Business Exit During the COVID-19 Pandemic: Non-Traditional Measures in Historical Context

Leland D. Crane   Ryan A. Decker   Aaron Flaaen
Adrian Hamins-Puertolas   Christopher Kurz

February 2022

Abstract

Lags in official data releases have forced economists and policymakers to leverage "alternative" or "non-traditional" data to measure business exit resulting from the COVID-19 pandemic. We first review official data on business exit in recent decades to place the alternative measures of exit within historical context. For the U.S., business exit is fairly common, with about 7.5 percent of firms exiting annually in recent years. The high level of exit is driven by very small firms and establishments. We then explore a range of alternative measures of business exit, including novel measures based on paycheck issuance and phone-tracking data, which indicate exit was elevated in certain sectors during the first year of the pandemic. That said, we find many industries have likely seen lower-than-usual exit rates, and exiting businesses do not appear to represent a large share of U.S. employment. As a result, exit appears lower than widespread expectations from early in the pandemic.

JEL codes: D22, E32, C55, C81

Keywords: Business exit, business death, COVID-19, big data, non-traditional data

All authors are from the Federal Reserve Board. First draft: October 2020. Tomaz Cajner provided critical support and feedback for this project. We thank Tim Dore, Nathan Goldschlag, John Haltiwanger, Traci Mach, Brendan Price, John Stevens, and seminar participants at the 2020 Banca d'Italia and FRB Joint Conference, the CAED 2020 seminar series, the Federal Reserve Board, the Boston Fed, the International Monetary Fund, the UNSW CAER seminar series, and the 2021 CRIW Summer Institute for helpful discussions. We thank Anbar Aizenman and Betsy Vrankovich for technical assistance and Rosemary Rhodes, Ashley Sexton, and Jacob Williams for research assistance. We thank Ray Sandza, Adam Liem, and Homebase for providing Homebase data and technical support. We thank SafeGraph for providing access to their data and Ryan Fox Squire in particular for answering questions related to the data. We thank ADP and Matt Levin, Ahu Yildirmaz, and Sinem Buber for access to and help with their payroll microdata. The analysis and conclusions set forth are those of the authors and do not indicate concurrence by other members of the research staff or the Board of Governors of the Federal Reserve System.

1 Introduction

Widespread business exit—death—resulting from the Pandemic Recession would have long-lasting consequences for the U.S. economy. Unfortunately, actual business exit is difficult to measure in real time since official statistics on business dynamics are released with substantial lags: Bureau of Labor Statistics (BLS) data on establishment deaths during the first year of the pandemic will become available in mid-2022, and Census Bureau data on firm deaths will likely not be public until 2023. As a result, economic commentators and policymakers have been relying on "alternative" or "non-traditional" data to measure business exit. For example, business electricity accounts show little imprint of the recent economic stress while vacancy rates for office and retail are reaching levels last seen during the Great Recession. Similarly, defaults jumped, but both "going out of business" search queries from Google and 30-day defaults returned to trend after brief elevation.[1] While the many indicators made available by private firms have improved understanding of recent economic developments, it is critical to be aware of historical patterns of business shutdown and how popular alternative indicators compare.

In this paper we review official data on business closures and deaths before the pandemic, providing a set of stylized facts that are necessary for evaluation of alternative indicators of business shutdown. We then evaluate a range of alternative indicators—including several new measures of exit we formulate—and discuss what they suggest about business exits during the first year of the COVID-19 pandemic.

Official data reveal business death to be a common occurrence, with about 7.5 percent of firms and 8.5 percent of establishments exiting in a typical year. Various measures of business closure—temporary and permanent—have been countercyclical in the past and rose notably during the Great Recession. Levels and cyclicality of business death are driven primarily by extremely small firms and establishments—those with fewer than 5 employees—though larger firms often permanently close individual establishments (locations) as part of geographic or industry restructuring. The historical facts we document are interesting independent of COVID-19 considerations.

Alternative indicators of exit during the pandemic's first year, on balance, suggest that exit has been elevated at least among small firms and establishments and particularly in the sectors most exposed to social distancing, though this elevated exit was partially offset by reduced exit in pandemic-friendly industries. A rough estimate is that the most troubled sector,

[1] We report these indicators in a prior working paper version of this paper, Crane et al. (2021).

other services (NAICS 81, which includes barber shops and nail salons), saw the permanent exit of more than 100,000 establishments *in excess of historically normal exit levels* during the 12 months of March 2020 through February 2021. Results for other sectors may have been more mixed; for example, within the leisure and hospitality sector, some businesses—like full-service restaurants—saw significantly elevated exit, while other businesses—such as those focused on outdoor recreation—saw exit rates similar to, or even below, those of previous years. The retail trade sector appears similar in that some industries—such as clothing stores—saw elevated exit, while others—such as grocery stores—saw below-normal exit. Our best non-traditional measures are more indicative of establishment than firm exit, though we do have some firm-based indicators with useful insights.

Taken together along with some prudent guesswork, our sector-level results suggest economywide *excess establishment exit*—that is, exit above and beyond pre-pandemic rates—was likely below 200,000 establishments during the first year of the pandemic, implying an exit rate about one-quarter to one-third above normal. This is roughly consistent with what we find from rough, preliminary estimates based on existing official Business Employment Dynamics (BED) data, which suggest roughly 185,000 excess establishment exits during the calendar year of 2020. We have less insight into *firm* exit, though given historical patterns 200,000 excess establishment exits would imply roughly 130,000 excess firm exits. Relative to popular discussion and early expectations, our results may represent an optimistic update to views about pandemic-related business failure. Throughout the paper, though, we emphasize the limitations of our non-traditional data.

We draw these inferences from a number of timely, high-frequency business exit indicators, some of which have been used in existing literature. Two key contributions in this respect, however, are the construction of employment-weighted shutdown indicators from ADP payroll data and a permanent business exit measure based on SafeGraph cell phone geolocation data. We also review more commonly used data on small business operations from Womply, Homebase, and the Census Bureau's Small Business Pulse Survey. As we show, these alternative measures are most useful in the context of historical patterns of business exit.

A key challenge is distinguishing between temporary shutdown and permanent shutdown (exit), since temporary shutdown was widespread in the early pandemic months (e.g., Cajner et al., 2020). U.S. statistical agencies provide data on business exits, but identifying exits is more difficult in alternative data sources. Typically what can be measured is whether a business is engaged in normal activities—e.g., receiving customer traffic, completing transactions, or paying workers. We use the term "shutdown" to refer broadly to businesses not engaged in normal activities, whether temporarily or permanently, and we attempt (loosely) to make guesses about actual exits based on how long businesses have been inactive. We use the terms "exit" and "death" interchangeably to refer to likely *permanent* shutdown.

A handful of papers study business closure early in the pandemic using, for example, new surveys (Bartik et al., 2020b) or official data on self-employment (Fairlie, 2020). Wang et al. (2020) and Greenwood et al. (2020) report that 2020 bankruptcy filings by small businesses through August were significantly *lower* than in prior years (though, importantly, *bankruptcy* is a different concept from exit). Bartik et al. (2020a) and Kurmann et al. (2021) measure business closures in Homebase data, and Chetty et al. (2020) measure early closures in Womply data. Hamilton (2020) uses Womply and Yelp data to estimate that as of July 2020, roughly 400,000 businesses had permanently closed during the pandemic; this number has been widely cited but is likely to be an overestimate in light of official and non-traditional data we review here, which were not available at the time Hamilton (2020) was written.

A number of studies by BLS researchers (Dalton et al., 2020a,b, 2021) track business closure in official business data; the authors use establishment microdata from the Current Employment Statistics (CES) along with establishment microdata with firm identifiers associated with the Quarterly Census of Employment and Wages (QCEW). These papers confirm that many establishment closures during April and May 2020 were temporary, though they note that closure rates stabilized somewhat by July. While the authors find that early establishment closure was far more elevated among small firms than among large firms, in late 2020 they observe an uptick in closures of establishments of firms with 250-500 employees; employment-weighted closures in this group remained historically elevated (by roughly 3 percentage points) into early 2021.[2]

We first provide general background on the importance of business exit, drawing from the literature and the unique aspects of the COVID-19 pandemic (Section 2). We explore historical patterns of business exit, summarizing them as a list of stylized facts, in Section 3. We review a range of official and non-traditional measures of business shutdown during 2020 in Section 4. We take stock and conclude in Section 5.

[2]See Dalton et al. (2021) for estimates through early 2021; see associated slides at `https://conference.nber.org/conf_papers/f152529.slides.pdf`. Employment-weighted closure rates for establishments of large (500+) firms also remained elevated, though only modestly.

2 Background on business exit

Exit, as part of a broader set of business dynamics patterns, generally enhances aggregate productivity as lower-productivity exiting businesses are replaced by higher-productivity entrants.[3] But in the pandemic environment, patterns of business exit may be driven by the geographic, industrial, and temporal onset of severe infection outbreaks and lockdowns rather than business productivity. As a result, many high-productivity businesses might fail during the COVID-19 episode, while some low-productivity businesses that otherwise would have exited may be saved by pandemic policies (see, e.g., Gournichas et al., 2021).

Outside of business exit situations, a large share of workers that face job separations return to their former employers ("recall hires;" see Fujita and Moscarini, 2017). Exit eliminates this recall option and, potentially, implies longer unemployment spells for workers. While the costs of exit-induced layoffs may be manageable during periods of strong labor markets, releasing workers onto labor markets at a time of high unemployment presents greater potential for long-term harm (Davis and von Wachter, 2011).

Business exit, particularly when it involves entire firms rather than single locations, also means the destruction of firm-specific forms of intangible capital—brand value and tacit knowledge about production or distribution—and costly reallocation of physical capital (Cooper and Haltiwanger, 2007). From the perspective of business owners, the exit of a firm means not only the loss of a job but also potentially the destruction of household wealth. And from the perspective of local economies, widespread business exits may alter the economic geography of neighborhoods and communities.

3 Historical patterns of business closure and death

Both the BLS and the Census Bureau publish official statistics on business closure and death. The BLS publishes *establishment* closure and death data through the quarterly BED product.[4] These data are based on the state and federal unemployment insurance data underlying the QCEW product, cover the near-universe of U.S. private nonfarm business

[3]See, e.g., Bartelsman et al. (2013), Decker et al. (2017), Decker et al. (2020), Foster et al. (2001), Foster et al. (2006), Foster et al. (2016), and Syverson (2011). For theoretical considerations see, e.g., Hopenhayn (1992) and Hopenhayn and Rogerson (1993). Some business exits occur as large firms restructure their activities across industry and geography, closing some establishments while opening others to better meet demand or to adjust to changing global supply chains (e.g., Davis et al., 2014; Fort et al., 2018).

[4]An "establishment" is a single business operating location (with few exceptions). A "firm" is a collection of one or more establishments under common ownership or operational control.

establishments with formal employees, and start in the early 1990s.

The BLS provides two measures of business shutdown: establishment "closures" are establishments that had positive employment in the third month of the previous quarter but zero employment (or no reported employment) in the third month of the current quarter, and establishment "deaths" are establishments that have been closed for four consecutive quarters.

Separately, the Census Bureau publishes both *firm* and *establishment* exit data through the annual Business Dynamics Statistics (BDS) product. Firms and establishments with positive employment in March of the previous year but no employment in March of the current year are counted as deaths. While the BDS only provides annual data, rather than quarterly as in the BED, the BDS has advantages of a longer time series (starting in the late 1970s, though we focus on post-1983 data) and ability to distinguish between firm and establishment deaths.

Figure 1 reports official data on business closures and deaths in recent decades. The top panel reports annual firm and establishment death rates from the BDS through 2018, with unweighted death rates (deaths as a share of establishments) on the left panel and employment-weighted death rates (employment at deaths as a share of employment) on the right panel. The bottom panel reports quarterly establishment closure and death rates from the BED (seasonally adjusted) through 2019, again with unweighted rates on the left and weighted rates on the right.[5]

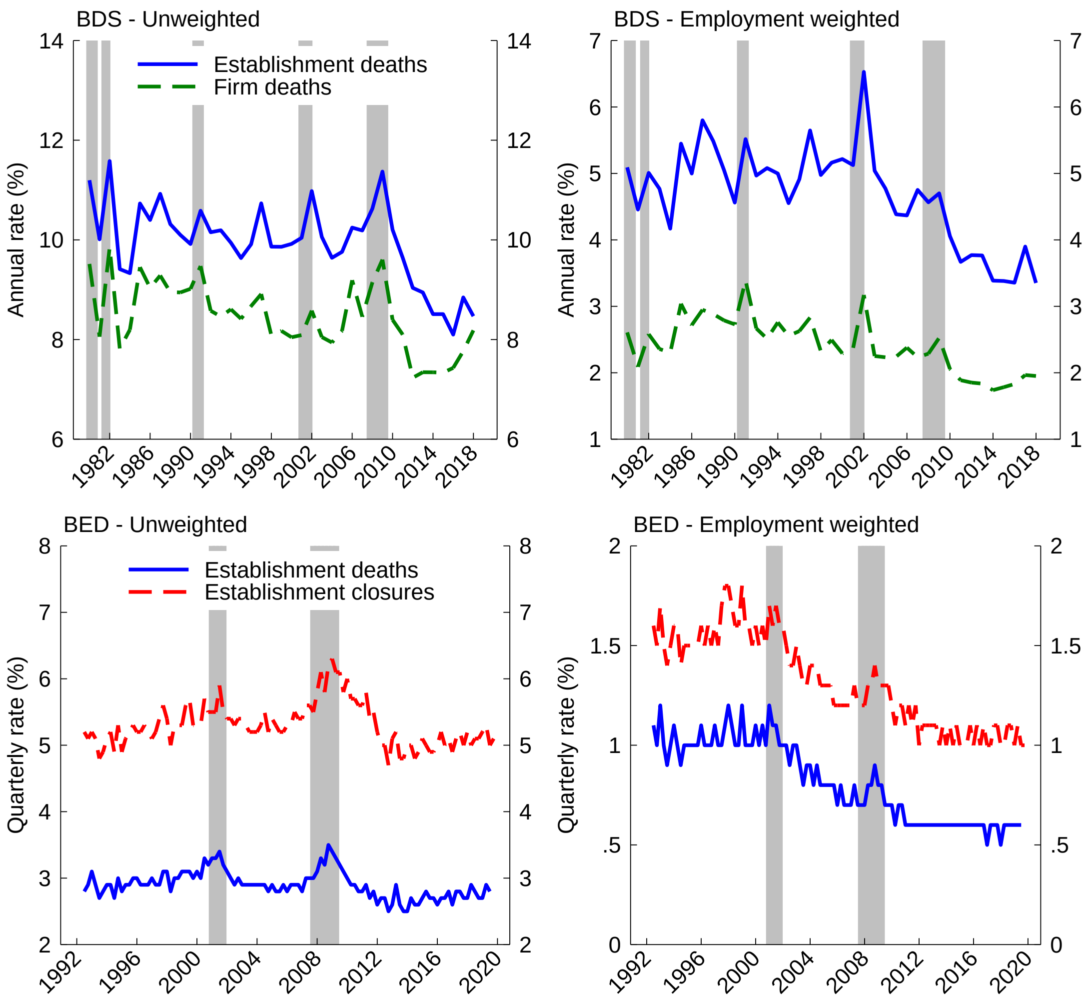
Figure 1 shows that business shutdown and death are common occurrences. In recent years (2015-2018), annual firm death rates have been around 7.5 percent of firms (about 400,000 per year), while establishment death rates have been around 8.5 percent at an annual frequency (about 600,000 per year) or just over 2.5 percent at a quarterly frequency.[6] A comparison of the left and right panels reveals that business death comprises a much smaller share of employment than of firms or establishments, implying that exit is concentrated among smaller businesses, as is exit cyclicality; we show this in more detail in Appendix A.[7]

Figure 1 also suggests that most measures of business shutdown are countercyclical, with particularly notable increases during the Great Recession (when firm exit rates rose by

[5]In all panels we use Davis et al. (1996) (DHS) denominators, where the current and previous quarters' or years' values are averaged (using longitudinally precise lag values where possible).

[6]Quarterly rates need not aggregate to annual rates due to short-lived establishments or potential discrepancies between BDS and BED data.

[7]We also note that exit rates are driven largely by younger firms. Among the smallest firms (those with fewer than five employees), those with age of zero saw exit rates above 20 percent in recent years, while firms of age ten or above saw exit rates well below 10 percent.



Note: DHS denominators. BDS data are noisy around Economic Census years (2's and 7's).
Establishments are single operating business locations.
Firms are collections of one or more establishments under common ownership or operational control.
Source: BLS Business Employment Dynamics (BED), Census Bureau Business Dynamics Statistics (BDS).

Figure 1: Historical patterns of business shutdown

roughly 1.5 percentage points from the previous expansion low). In Appendix A we report correlations and regressions demonstrating the countercyclicality of exit, with results that are broadly consistent with those of Tian (2018).

