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Measures in Historical Context**

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

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## 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 countercyclical and fairly common, with about 7.5 percent of firms exiting annually in recent years. Both the high level and the cyclical nature of exit are 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. The evidence is mixed, however; 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. Actual exit is likely to have been lower than widespread expectations from early in the pandemic. Moreover, businesses have recently exhibited notable optimism about their survival prospects.

JEL codes: D22, E32, C55, C81

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# 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 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 “alternative” or “non-traditional” data to measure business exit. For example, Figure 1 shows several popular indicators of business distress. 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 have jumped, but both “going out of business” search queries from Google and 30-day defaults have returned to trend after brief elevation.<sup>1</sup> 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 measures of business exit provide valuable historical context in which to evaluate alternative indicators. These statistics reveal business death to be a common occurrence, with about 7.5 percent of firms and 8.5 percent of establishments 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 cyclicity 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 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, other services (NAICS 81, which includes barber shops and nail salons), saw the permanent exit of more than 100,000 establishments *above and beyond historically normal*

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<sup>1</sup>Monthly 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. Commercial real estate daily vacancy rates are a simple average across Chicago, Dallas, New York, Los Angeles, and San Francisco from Costar, a provider of real estate analytics; see Costar Group, Inc. (2020). Google Trends monthly data taken from <https://trends.google.com/trends/> with the search term “going out of business.” Small business delinquency rates are from Paynet, a provider of lending and banking analytics; see Paynet, Inc. (2020) and <http://sbinsights.paynetonline.com/loan-performance/>.

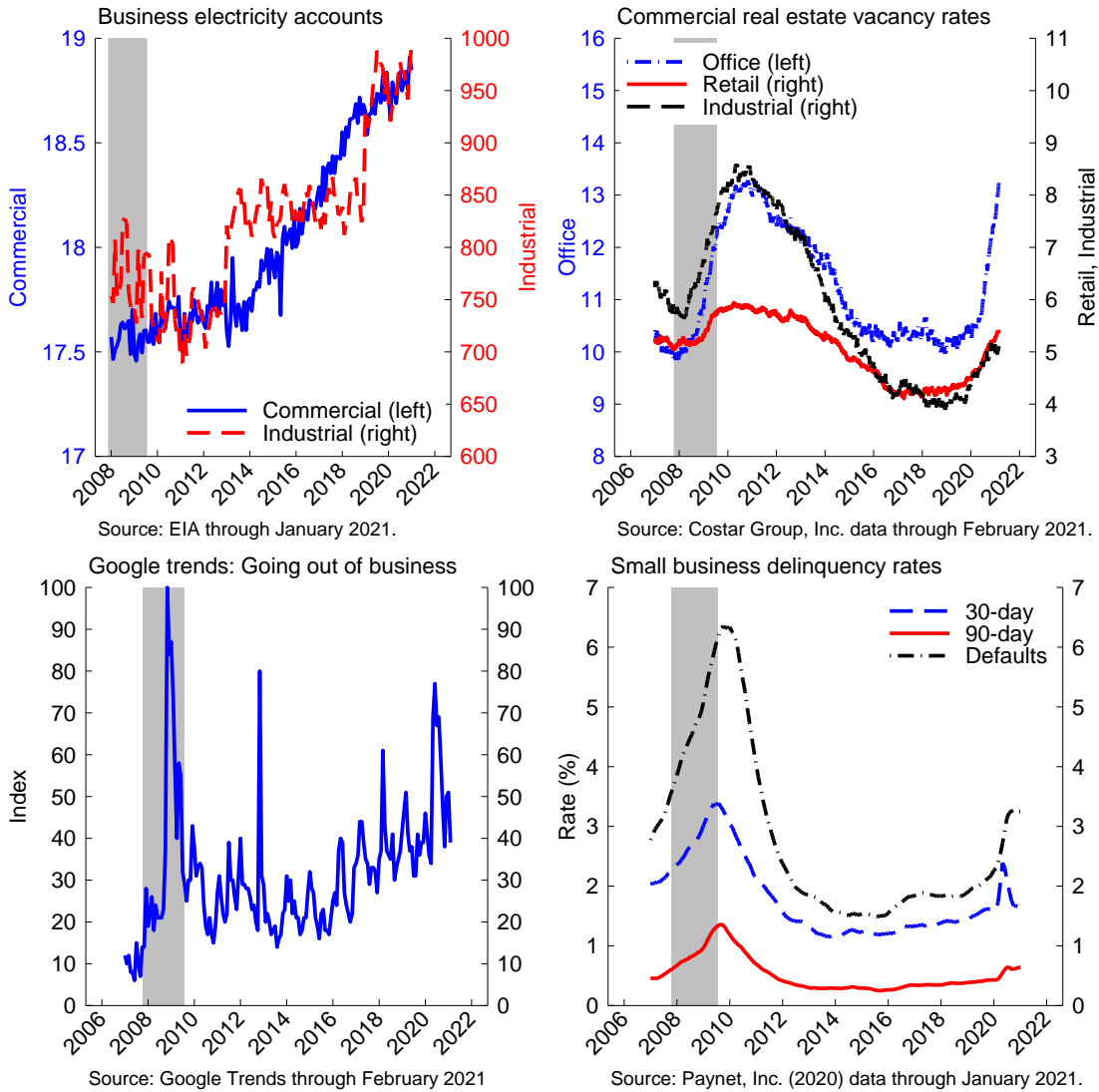


Figure 1: High-frequency indicators of business distress

*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 such that exit was not dramatically elevated in the sector overall. 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, so the sector overall likely saw only modestly elevated exit on net. Importantly, 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 calculations on official BED data through the second quarter of 2020, which suggest fewer than 150,000 excess establishment deaths during the first half of that year. We have less insight into *firm* exit, though given historical patterns 200,000 excess establishment exits would imply roughly 130,000 excess firm exits. Several sectors never saw elevated exit *expectations*, at least among small businesses, and as of March 2021 forward-looking firm exit expectations were actually below historical exit rates in most sectors. Relative to popular discussion, then, 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, which may lead us substantially to understate or overstate exit. Moreover, even as the pandemic ebbs, business exit risk may remain material as business owners face overdue credit bills, deferred rent, and other expenses that may have been delayed but not eliminated by pandemic norms and policies (see, e.g., Simon and Haddon, 2021; Gournichas et al., 2021).

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 permanent business exit measure based on SafeGraph cell phone geolocation data. We also review more commonly used data on small business operations (from Womply and Homebase) as well as Census Bureau survey data on business exit expectations.<sup>2</sup> 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 (death). U.S. statistical agencies provide data on business deaths, but identifying deaths is more difficult in alternative data sources.<sup>3</sup> 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 “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 pandemic. Bartik et al. (2020b)

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<sup>2</sup>We also provide brief discussion of more well-known private sector business registers like Yelp and Dun & Bradstreet.

<sup>3</sup>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).

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). Bartlett and Morse (2020) study business survival through April in the City of Oakland using survey data and other sources. 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. 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.<sup>4</sup> A particular challenge with earlier estimates is that a large number of early business closures may have been temporary, which is shown by Cajner et al. (2020) and corroborated in the data we describe below.

Dalton et al. (2020a) and Dalton et al. (2020b) are the only papers studying business closure in official business data; the authors use establishment microdata from the Current Employment Statistics (CES)—the data behind the widely followed U.S. monthly jobs report—along with firm information housed in internal BLS databases to study business closure through November 2020 by firm size and establishment industry. The authors find corroboration for the view that a large share of establishment closures during April and May were temporary, though they note that closure appears to have stabilized by July. They also find that establishment closure was far more elevated among small firms than among large firms (and more elevated among large multi-establishment firms than large single-establishment firms). As we argue below, the results in Dalton et al. (2020b) suggest that the share of employment associated with cumulative establishment closures through November 2020 was not dramatically above that of previous years, consistent with our findings. Interestingly, the authors find that about a quarter of job losses among restaurants are due to establishment closure, while the closure share is closer to half among drinking places.

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 official and non-traditional measures of business shutdown during 2020 in Section 4, then we survey data on small business expectations in Section 5. We take stock and conclude in Section 6.

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<sup>4</sup>The 400,000 figure has been cited not only by high-level government officials but also in academic literature, e.g., Wang et al. (2020). Data from the BED (reviewed below) suggest that, at most, 350,000 establishments—and even fewer firms—closed permanently during the second quarter of 2020.

## 2 Background on business exit

Business exit is a natural phenomenon in healthy market economies, and while the closure of weak businesses can be productivity enhancing, business exit imposes profound costs on, workers, owners, and communities. 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 entrants.<sup>5</sup> 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., Davis et al., 2014; Fort et al., 2018).

The pandemic-induced recession raises concerns about whether exit can be as productivity enhancing as it has been in the past. In the current 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, 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 during the pandemic than in the past.<sup>6</sup> 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 implies permanent job destruction, potentially detaching workers from the labor market and limiting the speed of the employment recovery.<sup>7</sup> 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 the pandemic period—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

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<sup>5</sup>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. (2001), Foster et al. (2006), Foster et al. (2016), and Syverson (2011). Exit, and firm turnover more generally, have been linked to industry cost structure since industries with lower sunk costs exhibit higher firm turnover (both theoretically and empirically); see Gschwandtner and Lambson (2012) and citations therein.

<sup>6</sup>Additionally, policies designed to save businesses and jobs may have saved businesses that would have exited had the pandemic never occurred, a source of potential misallocation that may result in additional exit in the near future; for example, see Gournichas et al. (2021).

<sup>7</sup>Exit is a significant contributor to overall job destruction. In Census Bureau Business Dynamics Statistics data for 2015-2018, establishment exit accounted for 32 percent of annual gross job destruction, while firm exit accounted for 17 percent.

workers that must form new employment relationships.

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 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* closure and death data through the quarterly Business Employment Dynamics (BED) product.<sup>8</sup> 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 nonfarm business establishments with formal employees, and start in the early 1990s.<sup>9</sup> 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.<sup>10</sup> 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.

#### 3.1 Levels and cyclicity

Figure 2 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 un-

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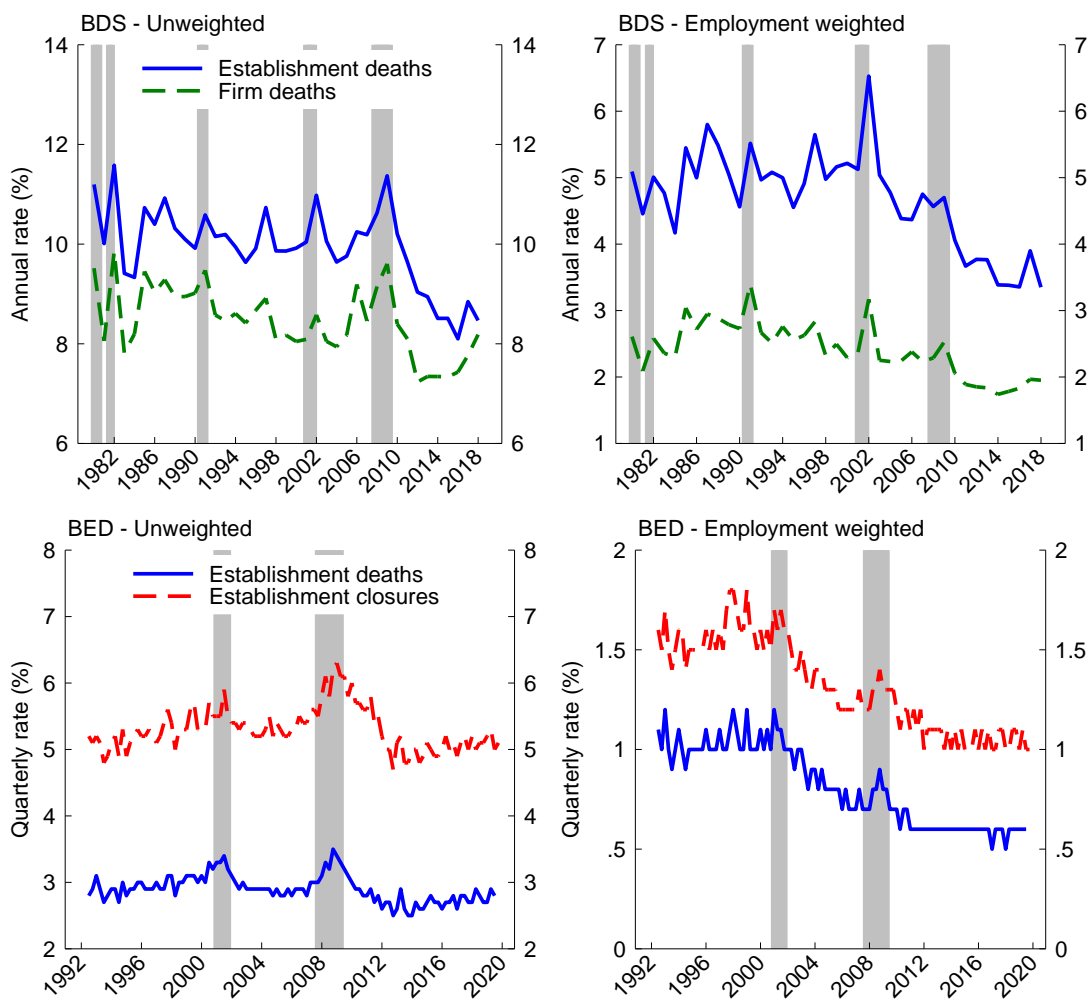
<sup>8</sup>An “establishment” is a single business operating location (with few exceptions). A “firm” is a collection of one or more establishments under common ownership or operational control.

