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

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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

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Abstract

Given lags in official data releases, economists have studied "alternative data" measures of business exit resulting from the COVID-19 pandemic. Such measures are difficult to understand without historical context, so we review official data on business exit in recent decades. Business exit is common in the U.S., with about 7.5 percent of firms exiting annually in recent years, and is countercyclical (particularly recently). Both the high level and the cyclicality of exit are driven by very small firms. We explore a range of alternative measures and indicators of business exit, including novel measures based on payroll events and phone-tracking data, and find tentative evidence that exit has been elevated during 2020. Evidence is somewhat mixed, however, and exiting businesses do not appear to represent a large share of U.S. employment.

JEL codes: D22, E32, C55, C81

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1 Introduction

A prominent concern arising from the COVID-19 recession of 2020 is that permanent business death could be widespread, with a range of lasting consequences for the U.S. economy. But business death is difficult to measure in real time since official statistics on business dynamics are released with substantial lags; for example, Bureau of Labor Statistics (BLS) data on establishment deaths during mid-2020 will become available in late 2021, 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 a number of private sector indicators of business stress and shutdown. While the wide range of indicators made available by private firms have enhanced policy discussions and improved understanding of 2020 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 in recent decades, providing a set of stylized facts that are necessary for evaluation of alternative indicators of business shutdown. We then discuss a range of alternative indicators and what they suggest about business exits during 2020 thus far.

Official measures of business exit provide valuable historical context on which to evaluate alternative indicators. These statistics reveal business death to be a common occurrence, with about 7.5 percent of firms exiting annually in recent years. 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—those with fewer than 5 employees—though larger firms often permanently close individual establishments (locations) as part of geographic or industry restructuring.

Alternative indicators of exit during 2020, on balance, suggest that exit has been elevated at least among small firms and particularly in industries most sensitive to social distancing. For example, we estimate that the permanent exit rate of restaurants has been elevated by about 50 percent, relative to historical rates, during 2020. Other businesses are faring better, however, and in some sectors exit *expectations* are actually below historical exit rates. Importantly, our evidence at this point is only suggestive: some indicators do not point to elevated exit, and others appear to be returning to historical rates such that the story could change in coming months.

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 exit indicators from ADP payroll data and a new permanent business exit measure based on SafeGraph cell phone geolocation data. We also review data on small business operations (from Womply and Homebase) as well as business electricity user accounts, commercial real estate vacancies, Google Trends for search terms "going out of business", small business loan delinquencies and defaults, and Census Bureau survey data on business exit expectations.[1] As we show, these alternative measures are most useful in the context of historical patterns of business exit.

[1] We also discuss the popular Yelp estimates of permanent business closure.

A key challenge is distinguishing between temporary shutdown and permanent shutdown (death). U.S. statistical agencies provide data on business deaths, but identifying deaths is more difficult in alternative data sources.[2] 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 deaths based on how long businesses have been inactive. We use the terms "death" and "exit" interchangeably to refer to likely permanent shutdown. We note, however, that the line between temporary and permanent shutdown may be blurry at times, as even temporary shutdown may have significant economic implications if it is prolonged or results in substantial business restructuring before reopening.

A handful of papers study business closure early in the COVID-19 episode. Bartik et al. (2020b) surveyed firms in late March and early April and found temporary closure rates around 40 percent with permanent closure rates around 2 percent. Fairlie (Forthcoming) exploits Current Population Survey (CPS) data on self-employment and finds that the number of operating businesses in June was down 8 percent since March (note that we limit our focus to employer businesses). Wang et al. (2020) report that 2020 bankruptcy filings by small businesses through August were significantly *lower* than in prior years, a topic also explored by Greenwood et al. (2020). Bartik et al. (2020a) and Kurmann et al. (2020) measure early business closures in Homebase data, and Chetty et al. (2020) measure early closures in Womply data. Dalton et al. (2020) study closures through August in Current Employment Statistics (CES) microdata. Ongoing measurement of business closure is critical—evidence suggests a large number of early closures were temporary (Cajner et al. (2020))—as is comparison with historical patterns.

We first provide general background on the importance of business exit, drawing from 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 direct, but nontraditional, measures of business shutdown during 2020 in Section 4, then we survey indirect measures—including data on small business expectations—in Section 5. While our inferences about 2020 are tentative and do not lead to strong conclusions, we take stock and conclude in Section 6.

2 Background

Business death is a routine phenomenon in healthy market economies. Existing literature shows that death, as part of a broader set of business dynamics patterns, enhances aggregate productivity as lower-productivity exiting businesses are replaced by higher-productivity

[2]Permanent business death can be difficult to define and measure even in official data; for example, Sadeghi (2008) shows that establishment death numbers depend materially on how long an establishment must be inactive to count as a death (see Charts 10 and 11 therein).

entrants.[3] These patterns also release resources, both employment and capital, to be used more efficiently elsewhere. 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., Fort et al. (2018)). However, business exit imposes profound costs on workers and business owners. Moreover, the COVID-19 recession raises unique concerns about the costs of potential business deaths during 2020.

Business exit implies permanent job destruction, potentially detaching workers from the labor market and limiting the speed of the employment recovery.[4] 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—such as 2020—is more concerning. Relatedly, business exit destroys the match-specific capital formed by a firm's relationship with its workers. As documented in Fujita and Moscarini (2017), a large share of workers return to their former employers after being separated ("recall hires"). Business exit eliminates this recall option, and potentially implies longer unemployment spells for workers that must form new employment relationships.

The pandemic-induced recession also raises concerns about whether exit can be as productivity enhancing as it has been in the past. In a pandemic recession, patterns of business exit may be driven by the geographic, industrial, and temporal onset of severe infection outbreaks rather than business productivity; therefore, we might expect even many high-productivity businesses to fail during the COVID-19 episode, potentially implying negative effects on aggregate productivity. In a broader sense, exit selection—–the relationship between business productivity and exit—–has been weakening for decades (Decker et al. (2020)), and patterns of business exit were less productivity enhancing during the Great Recession than in previous recessions (Foster et al. (2016)). For these reasons, we might expect exit selection to be less healthy in 2020 than in the past. Moreover, to the extent that the industry restructuring witnessed in 2020 (e.g., the decline in leisure & hospitality employment with gains in pandemic-friendly industries) is temporary, business exits that occur in declining industries may be especially wasteful.

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 (see Cooper and Haltiwanger (2007)). From the perspective of business owners, the exit of a firm means not only the loss of a job and career disruption but also potentially the destruction of household wealth. And from the perspective of local economies, widespread business deaths may permanently alter the economic geography of neighborhoods and communities.

[3]For theoretical considerations around the role of exit in aggregate productivity see, e.g., Hopenhayn (1992) and Hopenhayn and Rogerson (1993). For empirical explorations see, e.g., Bartelsman et al. (2013), Decker et al. (2017), Decker et al. (2020), Foster et al. (2016), Foster et al. (2001), Foster et al. (2006), and Syverson (2011).

[4]Exit is a significant contributor to overall job destruction. In Census Bureau BDS data for 2015-2018, establishment exit accounted for 32 percent of annual gross job destruction, while firm exit accounted for 17 percent.

3 Historical patterns of business closure and death

Both the Bureau of Labor Statistics (BLS) and the Census Bureau publish official statistics on business closure and death. The BLS publishes *establishment* exit data through the quarterly Business Employment Dynamics (BED) product.[5] These data are based on the state and federal unemployment insurance data underlying the Quarterly Census of Employment and Wages (QCEW) product, cover the near-universe of U.S. private business establishments, and start in the early 1990s. The BLS provides two measures of business shutdown: establishment "closures" are establishments that had positive employment in the previous quarter but zero employment (or no reported employment) in 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.[6] 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) and ability to distinguish between firm and establishment deaths.

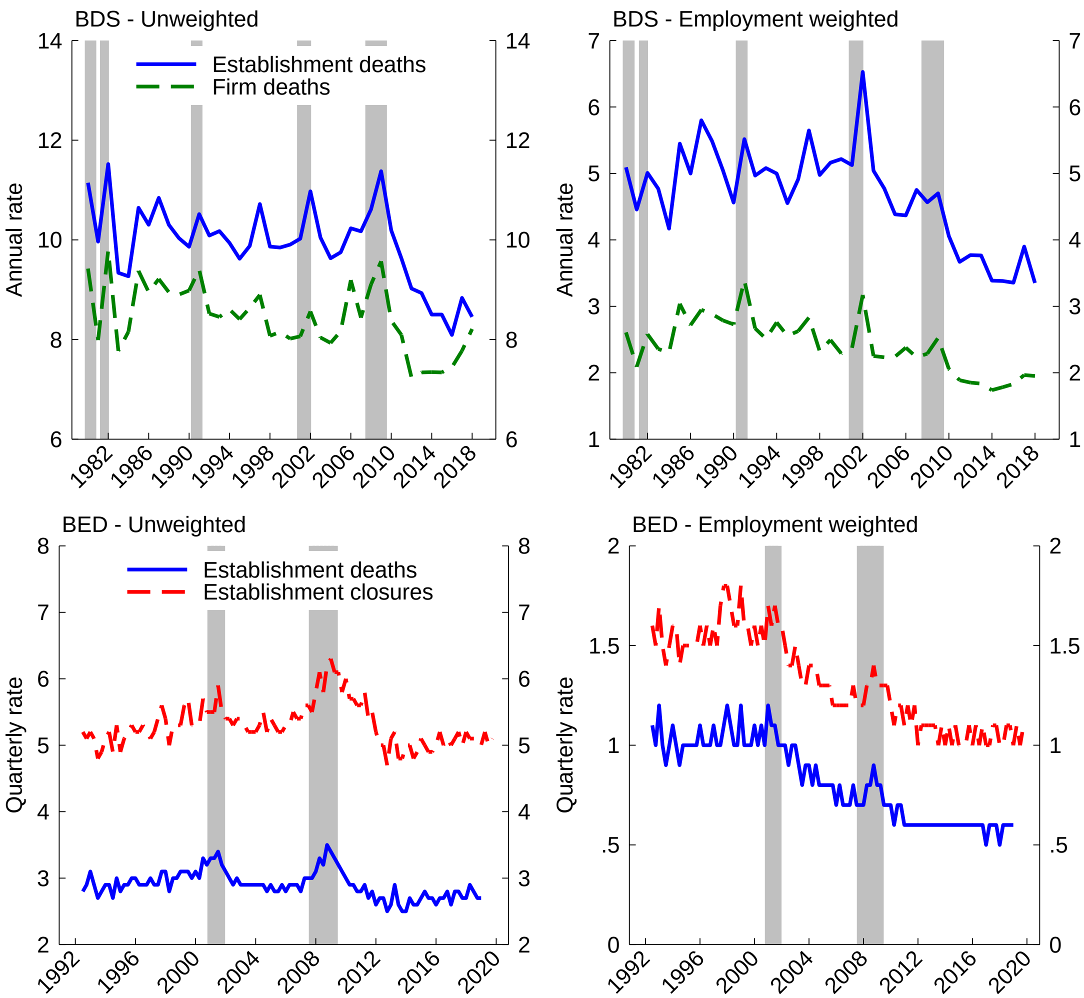
Figure 1 reports official data on business closures and deaths in recent decades. The top panel reports annual firm and establishment death rates from the BDS, with unweighted death rates (deaths as a share of establishments) on the left panel and employment-weighted death rates (employment at deaths as a share of employment) on the right panel. The bottom panel reports quarterly establishment closure and death rates from the BED (seasonally adjusted), again with unweighted rates on the left and weighted rates on the right.[7]

Figure 1 shows that business shutdown and death are common occurrences. In recent years (since 2014), annual firm death rates have been around 7.5 percent of firms, while establishment death rates have been around 8.5 percent at the annual frequency or just over 2.5 percent at the quarterly frequency. 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 we will explore further below). 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 roughly 1.5 percentage points from the previous expansion low). Finally, a comparison of quarterly deaths and quarterly closures in BED data indicates that temporary

[5] 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.