Finally, a comparison of quarterly deaths and quarterly closures in BED data indicates that temporary closure is common, affecting roughly 2 percent of establishments or about 0.5 percent of employment each quarter (the difference between the red and blue lines); this likely reflects some combination of typical seasonal business suspensions and temporary periods of business distress.

The above data suggest a set of stylized facts that must be kept in mind as alternative measures of business shutdown are examined:

- Annual *firm* exit rates have averaged around 7.5 percent in recent years—roughly 400,000 firms—or 2 percent of employment.
- Annual *establishment* exit rates have averaged around 8.5 percent in recent years—roughly 600,000 establishments—or 3.5 percent of employment.
- Quarterly establishment death rates have averaged about 2.5 percent, or about 0.5 percent of employment.
- Business exit is countercyclical; in particular, firm exit rates rose by about 1.5 percentage points in the Great Recession.
- The overall rate of business exit and the countercyclicality of exit are driven primarily by very small firms and establishments.
- Temporary business closure is common, affecting about 2 percent of establishments per quarter.

4 Has COVID-19 sparked a surge in business exit?

Official data on business exit are released with a lag: BED data on establishment *closures* are currently available through the second quarter of 2021, but BED *deaths* are only available through the third quarter of 2020. BDS data on *firm* deaths during 2020 will (presumably) not be available until late 2023.[8] In the meantime, we must rely on some guesswork along

[8] BED data are released with a lag of about two quarters, but deaths, by definition, are not observed until three quarters later. The BDS reports business activity as of March 12 of each year and is typically released

with non-traditional measures of business activity to assess the magnitude of business exit during the first year of the pandemic. We now describe several such measures.

4.1 ADP

We first describe business closure measures based on microdata from ADP, a provider of payroll processing services for businesses comprising about one-fifth of total private sector employment.[9] Key advantages of ADP data are their comprehensive coverage across business size and industry cells as well as the ability to observe both unweighted and employment-weighted business closure measures. A challenge—in the context of business shutdown—is that true shutdown cannot be distinguished from ADP client turnover, a limitation present in many non-traditional business microdata sources.

We observe paycheck issuance events at the business level, and we measure business shutdown based on the length of time a business goes without issuing pay. Since we have a long history of ADP data, we focus on comparing the 2020-2021 experience to the *average* experience from recent years (2015-2019) to abstract from typical patterns of ADP client turnover. We begin in mid-February 2020 and, for each week thereafter, we tally up the share of businesses that were operating in February 2020 but are in the midst of a shutdown period. We compare this share to the same-week average for the 2015-2019 period (i.e., the average for February cohorts of businesses starting in each of 2015-2019). For example, the 2019 cohort data extend from February 2019 through February 2020.

The top-left panel of Figure 2 shows the results for various shutdown durations. The blue line shows the share of businesses that are in a shutdown spell of at least 25 days, in 2020 relative to the 2015-2019 average for a given week. By late April of 2020, the share of businesses in a 25-day (or more) shutdown spell was nearly 12 percentage points higher than it was at the same time in past years. After that time, however, closed businesses reopened such that the share of businesses that were shut down returned to the historical pace by late August. The red line uses a more stringent criterion for measuring shutdowns, reporting the

with a lag of about two years (similar to the County Business Patterns product). Since much of the economic fallout from COVID-19 occurred after March 12, 2020, most pandemic-related exit will be observed in the BDS data for 2021.

[9] Cajner et al. (2018) describe ADP microdata in detail and document representativeness across business size and industry. Cajner et al. (2020) use ADP data to explore various dimensions of the early pandemic recession including business shutdowns and reopenings, temporary versus permanent job losses, and wage dynamics. Some ADP clients may process payroll at the establishment level, while others may process at the firm level or something in between; we follow Cajner et al. (2018) in treating ADP units as establishments. We apply sampling weights to ADP payroll units from the QCEW (with weights in terms of NAICS sector and establishment size as of March of 2020).

share of businesses that were in shutdown spells of at least 36 days (relative to the same measure in previous years). This ensures that businesses that pay on any pay frequency—weekly, biweekly, or monthly—do not count as spurious shutdowns. By late April 2020, the share of businesses that were in shutdown spells of at least 36 days exceeded historical patterns by more than 6 percentage points. The black line focuses on shutdown spells of 70 days or more.

In short, while business shutdown, including shutdown spells of more than two months, was elevated in the late spring of 2020, by the end of August we observe no evidence of excessive, ongoing business inactivity; in fact, shutdown was well *below* normal by late 2020. The historically low pace of shutdowns in late 2020 likely reflects, in large part, increased client retention during 2020, which has been noted in ADP earnings calls. This makes the data difficult to interpret; still, though, if permanent death were extremely elevated in 2020, it would likely be reflected in this large dataset. So the data, while not dispositive, are at least suggestive.

The top-right panel of Figure 2 shows the same concepts in an employment-weighted form. To calculate employment-weighted shutdowns in any given week, we identify businesses that meet a given shutdown criterion (25 days, 36 days, or 70 days) then calculate their employment share based on their February 15, 2020 employment as a share of total employment. Hence, the top-right panel of Figure 2 shows the share of February's employment that is associated with businesses that shut down in some weeks thereafter, in 2020 compared with the 2015-2019 average for that week. Employment-weighted shutdown also peaked in late April/early May 2020, when businesses inactive for at least 25 weeks accounted for a share of February employment that exceeded past years' share by about 5 percentage points. In employment terms, extremely long shutdown spells of 70 days or more were barely more common in mid-2020 than in past years; and by late August the share of employment attached to closed businesses was lower than average.

The differences between the top-left and the top-right panels of Figure 2 suggest that the elevated shutdown in spring 2020 relative to past years was driven largely by smaller units. We can see this more clearly in the bottom-left panel of Figure 2, which shows (unweighted) 70-day shutdown rates in 2020 relative to 2015-2019 averages, separated by business size. The black line shows that shutdown rates among the largest units—those with at least 500 employees—were similar to the pace of previous years for much of 2020 then dipped even lower by the end of the year. Smaller units saw significantly elevated shutdown rates in late April/early May 2020, but by August all business sizes saw shutdown rates well below

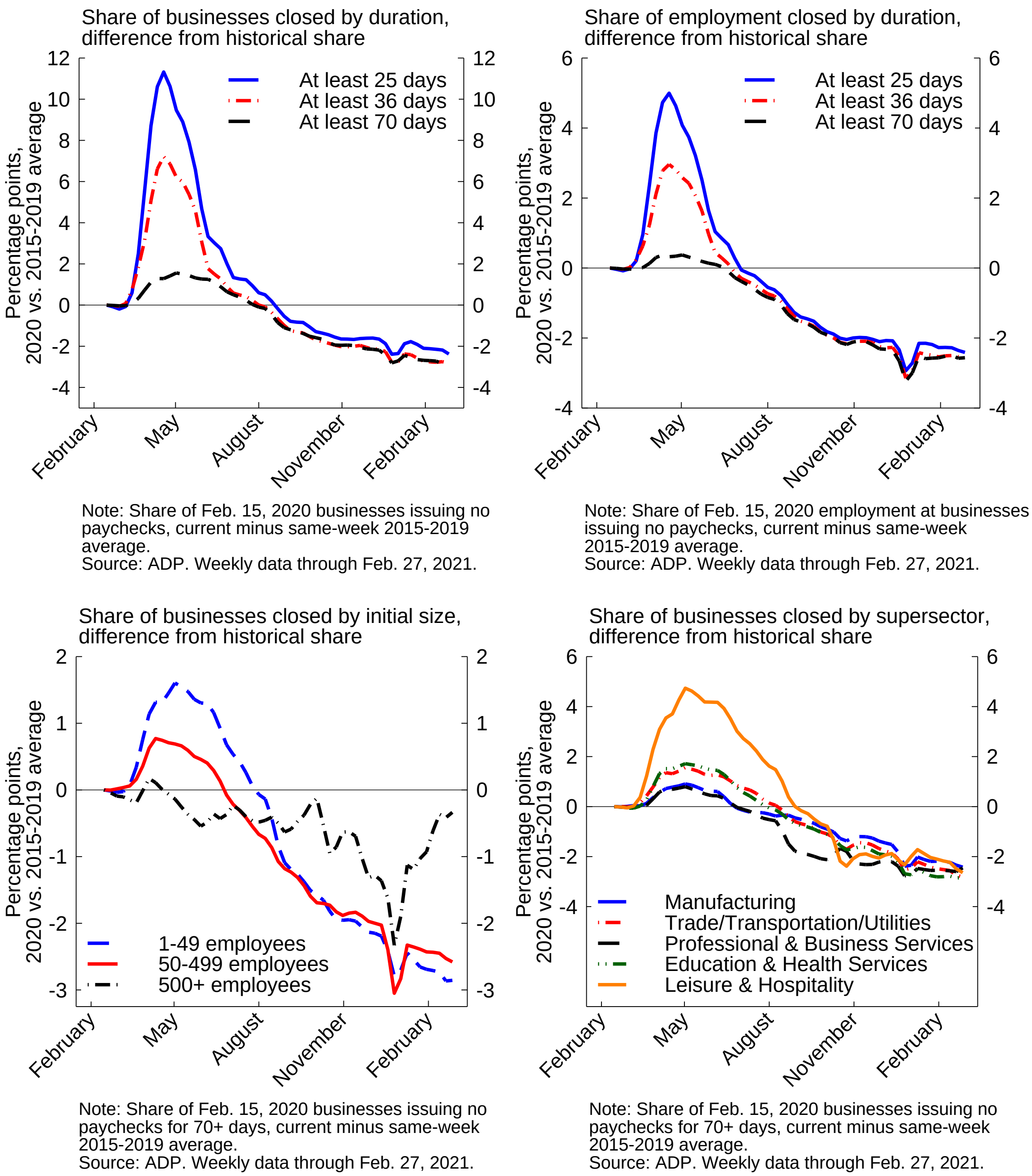


Figure 2: Measures of business closure from ADP payroll data (2020-2021 relative to 2015-2019 average)

historical patterns. The bottom-right panel shows that shutdown patterns do vary some across sectors; but while the mid-2020 cross-sector variation is intuitive, the shutdown rates show striking convergence by the end of the year.

Taken at face value, the ADP data suggest that business shutdown was elevated during the middle of the year, but on net these excess shutdowns were all temporary; since then, shutdown has moved well below historical rates, even among small units. This is a striking result, since the ADP data are reasonably comprehensive in terms of coverage across sectors and establishment size classes. If permanent business shutdown were accounting for a sizeable share of businesses and employment, we would expect to see evidence of it in the large sample of ADP clients. We again emphasize, however, that ADP data can be affected by patterns of client turnover in addition to true business shutdown, and elevated customer retention is readily apparent late in 2020.

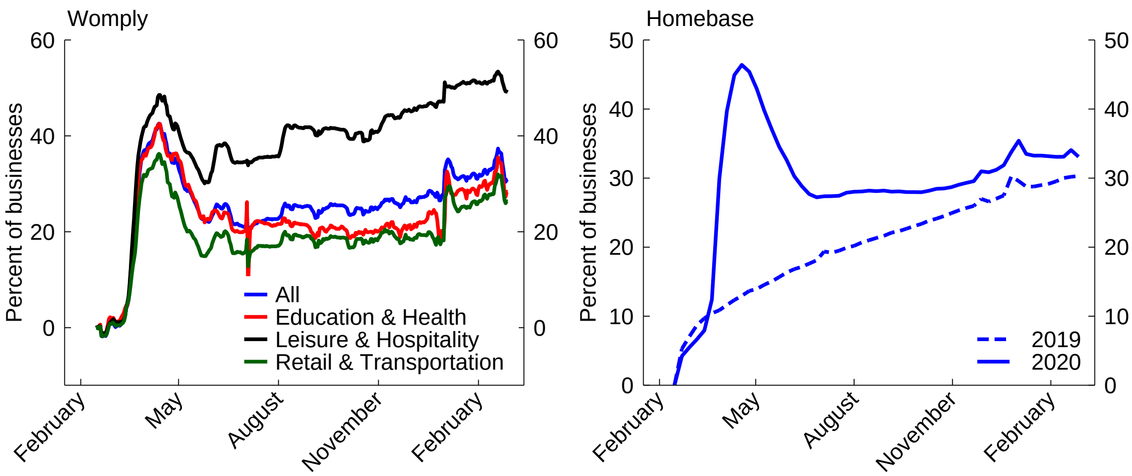
4.2 Small business trackers

We next turn to two popular measures of small business activity, shown on Figure 3. The left panel reports daily data from Womply, a credit card transaction processor, on the share of firms that have ceased processing point-of-sale transactions since mid-February.[10] The right panel reports weekly data from Homebase, a provider of clock-in/clock-out tracking software, showing the share of firms that have stopped reporting clock events since mid-February 2020 (and, conveniently, we can observe 2019 data for Homebase as well).[11] In both cases, the sample of businesses is restricted to those that were operating in February 2020 (or February 2019 for the 2019 Homebase line), abstracting from entry into the sample (consistent with our ADP-based exercises above). Importantly, presence in these datasets may be less costly than for ADP data; for example, many Homebase clients use a free tier of the service, which implies different selection considerations than may be present in ADP data.

[10] Womply is a credit card analytics firm that aggregates data on card transactions. Data reported by Womply reflect card transactions (or lack thereof) among small businesses as defined by the Small Business Administration (see Chetty et al. (2020) or https://www.womply.com/blog/data-dashboard-how-coronavirus-covid-19-is-impacting-local-business-revenue-across-the-u-s/). We follow Chetty et al. (2020) in treating Womply businesses as firms.

[11] Homebase provides clock-in/clock-out software for small businesses and can therefore observe employment activity in close to real time. As of early 2020, Homebase data included over 60,000 establishments with about 500,000 (hourly) employees. Coverage is concentrated among very small establishments (mostly those with fewer than 20 employees) in retail and service industries that happened to be particularly affected by social distancing. We aggregate Homebase establishment data to the firm level using their (anonymized) company identifier. See Kurmann et al. (2021) for extensive detail on Homebase representativeness, and see http://joinhomebase.com/data for more details on Homebase data.

These measures, which are focused on small *firms* in customer-facing industries, suggest that business shutdown rose sharply in March and April, but many closed businesses re-opened in May and June. Still, the recent observations indicate that shutdown was indeed elevated during the first year of the pandemic. Homebase data suggest that, as of the end of February 2021, shutdown in well-covered industries was elevated by roughly 3 percentage points relative to the same time a year earlier. Given the Homebase comparison to 2019, the absence of historical comparisons for Womply data warrants extreme caution in interpreting Womply shutdown rates.



Note: Share of Feb. 15, 2020 firms with no transactions.
Daily data through Feb. 28, 2021.
Source: Womply data from
https://www.tracktherecovery.org and Chetty et al. (2020).

Note: Share of Feb. 15, 2020 firms with no clock-ins.
Weekly data through week ending Feb. 27, 2021.
Source: Homebase.

Figure 3: Small consumer business closures

Small businesses have also been the subject of a number of surveys, most notably the Census Bureau's Small Business Pulse Survey. This survey is particularly interesting because it asks respondents about their exit *expectations*. We describe the survey and notable results in Appendix C. Most notably, for a time in mid-to-late 2020, small business exit expectations were elevated roughly one-quarter relative to historical actual exit rates.

Importantly, a limitation that is common to ADP, Womply, and Homebase data—as well as surveys like the Small Business Pulse Survey—is the possibility that exit patterns are driven by client or respondent attrition rather than business shutdown. Our comparisons to past-year patterns in ADP and Homebase data are designed to provide perspective on this; roughly speaking, the question is not whether we observe exit in these data but, rather, whether we see excess exit relative to historical patterns. We next turn to a measure that is not subject to the particular limitation of client or respondent attrition.

4.3 SafeGraph

SafeGraph is a company that aggregates anonymized location data from numerous mobile device applications to provide insights about physical places.[12] The company temporarily made their micro-level data available to researchers and government agencies studying the impact of COVID-19, and a number of papers have used the data in studying the effects of COVID-19 (e.g., de Vaan et al., 2021; Farboodi et al., 2021). The company links location data from roughly 45 million mobile devices to a registry of around 6 million points of interest nationwide to record, at a daily frequency, individual visits to these points of interest.[13]

In Appendix B we describe a methodology that identifies temporary closure and likely permanent establishment exit based on patterns of visits to business locations. These indicators should be considered as establishment, not firm, indicators since they are based on business operating locations.[14] This customer and worker *traffic*-based measure of business operation is distinct from the payroll- or revenue-based measures of ADP, Womply, and Homebase described above. Importantly, traffic-based measures of business shutdown are more useful in some industries than in others; for example, in construction, traffic patterns may not be useful as workers may report to various construction sites each day. Similarly, such measures applied to industries like landscaping services or food trucks would also be problematic. Generally speaking this methodology is appropriate for industries that rely on consumer (and worker) visits to businesses at a stationary location, a situation that applies to many retail and service businesses. It is convenient, though, that industries likely to be most sensitive to social distancing concerns are also those for which our traffic-based closure measure may be most appropriate.