<sup>9</sup>The main omissions are railroads and some nonprofits that do not participate in state unemployment insurance systems.

<sup>10</sup>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 nonfarm business establishments with employees (the main exceptions are railroads and certain financial entities).



weighted 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.<sup>11</sup>



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 2: Historical patterns of business shutdown

<sup>11</sup>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). 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.

Figure 2 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 the annual frequency (about 600,000 per year) or just over 2.5 percent at the quarterly frequency.<sup>12</sup> 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).<sup>13</sup> Figure 2 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 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 and real GDP growth (at quarterly frequency for comparisons with BED measures and annual frequency for comparisons with BDS measures).<sup>14</sup>

Table 1: 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).

<sup>12</sup>Quarterly rates need not aggregate to annual rates due to short-lived establishments or potential discrepancies between BDS and BED data.

<sup>13</sup>The BLS BED program also publishes quarterly employment-weighted *firm closure* rates, not shown on Figure 2, which have averaged about 0.7 percent per quarter in recent years. The BLS defines a firm based on tax identifiers, while the Census Bureau definition is based on overall ownership or control.

<sup>14</sup>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.

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 (again suggesting that smaller units drive exit cyclicity), and firm death is less countercyclical than establishment death.<sup>15</sup> The specifications in Table 1 show consistent countercyclicity 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 for exit measures in which firm exit is less countercyclical.<sup>16</sup>

In appendix Table A1 we detrend exit measures with the method of Hamilton (2018) and find broadly similar results, though firm exit correlations with GDP are weaker under this alternative detrending approach. In appendix Table A2 we report results specifying the GDP component of the correlations using detrending methods of Hamilton (2018) and Hodrick and Prescott (1997), in which countercyclical exit is still evident though the results for firms versus establishments and weighted versus unweighted exit are less clear cut. In appendix Table A12 we report correlations of exit with industrial production, another business cycle concept, and find that exit is countercyclical with respect to this indicator as well.

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 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).

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 cyclicity) and firm exit being less cyclical than establishment exit.<sup>17</sup> 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 1, the data consistently show that exit is countercyclical, particularly in recent years.<sup>18</sup>

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<sup>15</sup>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 show in appendix Table A3. Separately, as can be seen from Figure 2, 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; see the appendix for analysis of Census year and vintage issues.

<sup>16</sup>We intentionally begin the BDS-based sample in 1984 for Table 1 and all other results unless otherwise noted. Firm countercyclicity is highly sensitive to specific years included in the 1980-1983 period. In the appendix Table A8 we report correlations for the entire 1978-2018 period covered by the current BDS vintage, in which firm exit countercyclicity is nevertheless apparent.

<sup>17</sup>The 2003-2018 period included in the Table 2 estimates includes three Economic Census years (2007, 2012, and 2017), but the coefficients are little changed if Census years are omitted (see appendix Table A7). Separately, coefficients are similar in regressions without state fixed effects or with both state and year fixed effects (see appendix Table A9).

<sup>18</sup>See Clementi and Palazzo (2016) for a model-based exploration of exit countercyclicity. Lee and Mukoyama (2015) document the relative cyclicity of entry and exit of manufacturing plants.

Table 2: Business cycle comovement: States

	Establishment death		Firm death	
	Unweighted	Weighted	Unweighted	Weighted
Change in unemp	0.56*** (0.03)	0.22*** (0.02)	0.37*** (0.02)	0.11*** (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.

### 3.2 Exit by firm and establishment size

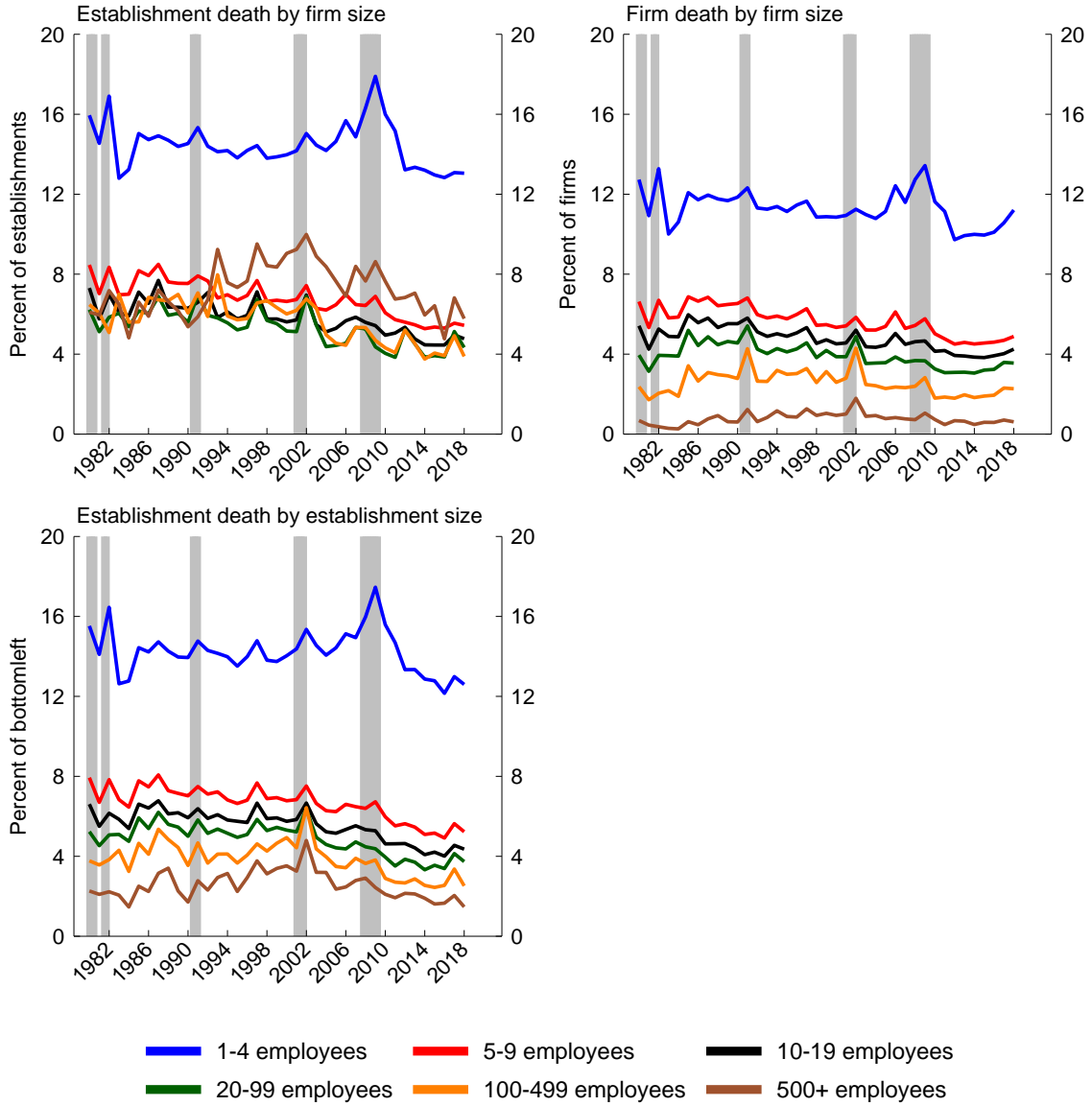
As noted above, the differences between unweighted and weighted exit rates suggest that business exit is concentrated among smaller units, in terms of both overall exit rates and the cyclicity of exit. This is made clear by Figure 3, which plots annual establishment death rates (left panels) and firm 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.<sup>19</sup> 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.<sup>20</sup>

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. More broadly, Figure 3 makes apparent that the rise in business death during the Great Recession was driven in large part by small firms and establishments.

<sup>19</sup>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.

<sup>20</sup>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 3: Business death rates by firm and establishment size

### 3.3 Stylized facts about business shutdown

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 countercyclicity 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 for the second quarter of 2020, but BED *deaths* for that quarter will not be available until fall of 2021. BDS data on *firm* deaths during 2020 will (presumably) not be available until 2023, as currently available data extend only through March of 2018.<sup>21</sup> In the meantime, we must rely on some guesswork along with non-traditional measures of business activity to assess the magnitude of business exit during 2020. We now describe several such measures.

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<sup>21</sup>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 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. Statistical agencies face significant challenges when measuring business exit (and entry), and this paper is in no way a criticism of the agencies for the lags with which these data are released. Administrative source data are complicated and require careful processing, particularly when longitudinal aspects are important (as is the case for business exit). Moreover, in the past business exit has not typically received the intense research, policy, and media focus that has characterized the pandemic period; in recent decades, exit has been less volatile than entry, and the latter tends to receive more focus given its relationship to entrepreneurship. Therefore it is not surprising that exit is not measured and published in real time, even in proxy form. The Census Bureau publishes proxy indicators of business *entry*—in the form of business tax application data—in near real time as part of the Business Formation Statistics product.

## 4.1 Business Employment Dynamics data

While official data on firm and establishment deaths are not yet available, we can observe BLS data on establishment *closures*—which include potentially temporary closures—during the first and second quarters of 2020 in the BED. These data, which measure establishments paying no employees in the third month of a given quarter (but with positive employment in the third month of the previous quarter), provide a view into establishment shutdowns during the first half of the year from high-quality administrative data.<sup>22</sup> Figure 4 shows establishment closure rates (left panel) and numbers of establishments and jobs associated with closures (right panel).

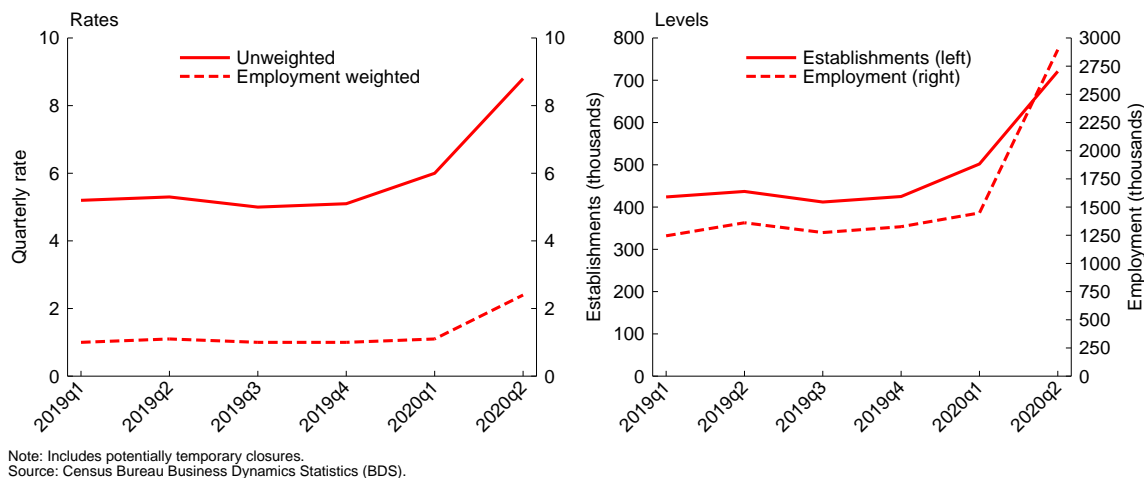


Figure 4: Establishment closure rates, 2019-2020

Focusing on the left panel of Figure 4, the quarterly share of establishments closing rose from just above 5 percent during 2019 to 6 percent during the first quarter of 2020 and nearly 9 percent during the second quarter. Second-quarter closures accounted for nearly 2.5 percent of private sector employment.<sup>23</sup> The right panel shows that 500,000 establishments closed during the first quarter followed by more than 700,000 establishments in the second quarter; the second-quarter pace exceeded the 2019 average pace by nearly 300,000 establishments. Those establishments that closed in the second quarter were associated with nearly 2.9 million jobs.

Historically, roughly half of establishment closures result in permanent deaths.<sup>24</sup> If this historical share held during the first quarter of 2020 it would imply about 250,000 establishment deaths, roughly 25,000 above the 2019 quarterly average. For the second quarter,

<sup>22</sup>The BLS has noted that data collection for the BED (and the related QCEW product) may have been affected by the pandemic; see <https://www.bls.gov/covid19/effects-of-covid-19-pandemic-on-business-employment-dynamics.htm>.

<sup>23</sup>The BED quarterly employment-weighted *firm closure* rate, not shown on Figure 4, rose from an average rate of 0.7 percent in 2019 to 0.8 percent in 2020q1 and 1.8 percent in 2020q2.

<sup>24</sup>This share averaged 53 percent during the 2018q1-2019q3 period.