[6] We use the current BDS redesign vintage introduced in September 2020. In the appendix we report differences between the 2020 BDS vintage and the legacy vintage; our broad conclusions about historical exit patterns are apparent in both vintages. BDS data are based on the Census Bureau's Business Register, which covers nearly the universe of private business establishments in the U.S. and underlies the County Business Patterns (CBP), Statistics of U.S. Businesses (SUSB), and Longitudinal Business Database (LBD).

[7] In all panels we use Davis et al. (1996) (DHS) denominators, where the current and previous quarters' or years' values are averaged. In the appendix, we repeat the correlation and regression calculations that follow using the lag (i.e., initial) values only as denominators. Our main stylized facts are unaffected by this specification choice.



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

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.

We can more clearly observe the countercyclicality of business shutdown with some simple correlations, reported on Table 1. We compare our firm and establishment shutdown measures—detrended with linear trends—with the change in the unemployment rate (at quarterly frequency for comparisons with BED measures and annual frequency for comparisons with BDS measures).[8]

Table 1: Business cycle correlations

	Unemployment		GDP	
	Unweighted	Weighted	Unweighted	Weighted
BED: Establishment closures (BLS)	.34	.22	-.41	-.12
BED: Establishment deaths (BLS)	.37	.32	-.45	-.20
BDS: Establishment deaths (Census)	.38	.15	-.32	.00
BDS: Firm deaths (Census)	.12	.02	-.19	-.04

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 growth of GDP on BDS annual timing (April-March). BED data cover 1992q3-2019q1 (deaths) or 1992q3-2019q4 (closures). BDS data cover 1980-2018.

Simple correlations are mostly consistent with countercyclical 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 (suggesting that smaller units drive exit cyclicality), and firm death is less countercyclical than establishment death.[9] Employment-weighted firm death is basically acyclical in these specifications, which is intuitive if large firms rarely exit; however, this finding is specification dependent, as alternative time periods and detrending approaches produce some cyclicality of employment-weighted firm exit.[10]

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

[8] 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) are difficult to interpret given persistence of unemployment rates in the aftermath of recessions.

[9] Correlations of the *level* of establishment deaths or firm deaths (rather than death *rates*) with the change in unemployment are generally even stronger than the rate correlations. 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 Economic Censuses. The previous vintage of BDS data displayed somewhat different patterns; see the appendix for extensive discussion Census year and vintage issues.

[10] In the appendix, we show a version of this table based on the detrending methodology of Hamilton (2018). Most of the results are robust to this methodological choice; however, we find that the omission of data from the early 1980s—which are omitted from detrended data under the Hamilton (2018) methodology—does somewhat affect firm-based correlations.

during the post-2002 period, thereby avoiding the need to detrend the exit series (and also avoiding the potentially problematic 2002 observation, discussed more in the appendix). Table 2 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 2: Business cycle comovement: States

	Establishment death		Firm death	
	Unweighted	Weighted	Unweighted	Weighted
Change in unemp	0.56***	0.22***	0.36***	0.11***
	(0.03)	(0.02)	(0.02)	(0.01)
Observations	816	816	816	816
State FE	Yes	Yes	Yes	Yes

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.[11] 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. In the appendix we show that results are similar without state fixed effects. Taken together with Table 1, the data consistently show that exit is countercyclical, particularly in recent years.[12]

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 2, which plots annual establishment death rates (left panel) and firm death rates (right panel), both reported by *firm* size categories.

The smallest firms—–those with fewer than 5 employees—–exit at rates typically around 12 percent, markedly higher than any other firm size classes.[13] 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-

[11] The 2003-2018 period included in the Table 2 estimates includes several Economic Census years (2007, 2012, and 2017), but the coefficients are little changed if Census years are omitted. Separately, coefficients are still positive and significant without state fixed effects. See the appendix for these results.

[12] See Clementi and Palazzo (2016) for a model-based exploration of exit countercyclicality.

[13] 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.

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 2: Business death rates by *firm* size

highest 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.[14] The figure also makes apparent that the rise in business death during the Great Recession was driven in large part by small firms.

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, or 2 percent of employment.
- Annual establishment exit rates have been around 8.5 percent in recent years, or 3.5 percent of employment.
- Average quarterly establishment death rates have been 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.
- Temporary business closure is common, affecting about 2 percent of establishments per quarter.

[14]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.

4 Has COVID-19 sparked a surge in business death? Direct measures

The critical question in 2020 is whether the recession induced by the COVID-19 pandemic has sparked a surge in business deaths. Official data on business exit are released with a lag: BED data on establishment *closures* for the first quarter of 2020 will be available in October 2020; closure data for the second quarter will be released early in 2021. BED data on establishment *deaths*, which require four quarters of closure, will be available a year later. BDS data on *firm* deaths during 2020 will (presumably) not be available for a few years, as currently available data extend only through March of 2018.[15] In the meantime, we must rely on alternative measures of business activity to assess the magnitude of business exit during 2020. We now describe several such measures.

4.1 ADP

Our most direct measures of business closure come from providers of business services from which we can observe, nearly in real time, indicators of business activity. 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.[16] 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 of these data is that 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.

In the ADP data, we observe paycheck issuance events at the business level. Pay frequency varies by business—some pay weekly, biweekly, or monthly—so 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 experience to the average experience from recent years (2015-2019); this allows us to abstract from typical patterns of ADP client turnover and determine the extent to which business shutdown has been elevated in 2020 relative to normal. We begin in mid-February 2020 and, for each week thereafter, we tally up the share of businesses that were operating in February but are in the midst of a shutdown period. We compare this share to the same-week average for the 2015-2019 period. In all cases, we apply sampling weights to ADP payroll units from the QCEW (with weights in terms of NAICS sector and establishment size).[17]

[15] If the Census Bureau releases BDS updates annually as in the past, data covering the pandemic months of 2020 will not be available until 2023.

[16] 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.

[17] We also take the 2-week trailing moving average of all series to smooth through volatility associated with varying pay frequencies across businesses.

The top-left panel of Figure 3 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.

Importantly, businesses that issue paychecks at a monthly frequency count as shutdowns according to the blue line, as they typically issue paychecks more than 25 days apart. Staying in the top-left panel of Figure 3, the red line uses a more stringent criteria for measuring shutdowns, reporting the 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. The red line indicates that 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; if permanent shutdown were elevated among ADP data businesses, we would expect to see it in this line at least. While the share of businesses shut down for 70 days or more exceeded historical averages by about 2 percentage points in early May, it has since returned to normal.

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. Moreover, the top-right panel of Figure 3 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 criteria (25 days, 36 days, or 70 days) then calculate their employment share based on their February 15 employment as a share of total February employment. Hence, the top-right panel of Figure 3 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 peaked in late April/early May 2020, when businesses inactive for at least 25 weeks accounted for a share of February employment that exceed past years 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 actually lower than average.

The differences between the top-left and the top-right panels of Figure 3 suggest that the elevated shutdown in 2020 relative to past years has been driven largely by smaller units. We can see this more clearly in the bottom-left panel of Figure 3, which shows (unweighted) shutdown rates in 2020 relative to 2015-2019 averages, separated by business size (we focused on shutdown spells of at least 70 days). The black line shows that shutdown rates among the largest units—those with at least 500 employees—have been similar to the pace of previous years, if not even a bit lower. Smaller units saw significantly elevated shutdown rates in late April/early May 2020, but by August all business sizes saw shutdown rates below historical