We first illustrate these measures for full-service ("sit-down") restaurants (NAICS 722511), an industry that is well-suited to this methodology, has good coverage in SafeGraph data, and is sensitive to social distancing concerns and restrictions; note that this industry excludes fast food and takeout establishments (NAICS 722513).[15] The blue bars in Figure 4

[12] To enhance privacy, SafeGraph excludes census block group information if fewer than five devices visited an establishment in a month from a given census block group.

[13] While the exact universe of businesses covered by SafeGraph data is difficult to define, with 6 million points of interest it is likely that a large share of U.S. establishments are covered. The BLS QCEW data for March 2019 show about 10 million establishments in the U.S., which may be an overstatement of employer businesses since Census Bureau BDS data for 2018 show about 7 million establishments.

[14] The methodology outlined in Appendix B turns out to be similar in spirit to what is done in de Vaan et al. (2021) when studying the spillover effects of store closure.

[15] Our measure is reasonably robust to the notable shift of restaurants to carry-out service, which is evident in the SafeGraph data based on changes in the duration of consumer visits (see Appendix B Figure B1). Carry-out or delivery service still requires a visit to the business address.

show estimates of temporary restaurant closure during each month of 2020 and into early 2021, based on a calibrated drop in consumer visits relative to normal patterns for that establishment. This fraction was low, around 3-4 percent in the months before the pandemic, but then jumped to over 50 percent in the months of March and April as social distancing policies were put in place. The overall contour of the remaining months fits the pattern of impacts of COVID-19: some declines in temporary closure in the summer months followed by increases in November, 2020 to January, 2021 before declining once again.

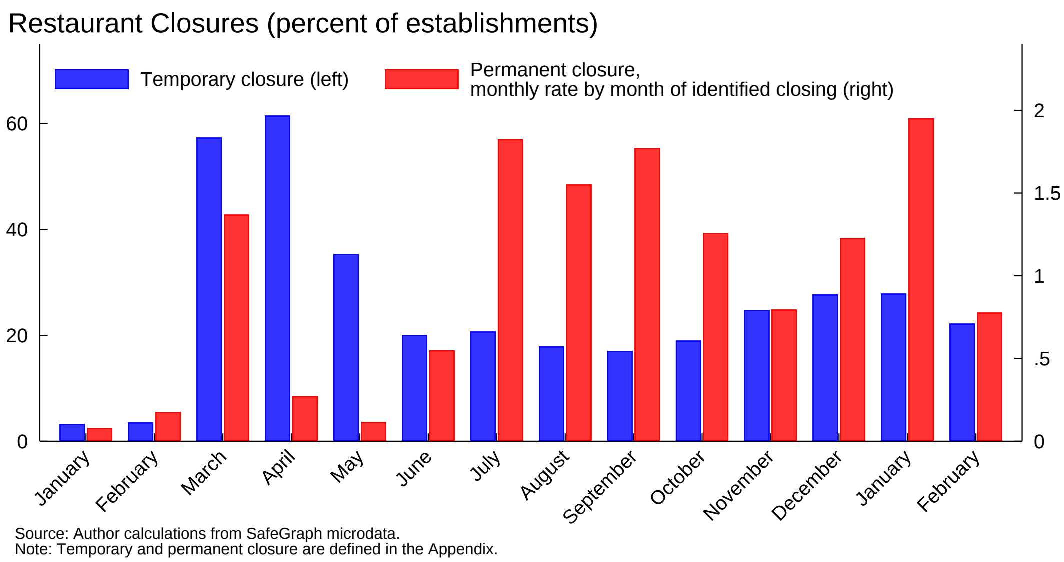


Figure 4: Measures of restaurant closure inferred from weekly visits

Estimates of the flow of restaurants that may have *permanently* closed each month are shown by the red bars in Figure 4; these are restaurants that closed during the indicated month and had still not reopened by February, 2021.[16] The monthly exit rate jumps in March 2020 to around 1.5 percent of restaurants and then increases again in July and August. The cumulative 12-month exit rate of restaurants, from March 2020 to February 2021, based on this measure is 13.5 percent, about 70 percent above rates seen in official data in recent years (around 8 percent). The data therefore suggest that establishment exit has been substantially elevated in the full-service restaurant industry.

We also calculate cumulative (March 2020 to February 2021) permanent closure rates

[16]The monthly estimates for the short benchmark period of January-February of 2020 (0.1 to 0.2 percent) are below the average monthly rate of exit for restaurants from BDS data (0.7 percent), so our SafeGraph measure might understate exit somewhat in other periods. BDS data for NAICS 7225—a broader category than the six-digit industry studied here—indicate recent (2015-2018) annual establishment death rates of 8.1 percent, implying monthly rates of a bit under 0.7 percent.

Table 1: Excess exit estimates for selected industries, SafeGraph

Industry (NAICS)	Initial establishments (QCEW, 1000s)	Historical rate (BDS)	Pandemic rate	Excess rate	Excess exits (1000s)
Broad sectors					
Retail trade	1,048	7.6	8.6	1.0	11
Arts, Entertainment, & recreation	165	10.1	7.6	-2.5	-4
Accommodation & food services	737	8.5	9.7	1.2	9
Other services	875	6.0	20.1	14.1	123
Four-digit industries					
Furniture stores	22	7.2	10.2	3.0	1
Home furnishings stores	25	7.2	11.4	4.3	1
Electronics & appliance stores	44	14.1	12.1	-2.0	-1
Building material & supplies dealers	53	4.7	5.3	0.6	0
Grocery stores	90	8.4	7.2	-1.2	-1
Clothing stores	82	8.6	16.0	7.3	6
Museums, historical sites, etc.	9	3.8	6.1	2.4	0
Other amusement & recreation	85	9.3	8.3	-1.0	-1
Traveler accommodation	64	7.5	8.8	1.3	1
Restaurants & other eating places	578	8.1	9.3	1.2	7
Automotive repair & maintenance	165	7.8	16.7	8.9	15
Personal care services	135	10.4	22.7	12.3	17

Note: Industries listed in NAICS order. Initial establishment counts from QCEW, 2020q1. Historical rate is average the establishment exit rate for 2015-2018 from BDS. Pandemic rate is the SafeGraph-based exit rate estimate for the 12 months of March 2020-February 2021. Excess rate is the difference between pandemic and historical rates. Excess deaths are equal to excess rate multiplied by establishment count.

using the SafeGraph-based measure for a number of other industries and compare the experience of the first 12 months of the COVID-19 pandemic to historical exit rates in official data. Table 1 reports the establishment count, the average 2015-2018 establishment exit rate from the BDS, the SafeGraph-based exit rate for March 2020-February 2021, the implied excess exit rate, and the implied count of establishment deaths in excess of historical norms. The industry-level variation in excess business closure largely aligns with intuition, as shown in the Excess Rate column of Table 1. The *Broad sectors* portion of the table shows these comparisons for select 2-digit NAICS sectors, while the bottom portion shows select 4-digit industry-group detail. The highest rates of excess closure are in industries relying heavily on in-person contact (other services, such as personal care services), experiencing COVID-related reductions in demand (automotive repair), or that are close substitutes with e-commerce (clothing stores). Some industries with lower-than-typical closure rates include grocery stores and electronics and appliance stores, two industries that have seen increased demand during the pandemic.

A few other industry results are worth discussing. The excess closure rate for accommodation and food services (and traveler accommodation in particular) is lower than might be expected given the large drops in travel and restaurant dining induced by the pandemic (and the elevated exit we observe among full-service restaurants mentioned above). But anecdotes suggest that certain areas of this sector have been resilient to the pandemic; indeed, the aggregated industry group of restaurants and other eating places (NAICS 7225) saw exit rates only modestly above historical averages, suggesting that the high rate we document for full-service restaurants was partially offset by low exit rates among limited-service restaurants like pizza delivery, fast food, and takeout. More broadly, the Paycheck Protection Program (PPP) targeted the accommodation and food services sector for special treatment, allowing virtually the entire sector to qualify rather than just small firms, potentially facilitating continued operation despite large declines in revenue.[17]

The low excess closure rate for "arts, entertainment, and recreation" is also striking, though this industry is a mix between establishments that specialize in pandemic-friendly outdoor recreation (i.e., golf courses, skiing facilities) and those that specialize in riskier indoor recreation (i.e., museums, fitness centers, and bowling alleys). The retail trade sector likely saw considerable heterogeneity in exit rates, combining high-exit industries like clothing stores with low-exit industries like grocery stores and (likely) nonstore retail. We expect exit rates to have been lower in the broad sectors not reported here, with the possible exceptions of mining (due to oil market developments) and, perhaps, education.

The implications of our SafeGraph-based estimates can be found in the last column of Table 1. Our SafeGraph-based estimates suggest excess exit of about 123,000 establishments in other services, with negligible figures in other sectors. Since we expect other sectors that are less well measured by SafeGraph to have fared better than the social distance-sensitive sectors we report here, excess exit for the economy as a whole was likely not far off from the figure for the worst-off sector of other services. That is, these data suggest that excess exit was likely below (say) 200,000 establishments during the first year of the pandemic. Of course, this is a guess in which we assume that sectors other than other services saw relatively low excess exit; if our guess is reasonable, it would imply that overall exit has been elevated by about one-third, as the average annual number of establishment exits during 2015-2018 was about 600,000 in BDS data.[18]

[17] While the PPP was generally only available to small *firms*, for the accommodation and food services sector the size qualification was based on *establishment* size. Decker et al. (2021) find that more than 99 percent of establishments in that sector met the size qualification.

[18] Our mixing of BLS and Census Bureau data on Table 1 is not without consequence and is its own source

In summary, the measure inferred from mobile device location data—which is not subject to the customer attrition concerns of our previously described data sources—appears to show elevated exit rates for restaurants and other industries that align with the effects from COVID-19. However, this unconventional measure of business exit should be treated with caution as there remain several unique features of our measurement that may not align with true business exit.

4.4 Comparing our estimates to early official data

While official data on firm and establishment exits are not yet available for the full time period we study, we can use existing BED data on establishment closures, openings, and births for the full calendar year of 2020 to assess the sensibility of our estimates from non-traditional data. In the BED, establishment closures in 2020 exceeded their 2019 pace, but openings were also elevated. Moreover, by comparing openings with births, we can quantify *re*openings of previously closed establishments and thereby estimate the number of closed establishments that have not reopened.

In the BED data, cumulative closures during the four quarters of 2020 totaled 2,176,000, while cumulative reopenings during the second quarter of 2020 through the first quarter of 2021 totaled 1,116,000. The difference between these figures—1,060,000—is a reasonable estimate of establishment exits throughout 2020. The average annual exit pace of 2015-2019 in BED data was 875,000, suggesting that 2020 saw excess exits of roughly 185,000, remarkably consistent with our estimate above (though covering a slightly different time period).[19]

Data on *actual* establishment exits are currently available through the third quarter of 2020; the annualized pace of actual exits for the first three quarters of 2020 is 1,117,000, which would imply an excess exit rate close to 242,000. This is higher than, but still reasonably consistent with, our estimate based on non-traditional data; moreover, *closures* declined after the third quarter, so we might expect actual exits to have been lower in that as-yet unmeasured quarter as well.

of uncertainty since the BLS reports about 35 percent more private establishments than the Census Bureau. Scaling down the establishment counts from Table 1 using a 1.35 ratio suggests an upper bound estimate of 150,000 excess establishment exits, which would imply overall exit was elevated by about one-fourth.

[19] This method of exit estimation does require strong assumptions about the timing of closings and re-openings, but applying the method to 2019 produces an exit estimate of 929,000 for the full year, compared with actual exits of 928,000. The method's root mean squared error for the 1993-2019 period is just 10,000 establishments. Applying the method to the 2020:Q2-2021:Q1 period (instead of the calendar year of 2020) yields 983,000 estimated exits, implying even fewer excess exits.

We emphasize that our non-traditional data-based exit estimates for the first full year of the pandemic were computable in March of 2021, at which time BED data on establishment closures were only available through the third quarter of 2020, with no death data for 2020 at all, such that the above BED-based estimates were still infeasible for a few quarters yet.

5 Taking stock

Our analysis suggests that business exit likely was elevated during the first year of the pandemic. We find direct evidence of increased *establishment* deaths among full-service restaurants, personal care services, automotive repair, and certain retailers (apparent in SafeGraph data) and suggestive direct evidence for elevated deaths among small *firms* in related industries generally (apparent in Womply and Homebase data). But within sectors, variation across industries appears to be partially or fully offsetting, such that most sectors likely did not see dramatically elevated exit; the primary exception is other services, where we estimate that establishment exits exceed prior years' pace by about 120,000. From this estimate, and considering our relatively modest estimates for other sectors, we judgmentally estimate that excess establishment exits were below 200,000 during the first year of the pandemic.[20] Separately, our ADP data suggest that any rise in business exits has not reached larger business units or even enough smaller units to account for a material share of employment, which would be consistent with past patterns of employment-weighted exit cyclicality. This result may be somewhat too optimistic, however, as Dalton et al. (2021) find employment-weighted exit to be materially elevated among certain firm size classes.

Excess establishment exits below 200,000 during the first year of the pandemic—and excess firm exits below 150,000, if historical shares continued—with little associated excess job destruction would likely be a positive outcome relative to widespread expectations from early in the pandemic. Some of the detrimental consequences of elevated exit—permanent job dislocation, potential productivity impacts if selection works adversely, and the destruction of intangible and physical capital—may be modest in aggregate if exit does not reach large firms or a greater number of small firms. Other detrimental consequences—loss of job and wealth for business owners and abrupt changes to local economic geography—have welfare

[20]Our results seem roughly consistent with the Census Bureau's weekly, pandemic-inspired Small Business Pulse Survey business expectations during the pandemic; see Appendix C. Separately, early evidence suggests business entry surged during the first year of the pandemic (Dinlersoz et al., 2021). Combined with our evidence of lower-than-expected exit, it may be that the effect of the pandemic on the net change in establishment or firm numbers will turn out to be modest.

implications even if exit is not substantial on an activity-weighted basis.

Given various dataset-specific caveats mentioned in the main text, we view our evidence as suggestive, not conclusive. Early and ongoing policy actions may have helped businesses in the relatively optimistic sectors to survive the worst of the pandemic and sustainably reopen.[21] Alternatively, it may be that our imperfect indicators understate the magnitude of the surge in exit. Ultimately, we will not have certainty about business exit during 2020-2021 until high-quality official data are published. In this respect, our work highlights the importance of timely data production and underscores of the value of official data producers.

[21] While it is still too early for clear results, several studies find evidence of material positive effects of the Paycheck Protection Program (PPP); see, for example, Autor et al. (2020), Doniger and Kay (2021), and Kurmann et al. (2021).

References

Autor, David, David Cho, Leland D. Crane, Mita Goldar, Byron Lutz, Joshua Montes, William B. Peterman, David Ratner, Daniel Villar, and Ahu Yildirmaz, "An Evaluation of the Paycheck Protection Program Using Administrative Payroll Microdata," Working paper 2020.

Bartelsman, Eric, John Haltiwanger, and Stefano Scarpetta, "Cross-Country Differences in Productivity: The Role of Allocation and Selection," *American Economic Review*, 2013, *103* (1), 305–334.

Bartik, Alexander W., Marianne Bertrand, Feng Lin, Jesse Rothstein, and Matt Unrath, "Measuring the Labor Market at the Onset of the COVID-19 Crisis," *BPEA Conference Draft, Summer*, 2020.

Bartik, Alexander W, Marianne Bertrand, Zoë B Cullen, Edward L Glaeser, Michael Luca, and Christopher T Stanton, "The Impact of COVID-19 on Small Business Outcomes and Expectations," *Proceedings of the National Academy of Sciences*, 2020, *117* (30), 17656–17666.

Buffington, Catherine, Carrie Dennis, Emin Dinlersoz, Lucia Foster, and Shawn Klimek, "Measuring the Effect of COVID-19 on U.S. Small Businesses: The Small Business Pulse Survey," CES Working Paper CES-20-16, U.S. Census Bureau: Center for Economic Studies 2020.

Cajner, Tomaz, Leland Crane, Ryan A. Decker, Adrian Hamins-Puertolas, Christopher Kurz, and Tyler Radler, "Using Payroll Processor Microdata to Measure Aggregate Labor Market Activity," FEDS Working Paper 2020-089, Board of Governors of the Federal Reserve System (U.S.) April 2018.