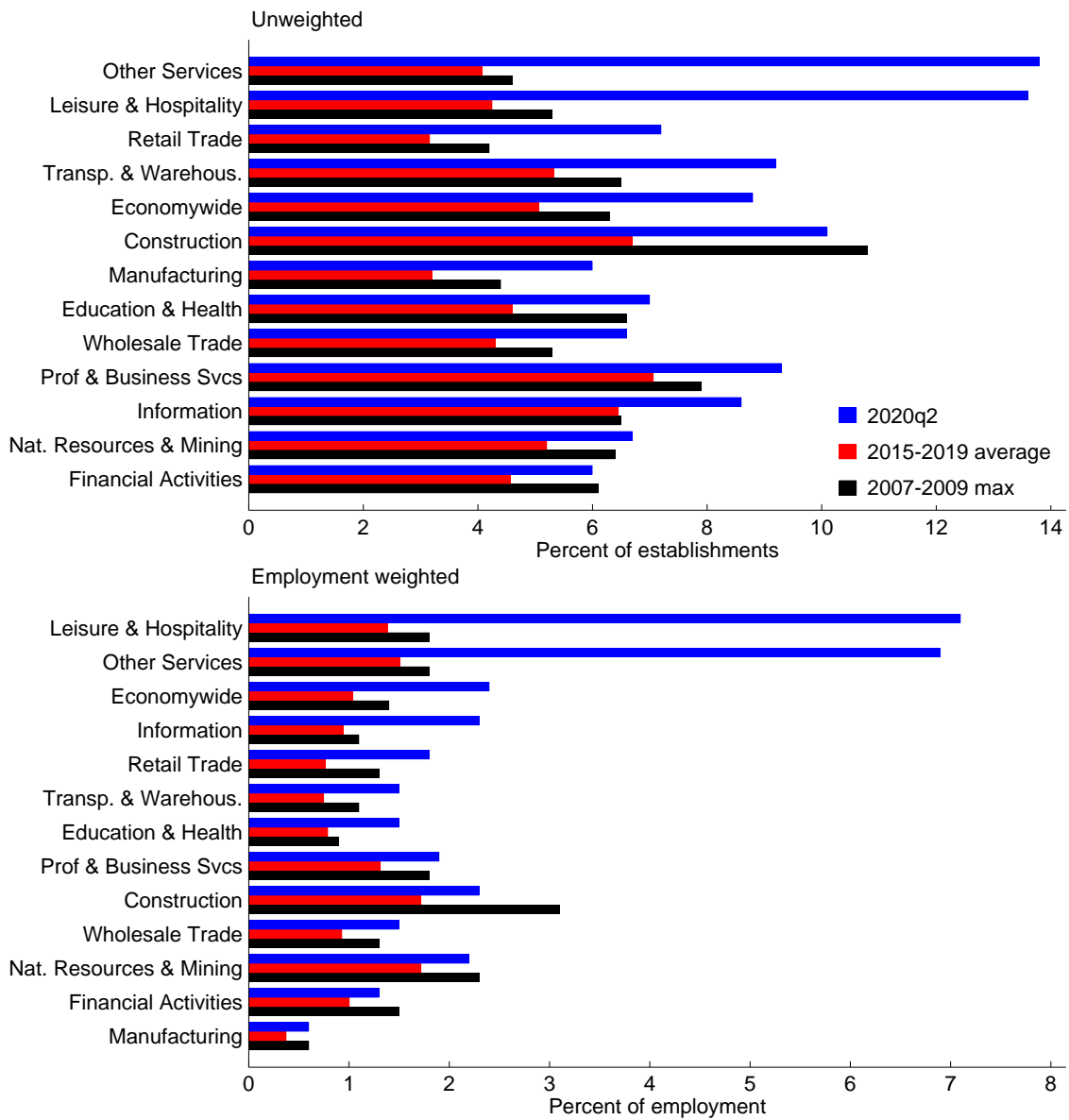
the historical share would imply about 350,000 total establishment deaths, about 125,000 above the 2019 quarterly average. But we expect that establishment closures during the early months of the pandemic are more likely than normal to have been temporary (Cajner et al., 2020) such that 600,000 total establishment deaths and 150,000 excess establishment deaths are extreme upper bound estimates for actual deaths during the entire first half of 2020. Firm deaths (and excess deaths) would be lower still, historically equalling about two-thirds of establishment deaths. Moreover, as we argue in section 4.1 above, Dalton et al. (2020b) estimates from BLS microdata suggest that cumulative establishment closures—at least on an employment-weighted basis—were roughly back on historical patterns by late 2020. For these reasons, the Womply-based estimate of Hamilton (2020)—constructed well before BED data were available—that 400,000 firms closed permanently between March and early July is probably somewhat too high.

The elevated establishment closure patterns of 2020 varied widely across sectors. Figure 5 shows closure rates (top panel) and employment-weighted closure rates (bottom panel) during the second quarter (in blue) compared with the average of 2015-2019 (red) and the maximum quarterly closure rate seen during the Great Recession of 2007-2009. Every sector saw higher rates in the second quarter than the 2015-2019 average. Most sectors also saw higher closure rates than the peaks of the Great Recession; the exceptions are construction, financial activities, and (in employment-weighted terms) natural resources & mining and manufacturing. While all sectors saw high closure rates, other services and leisure & hospitality saw extremely high rates, exceeding 13 percent of establishments and 6.5 percent of private sector employment.

The closure rates seen in Figures 4 and 5 are instructive about the early pandemic period, but two caveats are notable. First, to count as closed, an establishment must have no paid employment for the third month of the quarter; establishments with paid workers in June but none in April or May do not count as closed. Second, existing literature and data we discuss below find that a large share of businesses that closed in the early months of the pandemic reopened thereafter; our rough permanent death calculations above are only suggestive upper bounds.

Here it is also useful to note that Dalton et al. (2020b), using BLS microdata but not actual BED data, find that about 2.3 percent of February 2020 employment at small firms and roughly 0.5 percent of February 2020 employment at large firms was associated with establishments that were closed three quarters later in November 2020, for a rough overall employment-weighted closure rate of about 1.4 percent for that period (calculated as the weighted average of small and large firm rates using 2019 BED firm size weights; see Bureau of Labor Statistics, 2020b, for BED firm size data). Roughly speaking (and ignoring seasonality), this implies an annualized employment-weighted closure rate of 1.9 percent. This is close to the typical annual employment-weighted establishment *death* rate seen in BDS data for recent years (top-right panel of Figure 2) and actually well below typical annual employment-weighted establishment *closure* rates reported in BED data, consistent with the notion that business death has not accounted for a dramatically elevated share of job de-





Note: Quarterly rates (seasonally adjusted). Bars ordered by difference between 2020q2 and 2015-2019 average. Includes temporary closures.  
 Source: BLS Business Employment Dynamics.

Figure 5: Establishment closure rates, 2020q2 versus history

struction during the pandemic.<sup>25</sup> Based on these data, we might expect BED closure rates to come down in the third quarter reading (to be released in late April 2021).

## 4.2 ADP

The best non-traditional measures of business closure come from business databases in which data are generated automatically from actual business activity such as revenue transactions, payroll events, or customer traffic. 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.<sup>26</sup> 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. Another 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.

In the ADP data, we observe paycheck issuance events at the business level. Pay frequency varies by business—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 2020-2021 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 the pandemic 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 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).<sup>27</sup> In all cases, 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).<sup>28</sup>

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<sup>25</sup>In addition to quarterly data, the BLS BED program publishes year-over-year establishment closure data timed to March of each year; see Bureau of Labor Statistics (2020a). Annual employment-weighted establishment closure rates in BED data average 2.8 percent during 2015-2019.

<sup>26</sup>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.

<sup>27</sup>For example, the 2019 cohort data extend from February 2019 through February 2020.

<sup>28</sup>We also take the 2-week trailing moving average of all series to smooth through volatility associated with varying pay frequencies across businesses. Importantly, in this and subsequent sections our exit measure differs slightly from the DHS-based measure from BDS and BED data to which we make comparisons. In non-traditional sources we will generally calculate exit rates as the share of firms operating at the beginning of the pandemic that are not operating at some later date. This is necessary so that we do not have to make assumptions about firm entry in the intervening period, which would be required for calculating the DHS denominator. As we show in the appendix, in BDS data the choice of denominator is not quantitatively critical for the types of exit measures we study, so we prefer to view this conceptual discrepancy as a

The top-left panel of Figure 6 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 routinely 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 6, 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 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.

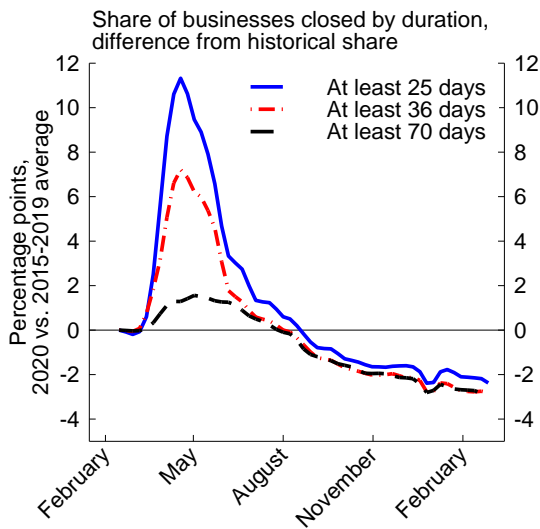
In short, while business shutdown, including shutdown spells of more than two months, was elevated in the late Spring of 2020, by the end of August we observe no evidence of excessive, ongoing business inactivity; in fact, shutdown was well *below* normal by late 2020. We even do not see evidence of a shutdown surge during the latest wave of Covid cases of November-February. 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.<sup>29</sup> So the data, while not dispositive, are at least suggestive.

The top-right panel of Figure 6 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 15 employment. Hence, the top-right panel of Figure 6 shows the share of February’s employment that is associated with businesses that shut down in some weeks thereafter, in 2020 compared with

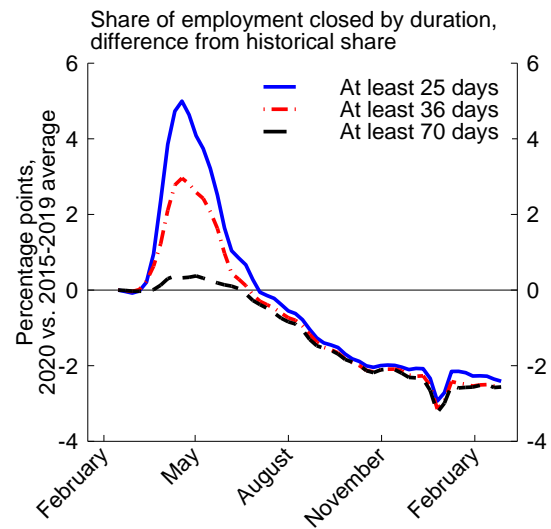
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source of measurement error with magnitude that is small relative to other sources of error inherent in our non-traditional measures.

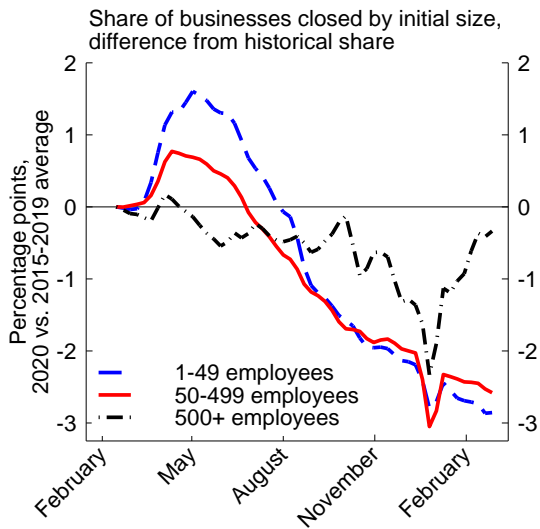
<sup>29</sup>ADP’s fiscal Q4 2020 earnings report (ADP, 2020b), which covered the calendar quarter ending June 30, 2020, noted an overall decline in client retention in that quarter. But retention rebounded, apparently; the fiscal Q2 2021 report (ADP, 2020a), which covered the calendar quarter ending December 31, 2020, noted “an all-time high retention level” and “record client retention.” In the associated earnings call, ADP executives noted that retention has been trending higher in recent years generally and that composition shifts from older to newer ADP products naturally raise retention. Higher-than-historical retention helps explain why relative shutdown measures shown on Figure 6 moved below zero after the summer surge.



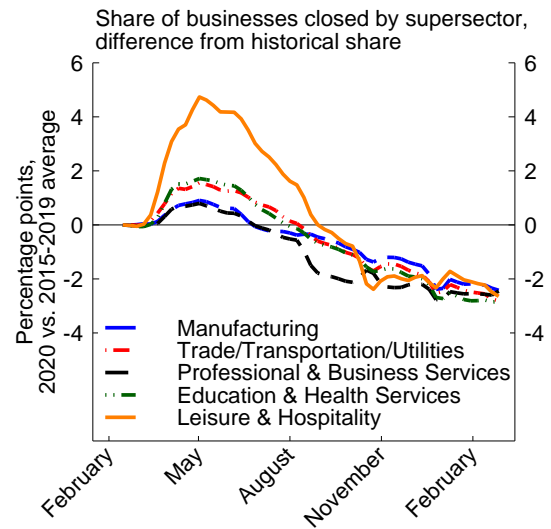
Note: Share of Feb. 15, 2020 businesses issuing no paychecks, current minus same-week 2015-2019 average. Source: ADP. Weekly data through Feb. 27, 2021.