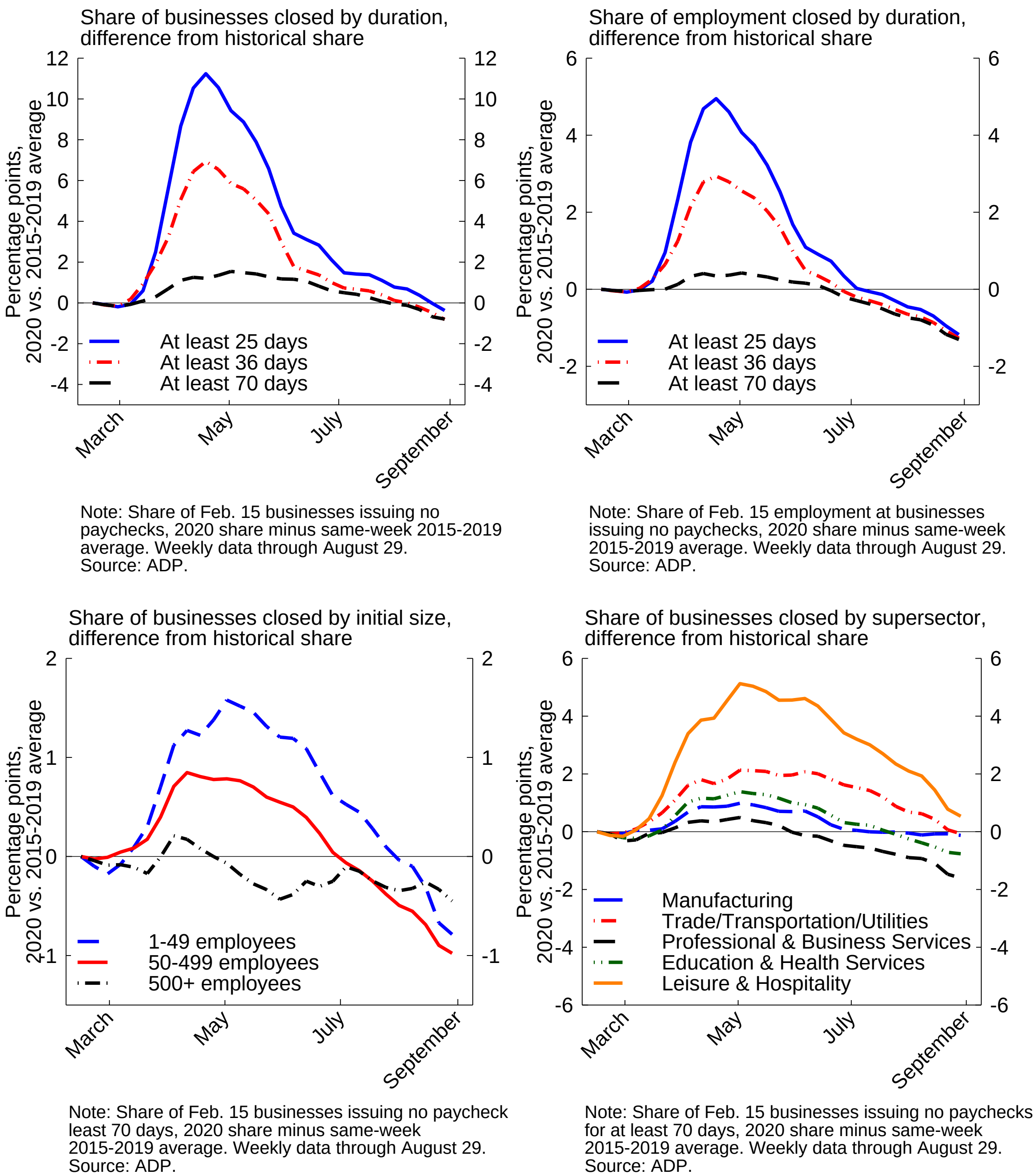


Figure 3: Measures of Business Closure from ADP Payroll Data (2020 relative to 2015-2019 average)

patterns.

Shutdown patterns do vary some across sectors. The bottom-right panel of Figure 3 shows unweighted 70-day shutdown rates for selected supersectors. The leisure & hospitality supersector (NAICS 71-72) has had historically high shutdown rates throughout the pandemic, with shutdown rates exceeding past patterns by more than 5 percentage points in early May. By late August, however, shutdown among leisure & hospitality businesses was only modestly elevated relative to previous years, while shutdown activity was below average in other supersectors. Professional and business services (NAICS 54-56) is doing particularly well.

Taken at face value, the ADP data show that business shutdown was elevated during the middle of the year but has since returned to normal or even below-normal rates, even among small establishments. This is a striking result, since the ADP data are reasonably comprehensive in terms of coverage across sectors and establishment size classes (see Cajner et al. (2018)) and allow us to study how shutdown activity is affecting employment; the measures we will review below are more limited in at least some of these dimensions. As such, the ADP data provide some good news, suggesting that permanent shutdown is not materially elevated as we enter Fall 2020 and providing detail on the relative success of different sectors.

Importantly, however, ADP data can be affected by patterns of client turnover in addition to true business shutdown. It is possible that during 2020, ADP has been especially successful at client retention for various reasons, which would appear in our data as lower-than-usual shutdown rates. Moreover, ADP data, like other client-based databases, are subject to unobservable selection around which businesses choose to engage ADP for payroll services. For these reasons, it is critical that we add other data sources to our study.[18]

4.2 Small business trackers

We next turn to popular measures of small business activity, shown on Figure 4. The left panel reports data from Womply, a credit card transaction processor, on the share of firms that have ceased processing point-of-sale transactions since mid-February.[19] The right panel reports 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 (and, conveniently, we can observe 2019 data for Homebase as well).[20] In both cases, the sample

[18]Additionally, as noted above, some ADP payroll units are closer to firms than establishments. As such, we would expect relatively low exit rates in ADP data relative to pure establishment-based measures.

[19]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 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, though it is unclear from Womply documentation whether this is accurate.

[20]Homebase provides clock-in/clock-out software for small businesses and can therefore observe employment activity in close to real time. Homebase data have been a good predictor of official CES monthly job gains since the job recovery began in May; see Reinicke (2020). As of early 2020, Homebase data included over

of businesses is restricted to those that were operating in February, abstracting from entry into the sample (consistent with our ADP-based exercises above). Entry into these datasets may be less costly than entry into ADP data; for example, many Homebase clients use a free tier of the service, which implies different selection dynamics than may be present in ADP 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 reopened in May and June. Still, the recent observations indicate that shutdown is indeed elevated in 2020. Homebase data suggest that shutdown in well-covered industries is currently elevated by roughly 5 percentage points relative to the same time in 2019. Moreover, Womply data are qualitatively consistent with ADP in showing more shutdown activity in leisure & hospitality than other sectors.

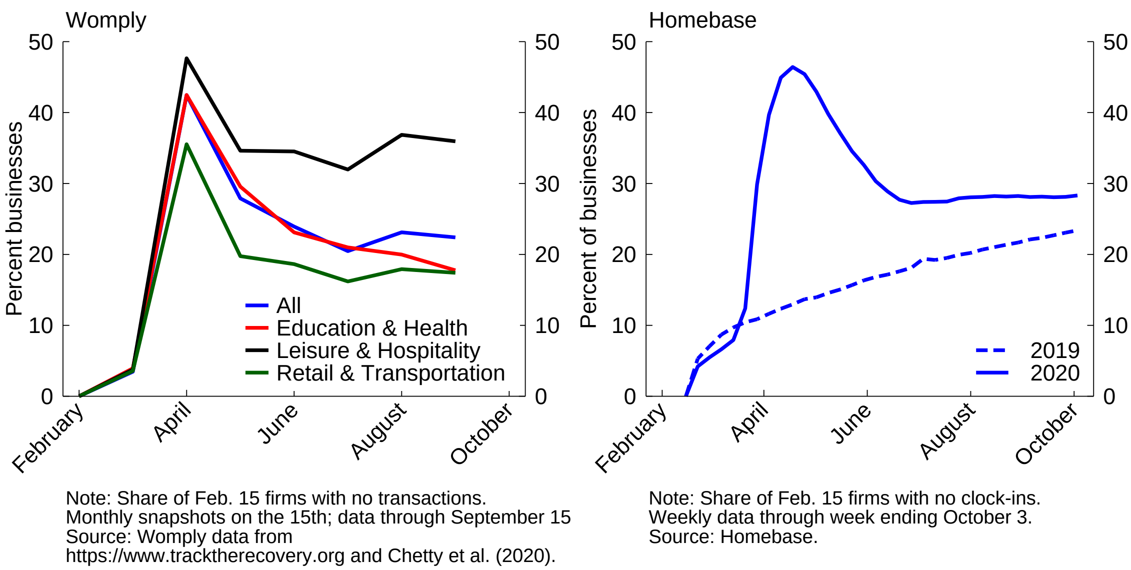


Figure 4: Small consumer business closures

Importantly, a limitation that is common to ADP, Womply, and Homebase data is the possibility that exit patterns are driven by client 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. This makes the Womply data particularly difficult to interpret; and, indeed, exit appears far more elevated

60,000 establishments with about 500,000 (hourly) employees. Coverage is concentrated among very small establishments (mostly those with fewer than 20 employees) in service industries that happened to be particularly affected by social distancing (coverage is best in accommodation and food services—NAICS 72—where Homebase has almost 2.5 percent of all U.S. establishments with fewer than 50 employees). We aggregate Homebase establishment data to the firm level using their (anonymized) company identifier. See Kurmann et al. (2020) for extensive detail on Homebase representativeness, and see http://joinhomebase.com/data for more details on Homebase data.

in Womply than in our other sources, suggesting that normal attrition may be higher in Womply data.[21] Moreover, in the Homebase data, attrition rose steadily during 2019 such that, if current trends continue, 2020 closures appear on track to converge with the 2019 pace. As such, these data are suggestive of, but do not provide unambiguous confirmation of, elevated exits during 2020. We next turn to a measure that is not subject to the particular limitation of client attrition.[22]

4.3 SafeGraph

We construct a final direct measure of business shutdown based on information from SafeGraph, a data company that aggregates anonymized location data from numerous mobile device applications in order to provide insights about physical places.[23] The company has temporarily made their micro-level data available to researchers and government agencies studying the impact of COVID-19. The location data from roughly 45 million mobile devices is linked to a registry of around 6 million points of interest nationwide to record, at a daily frequency, individual visits to these points of interest.[24]

In the appendix we describe a methodology that translates patterns of consumer visits to business locations into indicators of temporary closure and likely permanent establishment exit (these indicators are best thought of as establishment, not firm, indicators since they are based on business operating locations).[25] Figures 5 and 6 present these measures for sit-down restaurants, an industry that is well-suited to this methodology with good coverage in SafeGraph data.[26]

[21] As noted on the Womply website, businesses that change their method of accepting payment will appear as closures in Womply data; for example, restaurants that close on-site dining and do business via third-party delivery services will appear as closed. This may be partly responsible for the particularly high closure rate in leisure & hospitality.

[22] It is also possible that patterns of client churn may be time-varying; for example, if providers of business services made extra efforts to retain clients in 2020, a surge in true exits could be hidden by a lower pace of client attrition.

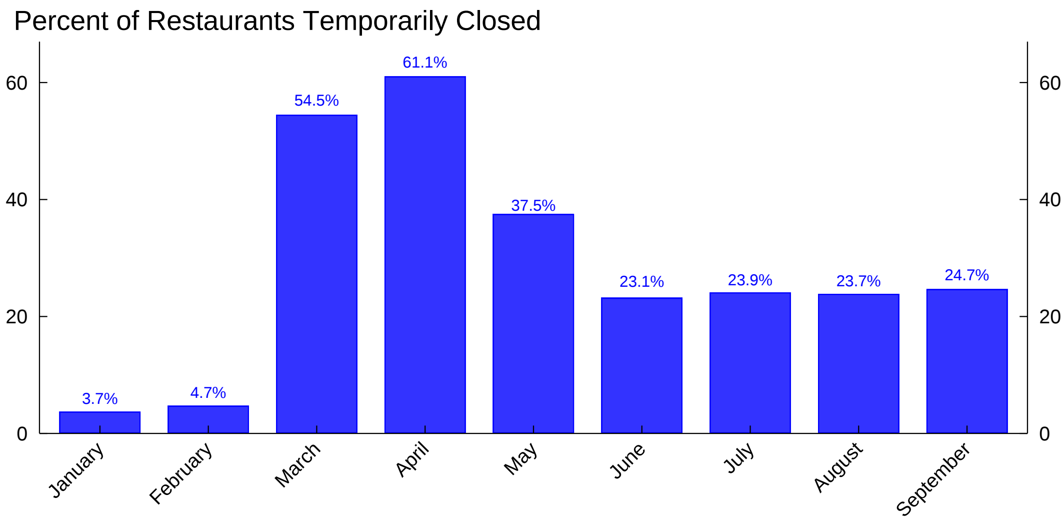
[23] To enhance privacy, SafeGraph excludes census block group information if fewer than five devices visited an establishment in a month from a given census block group.