_ , Leland D. Crane, Ryan A. Decker, John Grigsby, Adrian Hamins-Puertolas, Erik Hurst, Christopher Kurz, and Ahu Yildirmaz, "The U.S. Labor Market during the Beginning of the Pandemic Recession," *BPEA Conference Draft, Summer*, 2020.

Chetty, Raj, John N. Friedman, Nathaniel Hendren, Michael Stepner, and the Opportunity Insights Team, "How Did COVID-19 and Stabilization Policies Affect Spending and Employment? A New Real Time Economic Tracker Based on Private Sector Data," Working Paper June 2020.

Clementi, Gian Luca and Berardino Palazzo, "Entry, Exit, Firm Dynamics, and Aggregate Fluctuations," *American Economic Journal: Macroeconomics*, 2016, *8* (3), 1–41.

Cooper, Russell W. and John C. Haltiwanger, "On the Nature of Capital Adjustment Costs," *The Review of Economic Studies*, 2007, *73* (3), 611–633.

Crane, Leland D., Ryan A. Decker, Aaron Flaaen, Adrian Hamins-Puertoilas, and Christopher Kurz, "Business Exit During the COVID-19 Pandemic: Non-Traditional Measures in Historical Context," FEDS Working Paper 2018-005, Board of Governors of the Federal Reserve System (U.S.) January 2021.

Dalton, Michael, Elizabeth Weber Handwerker, and Mark A. Loewenstein, "Employment Changes by Employer Size Using the Current Employment Statistics Survey Microdata during the COVID-19 Panedmic," *Covid Economics*, September 2020, *1* (46), 123–136.

_ , _ , and _ , "An Update on Employment Changes by Employer Size during the COVID-19 Pandemic: A Look at the Current Employment Statistics Microdata," *BLS Working Papers*, December 2020, (532).

_ , Jeffrey Groen, Elizabeth Weber Handwerker, and Mark A. Loewenstein, "Analyzing Near-Current Employment Changes by Employer Characteristics using the Quarterly Census of Employment and Wages and Current Employment Statistics," Technical Report July 2021.

Davis, Steven J. and Till von Wachter, "Recessions and the Costs of Job Loss," *Brookings Papers on Economic Activity*, 2011, *Fall.*

Davis, Steven J, John C Haltiwanger, and Scott Schuh, *Job Creation and Destruction*, The MIT Press, 1996.

Davis, Steven J., John Haltiwanger, Kyle Handley, Ron Jarmin, Josh Lerner, and Javier Miranda, "Private Equity, Jobs, and Productivity," *American Economic Association*, 2014, *104* (12), 2956–3990.

de Vaan, Mathijs, Saqib Mumtaz, Abhishek Nagaraj, and Sameer B. Srivastava, "Social Learning in the COVID-19 Pandemic: Community Establishments' Closure Decisions Follow Those of Nearby Chain Establishments," *Management Science*, 2021, *67* (7), 4446–4454.

Decker, Ryan A., John Haltiwanger, Ron S. Jarmin, and Jarmin Miranda, "Declining Dynamism, Allocative Efficiency, and the Productivity Slowdown," *American Economic Review: Papers & Proceedings*, 2017, *107* (5), 322–326.

_ , _ , _ , and Javier Miranda, "Changing Business Dynamism and Productivity: Shocks versus Responsiveness," *American Economic Review*, 2020, *110* (12), 835–850.

_ , Pablo N. D'erasmo, and Hernan Moscoso Boedo, "Market Exposure and Endogenous Firm Volatility over the Business Cycle," *American Economic Journal: Macroeconomics*, 2016, *8* (1), 148–198.

Decker, Ryan, Robert Kurtzman, Byron Lutz, and Christopher Nekarda, "Across the Universe: Policy Support for Employment and Revenue in the Pandemic Recession," *American Economic Association: Papers and Proceedings*, May 2021.

Dinlersoz, Emin, Timothy Dunne, John Haltiwanger, and Veronika Penciakova, "Business Formation: A Tale of Two Recessions," *American Economic Association: Papers and Proceedings*, May 2021.

Doniger, Cynthia L. and Benjamin Kay, "Ten Days Late and Billions of Dollars Short: The Employment Effects of Delays in Paycheck Protection Program Financing," FEDS Working Paper 2021-003, Board of Governors of the Federal Reserve System (U.S.) January 2021.

Fairlie, Robert, "The impact of COVID-19 on small business owners: Evidence from the first three months after widespread social-distancing restrictions," *Journal of Economics & Management Strategy*, 2020, *29* (4), 727–740.

Farboodi, Maryam, Gregor Jarosch, and Robert Shimer, "Internal and external effects of social distancing in a pandemic," *Journal of Economic Theory*, 2021, *196*, 105293.

Fort, Teresa C., Justin R. Pierce, and Peter K. Schott, "New Perspectives on the Decline in US Manufacturing Employment," *Journal of Economic Perspectives*, 2018, *32* (2), 47–72.

Foster, Lucia, Cheryl Grim, and John Haltiwanger, "Reallocation in the Great Recession: Cleansing or Not?," *Journal of Labor Economics*, 2016, *34* (S1), S293–S331.

— **, John C. Haltiwanger, and C. J. Krizan**, "Aggregate Productivity Growth: Lessons from Microeconomic Evidence," in "New Developments in Productivity Analysis," University of Chicago Press, January 2001, pp. 303–372.

— **, John Haltiwanger, and C. J. Krizan**, "Market Selection, Reallocation, and Restructuring in the U.S. Retail Trade Sector in the 1990s," *The Review of Economics and Statistics*, 2006, *88* (4), 748–758.

Fujita, Shigeru and Giuseppe Moscarini, "Recall and Unemployment," *American Economic Review*, December 2017, *107* (12), 3875–3916.

Gournichas, Pierre Olivier, Sebnem Kalemli-Ozcan, Veronika Penciakova, and Nick Sander, "COVID-19 and SMEs: A 2021 "Time Bomb"?," *American Economic Association: Papers and Proceedings*, May 2021.

Greenwood, Robin, Ben Iverson, and David Thesmar, "Sizing Up Corporate Restructuring in the COVID Crisis," *BPEA Conference Draft, Fall*, 2020.

Hamilton, James, "Why You Shoud Never Use the Hodrick-Prescott Filter," *Review of Economics and Statistics*, December 2018, *100*, 831–843.

Hamilton, Steven, "From Survival to Revival: How to Help Small Businesses through the COVID-19 Crisis," *The Hamilton Project Policy Proposal*, 2020, (2020-14).

Hodrick, Robert J. and Edward C. Prescott, "Postwar U.S. Business Cycles: An Empirical Investigation," *Journal of Money, Credit, and Banking*, 1997, *29* (1), 1–16.

Hopenhayn, Hugo A., "Exit, Selection, and the Value of Firms," *Journal of Economic Dynamics and Control*, 1992, *16* (3-4), 621–653.

— **and Richard Rogerson**, "Job Turnover and Policy Evaluation: A General Equilibrium Analysis," *Journal of Political Economy*, 1993, *101* (5), 915–938.

Kurmann, Andre, Etienne Lale, and Lien Ta, "The Impact of COVID-19 on U.S. Employment and Hours: Real-Time Estimates with Homebase Data," mimeo 2021.

Lee, Yoonsoo and Toshihiko Mukoyama, "Entry and Exit of Manufacturing Plants over the Business Cycle," *European Economic Review*, 2015, *77*, 20–27.

Syverson, Chad, "What Determines Productivity?," *Journal of Economic Literature*, 2011, *49* (2), 326–365.

Tian, Can, "Firm-level Entry and Exit Dynamics over the Business Cycle," *European Economic Review*, 2018, *102*, 298–326.

Wang, Jialan, Jeyul Yang, Benjamin Iverson, and Raymond Kluender, "Bankruptcy and the COVID-19 Crisis," Working Paper, University of Illinois at Urbana-Champaign 2020.

Online appendix, not for publication

A Appendix for historical results

A.1 Business cycle correlations

We can observe the countercyclicality of business shutdown with some simple correlations, reported on Table A1. We compare our firm and establishment shutdown measures—detrended with linear trends—with the change in the unemployment rate and real GDP growth (at quarterly frequency for comparisons with BED measures and annual frequency for comparisons with BDS measures).[22]

Table A1: Business cycle correlations

	Unemployment		GDP	
	Unweighted	Weighted	Unweighted	Weighted
BED: Establishment closures (BLS)	.34	.23	-.40	-.13
BED: Establishment deaths (BLS)	.36	.31	-.44	-.21
BDS: Establishment deaths (Census)	.64	.37	-.46	-.16
BDS: Firm deaths (Census)	.41	.31	-.32	-.18

Note: Exit rates detrended linearly. BED correlated with quarterly change in unemployment rate or change in log real GDP. BDS correlated with annual change in unemployment rate or change in log real GDP on BDS annual timing (April-March). BED data cover 1992q3-2019q1 (deaths) or 1992q3-2019q4 (closures). BDS data cover 1984-2018 (trends estimated on full 1978-2018 sample).

These simple correlations are consistent with countercyclical shutdown and exit (i.e., positive correlations with the change in unemployment and negative correlations with GDP growth). Employment-weighted establishment shutdowns are less countercyclical than unweighted establishment shutdowns (implying that smaller units drive exit cyclicality), and firm death is less countercyclical than establishment death.[23] The specifications in Table A1 show consistent countercyclicality of both establishments and firms; in unreported results we find that these are somewhat sensitive to inclusion of the 1980-1983 period, a volatile period

[22]We first take the average of the unemployment rate for the period; that is, we calculate the quarterly average of monthly data for BED comparisons, and we calculate the annual average of monthly data for BDS comparisons (where the BDS year t is defined as April $t-1$ through March t). We then take the difference in these quarterly or annual averages. Correlations with the level of unemployment rates (rather than the change) would be difficult to interpret given persistence of unemployment rates in the aftermath of recessions. For GDP correlations, we calculate the change in the log of real GDP at quarterly frequency for BED comparisons or the change in the log of annual GDP (on BDS timing) for BDS comparisons, where annual GDP is the average of the quarterly GDP level for the year.

[23]Correlations of the *level* of establishment deaths or firm deaths (rather than death *rates*) with the change in unemployment also indicate countercyclical exit, as we will show below on Table A5. Separately, as can be seen from Figure 1, BDS data show notable spikes in exit in 2002 and other years ending in 2 or 7, suggesting there may be data challenges created by semi-decadal Economic Censuses. The previous vintage of BDS data displayed somewhat different patterns; we analyze Census year and vintage issues further below.

for exit measures in which firm exit is less countercyclical.[24] Further below, we explore sensitivity to detrending specifications as well as correlations with industrial production instead of GDP.

The time series correlations above are suggestive but limited. We can gain more business cycle variation using state-level data. Here we simplify by focusing on annual BDS data during the post-2002 period, thereby avoiding the need to detrend the exit series (and also avoiding the potentially problematic 2002 observation, discussed further below). Table A2 reports results from regressions of exit rates on the annual change in unemployment with state fixed effects (such that we study within-state business cycle fluctuations).

Table A2: Business cycle comovement: States

	Establishment death		Firm death	
	Unweighted	Weighted	Unweighted	Weighted
Change in unemp	0.56***	0.22***	0.37***	0.11***
	(0.03)	(0.02)	(0.02)	(0.01)
Observations	816	816	816	816
State FE	Yes	Yes	Yes	Yes
Year FE	No	No	No	No

Note: Regression of annual exit rates on annual change in unemployment rates, 2003-2018. Unemployment rate changes timed to correspond with BDS annual timing (April-March).
***denotes statistical significance with $p < 0.01$.
Source: Author calculations from Business Dynamics Statistics and BLS unemployment data.

Business cycle correlations are substantial within states, with unweighted exit rates being more cyclical than weighted rates for both establishments and firms (i.e., smaller units drive the cyclicality) and firm exit being less cyclical than establishment exit.[25] For example, the interpretation of the first coefficient is that a one percentage point rise in the state unemployment rate is associated with a 0.56 percentage point increase in the establishment exit rate. Taken together with Table A1, the data consistently show that exit is countercyclical, particularly in recent years.[26]

[24]We intentionally begin the BDS-based sample in 1984 for Table A1 and all other results unless otherwise noted. Firm countercyclicality is highly sensitive to specific years included in the 1980-1983 period. In Table A10 below we report correlations for the entire 1978-2018 period covered by the current BDS vintage, in which firm exit countercyclicality is nevertheless apparent.

[25]The 2003-2018 period included in the Table A2 estimates includes three Economic Census years (2007, 2012, and 2017), but the coefficients are little changed if Census years are omitted (see Table A9 and discussion below). Separately, coefficients are similar in regressions without state fixed effects or with both state and year fixed effects (see Table A11 and discussion below).

[26]See Clementi and Palazzo (2016) for a model-based exploration of exit countercyclicality. In addition,

Important aspects of the cyclical properties of exit—as well as entry—are documented by Tian (2018) for the 1979-2013 period. That paper's results for the change in GDP and change in unemployment as cyclical indicators are broadly consistent with ours, as are the results for firm and establishment size; importantly, though, Tian (2018) finds more ambiguous results for the level of GDP.[27] Even in GDP levels, however, Tian (2018) still finds countercyclicality for the industrial, construction, transportation, and wholesale sectors, with acyclicality or procyclicality for retail trade, finance, insurance, and real estate, and services. We find somewhat mixed results for GDP levels that can depend on the detrending methodology, as discussed further below. We view our results as complementary to Tian (2018), adding additional specifications as well as state-level variation.

A.2 Exit by firm and establishment size

As noted above, the differences between unweighted and weighted exit rates suggest that business exit is concentrated among smaller units, in terms of both overall exit rates and the cyclicality of exit. This is made clear by Figure A1, which plots annual establishment death rates (left panels) and firm death rates (right panel), reported by both firm (top panels) and establishment (bottom panel) size categories.

The smallest *firms*—those with fewer than 5 employees—have the highest exit rates, with typical rates markedly above any other firm size classes.[28] Firm death rates decline monotonically with firm size. Establishment death rates are likewise highest among the smallest firms, but the largest firms—–those with 500 or more employees—–have the second-highest establishment death rates; intuitively, large multi-unit firms may close many establishments per year (and potentially open many others) as part of geographic or industrial restructuring.[29]

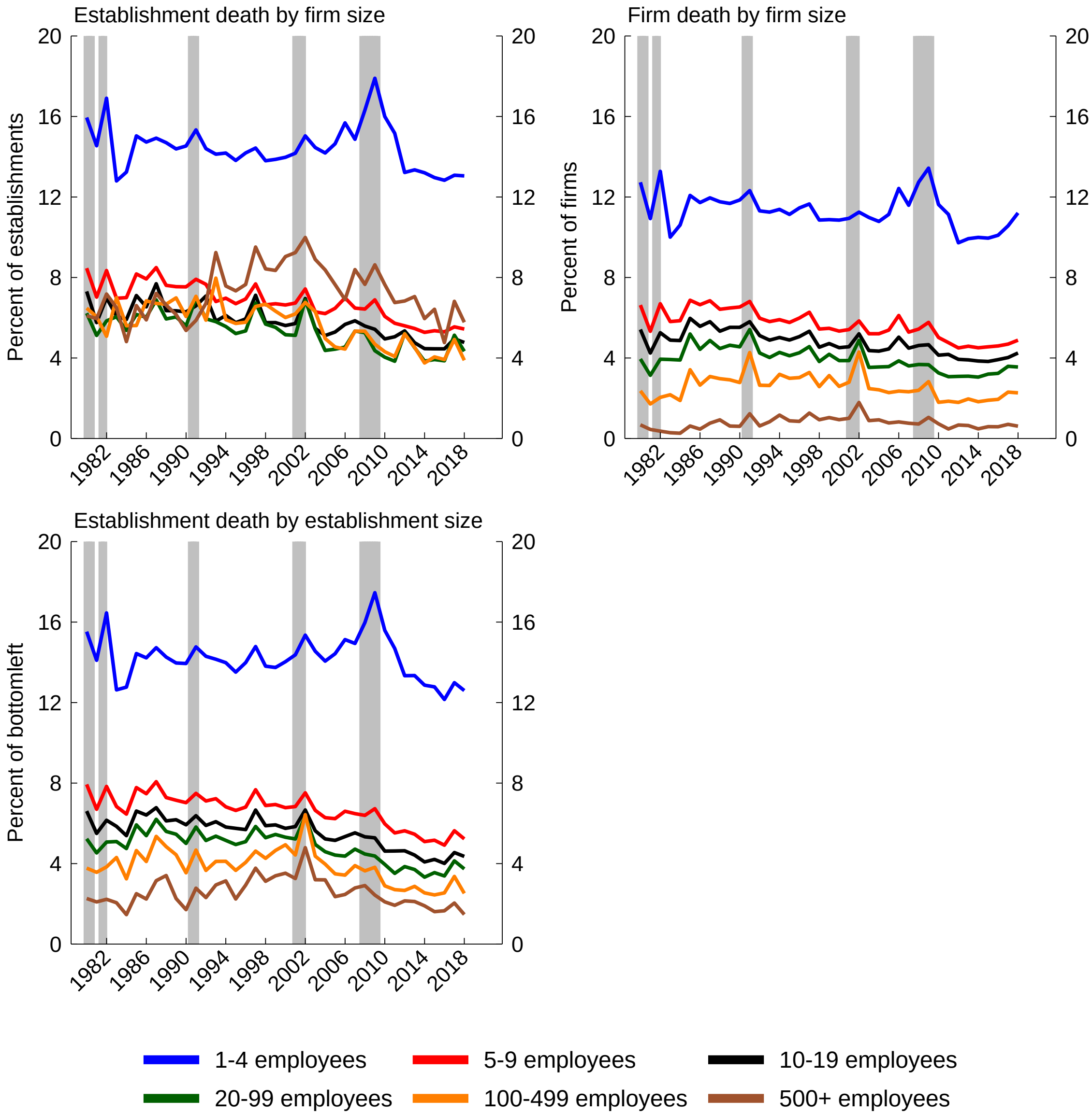
The bottom panel shows that establishment death rates decline monotonically in *establishment* size, with the smallest establishments exiting at rates well above other size classes.