Note: Share of Feb. 15, 2020 employment at businesses issuing no paychecks, current minus same-week 2015-2019 average. Source: ADP. Weekly data through Feb. 27, 2021.



Note: Share of Feb. 15, 2020 businesses issuing no paychecks for 70+ days, current minus same-week 2015-2019 average. Source: ADP. Weekly data through Feb. 27, 2021.



Note: Share of Feb. 15, 2020 businesses issuing no paychecks for 70+ days, current minus same-week 2015-2019 average. Source: ADP. Weekly data through Feb. 27, 2021.

Figure 6: Measures of business closure from ADP payroll data (2020-2021 relative to 2015-2019 average)

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 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. As we argue in section 4.1, BLS data studied by Dalton et al. (2020b) are also consistent with the notion that employment-weighted shutdown was not markedly above historical norms by the third quarter of 2020.

The differences between the top-left and the top-right panels of Figure 6 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 6, 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—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 historical patterns.

Shutdown patterns do vary some across sectors. The bottom-right panel of Figure 6 shows unweighted 70-day shutdown rates for selected supersectors. The leisure & hospitality supersector (NAICS 71-72) had historically high shutdown rates during spring and summer 2020, 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, and by late-2020 the sector had joined other sectors in seeing historically low shutdown activity. Indeed, the cross-sector convergence is striking.

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 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 has not been materially elevated since the pandemic began and that the industries hit hardest in the spring also saw relatively large recoveries.

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 during the second half of the pandemic. 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.<sup>30</sup>

### 4.3 Small business trackers

We next turn to popular measures of small business activity, shown on Figure 7. 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.<sup>31</sup> 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 (and, conveniently, we can observe 2019 data for Homebase as well).<sup>32</sup> 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 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 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.

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

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<sup>30</sup>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.

<sup>31</sup>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, though it is unclear from Womply documentation whether this is accurate.

<sup>32</sup>Homebase provides clock-in/clock-out software for small businesses and can therefore observe employment activity in close to real time. Homebase data were a good predictor of official CES monthly job gains during the initial late-spring recovery; see Reinicke (2020). 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 (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.

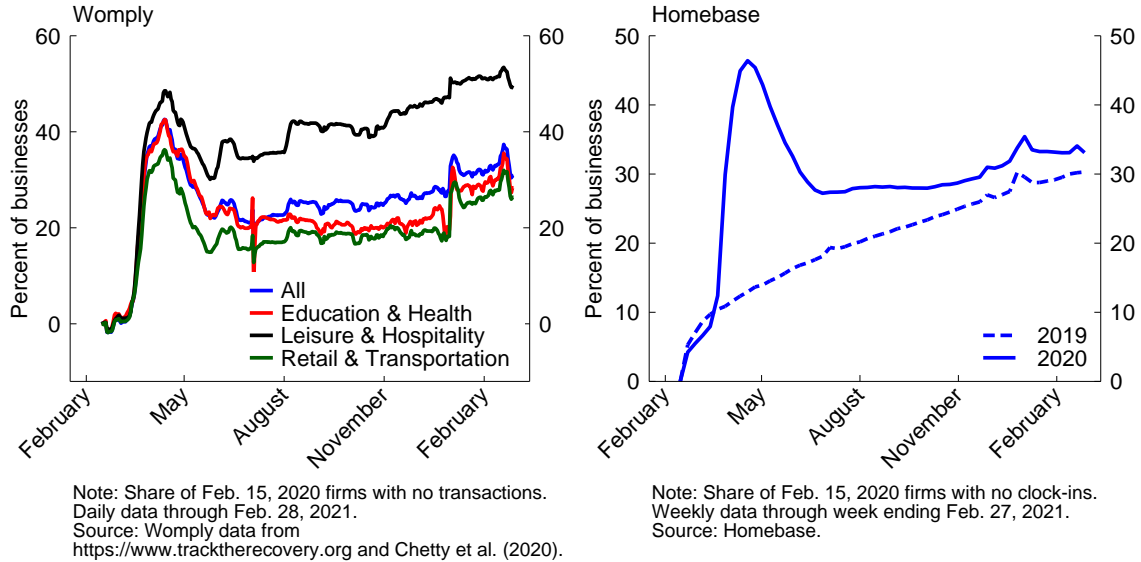


Figure 7: Small consumer business closures

in Womply than in our other sources, suggesting that normal attrition may be higher in Womply data.<sup>33</sup> The Homebase data provide better context and are suggestive of, though not unambiguous confirmation of, elevated exits during the pandemic. We next turn to a measure that is not subject to the particular limitation of client attrition.

#### 4.4 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 to provide insights about physical places.<sup>34</sup> 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.<sup>35</sup>

<sup>33</sup>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 and persistent closure rate in leisure & hospitality.

<sup>34</sup>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.

<sup>35</sup>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)).

In the appendix we describe a methodology that translates patterns of 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). This is a customer and worker *traffic*-based measure of business operation, as opposed to 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).<sup>36</sup> Figure 8 shows estimates of restaurants that were closed—at least temporarily—during each month of 2020 and into early 2021, based on severe drops 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. The significant share of closures identified after the initial March-April surge could be comprised of restaurants responding to renewed social distancing policies, restaurants that have permanently closed, or restaurants that are in fact open but operating at extremely low levels of activity.

Estimates of the flow of restaurants that may have *permanently* closed each month are shown by the red bars in Figure 9; these are restaurants that closed during the indicated month and had still not reopened by February, 2021. 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.<sup>37</sup> The monthly exit rate jumps in March

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<sup>36</sup>Importantly, 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 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.

<sup>37</sup>BDS data for NAICS 7225 indicate recent (2015-2018) annual establishment death rates of 8.1 percent, implying monthly rates of a bit under 0.7 percent. This suggests that SafeGraph closure rates may be a bit too low, though it is difficult to tell since seasonality may play a large role in monthly patterns. Perhaps more importantly, exit rates for the narrow full-service restaurant industry are not available in public data. The overall rate for NAICS 7225 may understate exit rates for full-service restaurants if, for example, fast food establishments (in NAICS 722513) have lower exit rates.



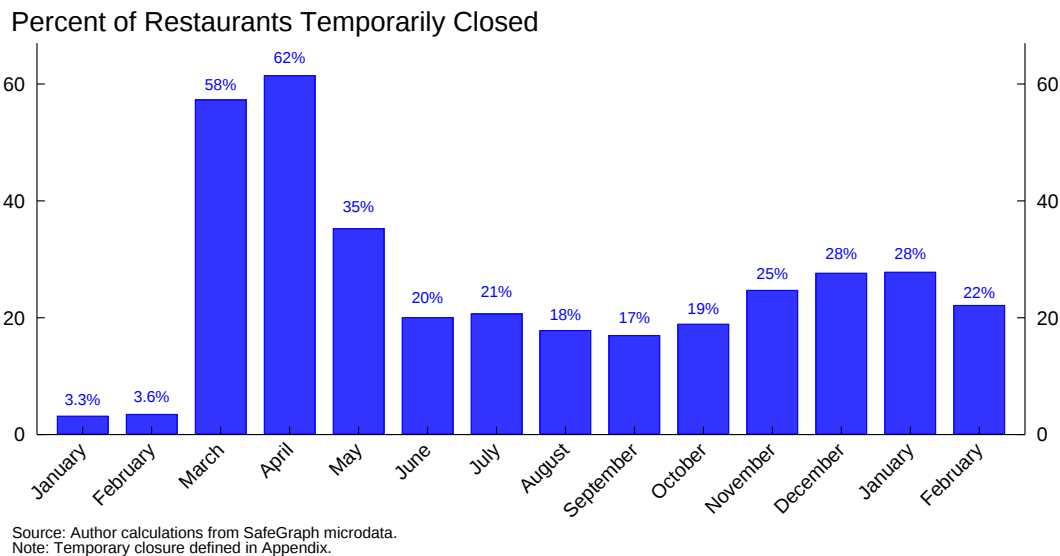


Figure 8: Temporary restaurant closures inferred from weekly visits

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 (the black line in Figure 9) 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.

#### 4.4.1 Case study of New York City and Oklahoma

A concrete way to evaluate 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 Oklahoma.<sup>38</sup> Panel A of Figure 10 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 10 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.<sup>39</sup>

The left panel of Figure 11 shows, perhaps unsurprisingly, that the relative magnitudes

<sup>38</sup>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.

<sup>39</sup>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.

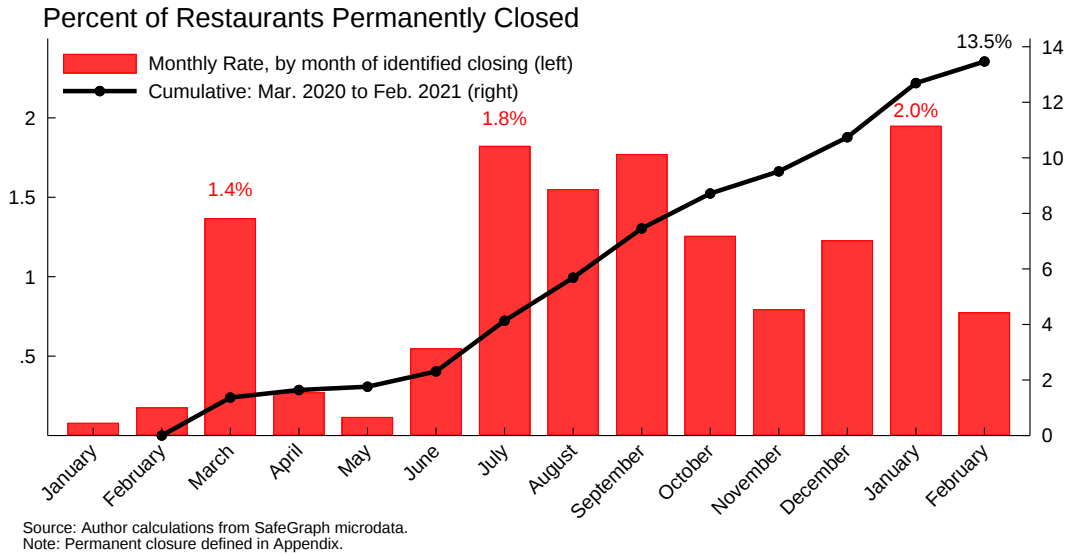


Figure 9: Permanent restaurant closures inferred from weekly visits

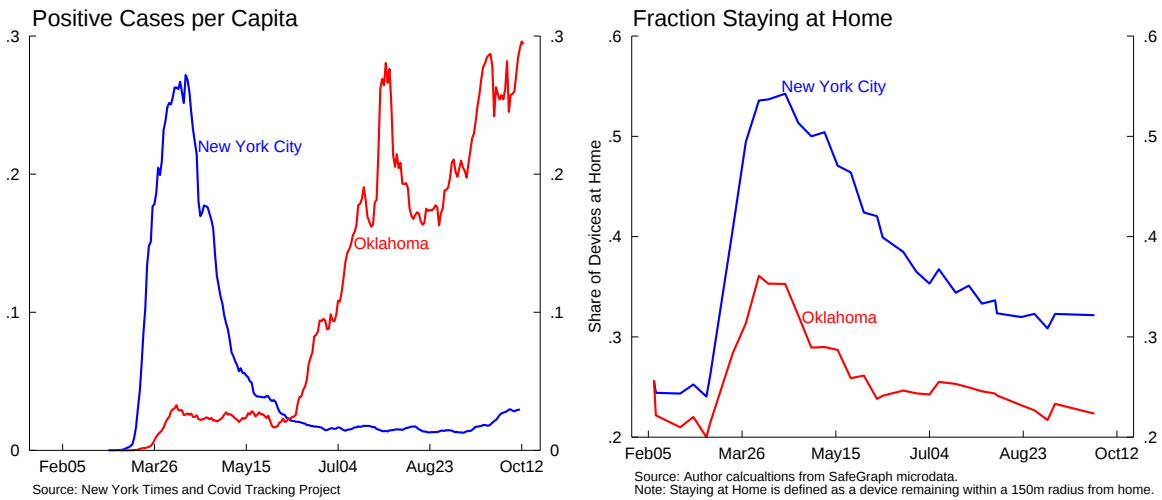


Figure 10: Cases per capita and social distancing: New York City vs Oklahoma

of temporary restaurant closure for New York City and Oklahoma align with the experience of cases and social distancing practices. Over 80 percent of restaurants appeared to be temporarily closed in New York City during the months of March and April, whereas this figure was significantly below the national average in Oklahoma. The right panel of Figure 11 plots the cumulative share of restaurants that are permanently closed. The blue line indicates that over 30 percent of sit-down restaurants in New York City could have permanently closed during the 12-month period following the start of the pandemic.<sup>40</sup> This cumulative share is considerably lower in Oklahoma, and indeed appears to be lower than the (national) rate of closure in a typical year.