[24] 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. While there are certain industry scope differences between the BLS and Census business registers, a discrepancy exists even when the two sources are restricted to common scope (see Barnatchez et al. (2017)).

[25] Of course, 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 constructions 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 to business visits at a stationary location, a situation that applies to many retail and service businesses.

[26] Importantly, our measure is 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 Figure B1). Carry-out or delivery service still requires a visit to the business address, which is the basis of the concept of SafeGraph visits.

Figure 5 shows estimates of restaurants that were closed——at least temporarily—during each month of 2020, based on severe drops in consumer visits relative to normal patterns for that establishment. This fraction was low, around 4-5 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. It is notable that a significant fraction of restaurants were even closed as of September, 2020. The significant share of closures identified later in the sample could be comprised of restaurants responding to renewed social distancing policies put in place during the summer, restaurants that have permanently closed, or restaurants that are in fact open but operating at extremely low levels of activity.



Source: Author calculations from SafeGraph microdata.
Note: Temporary closure defined in Appendix.

Figure 5: Temporary restaurant closures 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 6; these are restaurants that closed during the indicated month and had still not reopened by the end of August. The monthly estimates for the short benchmark period of January-February of 2020 (0.2 to 0.4 percent) are only slightly below the monthly rate of exit for restaurants cited above from BDS data (0.7 percent).[27] The monthly exit rate jumps in March to around 3 percent of restaurants and then increases sharply again in July and August. By August the cumulative 2020 exit rate of restaurants based on this measure (the black line in Figure 6) is around 12 percent, already well above the annual rates seen seen in official data in recent years (around 8 percent).

A concrete way to demonstrate the validity of the signal from this measure is to study variation in business closure across two geographies with significant differences in the contour of COVID-19 cases and social distancing policies: New York City and the state of

[27]BDS data for NAICS 7225 indicate recent (2015-2018) annual establishment death rates of about 8 percent, implying monthly rates of a bit under 0.7 percent.

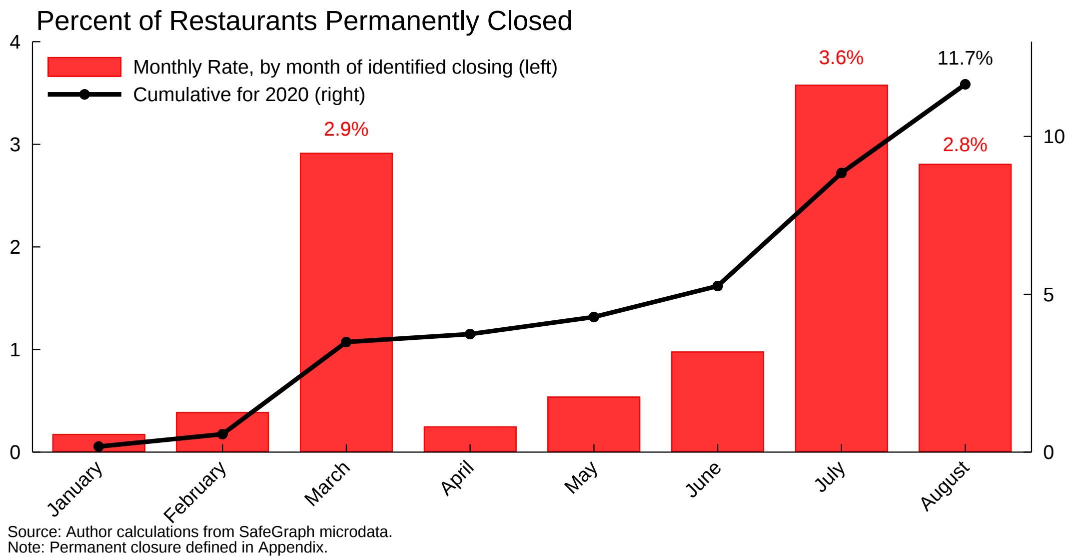


Figure 6: Permanent restaurant closures inferred from weekly visits

Oklahoma.[28] Panel A of Figure 7 shows the early spike in cases in New York City as well as the much later jump beginning in July in the state of Oklahoma. Panel B of Figure 7 shows one measure of social distancing—also derived from SafeGraph data—which identifies the share of mobile devices remaining close to home in a given day. This figure shows much larger and more pervasive changes in behavior regarding social distancing practices in New York City than in Oklahoma.[29]

The left panel of Figure 8 shows, perhaps unsurprisingly, that the relative magnitudes of temporary restaurant closure for New York City and Oklahoma align with the experience of cases and social distancing practices. Close to 80 percent of restaurants appeared to be temporarily closed in New York City during the month of April, whereas this figure was significantly below the national average in Oklahoma. The right panel of Figure 8 plots the cumulative share of restaurants that are permanently closed. The blue line indicates that close to 25 percent of sit-down restaurants in New York City could be permanently closed as of August.[30] This cumulative share is considerably lower in Oklahoma; however, the monthly rate has picked up in the latter months of the 2020 summer, which could align with the increase in cases of COVID-19 shown above.

[28] It is worth noting that identified visits in dense urban locations such as New York City may be subject to greater measurement error. SafeGraph uses a variety of tools to separate out visits from dense POIs; however, occasionally a POI will be excluded from data on visits due to being completely enclosed within another POI.

[29] These social distancing practices are, of course, also influenced by official regulations in each geography. For example, inside dining was not allowed in New York City through the end of September, whereas restrictions on dining for Oklahoma were minimal.

[30] Restrictions on inside dining in New York City were lifted on September 30; hence, it is possible that these numbers could revise based on the our methodology for forecasting revisions described in the Appendix.

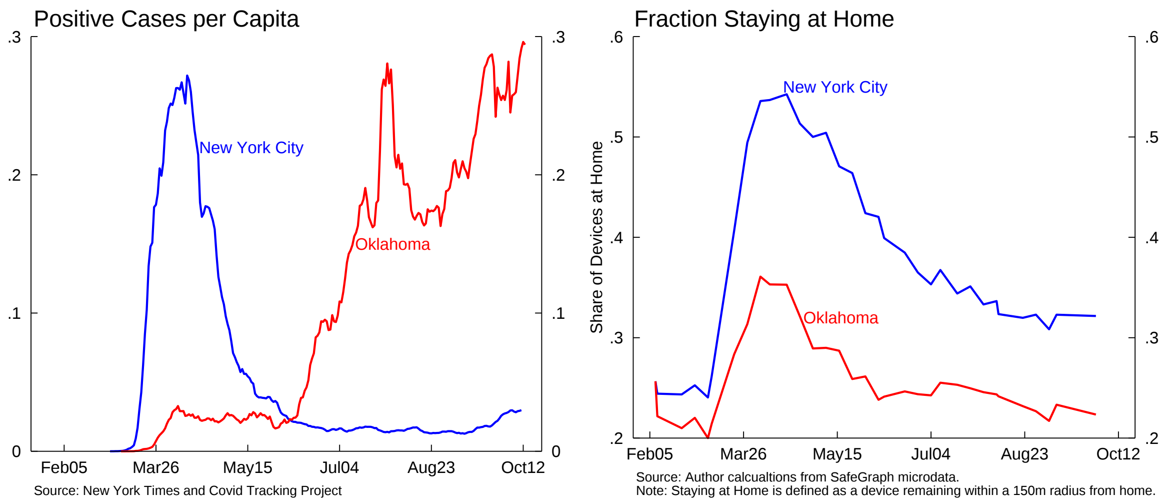


Figure 7: Cases per Capita and Social Distancing: New York City vs Oklahoma

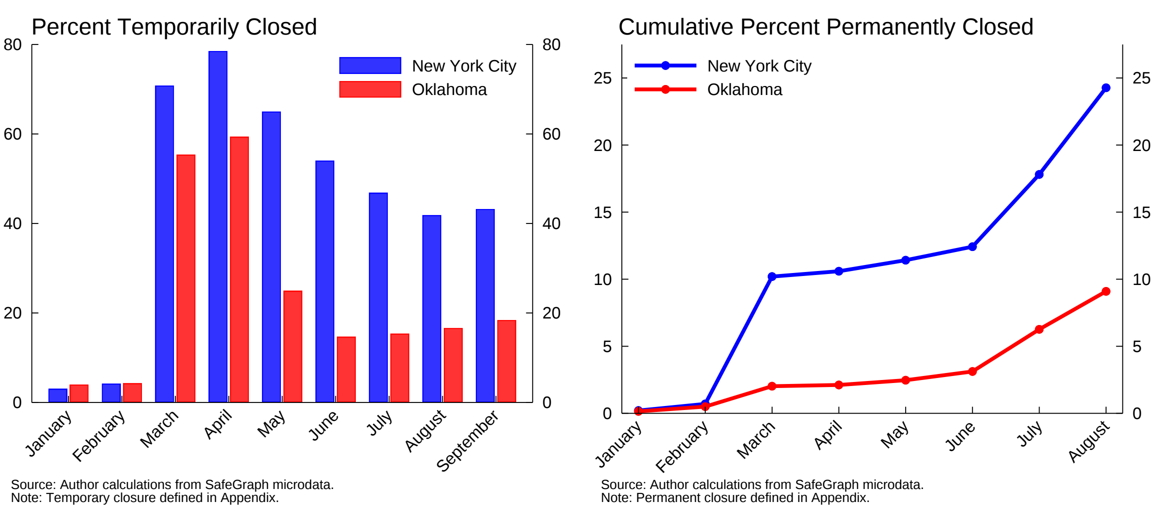


Figure 8: Restaurant Closures: New York City vs Oklahoma

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 but does face other measurement limitations—appears to show elevated exit rates for restaurants that align with the effects from COVID-19. The data suggest annual restaurant exits running at a pace about 4 percentage points—or 50 percent—higher than recent history. 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. In ongoing work in progress, we are expanding this methodology to other industries.