Lee and Mukoyama (2015) document the relative cyclicality of entry and exit of manufacturing plants.

[27] We view the level of the unemployment rate as a problematic cyclical indicator, since it tends to stay elevated long after recessions end.

[28] Among firms with fewer than 5 employees, death rates are highest among firms that entered during the previous year. In many years, death rates among these new, extremely small firms exceed 30 percent. Yet even among firms at least 10 years old, death rates in this size category are around 10 percent. This may in part reflect gradual downsizing of previously large firms that enter this small size category during their final year.

[29] Decker et al. (2016) find that, on net, multi-unit firms open establishments during expansions and close establishments during recessions, which the authors rationalize using a model in which firms adjust product or market in response to aggregate shocks; this can help explain the countercyclicality of firm volatility.



Note: Unweighted exit rates with DHS denominators. BDS data are noisy in Economic Census years (2's and 7's).
Source: Census Bureau Business Dynamics Statistics (BDS).

Figure A1: Business death rates by firm and establishment size

More broadly, Figure A1 makes apparent that the rise in business death during the Great Recession was driven in large part by small firms and establishments.

A.3 Alternative detrending specifications

In our main results for nationwide business cycle correlations, we detrend BED and BDS shutdown measures using simple linear trends. Table A3 repeats the results of Table A1 using the detrending methodology of Hamilton (2018) instead; since the Hamilton (2018) method loses the first three years of data from the detrended series, we also report correlations based on linear trends covering the same periods. The results are largely unaffected by detrending methodology, though we observe some differences for firm-based measures.

Table A3: Business cycle correlations: Hamilton (2018) detrending

	Unemployment		GDP	
	Unweighted	Weighted	Unweighted	Weighted
Hamilton (2018) method:				
BED: Establishment closures (BLS)	.39	.18	-.35	-.14
BED: Establishment deaths (BLS)	.36	.22	-.33	-.24
BDS: Establishment deaths (Census)	.41	.13	-.19	-.02
BDS: Firm deaths (Census)	.16	.09	-.05	-.05
Linear method:				
BED: Establishment closures (BLS)	.37	.20	-.41	-.11
BED: Establishment deaths (BLS)	.37	.27	-.46	-.19
BDS: Establishment deaths (Census)	.64	.37	-.46	-.16
BDS: Firm deaths (Census)	.41	.31	-.32	-.18

Note: BED correlated with quarterly change in unemployment rate or change in log real GDP. BDS correlated with annual change in unemployment rate or change in log real GDP on BDS annual timing (April-March). BED trends estimated on data for 1992q3-2019q1 (deaths) or 1992q3-2019q4 (closures); BED correlations omit first three quarters of data consistent with Hamilton (2018) methodology (and therefore do not match Table A1 exactly). BDS trends estimated on data for 1978-2018; BDS correlations based on data for 1984-2018.

Separately, our main specifications use the annual or quarterly change in the log of real GDP for GDP-based correlations. An alternative approach to specifying GDP is to extract the cyclical component using a detrending method. Table A4 reports our main correlations in which GDP is expressed as the cyclical component of real GDP from the Hamilton (2018) method (left panel) and the HP filter of Hodrick and Prescott (1997) parameterized as is common in the literature (right panel); exit rates are detrended linearly as in the main text.

The finding that establishment exit is more countercyclical than firm exit and unweighted exit rates are more countercyclical than employment-weighted exit rates still hold under the

Table A4: Business cycle correlations: Alternative GDP specifications

	Hamilton (2018)		Hodrick and Prescott (1997)	
	Unweighted	Weighted	Unweighted	Weighted
BED: Establishment closures (BED)	-.57	-.04	-.15	-.14
BED: Establishment deaths (BED)	-.44	.04	-.07	-.08
BDS: Establishment deaths (BDS)	-.29	.02	-.27	-.22
BDS: Firm deaths (BDS)	-.17	.00	-.08	-.22

Note: Exit rates detrended linearly. BED correlated with quarterly change in unemployment rate or growth of GDP. BDS correlated with annual change in unemployment rate or detrended real GDP on BDS annual timing (April-March). HP filter parameter set at 6.25 for annual data (BDS correlations) and 1600 for quarterly data (BED correlations. BED data cover 1992q3-2019q1 (deaths) or 1992q3-2019q4 (closures). BDS data cover 1984-2018 (trends estimated on 1978-2018 data).

Hamilton (2018) method for GDP detrending. The HP filter results are more puzzling; Hamilton (2018) recommends against using the HP filter, and we show it here only for reader convenience.

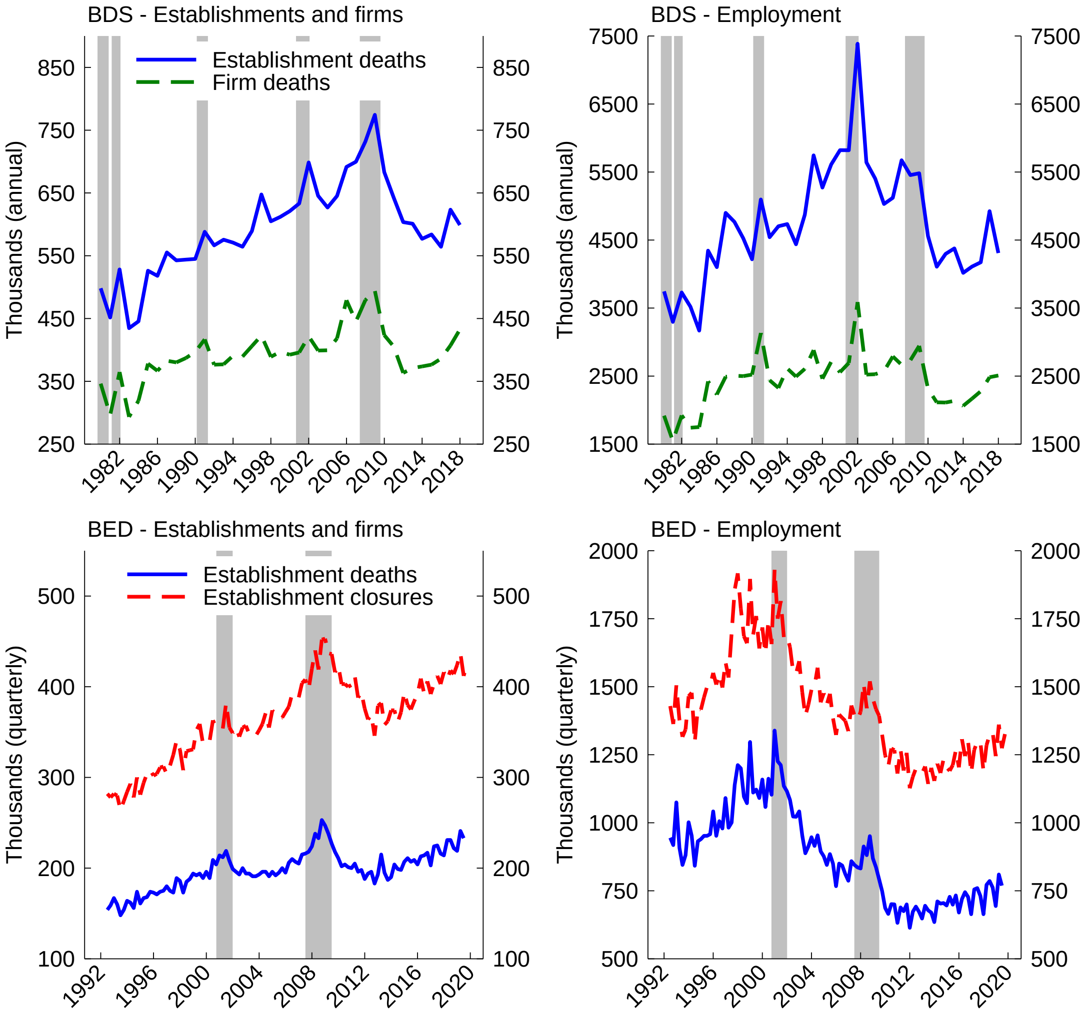
A.4 Exit levels

In the main text we focus on *rates* of establishment and firm exit. Firm and establishment exit *levels* (in terms of number of firms or establishments and their associated employment) behave in a broadly similar manner over the business cycle, though some interesting different trends are apparent. Figure A2 replicates Figure 1 using levels instead of rates.

The cyclicality of exit levels, particularly in recent years, is apparent in Figure A2. Interestingly, the number of exiting firms and establishments rose during the 1990s and 2000s (as the number of firms and establishments in the U.S. grew), but the number of exits declined after the Great Recession.

Table A5 reports business cycle correlations of exit level measures; that is, we correlate the (detrended) number of establishment closures and deaths and firm deaths with the change in the unemployment rate and the change in log real GDP. We report exit level measures detrended linearly (top) and using the Hamilton (2018) method (bottom).

The countercyclicality of establishment exit is apparent in levels, as is the weaker countercyclicality of employment-weighted quantities relative to unweighted quantities (with one exception of firm deaths under the Hamilton (2018) method). The countercyclicality of firm death is, as in other results, weaker than establishment death countercyclicality.



Note: BDS data are noisy around Economic Census years (2's and 7's). Establishments are single operating business locations. Firms are collections of one or more establishments under common ownership or operational control.
Source: BLS Business Employment Dynamics (BED), Census Bureau Business Dynamics Statistics (BDS).

Figure A2: Historical patterns of business shutdown levels

Table A5: Business cycle correlations: Exit levels

	Unemployment		GDP	
	Unweighted	Weighted	Unweighted	Weighted
Linear method:				
BED: Establishment closures (BLS)	.35	.21	-.40	-.10
BED: Establishment deaths (BLS)	.36	.23	-.45	-.13
BDS: Establishment deaths (Census)	.57	.34	-.29	-.05
BDS: Firm deaths (Census)	.39	.32	-.16	-.10
Hamilton (2018) method:				
BED: Establishment closures (BLS)	.29	.10	-.35	-.09
BED: Establishment deaths (BLS)	.29	.10	-.49	-.09
BDS: Establishment deaths (Census)	.32	.10	-.26	-.06
BDS: Firm deaths (Census)	.08	.13	-.13	-.12

Note: BED correlated with quarterly change in unemployment rate or change in log real GDP. BDS correlated with annual change in unemployment rate or change in log real GDP on BDS annual timing (April-March). BED trends estimated on data for 1992q3-2019q1 (deaths) or 1992q3-2019q4 (closures). BDS trends estimated on data for 1978-2018 but correlations estimated on data for 1984-2018.

A.5 Exit rate denominators

In the main results above, we calculate exit rates as the number of establishments (or firms) divided by the average aggregate number of establishments (or firms) in the two periods (the so-called "DHS denominator" after Davis et al., 1996). Likewise, we calculate employment-weighted exit rates as the jobs destroyed by exiting establishments (or firms) divided by average aggregate employment in the two periods. This convention follows the business dynamics literature generally and is consistent with how the BLS and the Census Bureau provide pre-calculated exit rates. But the denominators in these calculations are endogenous to exit and may be unintuitive to some readers, and our non-traditional measures use only initial values in denominators, so we examine the robustness of our main results to using the lagged (i.e., initial) establishment (or firm) count as the denominator for unweighted exit rates and using lagged aggregate employment as the denominator for weighted exit rates. Importantly, our main results about recent average exit rates (described in our stylized facts bullets in the main text) are not affected, to rounding, by this alternative denominator.

We study the sensitivity to denominator choice on Table A6 where we repeat our main correlations from Table A1 instead using the lag denominator. For simplicity, we focus on BDS data only, and we repeat the main results for ease of comparison (shown in the lines with the "(DHS)" parenthetical).

Business cycle correlations for unweighted exit rates are somewhat weaker—but still

Table A6: Business cycle correlations: Different denominator

	Unemployment		GDP	
	Unweighted	Weighted	Unweighted	Weighted
Establishment deaths (DHS)	.64	.37	-.46	-.16
Establishment deaths (lag denominator)	.60	.31	-.40	-.07
Firm deaths (DHS)	.41	.31	-.32	-.18
Firm deaths (lag denominator)	.35	.25	-.25	-.12

Note: Exit rates detrended linearly. BDS exit rates correlated with annual change in unemployment rate or change in log real GDP on BDS annual timing (April-March). BDS trends estimated on data for 1978-2018 but correlations estimated on data for 1984-2018. "DHS" refers to the Davis et al. (1996) two-period averaged denominator.

apparent—under the lag denominator, though employment-weighted exit rates are clearly less cyclical. We repeat our state-level regressions (from Table A1) with the lag denominator on Table A7; these are similar to the results with DHS denominators.

Table A7: Business cycle comovement: States (different denominator)

	Establishment death		Firm death	
	Unweighted	Weighted	Unweighted	Weighted
Change in unemp	0.52***	0.19***	0.33***	0.10***
	(0.03)	(0.02)	(0.02)	(0.01)
Observations	816	816	816	816
State FE	Yes	Yes	Yes	Yes
Year FE	No	No	No	No

Note: Regression of annual exit rates on annual change in unemployment rates, 2003-2018. Exit rates use lag denominator. Unemployment rate changes timed to correspond with BDS annual timing (April-March). Compare to Table A2.
***denotes statistical significance with $p < 0.01$.
Source: Author calculations from Business Dynamics Statistics and BLS unemployment data.

A.6 Using different sample periods

In the main text we explore official measures of business shutdown from two sources, BED (from the BLS) and BDS (from the Census Bureau). While BDS data have key advantages in terms of time coverage and firm identification, BDS data do face some limitations around years in which the Economic Census is conducted (those ending in 2 or 7) arising from difficulties with longitudinal linkages; these appear most significant in 2002, which was also a year in which the underlying Business Register (formerly Standard Statistical Establishment

List) source data were reorganized. In this section we briefly explore these limitations of the BDS.

We recreate the business cycle correlations in Table A1 omitting the year 2002 and, separately, omitting all Economic Census years; these can be found on Table A8. For ease of comparison, we repeat the correlations from Table A1 (calculated on all years in the data) and also show correlations in which specified years are omitted. Excluding 2002 or all Census years has little effect on the business cycle correlations of unweighted exit rates, but weighted rates are sensitive to these exclusions. Importantly, omitting these years is not necessarily the best practice, as they are likely to reflect real data in addition to potential noise.

Table A8: Business cycle correlations: Census year robustness

	Unemployment		GDP	
	Unweighted	Weighted	Unweighted	Weighted
Establishment deaths (all years)	.64	.37	-.46	-.16
Establishment deaths (ex. 2002)	.61	.29	-.42	-.03
Establishment deaths (ex. EC years)	.67	.34	-.52	-.11
Firm deaths (all years)	.41	.31	-.32	-.18
Firm deaths (ex. 2002)	.41	.22	-.32	-.09
Firm deaths (ex. EC years)	.44	.21	-.43	-.16

Note: Exit rates detrended linearly. BDS correlated with annual change in unemployment rate or change in log real GDP on BDS annual timing (April-March). "EC" is Economic Census (years ending in 2 or 7). BDS trends estimated on data for 1978-2018 but correlations estimated on data for 1984-2018.
Source: Author calculations from Business Dynamics Statistics, BLS (unemployment rates), and BEA (GDP).

Table A9 repeats the state-level panel regressions shown on Table A2 with the omission of Economic Census years.

Economic Census years do not substantially alter the result that exit rates are countercyclical, correlating positively with the change in unemployment rates and negatively with GDP growth, and that this cyclicality is driven by smaller units.

Our main results for BDS data use the years 1984-2018. BDS data are available for 1978-2018, but we omit the early period because correlations are heavily sensitive to the omission or inclusion of certain specific years during 1978-1983 (and particularly 1980-1983). This may be because the early 1980s recession years did not see elevated business exit in BDS data. That said, the inclusion of the entire 1978-1983 period does not qualitatively matter for our main results, as can be seen on Table A10.