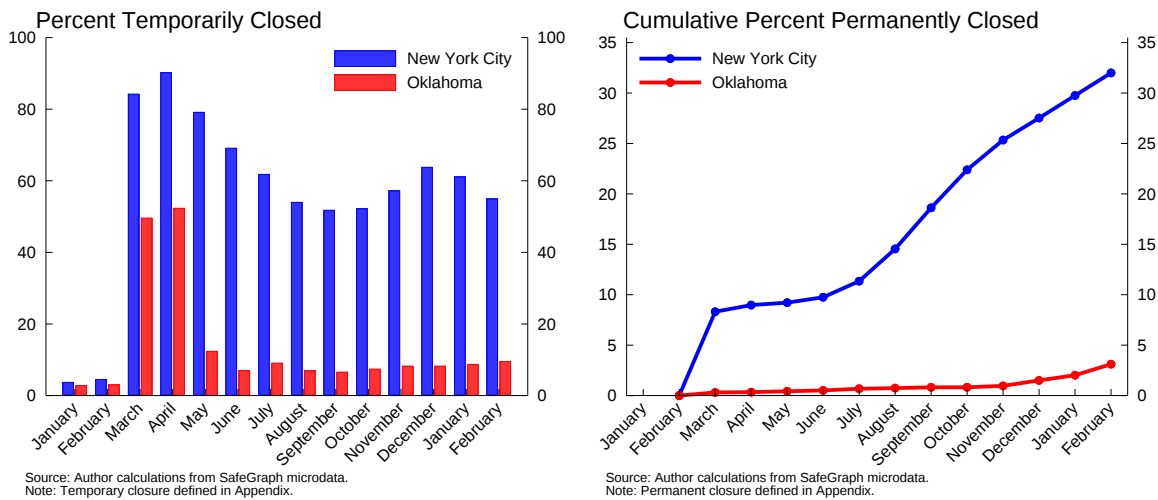


Figure 11: Restaurant closures: New York City vs Oklahoma

#### 4.4.2 Industry analysis

Finally, we calculate cumulative (March 2020 to February 2021) permanent closure rates using this 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. The industry-level variation in excess business closure largely aligns with expectations, as shown in Figure 12. The top panel shows these comparisons for select 2-digit NAICS sectors, while the bottom panel 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 close substitutes with e-commerce (clothing stores). Some industries with lower-than-typical

<sup>40</sup>While New York City was uniquely impacted by the pandemic, with dramatic declines in the number of people commuting into the city on a regular basis, there are some reasons to treat these numbers with even greater caution. Allocating a given GPS signal to a particular establishment is inherently more difficult with multi-storey buildings as is the case in urban areas such as New York City.

closure rates include grocery stores and electronics and appliance stores, two industries that have seen increased demand during the pandemic.

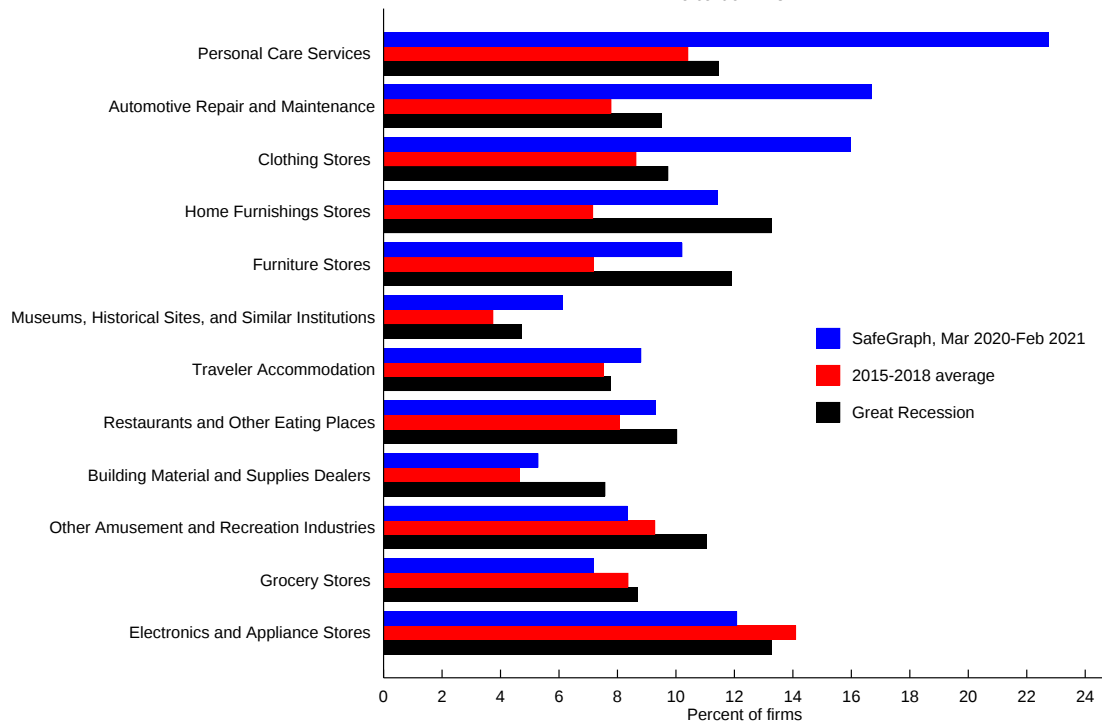
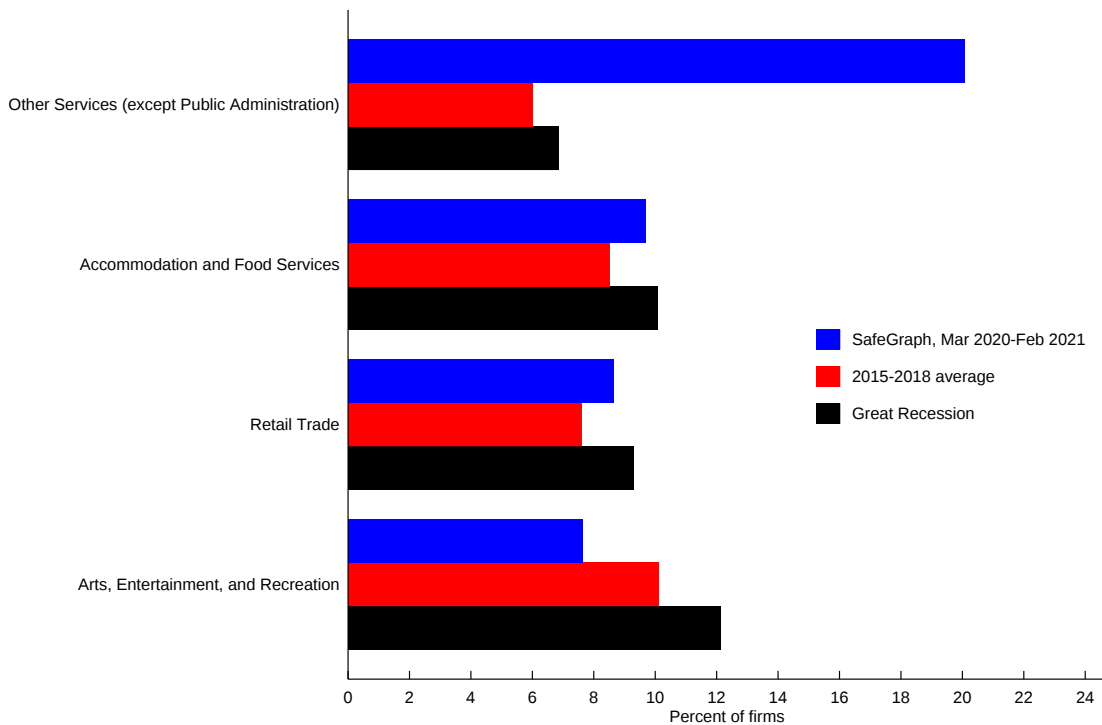
A few other industry results are worth discussing. The excess closure rate for accommodation and food services (and traveler accommodation in particular) is lower than expected given the large drops in travel and restaurant dining induced by the pandemic (and the elevated exit we observe among full-service restaurants on Figure 9). This sector saw dramatic employment declines and is still (as of March 2021) well below its pre-pandemic employment level. That said, 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 that likely benefited from shifts toward more at-home eating. 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 businesses, potentially facilitating continued operation despite large declines in revenue.<sup>41</sup>

The low excess closure rate for “arts, entertainment, and recreation” is also surprising, 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 (based on BED and ADP data reviewed above, Census Bureau survey data reviewed below, and anecdotal discussions), with the possible exceptions of mining (due to oil market developments) and, perhaps, education. Regardless, we again emphasize that our SafeGraph-based measures depend on traffic, and industries vary in how accurately traffic tracks business operational status; therefore, aggregated sector-level data as shown on the top of Figure 12 may not have a straightforward interpretation.

We report the implications of our SafeGraph-based estimates on Table 3. For each of the sectors and industries we show on Figure 12, we report the establishment count from the first quarter of 2019 from BLS QCEW data, 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. 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

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<sup>41</sup>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.



Note: Bars ordered by difference between SafeGraph 2020-2021 rate and BDS 2015-2018 average. Great Recession rate is average of years spanning April 2008-March 2010.  
 Source: Author calculations from SafeGraph microdata and Business Dynamics Statistics.

Figure 12: Cumulative permanent closures for selected industries, SafeGraph

likely below (say) 200,000 establishments during the first year of the pandemic—broadly consistent with our BED-based estimate described above, which suggested excess deaths below 150,000 during the first half of 2020. 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 number of establishment exits during 2015-2018 was about 600,000 in BDS data.<sup>42</sup>

Table 3: Excess exit estimates for selected industries, SafeGraph

Industry (NAICS)	Initial establishments (QCEW, 1000s)	Historical rate (BDS)	Pandemic rate	Excess rate	Excess exits (1000s)
<i>Broad sectors</i>					
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
<i>Four-digit industries</i>					
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 establishment exit rate for 2015-2018 from BDS. Pandemic rate is SafeGraph-based exit rate estimate for the 12 months of March 2020-February 2021. Excess rate is difference between pandemic and historical rates. Excess deaths are equal to excess rate multiplied by establishment count.

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

<sup>42</sup>Our mixing of BLS and Census Bureau data on Table 3 is not without consequence and is its own source of uncertainty around our estimates. As noted elsewhere, establishment counts in BLS and Census Bureau sources have diverged in recent years, with higher levels in BLS sources. This is particularly driven by divergence among the smallest establishments—which tend to have the highest exit rates. In 2018, BLS data reported about 35 percent more private sector establishments than Census Bureau data. Our estimate of fewer than 200,000 excess establishments is based on the BLS establishment count, while our suggestion that this number is about one-third above normal is based on Census Bureau annual death data (whereas the BLS does not, to our knowledge, publish *annual* establishment death figures). If we scale down the establishment counts from Table 3 using the 1.35 ratio just mentioned, we might suggest an upper bound estimate of 150,000 excess establishment exits, which would imply overall exit was elevated by about one-fourth.

other measurement limitations—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. Perhaps most importantly, we do not know how well our SafeGraph measure would have performed in past years—the January and February 2020 data might suggest some understatement—so the comparisons on which we based our excess death estimates should be treated with caution.

## 4.5 Other private sector business registers

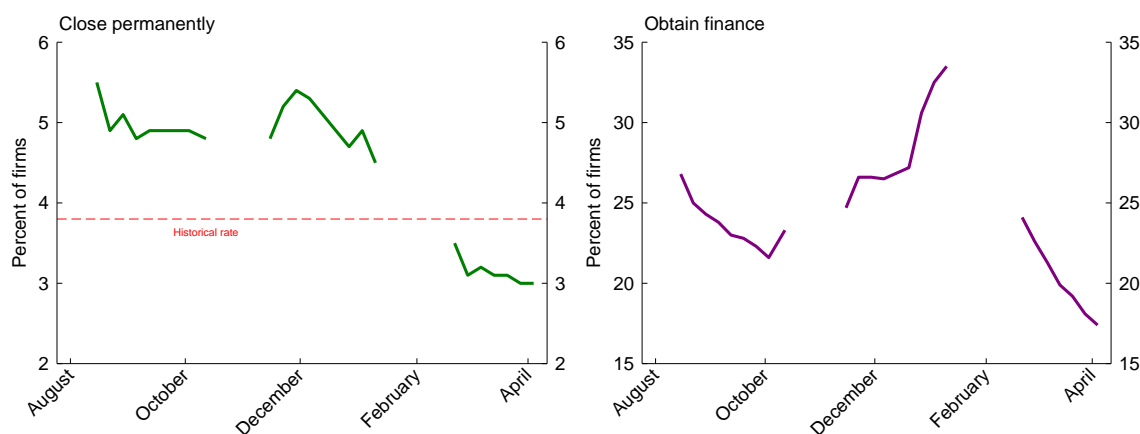
As noted above, an advantage of SafeGraph over the other non-traditional measures we study is that it is designed to capture the near universe of business establishments, and there are no customer attrition concerns. Certain other private sector business registers share these qualities; however, we see them as significantly limited for tracking business exit. In particular, business registers that do not have near-real-time sources of information on actual business activity—such as revenue transactions, payroll events, or customer traffic—for all businesses in the register are likely to poorly capture exit at high frequency, since business shutdown can typically be identified only with analyst effort.