4.4 Yelp

Yelp, the online platform for rating businesses, provides shutdown data including permanent shutdown estimates, but we find these difficult to interpret in their current form.[31] For example, Yelp reports the highest permanent closure rates among restaurants selling burgers and sandwiches, where estimated permanent closure rates from March through August were around 3.5 percent. On an annualized basis, this would imply exit rates around 7 percent, still below recent restaurant exit rates of around 8 percent. Other industries reported by Yelp show exit rates that are even lower relative to historical patterns. Given the general economic deterioration during 2020 and the indicators we describe above, it seems unlikely that actual exit rates are below historical averages as Yelp data suggest. That said, Yelp data may be useful to studying differences across industries. For example, Yelp indicates the lowest exit rates among lawyers, architects, and accountants, consistent with ADP data on the relative success of professional and business services industries.

5 Indirect indicators of business shutdown

Data from ADP, Womply, Homebase, SafeGraph, and Yelp described above can be thought of as *direct* measures of business shutdown as they provide information on the status of business operations. Figure 9 reports a variety of *indirect* indicators that are suggestive of business distress or shutdown that may yet provide clues about troubles facing businesses. Conveniently, for these measures we can report a longer history allowing comparison with the Great Recession.

The top-left panel of Figure 9 reports the number of U.S. commercial and industrial electricity users from the Energy Information Administration (EIA); a surge in business exits would presumably result in a drop in electricity accounts, but no such decline is apparent during 2020.[32] The top-right panel reports commercial real estate vacancy rates from Costar, a provider of real estate analytics; the office, retail, and industrial vacancies rose markedly during the Great Recession and are all elevated in 2020 (See Costar Group, Inc. (2020)).

[31] See *Yelp: Local Economic Impact Report* (2020).

[32] Electricity account data available from the EIA Electricity Data Browser at https://www.eia.gov/electricity/data/browser/. We thank Josh Blonz for this idea and Jacob Williams for data assistance.

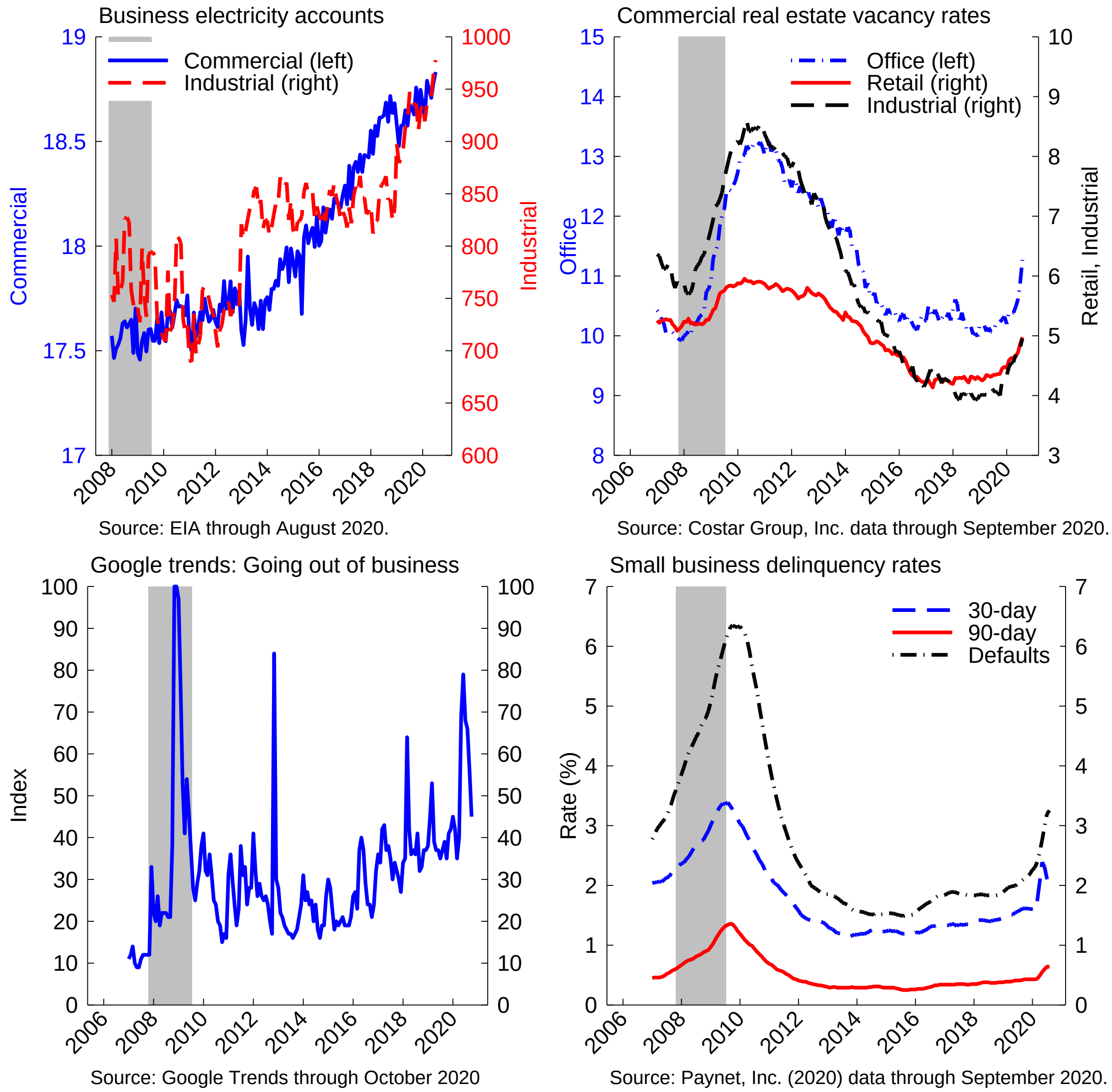


Figure 9: High-frequency indicators of business distress

While office vacancies may include vacancies arising from transitions to work-from-home arrangements, other types of vacancies, such as vacant retail spaces on the first floor of office buildings, may appear in the office vacancy series as well. Notably, commercial vacancies appear to have begun rising even before the onset of COVID-19, but the increase has been historically steep during 2020.

The bottom-left panel of Figure 9 reports Google Trends data on searches for the phrase "going out of business," which surged during the Great Recession and have clearly reached elevated levels at various points during 2020. The bottom-right panel shows measures of small business credit stress from Paynet, a provider of lending and banking analytics.[33] Defaults and delinquencies have risen sharply 2020.[34]

Fortunately, while most of the indirect indicators shown on Figure 9 are clearly elevated in 2020, none of them are yet consistent with patterns seen during the Great Recession. The indicators suggest that exit is likely elevated but not particularly excessive given a recessionary environment.

That said, many currently operating businesses are concerned about exit risk going forward. Figure 10 reports data on small business *expectations* from the Census Bureau's Small Business Pulse Survey of September 20-26.[35] Overall, about 5 percent of businesses expect to close permanently in the next 6 months (the red vertical line). In aggregate, this pace of business closure may not be markedly elevated relative to historical patterns; the *annual* exit rate for firms with fewer than 500 employees averaged about 7.8 percent during 2015-2018.[36]

Importantly, the expected exit rates vary widely by sector, and expected exit rates *relative to historical averages* vary widely as well. Figure 11 shows Pulse Survey expected exit on an (approximately) annualized basis (i.e., double the rates shown on Figure 10), in blue, compared with average firm death rates among small firms (fewer than 500 employees) from BDS data from 2015-2018, in red. In some sectors, expected death rates do indeed imply historically elevated rates (i.e., the blue bars are longer than the red bars). This is most notable in education and accommodation and food services, but several other sectors appear to have elevated exit expectations as well.[37] 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 restaurants have already seen exit rates at about 50 percent higher than normal (as suggested by our SafeGraph analysis) and then experience exit rates at double the usual pace over the next six months (as suggested by Pulse data). On the other hand, some sectors have exit expectations close to, or even somewhat below, historical averages.

[33]See Paynet, Inc. (2020) and http://sbinsights.paynetonline.com/loan-performance/

[34]Google Trends monthly data taken from https://trends.google.com/trends/ with the search term "going out of business."

[35]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).

[36]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.

[37]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. Small businesses are present in all of these industries, and some of these industries may be materially affected by ongoing social distancing concerns. It is also possible that the extremely elevated exit expectations in education are partly noise.