Table A9: Business cycle comovement: States (excluding Census years)

	Establishment death		Firm death	
	Unweighted	Weighted	Unweighted	Weighted
Change in unemp	0.57***	0.23***	0.36***	0.11***
	(0.03)	(0.02)	(0.03)	(0.01)
Observations	663	663	663	663
State FE	Yes	Yes	Yes	Yes
Year FE	No	No	No	No

Note: Regression of state annual exit rates on annual change in unemployment rates, 2003-2018 excluding Economic Census years. Unemployment rate changes timed to correspond with BDS annual timing (April-March). Compare to Table A2.

***denotes statistical significance with $p < 0.01$.

Source: Author calculations from Business Dynamics Statistics and BLS unemployment data.

Table A10: Business cycle correlations: Additional BDS years

	Unemployment		GDP	
	Unweighted	Weighted	Unweighted	Weighted
1978-2018:				
BDS: Establishment deaths (Census)	.39	.16	-.28	-.01
BDS: Firm deaths (Census)	.16	.06	-.15	-.02
1984-2018:				
BDS: Establishment deaths (Census)	.64	.37	-.46	-.16
BDS: Firm deaths (Census)	.41	.31	-.32	-.18

Note: Exit rates detrended linearly and correlated with annual change in unemployment rate or change in log real GDP on BDS annual timing (April-March). Trends estimated on full 1978-2018 sample.

A.7 Additional state results

Table A2 reports panel regressions relating state-level exit rates and changes in unemployment rates with state fixed effects. Table A11 reports the same regressions without state fixed effects (top panel) or with both state and year fixed effects (bottom panel). The fixed effects specification makes little difference. Moreover, in unreported results we find little effect of omitting Economic Census years from these regressions.

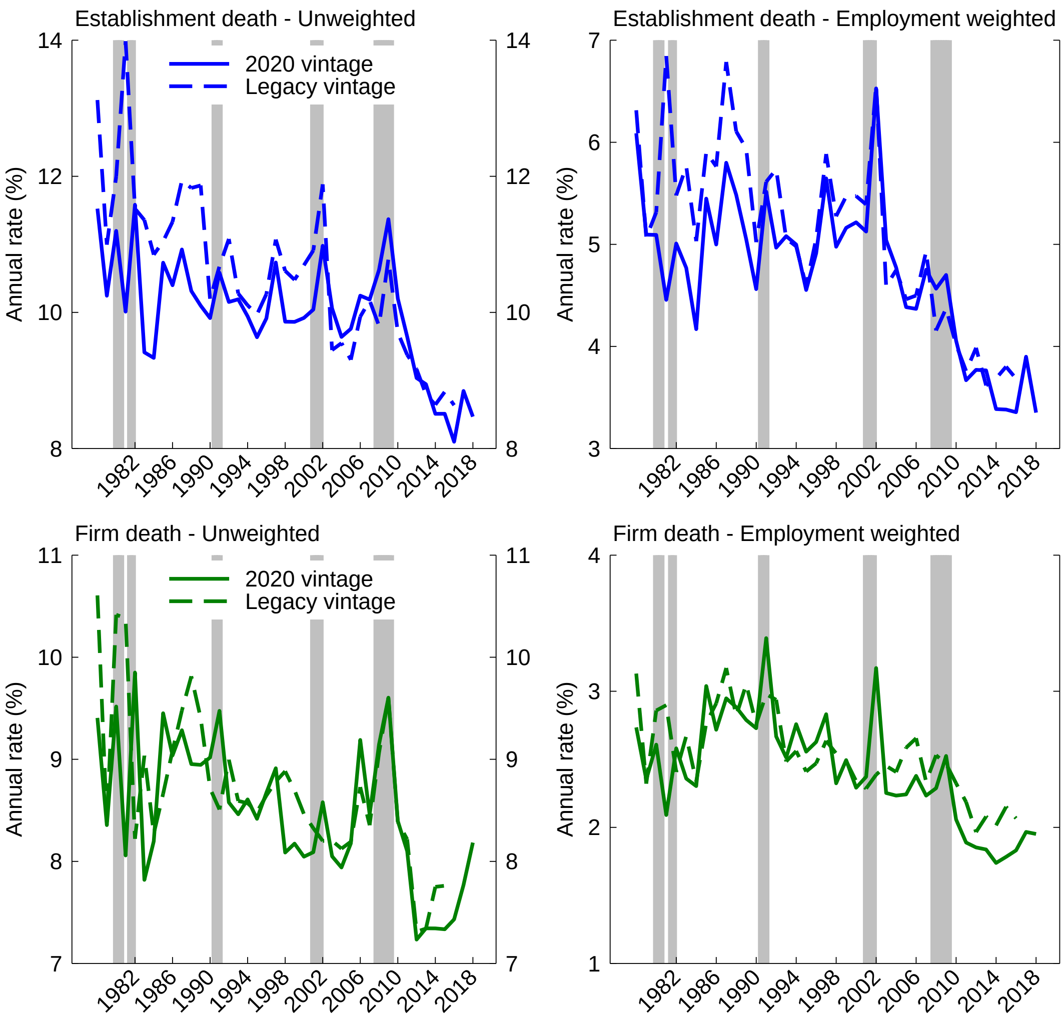
Table A11: Business cycle comovement: Alternative controls

	Establishment death		Firm death	
	Unweighted	Weighted	Unweighted	Weighted
Change in unemp	0.55***	0.22***	0.36***	0.11***
	(0.04)	(0.03)	(0.04)	(0.01)
Observations	816	816	816	816
State FE	No	No	No	No
Year FE	No	No	No	No
Change in unemp	0.46***	0.18***	0.40***	0.10***
	(0.03)	(0.03)	(0.03)	(0.02)
Observations	816	816	816	816
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Note: Regression of state annual exit rates on annual change in unemployment rates, 2003-2018. Unemployment rate changes timed to correspond with BDS annual timing (April-March). Compare to Table A2.
***denotes statistical significance with $p < 0.01$.
Source: Author calculations from Business Dynamics Statistics and BLS data.

A.8 2020 BDS redesign

Our main results feature the vintage of the BDS that was released in September 2020 (covering the period through 2018) after a substantial redesign. Relative to the previous BDS vintage, the redesigned product features extensive industry detail under consistent NAICS classification along with narrower tabulations across geography. The redesign also introduced expanded source data and improved longitudinal linking processes. The documentation released with the BDS redesign explores these issues and highlights ongoing challenges.[30] Importantly, overall patterns of business exit, and patterns around Economic Census years in particular, were slightly different in the previous vintage of BDS data. Figure A3 shows establishment (top panels) and firm (bottom panels) exit rates, unweighted (left panels) and employment weighted (right panels).



Note: DHS denominators. Establishments are single operating business locations.
Firms are collections of one or more establishments under common ownership or operational control.
Source: Census Bureau Business Dynamics Statistics (BDS).

Figure A3: Vintage differences in BDS data

These vintage differences also introduce differences in our business cycle correlations.

[30] See `https://www.census.gov/programs-surveys/bds/news-updates/updates/2018-bds-release.html`.

Table A12 reports business cycle correlations for both the legacy BDS vintage and the current (2020) vintage, where we omit the years 2017 and 2018 from the latter (including the detrending estimation) to have the same time coverage as the legacy vintage.

Table A12: Business cycle correlations: Vintage comparisons

	Unemployment		GDP	
	Unweighted	Weighted	Unweighted	Weighted
Establishment deaths (2020 vintage)	.63	.36	-.46	-.15
Establishment deaths (legacy vintage)	.40	.19	-.34	-.03
Firm deaths (2020 vintage)	.43	.30	-.34	-.19
Firm deaths (legacy vintage)	.39	.29	-.36	-.22

Note: Exit rates detrended linearly. BDS correlated with annual change in unemployment rate or growth of GDP on BDS annual timing (April-March). BDS trends estimated on data for 1978-2018 but correlations estimated on data for 1984-2018.

Differences between vintages are material for our quantitative estimates. On Table A13, we repeat our state-level regressions for vintage comparisons; our legacy vintage state-level data ended in 2014 so we first show regressions on the 2020 redesign data ending in 2014. We then show regressions on the legacy vintage data for the same years.

In the 2020 vintage, business cycle comovements with data for 2003-2014 are somewhat weaker than the comovements with data for 2003-2018 shown on Table A2. Within the 2003-2014 period, the legacy BDS vintage has notably weaker cyclicality in all categories than does the 2020 BDS vintage. That said, the countercyclicality of unweighted exit rates is confirmed in the older vintage, and there still exists modest countercyclicality even of weighted exit.

A.9 Industrial production

Our main business cycle correlations related exit rates with unemployment and GDP. Table A14 reports correlations using the growth rate of industrial production (IP) at quarterly frequency for BED correlations and annual frequency for BDS correlations. The left panel reports correlations with manufacturing IP, while the right panel reports correlations with total IP (which consists of manufacturing, mining, and utilities). Overall, exit measures are consistently countercyclical with respect to IP. Manufacturing IP correlations follow the familiar pattern in which establishment exit is more cyclical than firm exit, and employment-weighted exit is less cyclical than unweighted exit.

Table A13: Business cycle comovement: States, BDS vintages

	Establishment death		Firm death	
	Unweighted	Weighted	Unweighted	Weighted
2020 vintage, 2003-2014 data				
Change in unemp	0.43***	0.13***	0.32***	0.09***
	(0.03)	(0.02)	(0.03)	(0.01)
Observations	612	612	612	612
State FE	Yes	Yes	Yes	Yes
Year FE	No	No	No	No
Legacy vintage, 2003-2014 data				
Change in unemp	0.30***	0.04*	0.30***	0.05***
	(0.02)	(0.02)	(0.02)	(0.01)
Observations	612	612	612	
State FE	Yes	Yes	Yes	Yes
Year FE	No	No	No	No

Note: Regression of annual exit rates on annual change in unemployment rates, 2003-2014. Unemployment rate changes timed to correspond with BDS annual timing (April-March).
***denotes statistical significance with $p < 0.01$.
** denotes statistical significance with $p < 0.05$.
* denotes statistical significance with $p < 0.10$.
Source: Author calculations from Business Dynamics Statistics and BLS unemployment data.

Table A14: Business cycle correlations: Industrial production

	Manufacturing IP		Total IP	
	Unweighted	Weighted	Unweighted	Weighted
BED: Establishment closures (BLS)	-.39	-.17	-.39	-.17
BED: Establishment deaths (BLS)	-.52	-.28	-.53	-.28
BDS: Establishment deaths (Census)	-.41	-.14	-.14	-.17
BDS: Firm deaths (Census)	-.33	-.19	-.34	-.22

Note: Exit rates detrended linearly. BED correlated with quarterly growth of industrial production. BDS correlated with annual growth of industrial production on BDS annual timing (April-March). BED data cover 1992q3-2019q1 (deaths) or 1992q3-2019q4 (closures). BDS trends estimated on data for 1978-2018 but correlations estimated on data for 1984-2018.

B Appendix for SafeGraph closure measure

B.1 Details on SafeGraph data

The closure measure based on SafeGraph mobile device data comes from a combination of two different sources of information. First, the “Core” dataset is a registry of points of interest (POIs) in the U.S., with each POI listed with a SafeGraph identifier, industry information, location, and other time-invariant data.[31] SafeGraph has somewhere between 5-6 million POIs in their Core files, though not all of these POIs would be considered establishments with employment, as parks, etc., are also listed in the data. Coverage varies across industries but is generally good for consumer-facing businesses that have an incentive in advertising their business location to consumers. Overall, the POI database likely covers a significant share of U.S. business establishments, as Census Bureau and BLS data include between 7 and 10 million establishments.

The second dataset is the set of weekly “patterns” datasets that contain information about daily/weekly visits to individual establishments. The sample of devices delivering these data varies over time, but is often in the range of 40-45 million. The weekly pattern files are released each week and include visits from the previous week (a lag of 4 days or so).

B.2 Creating a longitudinally consistent sample

There are a number of challenges in creating a longitudinally consistent sample (across establishments, visits data, and the sample of devices). The first challenge comes from the impact of periodic revisions to the SafeGraph sample. Because establishments deemed closed are removed from the Core files, one must integrate historical Core snapshots in creating the universe of potential businesses. The earliest of these is from March 2020; hence, successfully identifying businesses during the process of closure becomes more challenging the further back one moves from March 2020.[32] Practically speaking, this feature limits the ability to compare measures of business exit between 2020 and equivalent months from earlier years.

A second challenge comes from revisions to the periodic patterns datasets that contain information on visits. These revisions often remove the historical visits from businesses deemed closed as of the time of the revision, a feature which could obviously bias estimates

[31] The nature of this dataset is roughly comparable to the Business Register maintained by the Census Bureau but is not as comprehensive.

[32] This ability declines gradually, as it typically takes some time for SafeGraph to identify a business as closed using their traditional methods.

of business closure. The latest of these revisions to the weekly data that extend back to early 2019 occurred in May, 2020. To correct for this potential bias in months prior to May, 2020, we utilize an earlier dataset from the monthly version of the patterns information dated from March 2020. These monthly snapshots that extend to early 2019 contain the daily detail to construct equivalent weekly estimates of visits. We use this supplementary information to fill out the sample with any POIs that could have been removed from the weekly patterns files.[33]

Finally, we adjust the weekly visits measure based on natural fluctuations in the underlying sample of mobile devices that are used to generate the data. This is particularly important during the peak of social distancing in March and April, as the sample of devices fell significantly during this time.[34] Using the SafeGraph-provided summary data on mobile devices recorded in given locations, we gross up the visits by the share of devices in a given location population, and use this variable for all of our longitudinal comparisons.

B.3 Sample selection

Sit-down restaurants (NAICS 722511) is a good fit for the SafeGraph data for several reasons. First, this industry has particularly good coverage in the SafeGraph data. When aggregating up to the four-digit NAICS category (7225) which includes limited service restaurants, the sample of SafeGraph establishments is reasonably close to official statistics (520 thousand vs 580 thousand according to the BLS QCEW). Second, unlike some industries where expenditure switching to e-commerce can translate to sales without physical movement, restaurants require movement to the geographic location of some form.[35] While there has clearly been a migration from in-person dining to carry-out service at these establishments in recent months, the carry-out transactions still require a visit that would be picked up by cellphone tracking data (and this is true whether it be a delivery service or picked up by the ultimate consumer). Figure B1 shows the shift in the distribution of visits toward carry-out, plotting the median duration of a visit to restaurants in the sample. The median duration of a visit

[33]Because the monthly patterns files use different criteria for identifying the sample of devices for measuring visits, we construct an establishment-specific scalar to adjust the visits derived from the monthly files based on overlap between the monthly-based and weekly-based estimates of visits. Those establishments lacking an overlap are adjusted using the overall sample adjustment ratio.

[34]This was due, at least in part, to the fact that mobile devices are not counted when there is no location data being recorded, a feature that occurs more often when devices are stationary.

[35]In reality, it is actually unclear whether COVID-induced switch to e-commerce would mitigate the impact on business closure. To the extent a greater reliance on internet-based shopping translates to direct delivery of merchandise, retail locations may be increasingly dispensable, with obvious implications for employment, at least at a spatial level.

before the onset of COVID-19 of just under 40 minutes indicates that casual dining and some form of carry-out service was likely a feature for these establishments even before the pandemic. Finally, sit-down restaurants have been particularly hard hit by the COVID-19 recession and have thus received substantial attention in the popular press in the context of closure.

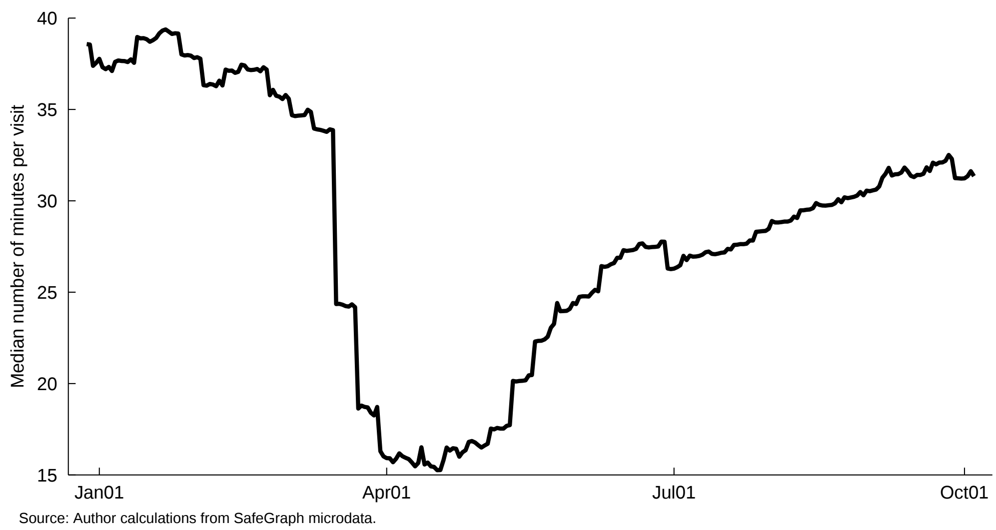


Figure B1: Median duration of restaurant visits

We merge the Core datasets for all restaurant establishments to the weekly patterns files that encompass January 2019 to August 2020, retaining establishment data whether or not visits are recorded. From there we create a balanced panel of weekly observations for each establishment, filling in visits as zero if no data were recorded in the patterns files. We impose a number of sample restrictions such as removing restaurants that record implausibly high numbers of visits in a given week. The resulting dataset identifies 335 thousand restaurants and covers around 85 weeks (≈28.5 million observations).