One such business register is maintained by Yelp, the online platform for rating businesses. The company occasionally provides shutdown data including permanent shutdown estimates, but we find these difficult to interpret in their current form (see *Yelp: Local Economic Impact Report* (2020)). 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. These results are not necessarily implausible, but still we find them difficult to interpret. In particular, we assume that Yelp underestimates exit because the company cannot directly verify business activity in real time but instead can only observe exit if website users contribute information or if analysts engage in significant effort. That said, Yelp data may be useful for 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 early in the pandemic.

The most prominent private sector business register is that of Dun & Bradstreet (D&B)—or its derivative, the National Establishment Time Series (NETS) of Walls & Associates. D&B uses many sources to maintain their business list, including yellow pages and surveys by analysts. Such business lists have many uses, but we expect them to be poorly suited for measuring recent exits. D&B-like business registers do not automatically update when business activity drops, in contrast to measures based on automated observation of business status. Instead, analysts must somehow identify business shutdown, an enormous task particularly for the millions of small businesses with high exit rates (and one for which even Yelp is likely better suited, given its access to effectively crowdsourced information). Indeed, Crane and Decker (2020) find that D&B/NETS data capture business dynamics poorly.

## 5 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.<sup>43</sup> Among other questions, businesses are asked about their expectations for the six months following the survey; the left panel of Figure 13 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.<sup>44</sup> The right panel reports the share of businesses expecting to need to obtain financial assistance or credit over the same period.



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 13: Small firms' expected needs over next six months

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 of an additional round of Paycheck Protection Program loans and other fiscal support.<sup>45</sup> During most of the pandemic death expectations have been well above the level consistent

<sup>43</sup>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).

<sup>44</sup>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.

<sup>45</sup>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.



with actual death rates from recent years (the dashed red line), though we do not know how 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 14 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.<sup>46</sup> In some sectors, recent expected death rates do indeed imply 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.<sup>47</sup> 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.

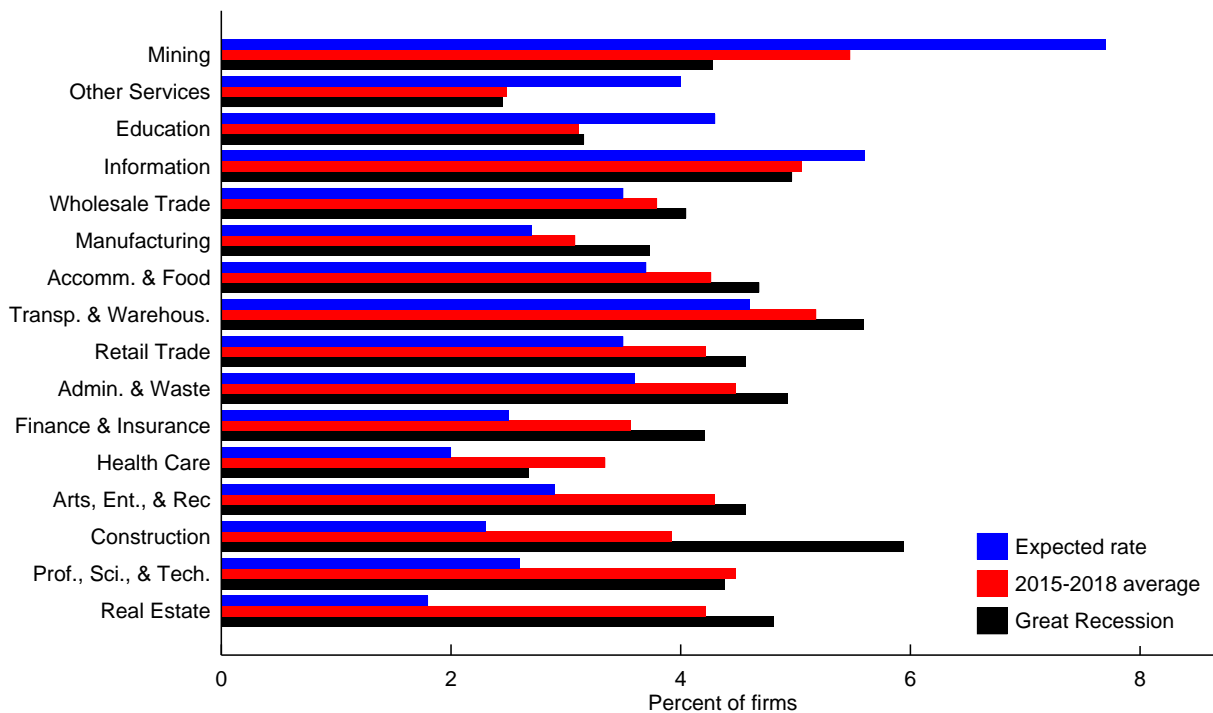
Some of the sectors with low exit expectations in recent surveys had much higher expectations earlier in the pandemic. Figure 15 reports *excess* exit expectations—that is, *expected* exit rates minus historical *actual* exit rates among firms with fewer than 500 employees—by sector (top panel) and by 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 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.<sup>48</sup>

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<sup>46</sup>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 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.

<sup>47</sup>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.

<sup>48</sup>The Census Bureau suppresses data for some states; all states with reported data in both time periods are shown on Figure 14. State historical actual exit rates are approximate (see footnote 46).



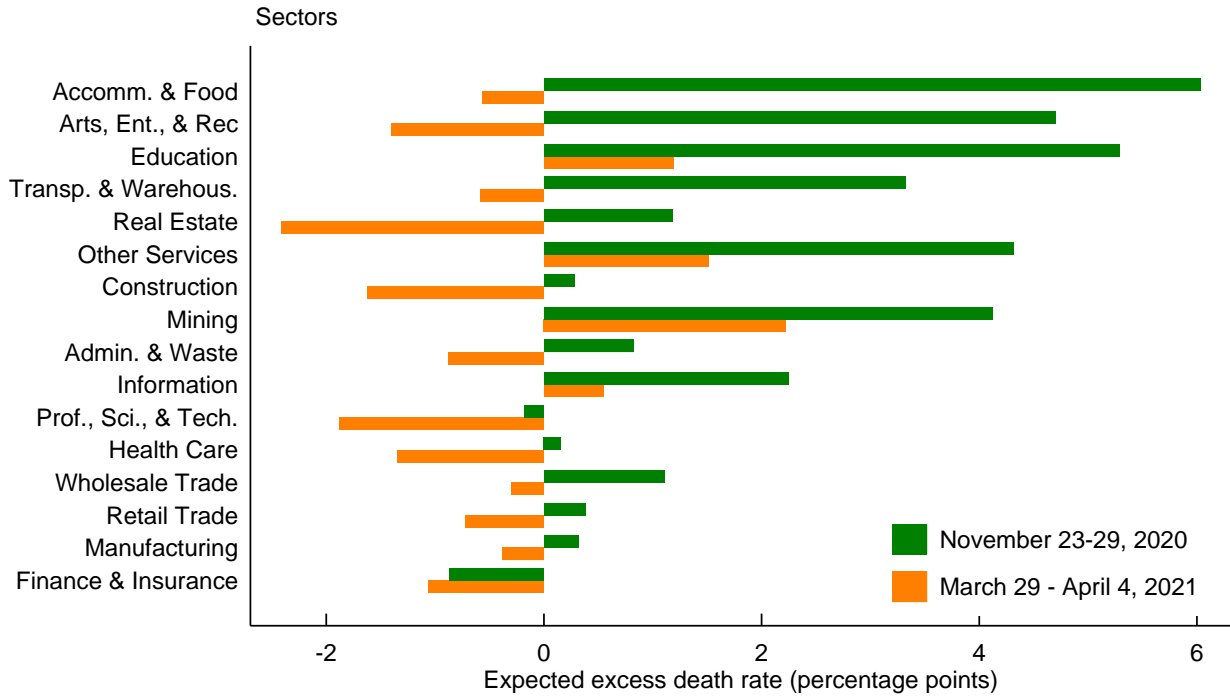
Note: Bars ordered by difference between expected rate and 2015-2018 average. Firms with fewer than 500 employees. Great Recession rate is average of years spanning April 2008-March 2010.  
 Source: Census Bureau Small Business Pulse Survey, Mar. 29-Apr. 4 2021 and Business Dynamics Statistics.

Figure 14: Exit expectations versus historical rates

## 6 Taking stock

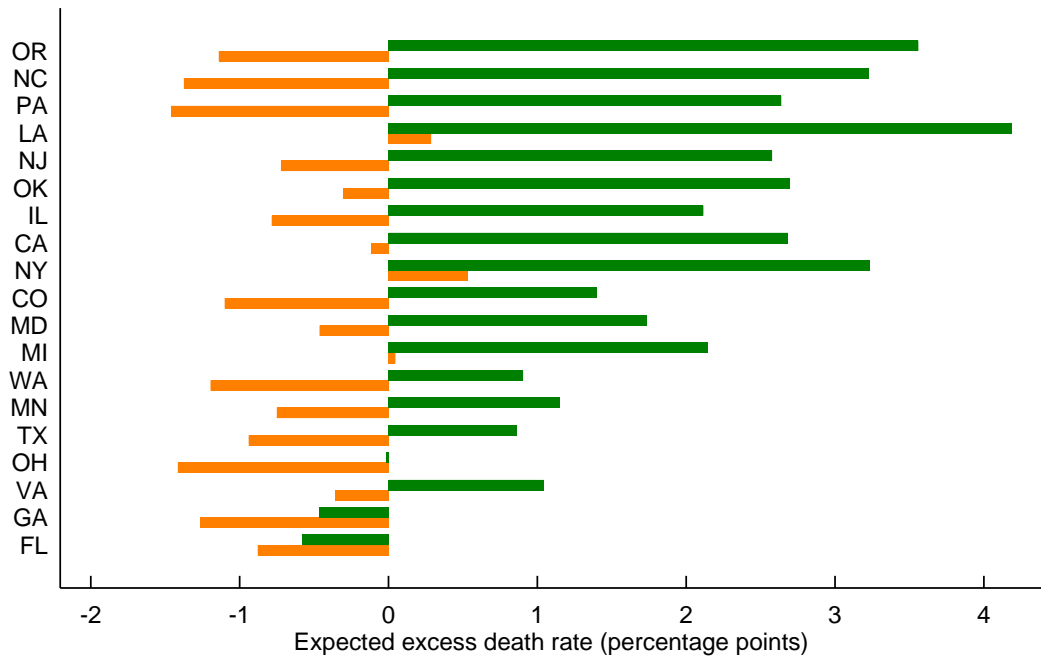
Our analysis suggests that business exit likely was elevated during the first year of the pandemic. We find direct evidence of increased *establishment* deaths among full-service restaurants, personal care services, automotive repair, and certain retailers (apparent in SafeGraph data) and suggestive 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 have not seen 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 figure, 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. 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 cyclicity. This view appears to be roughly corroborated in BLS data studied by Dalton et al. (2020b) (see section 4.1 above).

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



Source: Census Bureau Small Business Pulse Survey and Business Dynamics Statistics (BDS). Expected excess death rate is expected rate (over six months) minus half the sector average firm exit rate among firms with <500 employees, 2015-2018. Sectors ordered by change in excess death rate.

Reported states



Source: Census Bureau Small Business Pulse Survey and Business Dynamics Statistics (BDS). Expected excess death rate is expected rate (over six months) minus half the state average firm exit rate among firms with <500 employees, 2015-2018. States ordered by change in excess death rate.

Figure 15: Excess exit expectations, peak versus early March 2021

destruction would likely be a positive outcome relative to widespread expectations from early in the pandemic.<sup>49</sup> 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 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. 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 even be subdued in coming months. Exit expectations remain elevated in a handful of sectors and states but have generally improved dramatically since November 2020 and are significantly below normal in several sectors. Early and ongoing policy actions may have helped businesses in the relatively optimistic sectors to survive the worst of the pandemic and sustainably reopen.<sup>50</sup>

Alternatively, it may be that our imperfect indicators understate the magnitude of the surge in exit. Anecdotes of business failure certainly abound. Moreover, the situation could deteriorate further from here. Considerable uncertainty about the virus and its various strains remains. Business owners who have managed to stay open this long may face overdue financial obligations that will prove unmanageable, and liquidity contraction or distortions resulting from 2020 policies could leave many firms at high risk (Gournichas et al., 2021). Cross-industry demand reallocation associated with a transition out of the pandemic could be significant. Large firms may have been taking time to implement restructuring plans that could result in waves of establishment exits or the failure of smaller suppliers. Ultimately, we will not have certainty about business exit during 2020-2021 until the pandemic is behind us and high-quality official data are published. In this respect, our work highlights the importance of timely data production and reminds us of the value of official data producers.