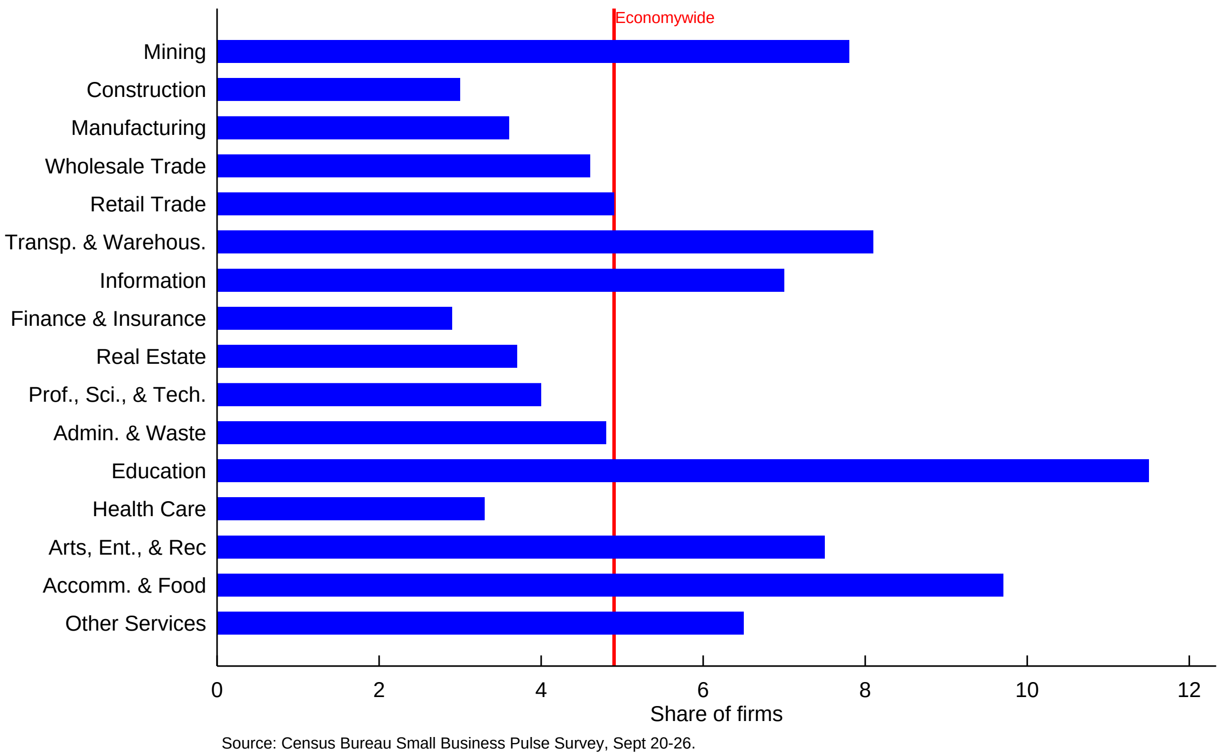


Figure 10: Share of small firms expecting to close permanently within 6 months

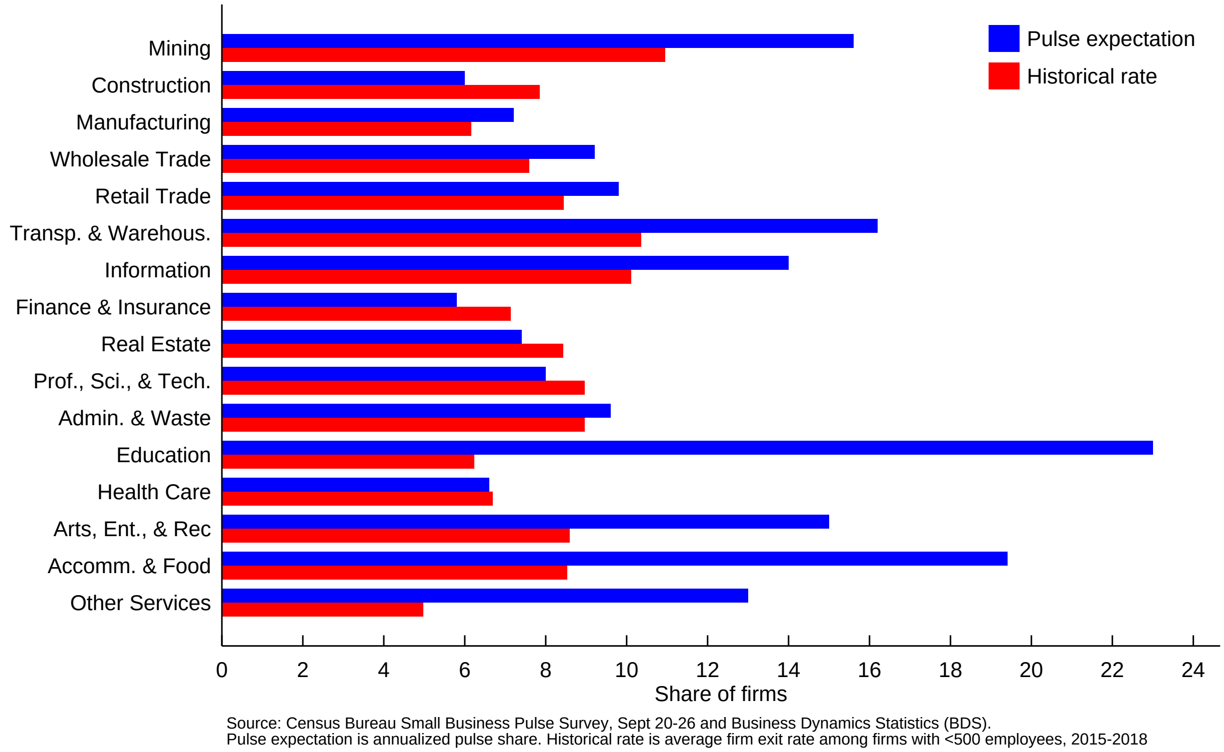


Figure 11: Closure expectations versus historical rates

6 Taking stock

The indicators shown above are not exhaustive. For example, data on bankruptcy filings may shed light on business closure rates, though many firms that shut down—particularly small firms—do so without declaring bankruptcy.[38] Comcast data on business internet accounts show historic declines in the first half of 2020 (Comcast (2020)). In ongoing work in progress, we are exploring other indicators.

Our analysis above does suggest that business exit is elevated or likely to be elevated in coming months. In particular, we find strong, direct evidence of increased business closures among restaurants (apparent in SafeGraph data) and suggestive direct evidence for related industries generally (apparent in Womply and Homebase data, with caveats noted above). For example, a restaurant exit rate around 12 percent through August, as indicated by SafeGraph, would already exceed recent years' rates by more than 4 percentage points, which would imply excess exits of more than 20,000 restaurant establishments.[39] We also

[38] Greenwood et al. (2020) describe the U.S. bankruptcy system and its capacity for handling a surge in filings. As the authors note, however, small firms do not typically utilize the business bankruptcy system when exiting, and since small firms account disproportionately for firm deaths, business bankruptcy data may provide little insight into overall business exit rates. See Wang et al. (2020) for further discussion of bankruptcy during the COVID-19 crisis.

[39] The BLS Quarterly Census of Employment and Wages (QCEW) for the first quarter of 2020 reports

find elevation of some indirect indicators: Google Trends, small business delinquencies and defaults, and retail vacancies.

Given various dataset-specific caveats mentioned in the main text, we view the evidence described in the previous paragraph as suggestive, not conclusive. Moreover, the ADP data suggest that any rise in business exits is not reaching larger business units or even enough smaller units to account for a material share of employment, which would be roughly consistent with past patterns of employment-weighted exit cyclicality. Some of the detrimental consequences of elevated exit—permanent job dislocation, potential productivity impacts if selection functions adversely, and the destruction of intangible and physical capital—may be negligible 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 implications even if exit is not substantial on an activity-weighted basis.

Looking ahead, exit expectations appear historically elevated among small businesses in mining, transportation & warehousing, information, education, leisure & hospitality, and other services (this latter sector includes many "local" businesses like beauty salons, auto mechanics, and churches). But some sectors appear set to do better than average, such as construction, finance & insurance, real estate, and professional, scientific, and technical services.

Whether the patterns we document will continue is difficult to tell; for example, a continued economic recovery could reduce exit rates, and if some deaths apparent in existing data were "pulled forward" from later this year then annualized exit rates could still converge to historical patterns (for example, the Homebase data in Figure 4 might be consistent with such a pattern). Early policy actions may have helped businesses in the relatively optimistic sectors to survive the worst of the pandemic and sustainably reopen.[40]

Alternatively, the situation could deteriorate further from here. Colder weather will render current service-industry adaptations (particularly outdoor dining) more difficult. More broadly, firms may be coasting on saved cash or funds received from government programs (e.g., the Paycheck Protection Program) that will soon run out. Large firms may have been taking time to implement restructuring plans that could result in waves of establishment exits or the exit of smaller suppliers. And most importantly, the ongoing pandemic may yet inflict unanticipated further damage on the U.S. economy.

577,220 privately owned restaurants, broadly defined (NAICS 7225); more narrowly, there were 252,847 full-service restaurants (NAICS 722511).

[40]Autor et al. (2020) estimate that the Paycheck Protection Program had a material effect, boosting June payroll employment by about 2.3 million.

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A Appendix for historical results

A.1 Linear detrending

In our main results for nationwide business cycle correlations, we detrend BED and BDS shutdown measures using simple linear trends. Table A1 repeats the results of Table 1 using the methodology of Hamilton (2018) instead; since the Hamilton (2018) loses the first three years of data from the detrended series, we also report correlations based on linear trends covering the same years. The results are largely unaffected by detrending methodology, though we observe some differences for firm-based measures. Importantly, the linear method correlations shown on Table A1 differ somewhat from those shown in Table 1 reflecting differences of time covered; in particular, Table 1 includes the years 1980-1982, which are omitted from Table A1. Our primary conclusions about the relative cyclicality of establishments and firms and the role of large versus small units appear strongest in more recent data (and can be clearly seen in state-level data for 2003-2018).

Table A1: Business cycle correlations: Hamilton (2018) detrending

	Unemployment		GDP	
	Unweighted	Weighted	Unweighted	Weighted
Hamilton (2018) method:				
BED: Establishment closures (BLS)	.38	.18	-.41	-.22
BED: Establishment deaths (BLS)	.37	.22	-.42	-.31
BDS: Establishment deaths (Census)	.33	.14	-.31	-.21
BDS: Firm deaths (Census)	.09	.09	-.16	-.27
Linear method:				
BED: Establishment closures (BLS)	.37	.19	-.46	-.15
BED: Establishment deaths (BLS)	.38	.28	-.49	-.23
BDS: Establishment deaths (Census)	.43	.26	-.35	-.12
BDS: Firm deaths (Census)	.20	.17	-.25	-.24

Note: Exit rates detrended following Hamilton (2018). BED correlated with quarterly change in unemployment rate or growth of GDP. BDS correlated with annual change in unemployment rate or growth of GDP on BDS annual timing (April-March). BED trends estimated on data for 1992q3-2019q1 (deaths) or 1992q3-2019q4 (closures); all correlations omit first three years of data consistent with Hamilton (2018) methodology. BDS trends estimated on data for 1980-2018; all correlations based on data for 1983-2018.

A.2 Exit rate denominators

In the main text, 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 Davis et al. (1996) in particular) 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, 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 A2 where we repeat our main correlations from Table 1 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).

Table A2: Business cycle correlations: Different denominator

	Unemployment		GDP	
	Unweighted	Weighted	Unweighted	Weighted
Establishment deaths (DHS)	.38	.15	-.32	.00
Establishment deaths (lag denominator)	.34	.08	-.29	.07
Firm deaths (DHS)	.12	.02	-.19	-.04
Firm deaths (lag denominator)	.07	-.05	-.14	.03

Note: Exit rates detrended linearly. BDS exit rates correlated with annual change in unemployment rate or growth of GDP on BDS annual timing (April-March). BDS data cover 1980-2018. "DHS" refers to the Davis et al. (1996) two-period averaged denominator. Compare to Table 1.

Business cycle correlations for unweighted exit rates are only slightly weaker under the lag denominator, though employment-weighted exit rates are clearly less cyclical. We repeat our state-level regressions (from Table 1) with the lag denominator on Table A3; these are almost identical to the results with DHS denominators.

Table A3: 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

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 2.
***denotes statistical significance with $p < 0.01$.
Source: Author calculations from Business Dynamics Statistics and BLS unemployment data.

A.3 Excluding noisy years

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 Economic Census years (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 appendix we briefly explore these limitations of the BDS.