B.4 Defining temporary and permanent closures

The approach taken here is to infer the likelihood of temporary and permanent business closure based on the degree and duration of declines in customer visits. Although one might expect business closure to be associated with zero visits from customers, there are several reasons that a non-zero threshold is appropriate. First, SafeGraph visits are based on a *sample* of devices that is not universal. Second, even a closed business would likely continue to record some visits due to periodic trips to the establishment by owners, management, or employees. Even after a prolonged or permanent closure, visits could be recorded if contractors or landlords continue to maintain the property. A third reason for a non-zero threshold to record closures is due to data error: while cellphone tracking from GPS signals is reasonably precise, there is some noise in where SafeGraph identifies a given visit, particularly if establishments are located close together.

For each case of temporary and permanent closures, we rely on additional data provided by SafeGraph to discipline the definitions based on visits data.

For temporary closures, we utilize an additional ad-hoc SafeGraph dataset resulting from an experimental analysis using machine-learning techniques to infer the operating status of POIs for the week of April 5th. The SafeGraph team trained a logistic model based on some POIs with known operating status, using a series of visit attributes during this week relative to a baseline week in early March. The resulting dataset released to researchers has a set of POIs with the predicted operational status from this model along with a confidence value.[36] Working backward, we retain only those POIs with high confidence values and use these data to identify simple rules in our sample of restaurants that most accurately align with temporary closure status. In addition to the year-over-year percent decline in visits, we consider lower and upper bounds of the absolute value of visits associated with not operating and operating status, respectively, as well as various forms of weekly smoothing (3-week vs 5-week moving averages) for visit declines.

Figure B2 shows the year-over-year percent change in visits based on operational status for the week of April 5th (identified by the dashed line in the chart). As expected, restaurants identified as closed had considerably lower average visits (78 percent declines) versus those remaining open (31 percent decline). Narrowing in on the heterogeneity in visit declines specifically for the week of April 5th, Figure B3 plots the densities of the year-over-year change in visits, separately for those identified as operational and not operational. While the overlap of the densities in Figure B3 indicates that any rule based on visit declines will be imperfect, there is significant separation such that visit patterns are highly informative for closure.

More formally, we calculate false positive (identifying closed establishments that are identified as operational) and false negative (identifying open establishments that are identified as not operational) rates across a wide variety of parameter values.[37] Ultimately, the best

[36] Further information on this dataset is available at `https://docs.safegraph.com/docs/operatingnot-operating-poi`.

[37] This terminology is somewhat misleading as the "true" data from SafeGraph are themselves estimates subject to error.

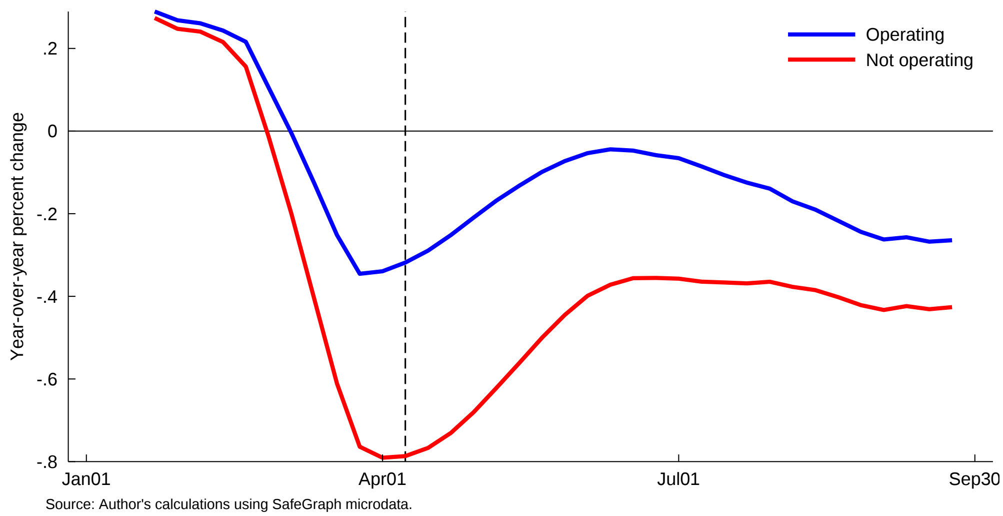


Figure B2: Year-over-year percent change in visits, by operational status
Week of April 5-11

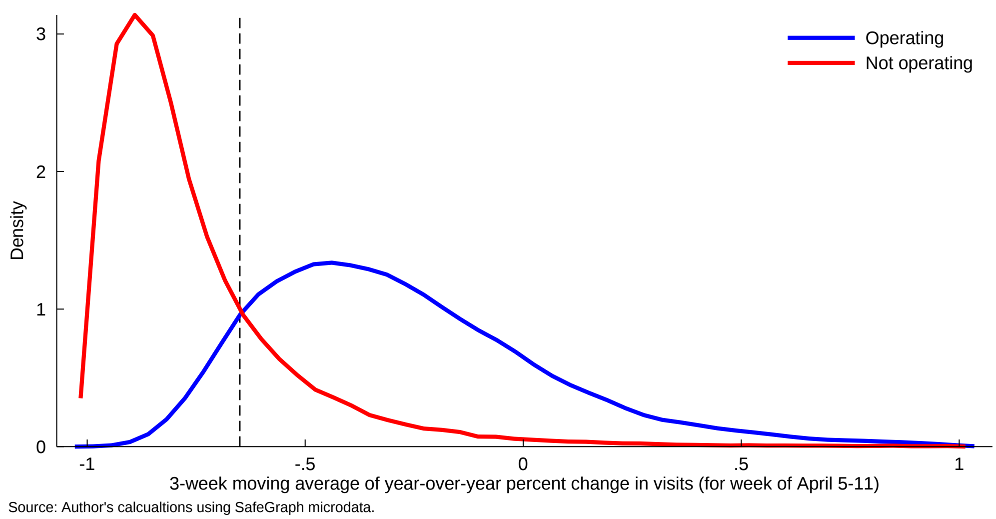


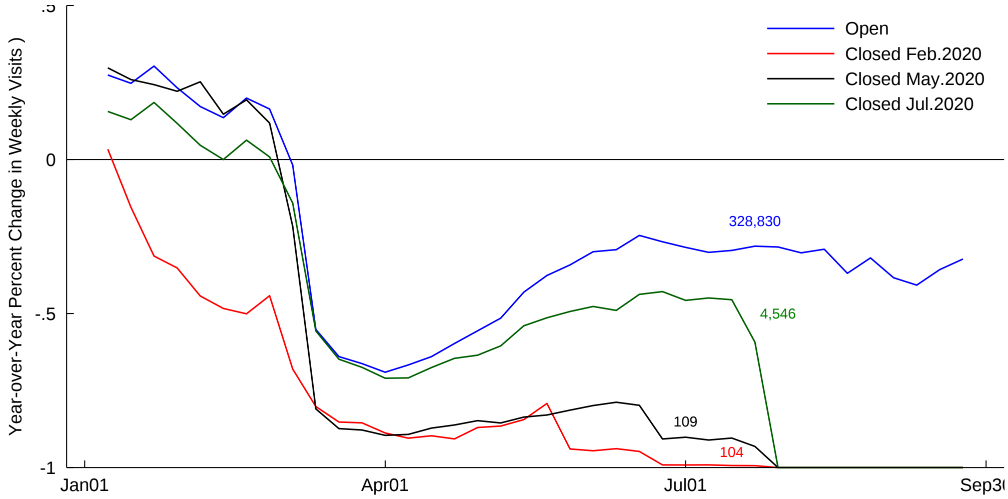
Figure B3: Density of year-over-year percent changes in visits, by operational status
Week of April 5-11

combination of parameters was a threshold of a 65 percent year-over-year decline in visits (using a 3-week moving average), a lower bound of 4 visits and an upper bound of 50 visits. This value of y/y declines is identified by the dashed line in Figure B3. It is worth pointing out that this procedure for identifying temporary closures is similar in spirit to that done in de Vaan et al. (2021) in their study of the spillover effects of store closure using SafeGraph Data. Some of their parameters are similar (and upper bound of 50 visits to guarantee open status) while others were different: de Vaan et al. (2021) use a ratio of visits of 0.2 relative to February levels, rather than the year-over-year metric used here.

For a formal definition of temporary closure, a business i is temporarily closed in week n if $\widetilde{v}^t_{i,n} = 1$:

$$\widetilde{v}^t_{i,n} = \begin{cases} 1 & \text{if } \frac{1}{3}\sum_{j=n-1}^{n+1}\left(\frac{v_{i,j}-v_{i,j-52}}{v_{i,j-52}}\right) < -0.65 \text{ and } v_{i,n} < 50 \\ 1 & \text{if } v_{i,n} \leq 4 \\ 0 & \text{if } \frac{1}{3}\sum_{j=n-1}^{n+1}\left(\frac{v_{i,j}-v_{i,j-52}}{v_{i,j-52}}\right) > -0.65 \text{ and } v_{i,n} > 4 \\ 0 & \text{if } v_{i,n} \geq 50 \end{cases} \tag{B1}$$

To guide definitions of permanent closures, we rely on recent additions to the "Core" data that identify the opening and closing dates of POIs. These data are not directly applicable for purposes of closure due to poor coverage; however, the patterns evident for POIs identified as permanently closed are nevertheless useful for constructing a universal definition based on observable characteristics. To verify these closures identified by SafeGraph, we group all restaurants according to closure date (including a category for those remaining open) and plot their average weekly visits over the sample period. Figure B4 plots the year-over-year percent change in weekly visits for some select closure dates. Overall, there is some evidence that the pattern of visits align with closure: visits drop in February for those restaurants identified as closing in that month, though the average visits of these establishments do not fall immediately to zero. The closures occurring during or after the COVID-19 pandemic show a similar pattern, with average visits remaining well below those that are identified as remaining open. The numbers corresponding to each line represent the number of establishment comprising the average visits—highlighting the lack of coverage for these indicators. The large jump in closures in July, 2020, could be due at least in part to increased surveillance of closures by SafeGraph. It is also somewhat puzzling that the contour of visits for the average of restaurants identified as closing in this month is only slightly below those identified as open.



Source: Author calculations from SafeGraph microdata. Numbers correspond to national number of restaurants in each group.

Figure B4: Weekly visits by identified closure Status

Figure B4 illustrates how the timing of a permanent closure is made difficult by the widespread temporary closures in March and April of 2020. Moreover, even for the closures identified as before the lockdowns resulting from COVID-19 (for example, the February 2020 closures shown by the red line), the weekly visits do not immediately drop to zero in subsequent weeks. Hence, the patterns in Figure B4 demonstrate that a threshold rule is likely also necessary for identifying a permanent closure. For simplicity, we therefore define a closure to be permanent if subsequent visits never rise above the visits threshold identified for temporary closure. Formally, an establishment i is identified as permanently closed in week n ($\widetilde{v}^p_{i,n}$) if:

$$\widetilde{v}^p_{i,n} = 1 \text{ if } \widetilde{v}^t_{i,n} = 1 \quad \text{and} \quad \forall m > n \quad \widetilde{v}^t_{i,m} = 1 \tag{B2}$$

We translate these measures to a monthly frequency t such that $\widetilde{v}^t_{i,t}$ equals one if any weeks $n \in t$ are equal to one; for permanent closures all subsequent (smoothed) weeks must be below the threshold to be recorded as closed in the month.

Using prior contour of revisions to refine estimate of closures

Unlike temporary closures, an important feature of the definition of permanent closures is that it is subject to revision as additional data become available. The set of permanently closed establishments in a month t can only decline as additional weeks of data outside of that month become available. Hence the initial estimate of permanent closures for a given month is an upper bound and will decline over time. One method of accounting for potential future revisions in real time is to calculate the equivalent revision schedule from prior months' data and apply that forecasted revision to the appropriate vintage of more recent data. As an example, Figure B5 illustrates the monthly closure rates for several months in our sample based on the number of weeks following the end of the respective month. As is clear from Panel A of Figure B5, the revisions are initially significant and then quickly decline. Panel B translates these estimates into a ratio relative to the initial estimate, and shows how one can apply the revision schedule from prior months to arrive at a forecast estimate for a subsequent vintage of a more recent estimate.[38]

B.5 Alternate threshold

Figures B6 and B7 show measures of temporary and potentially permanent closures when using a threshold of 80 percent y/y visit declines (versus 65 percent as in our main specifications). This more stringent threshold naturally leads to lower closure rates, with a cumulative death rate estimate of 9.3 percent (versus 13.5 percent in our main specification).

C Appendix for Business death expectations

Many currently operating businesses are concerned about exit risk going forward, though these expectations have fluctuated throughout the pandemic. The Census Bureau has conducted surveys of small businesses (those with one establishment and fewer than 500 employees) at weekly frequency through several phases since the pandemic began.[39] Among other questions, businesses are asked about their expectations for the six months following the survey; the left panel of Figure C8 reports the share of businesses expecting permanently to close within six months of the survey week; the red dashed line shows the historical *actual* exit rate for firms with fewer than 500 employees from BDS data for 2015-2018.[40] The right panel reports the share of businesses expecting to need to obtain financial assistance or credit over the same period.

Death expectations rose in November and December of 2020 corresponding with rising Covid case counts in the U.S.; this pessimism declined some through December and January then dropped substantially by mid-February, possibly related to the late-December passage

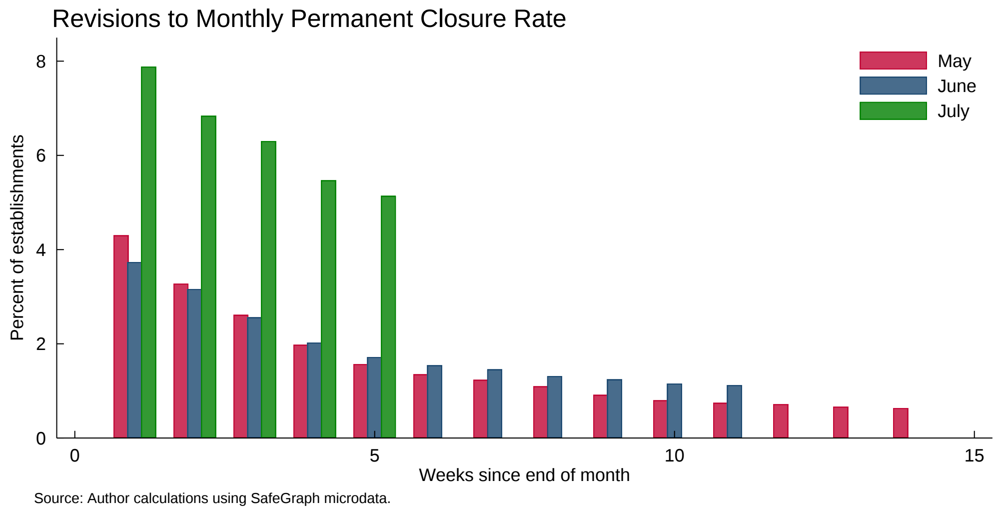
[38]Thanks to Brendan Price for helpful conversations on this methodology.

[39]See `https://www.census.gov/data/experimental-data-products/small-business-pulse-survey.html` for survey data, including time series views; for methodological details see Buffington et al. (2020).