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<sup>49</sup>Separately, early evidence suggests business entry may have 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.

<sup>50</sup>Autor et al. (2020) estimate that the Paycheck Protection Program (PPP) had a material effect, boosting June payroll employment by about 2.3 million. Given the particular vulnerability of the smallest businesses, even a small number of jobs saved could correspond with a large number of businesses; moreover, the Autor et al. (2020) identification strategies may understate effects on the smallest, most exit-prone businesses. We also note that the dramatic improvement in exit expectations shown in the Pulse survey coincided with the late-December refunding of the PPP program alongside other fiscal measures.

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# A Appendix for historical results

## A.1 Alternative detrending specifications

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 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 A1: Business cycle correlations: Hamilton (2018) detrending

	Unemployment		GDP	
	Unweighted	Weighted	Unweighted	Weighted
<i>Hamilton (2018) method:</i>				
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
<i>Linear method:</i>				
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 1 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 A2 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 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.

Table A2: 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).

## A.2 Exit levels

In the main text we focus on *rates* of establishment and firm exit. The behavior of establishment firm 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 A1 replicates Figure 2 using levels instead of rates.

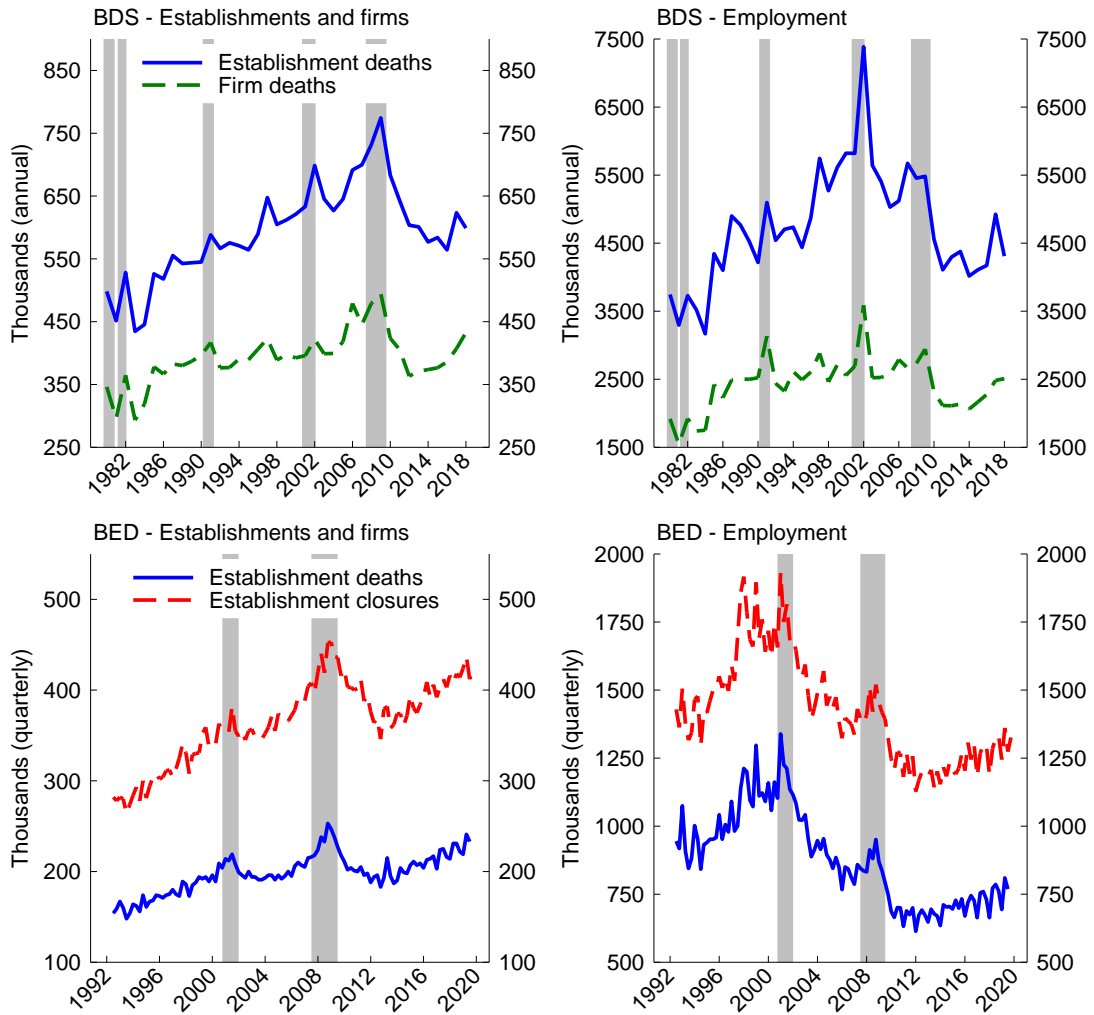
The cyclical nature of exit levels, particularly in recent years, is apparent in Figure A1. Interestingly, the number of exiting firms and establishments rose during the 1990s and 2000s (as the number of firms and establishments in the U.S. grew), but the number of exits declined after the Great Recession.

Table A3 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 countercyclicality.

## A.3 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 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



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 A1: Historical patterns of business shutdown levels

Table A3: Business cycle correlations: Exit levels

	Unemployment		GDP	
	Unweighted	Weighted	Unweighted	Weighted
<i>Linear method:</i>				
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
<i>Hamilton (2018) method:</i>				
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.

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 A4 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 A4: 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.

Business cycle correlations for unweighted exit rates are somewhat weaker—but still apparent—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 A5; these are similar to the results with DHS denominators.

Table A5: Business cycle comovement: States (different denominator)

	Establishment death		Firm death	
	Unweighted	Weighted	Unweighted	Weighted
Change in unemp	0.52*** (0.03)	0.19*** (0.02)	0.33*** (0.02)	0.10*** (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 2.

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

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

## A.4 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 1 omitting the year 2002 and, separately, omitting all Economic Census years; these can be found on Table A6. 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 A7 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 counter-cyclical, correlating positively with the change in unemployment rates and negatively with GDP growth, and that this cyclicity 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

Table A6: 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 A7: Business cycle comovement: States (excluding Census years)

	Establishment death		Firm death	
	Unweighted	Weighted	Unweighted	Weighted
Change in unemp	0.57*** (0.03)	0.23*** (0.02)	0.36*** (0.03)	0.11*** (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 2.

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

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

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 A8.

Table A8: Business cycle correlations: Additional BDS years

	Unemployment		GDP	
	Unweighted	Weighted	Unweighted	Weighted
<i>1978-2018:</i>				
BDS: Establishment deaths (Census)	.39	.16	-.28	-.01
BDS: Firm deaths (Census)	.16	.06	-.15	-.02
<i>1984-2018:</i>				
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.5 Additional state results

Table 2 reports panel regressions relating state-level exit rates and changes in unemployment rates with state fixed effects. Table A9 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.

## A.6 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 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.<sup>51</sup> 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 A2 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 A10 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.

<sup>51</sup>See <https://www.census.gov/programs-surveys/bds/news-updates/updates/2018-bds-release.html>.

Table A9: Business cycle comovement: Alternative controls

	Establishment death		Firm death	
	Unweighted	Weighted	Unweighted	Weighted
Change in unemp	0.55*** (0.04)	0.22*** (0.03)	0.36*** (0.04)	0.11*** (0.01)
Observations	816	816	816	816
State FE	No	No	No	No
Year FE	No	No	No	No
Change in unemp	0.46*** (0.03)	0.18*** (0.03)	0.40*** (0.03)	0.10*** (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 2.

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

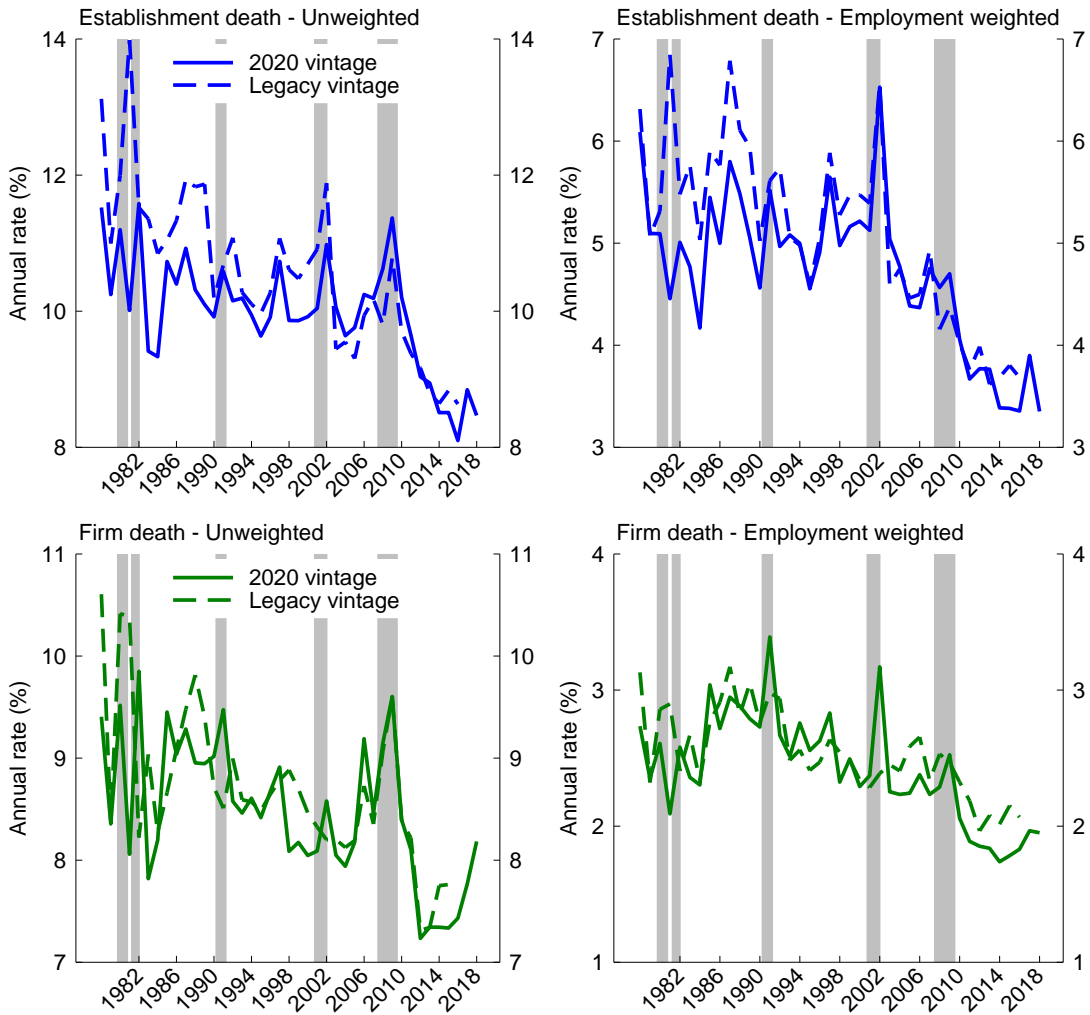
Source: Author calculations from Business Dynamics Statistics and BLS data.

Table A10: 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.





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 A2: Vintage differences in BDS data

Differences between vintages are material for our quantitative estimates. On Table A11, 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.

Table A11: Business cycle comovement: States, BDS vintages

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

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 cyclicity 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.7 Industrial production

Our main business cycle correlations related exit rates with unemployment and GDP. Table A12 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 A12: 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 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.<sup>52</sup> 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

<sup>52</sup>The nature of this dataset is roughly comparable to the Business Register maintained by the Census Bureau but is not as comprehensive.

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.<sup>53</sup> 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.<sup>54</sup>

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.<sup>55</sup> 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.<sup>56</sup> While there has clearly been a migration from in-person dining to carry-out service at these establishments in recent

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<sup>53</sup>This ability declines gradually, as it typically takes some time for SafeGraph to identify a business as closed using their traditional methods.

<sup>54</sup>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.

<sup>55</sup>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.

<sup>56</sup>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.

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.

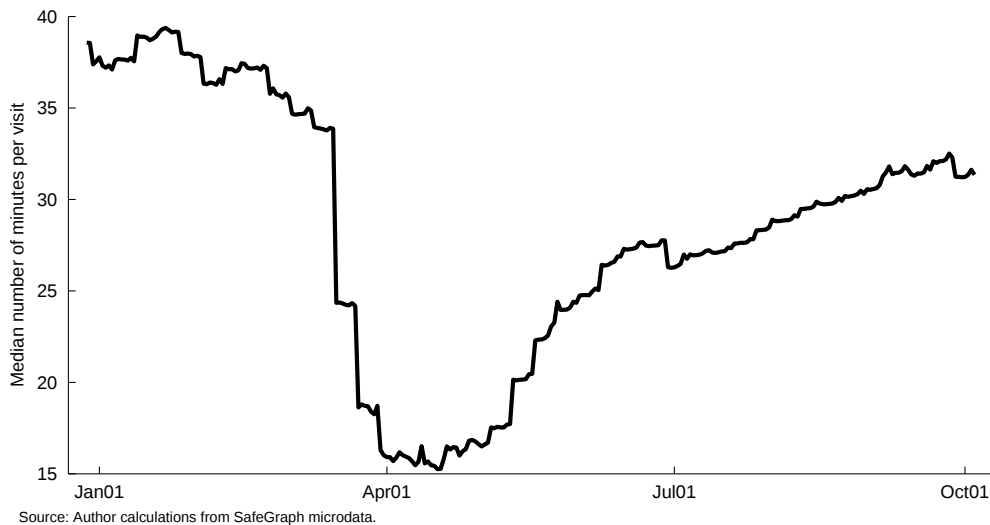


Figure B1: Median duration of restaurant visits

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 ( $\approx 28.5$  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.<sup>57</sup> 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.