We recreate the business cycle correlations in Table 1 omitting the year 2002 and omitting all Economic Census years; these can be found on Table A4. For ease of comparison, we repeat the correlations from 1 (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 A4: Business cycle correlations: Census year robustness

	Unemployment		GDP	
	Unweighted	Weighted	Unweighted	Weighted
Establishment deaths (all years)	.38	.15	-.32	.00
Establishment deaths (ex. 2002)	.34	.05	-.29	.12
Establishment deaths (ex. EC years)	.37	.09	-.32	.08
Firm deaths (all years)	.12	.02	-.19	-.04
Firm deaths (ex. 2002)	.11	-.08	-.18	.04
Firm deaths (ex. EC years)	.09	-.09	-.19	.01

Note: Exit rates detrended linearly. BDS correlated with annual change in unemployment rate or growth of GDP on BDS annual timing (April-March). "EC" is Economic Census (years ending in 2 or 7).
Source: Author calculations from Business Dynamics Statistics, BLS (unemployment rates), and BEA (GDP).

Table A5 repeats the state-level panel regressions shown on Table 2 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.

A.4 Additional state results

Table 2 reports panel regressions relating state-level exit rates and changes in unemployment rates with state fixed effects. Table A6 reports the same regressions without state fixed effects. The fixed effects make little difference. Moreover, in unreported results we find little effect of omitting Economic Census years from these pooled regressions.

Table A5: Business cycle comovement: States (excluding Census years)

	Establishment death		Firm death	
	Unweighted	Weighted	Unweighted	Weighted
Change in unemp	0.57***	0.23***	0.35***	0.11***
	(0.03)	(0.02)	(0.03)	(0.01)
Observations	663	663	663	663
State FE	Yes	Yes	Yes	Yes

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 2.
***denotes statistical significance with $p < 0.01$.
Source: Author calculations from Business Dynamics Statistics and BLS unemployment data.

Table A6: Business cycle comovement: Pooled states

	Establishment death		Firm death	
	Unweighted	Weighted	Unweighted	Weighted
Change in unemp	0.56***	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

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 2.
***denotes statistical significance with $p < 0.01$.
Source: Author calculations from Business Dynamics Statistics and BLS data.

A.5 2020 BDS redesign

Our main results feature the most recent vintage of the BDS, released in September 2020 after a substantial redesign. Relative to the previous BDS vintage, the redesigned product features extensive industry detail under consistent NAICS classification along 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.[41] 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 A1 shows establishment (top panels) and firm (bottom panels) exit rates, unweighted (left panels) and employment weighted (right panels).

These vintage differences also introduce differences in our business cycle correlations. Table A7 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. We also show these correlations omitting the years 1980-1982, since we showed previously that these years are material.

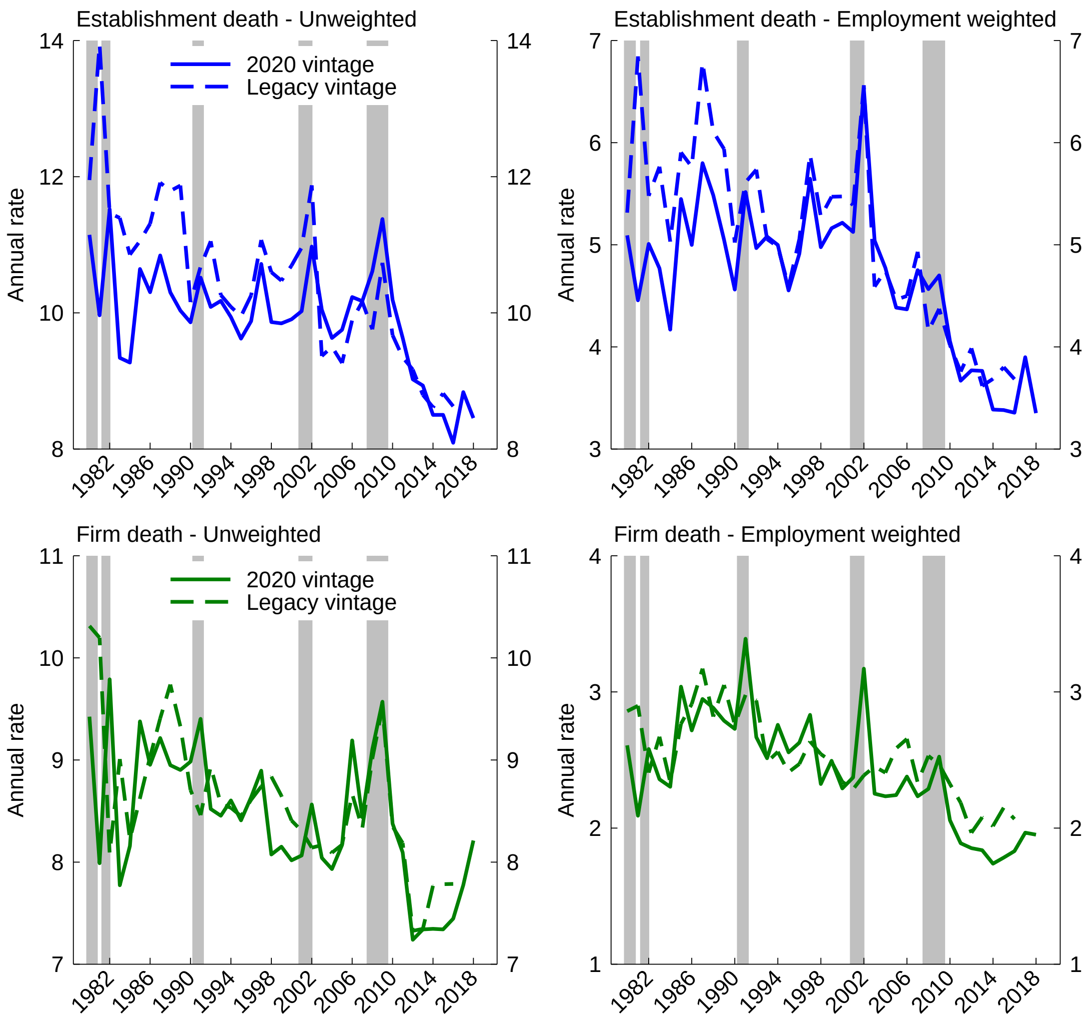
Table A7: Business cycle correlations: Vintage comparisons

	Unemployment		GDP	
	Unweighted	Weighted	Unweighted	Weighted
1980-2016				
Establishment deaths (2020 vintage)	.37	.14	-.32	.02
Establishment deaths (legacy vintage)	.38	.16	-.32	.00
Firm deaths (2020 vintage)	.13	.02	-.20	-.04
Firm deaths (legacy vintage)	.29	.15	-.28	-.10
1983-2016				
Establishment deaths (2020 vintage)	.42	.25	-.34	-.10
Establishment deaths (legacy vintage)	.32	.15	-.26	-.01
Firm deaths (2020 vintage)	.21	.18	-.27	-.23
Firm deaths (legacy vintage)	.32	.22	-.29	-.18

Note: Exit rates detrended linearly. BDS correlated with annual change in unemployment rate or growth of GDP on BDS annual timing (April-March). "EC" is Economic Census (years ending in 2 or 7).

These vintage differences are material, as are the early 1980s, for our quantitative estimates. On Table A8, we repeat our state-level regressions for vintage comparisons; our legacy vintage 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.

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



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 A1: Vintage differences in BDS data

Table A8: 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***	0.31***	0.09***
	(0.03)	(0.02)	(0.03)	(0.01)
Observations	612	612	612	612
State FE	Yes	Yes	Yes	Yes
Legacy vintage, 2003-2014 data				
Change in unemp	0.29***	0.04*	0.28***	0.05***
	(0.02)	(0.02)	(0.02)	(0.01)
Observations	612	612	612	
State FE	Yes	Yes	Yes	Yes

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.

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 2. 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.

B Data 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.[42] 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

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

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.[43] 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 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.[44]

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.[45] 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.

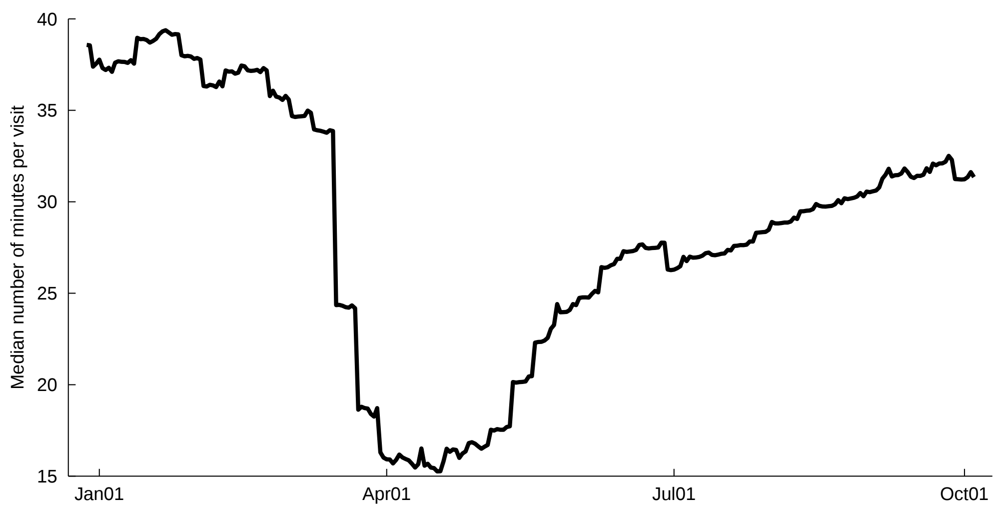
[43] This ability declines gradually, as it typically takes some time for SafeGraph to identify a business as closed using their traditional methods.

[44] 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.

[45] 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.

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 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.[46] 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 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.



Source: Author calculations from SafeGraph microdata.

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

[46] 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.

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 as contractors 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.

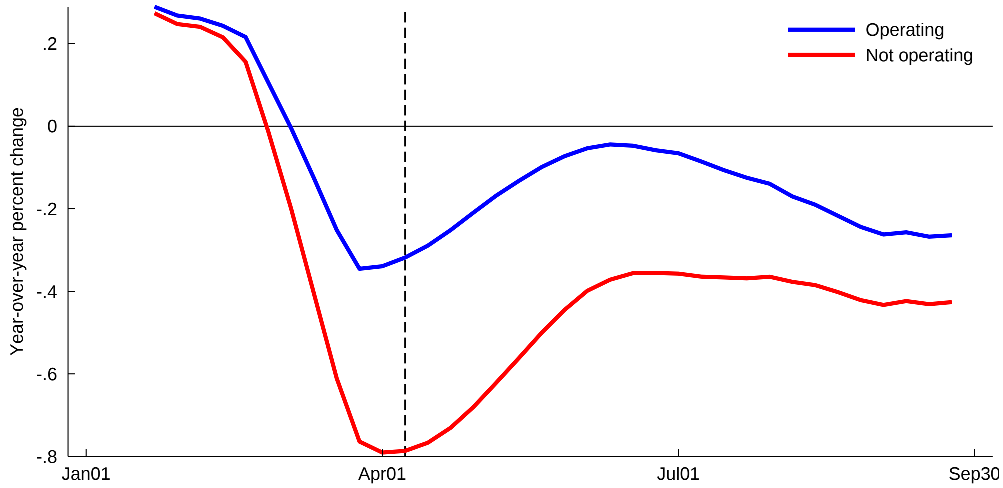
For each case of temporary and permanent closures, we rely on additional data provided by SafeGraph to discipline the definitions based on visits data.