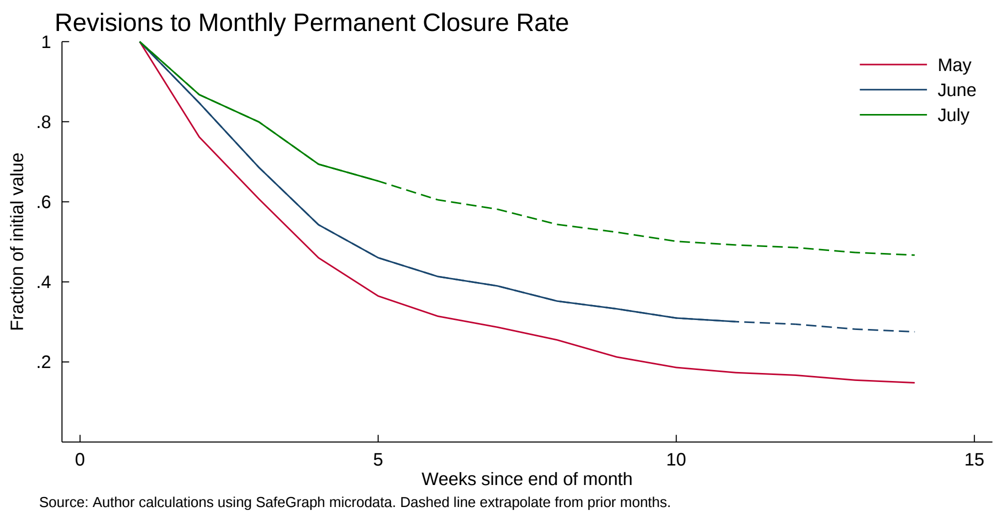
[40]We estimate the historical six-month exit rate by taking half of the historical annual exit rate. This is only approximate—actual six-month exit rates can be higher due to firms that enter and exit within a year.

Figure B5: Revisions by month of identified permanent closure

(a) Panel A: Monthly estimate by week



(b) Panel B: Revisions contour relative to initial reading



of an additional round of Paycheck Protection Program loans and other fiscal support.[41] During most of the pandemic death expectations have been well above the level consistent with actual death rates from recent years (the dashed red line), though we do not know how

[41]The Pulse survey also asks questions about past closures; these are more difficult to interpret given the possibility that non-response to the survey is correlated with business death. Even the forward-looking measure we report may suffer from selection bias among respondents, and it is possible that industries showing relatively low expectations of exit have already seen many exits. That said, the Pulse survey is constructed using the Census Bureau's Business Register as a sampling frame, and the Census Bureau applies appropriate sampling weights to responses, so the survey is of high scientific quality. Importantly, the sample is limited to small employer businesses with only one establishment.

Figure B6: Temporary closures: 80 percent threshold

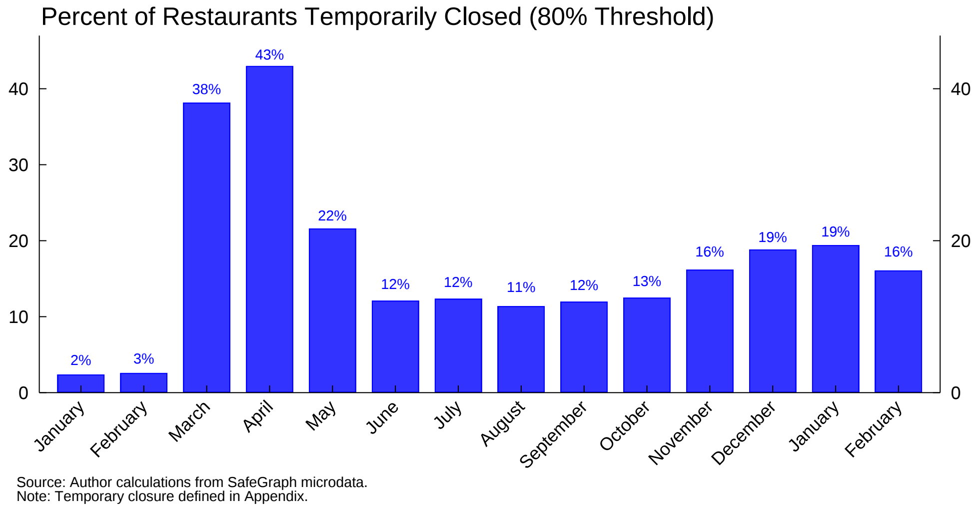
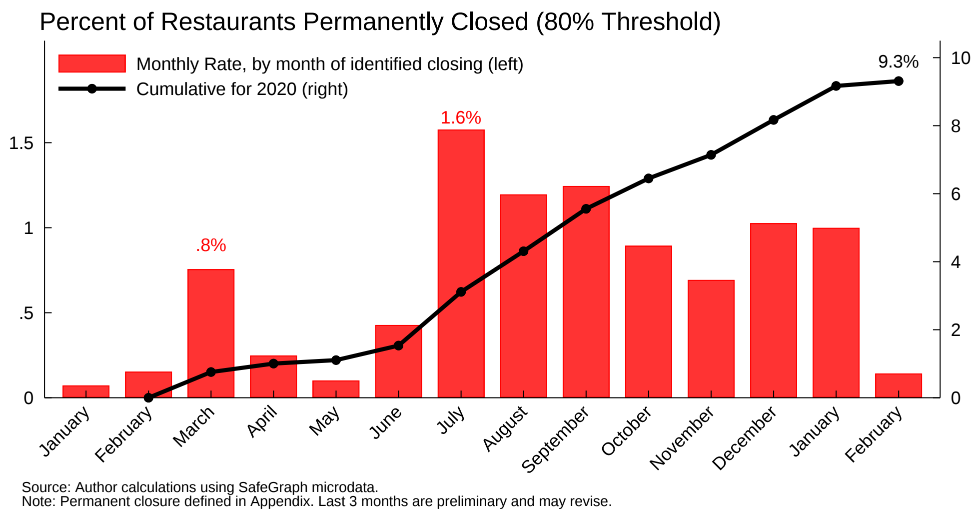


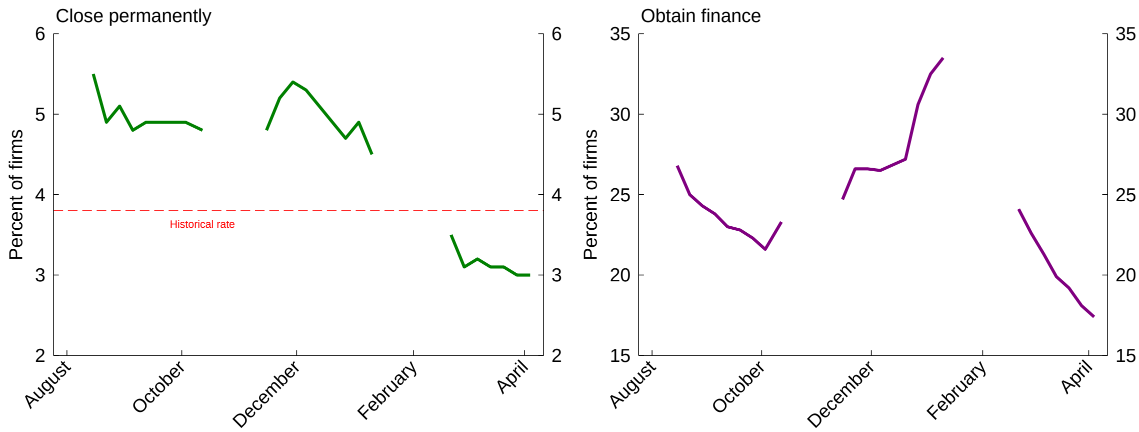
Figure B7: Potentially permanent closures: 80 percent threshold



well these death expectations would predict actual death in normal times.

Importantly, the expected exit rates vary widely by sector, and expected exit rates *relative to historical averages* vary widely as well. Figure C9 shows Pulse Survey expected exit as of early April 2021, in blue, compared with average six-month actual death rates among small firms (fewer than 500 employees) from BDS data from 2015-2018, in red, and from the Great Recession, in black.[42] In some sectors, recent expected death rates do indeed imply

[42] BDS exit rates are divided by two to approximate 6-month exit rates comparable to Pulse expectations. Sector-level historical actual exit rates are approximate; in particular, the lagged firm count in the DHS exit rate denominator is constructed with the previous-year total firm count ($firms_{t-1}$) rather than with longitudinally precise lagged firm count ($firms_t - entrants_t + deaths_t$) because publicly available BDS data



Note: Six month expectations. Data correspond to end of survey week. Gaps indicate between-survey periods. Historical rate is 2015-2018 actual small business death rate.
Source: Census Bureau Business Dynamics Statistics and Small Business Pulse Survey; data through March 29 - April 4 2021.

Figure C8: Small firms' expected needs over next six months

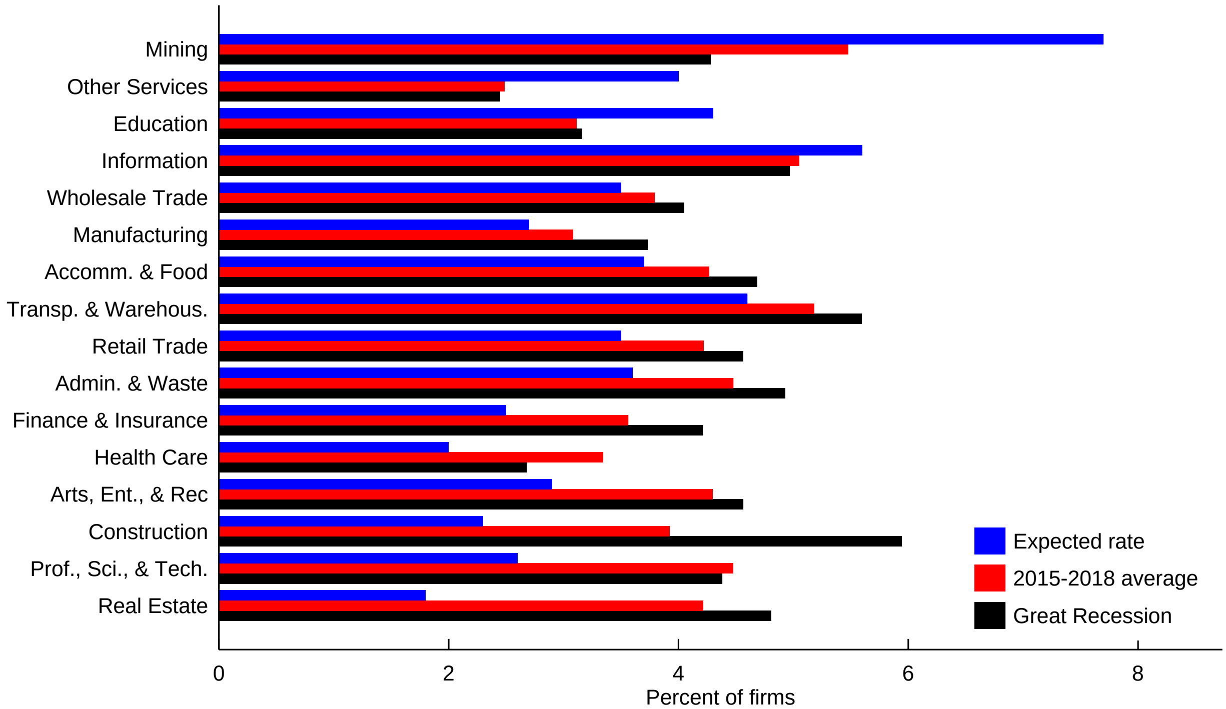
historically elevated rates (i.e., the blue bars are longer than the red and black bars). This is most notable in mining, other services, and education.[43] Some of these sectors have already seen considerable exit such that these expectations, if realized, would result in material permanent impacts; for example, consider the possibility that the other services sector has already seen exit rates at more than three times the normal rate over the past year (as suggested by our SafeGraph analysis) and then experiences exit rates at nearly double the usual pace over the next six months.

On the other hand, some sectors have recent exit expectations close to, or even below, historical averages. The most notable of these are accommodation and food services and arts, entertainment, and recreation—that is, the leisure and hospitality supersector. This is a notable development.

Some of the sectors with low exit expectations in recent surveys had much higher expectations earlier in the pandemic. Figure C10 reports *excess* exit expectations—that is, *expected* exit rates minus historical *actual* exit rates among firms with fewer than 500 employees—by sector (top panel) and by state (bottom panel). The green bars show expectations as of late November, when overall exit expectations were highest; the orange bars show expectations in the latest data from early April 2021. Sectors and states are listed in order of their improvement; the most improved sector is accommodation and food services, where excess

are not tabulated at the sector-by-age-by-size level, and age is necessary for constructing the longitudinal measure. The resulting error is likely negligible.

[43]The education sector includes not only K-12 schools and colleges/universities but also trade training programs such as cosmetology schools, flight training, and technical programs as well as sports and recreation training, exam preparation programs, and driving schools.



Note: Bars ordered by difference between expected rate and 2015-2018 average. Firms with fewer than 500 employees. Great Recession rate is average of years spanning April 2008-March 2010.
Source: Census Bureau Small Business Pulse Survey, Mar. 29-Apr. 4 2021 and Business Dynamics Statistics.

Figure C9: Exit expectations versus historical rates

death expectations once exceeded historical death rates by about six percentage points but are now below historical rates. Arts, entertainment, and recreation has also improved substantially. Notably, several sectors did not see dramatically elevated death expectations even during the November peak. Data on states also show wide heterogeneity in improvement—and in peak exit expectations.[44]

[44]The Census Bureau suppresses data for some states; all states with reported data in both time periods are shown on Figure C9. State historical actual exit rates are approximate (see footnote 42).

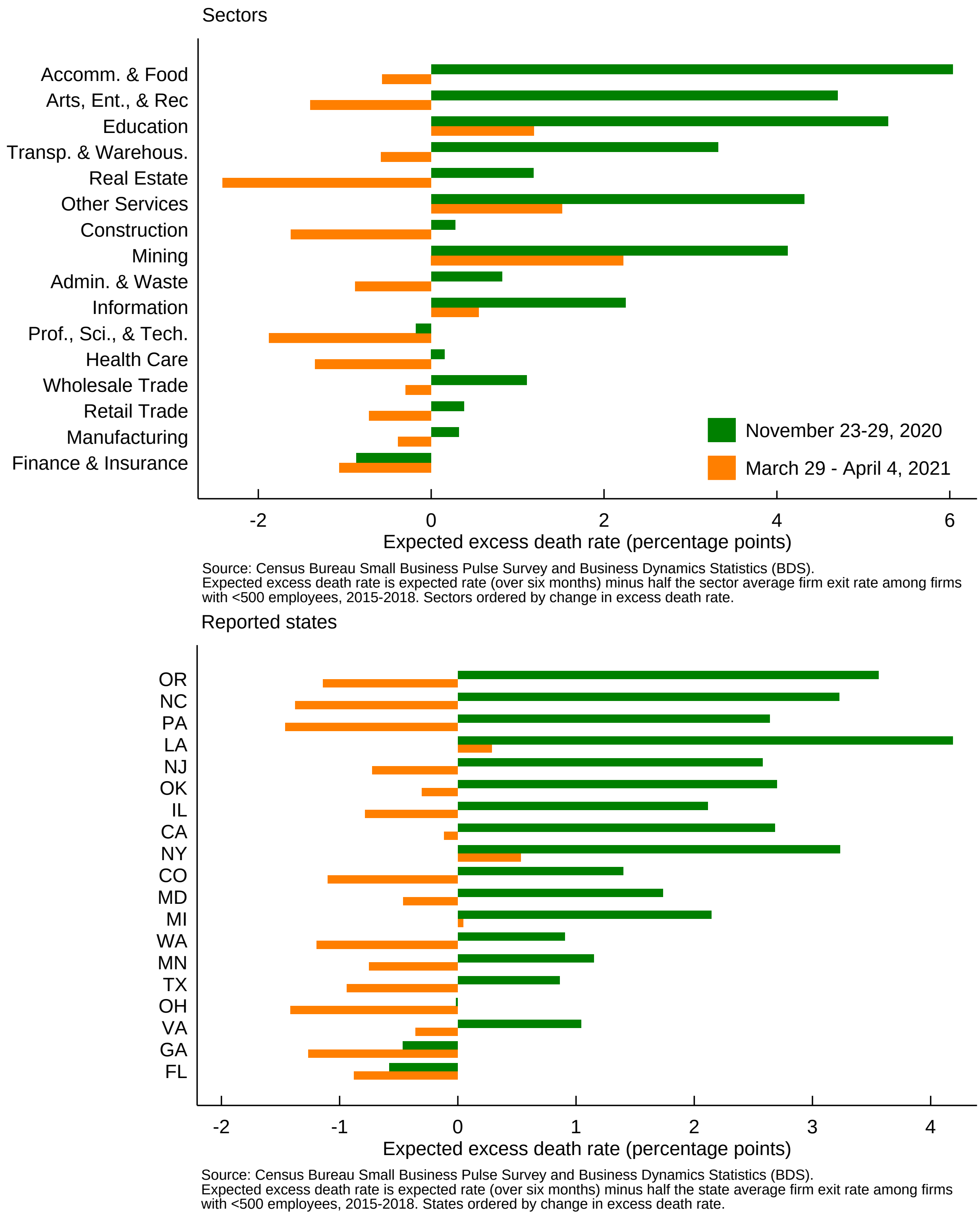


Figure C10: Excess exit expectations, peak versus early March 2021