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.[47] 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.

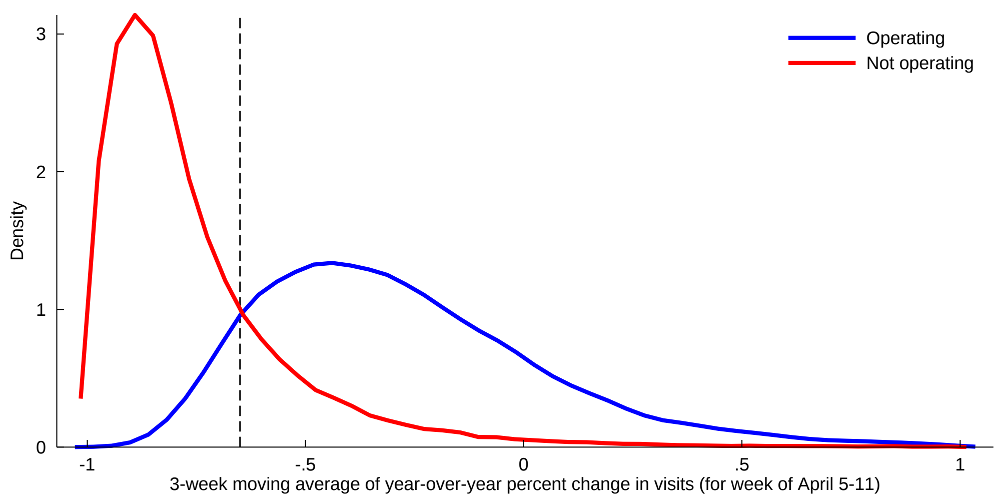
More formally, we calculate false positive (identifying closed establishments that are iden-

[47]Further information on this dataset is available here.



Source: Author's calculations using SafeGraph microdata.

Figure B2: Year-Over-Year Percent Change in Visits, by Operational Status Week of April 5-11



Source: Author's calcualtions using SafeGraph microdata.

Figure B3: Density of Year-Over-Year Percent Changes in Visits, by Operational Status, Week of April 5-11 tified as operational) and false negative (identifying open establishments that are identified as not operational) rates across a wide variety of parameter values.[48] Ultimately, the best 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. For a formal definition of temporary closure, a business i is temporarily closed in week n if $\widetilde{v}^t_{i,n} = 1$:

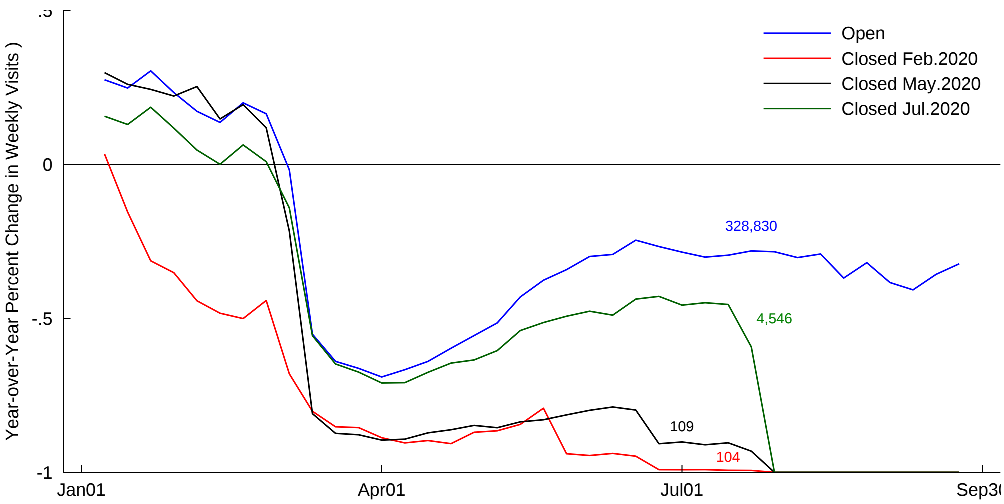
$$\widetilde{v}^t_{i,n} = \begin{cases} 1 & \text{if } \frac{1}{3}\sum_{j=n-1}^{n+1}\left(\frac{v_{i,j}-v_{i,j-52}}{v_{i,j-52}}\right) < -0.65 \quad \text{and} \quad v_{i,n} < 50 \\ 1 & \text{if } v_{i,n} \leq 4 \\ 0 & \text{if } \frac{1}{3}\sum_{j=n-1}^{n+1}\left(\frac{v_{i,j}-v_{i,j-52}}{v_{i,j-52}}\right)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

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.

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:

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

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



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

Figure B4: Weekly Visits by Identified Closure Status

We translate these measures to a monthly frequency t such that $\widetilde{v}_{i,t}^{t}$ 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.[49]

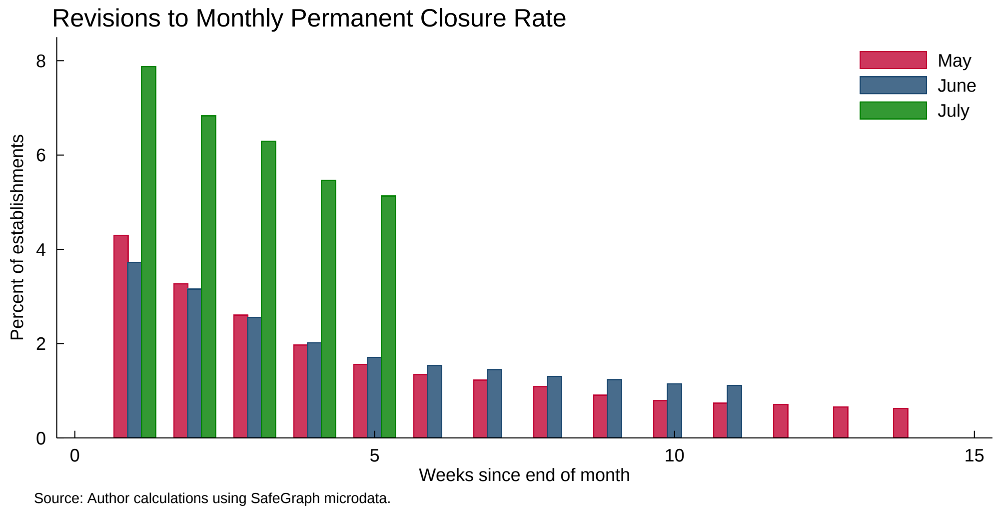
B.5 Alternate Thresholds

Figures B6 and B7 show measures of temporary and potentially permanent closures when using a threshold of 80 percent y/y visit declines.

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

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

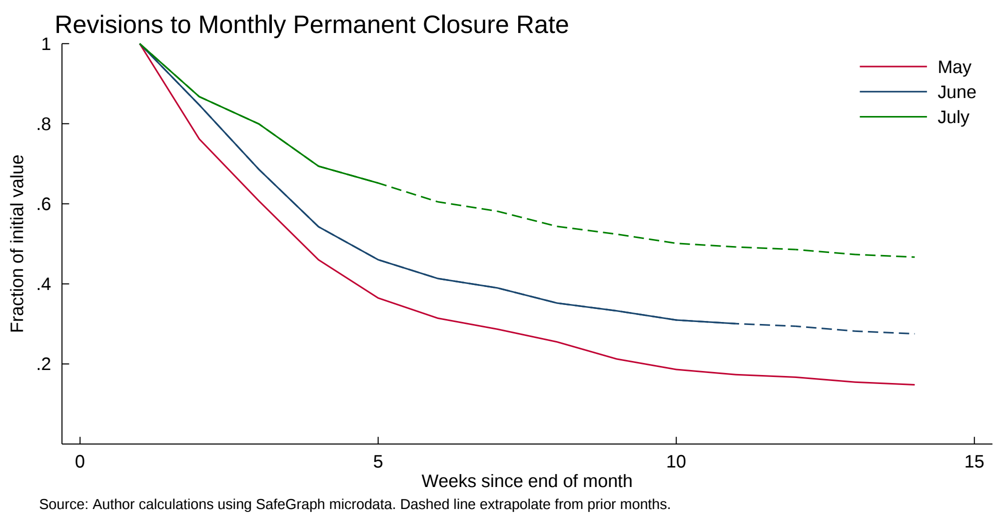


Figure B6: Temporary Closures: 80 Percent Threshold

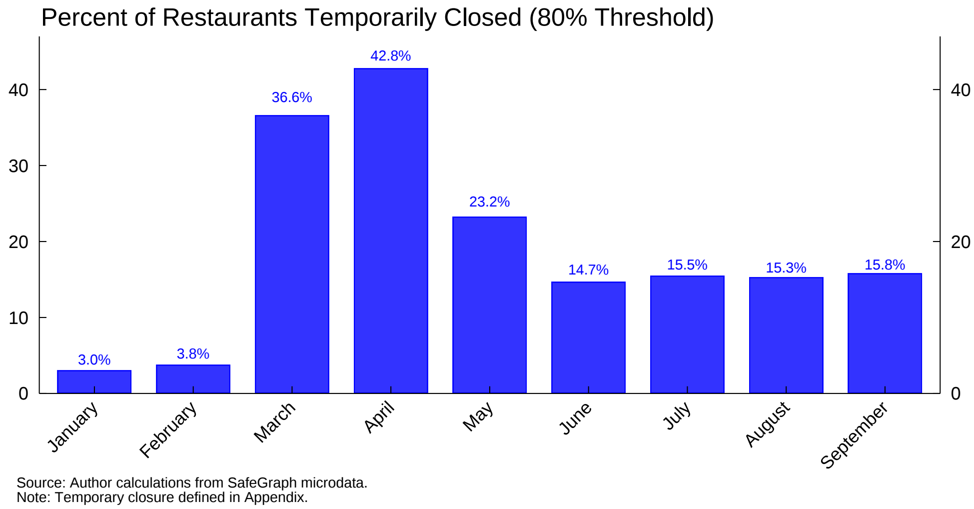


Figure B7: Potentially Permanent Closures: 80 Percent Threshold