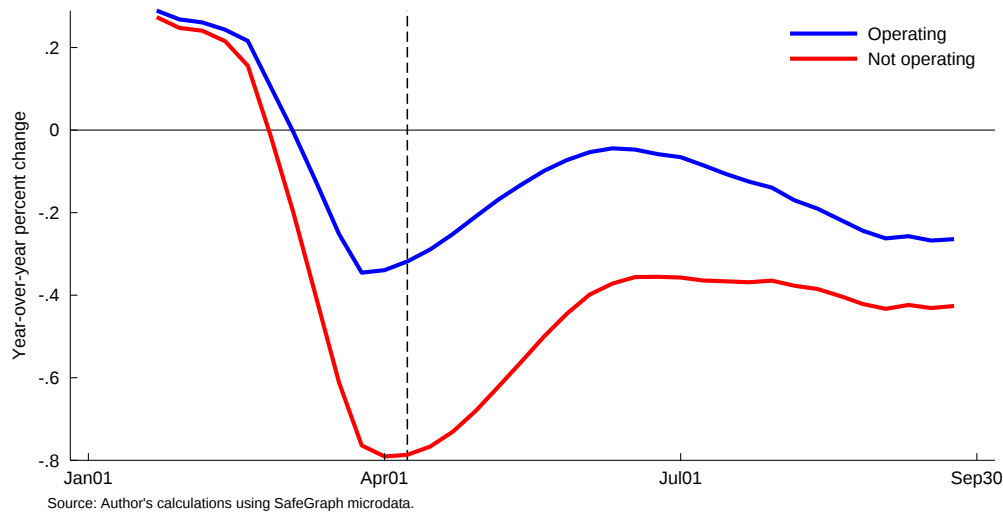


Figure B2: Year-over-year percent change in visits, by operational status Week of April 5-11

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

<sup>57</sup>Further information on this dataset is available at <https://docs.safegraph.com/docs/operatingnot-operating-poi>.

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.

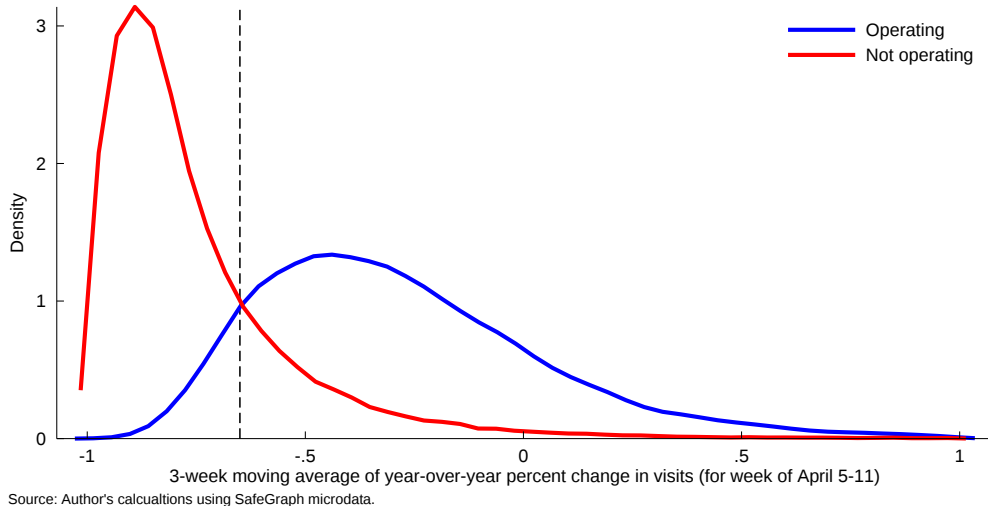


Figure B3: Density of year-over-year percent changes in visits, by operational status  
Week of April 5-11

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.<sup>58</sup> 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  $\tilde{v}_{i,n}^t = 1$ :

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

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

<sup>58</sup>This terminology is somewhat misleading as the “true” data from SafeGraph are themselves estimates subject to error.

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.

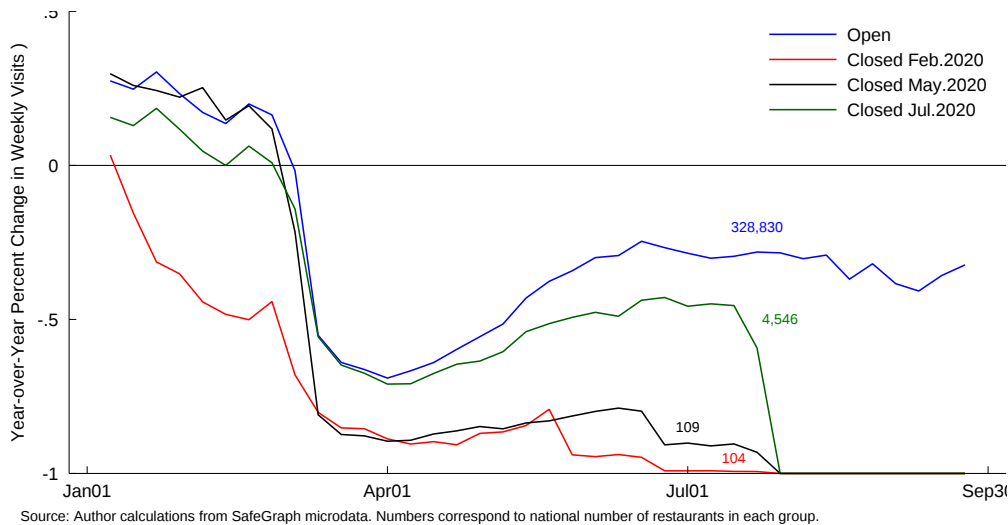


Figure B4: Weekly visits by identified closure Status

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  $n$  ( $\tilde{v}_{i,n}^p$ ) if:

$$\tilde{v}_{i,n}^p = 1 \text{ if } \tilde{v}_{i,n}^t = 1 \quad \text{and} \quad \forall m > n \quad \tilde{v}_{i,m}^t = 1 \quad (\text{B2})$$



We translate these measures to a monthly frequency  $t$  such that  $\tilde{v}_{i,t}^t$  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.<sup>59</sup>

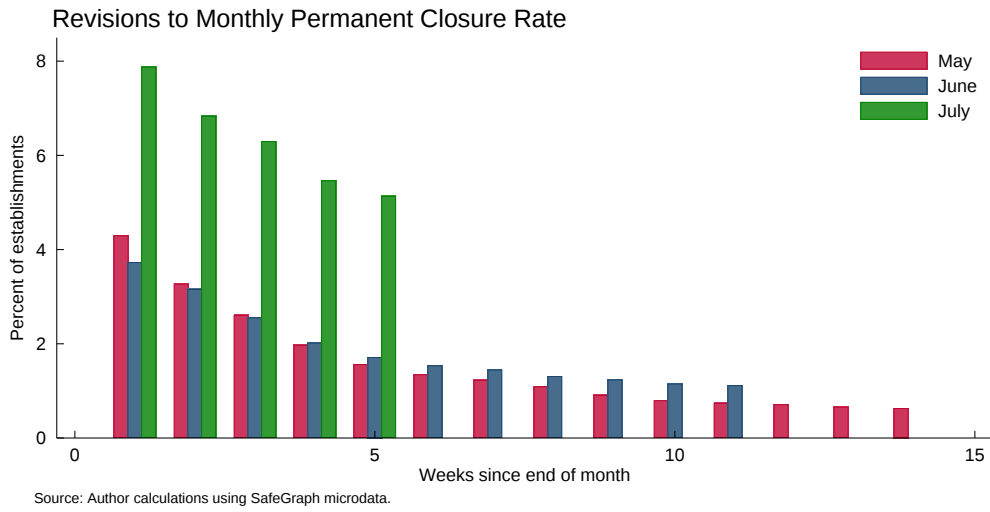
### B.5 Alternate threshold

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); these figures can be compared to Figures 8 and 9. 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).

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<sup>59</sup>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

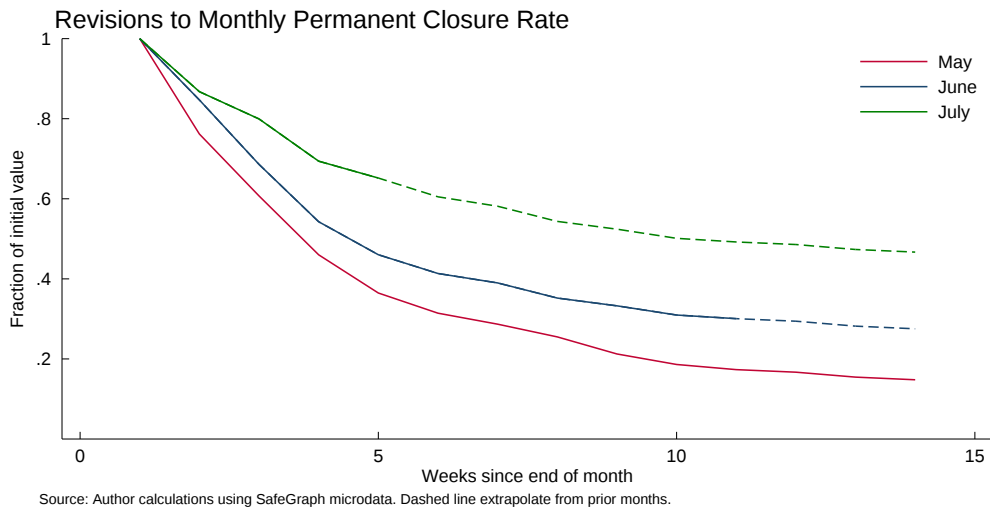
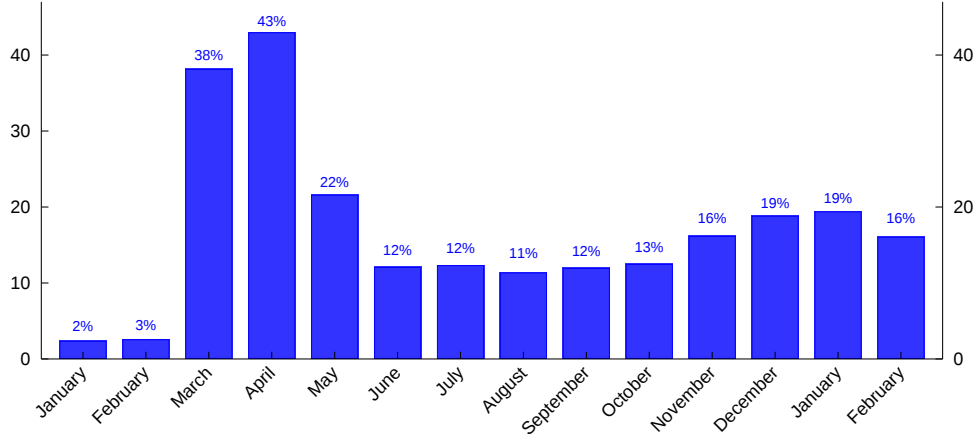
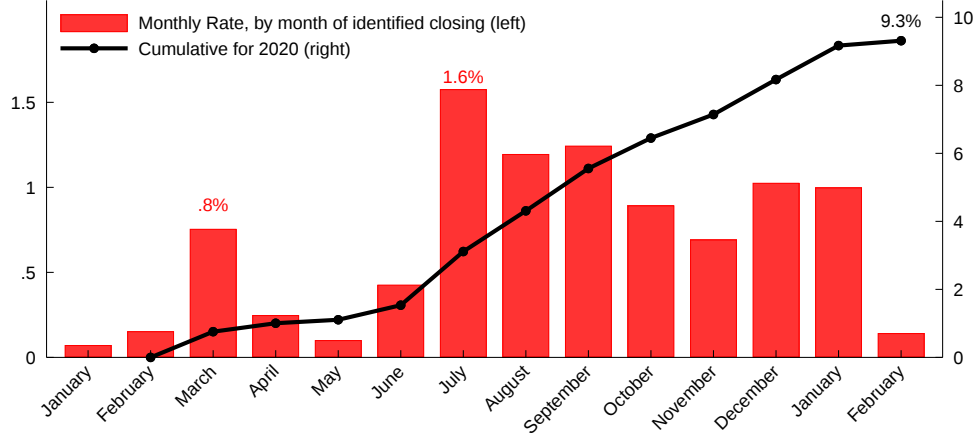


Figure B6: Temporary closures: 80 percent threshold  
 Percent of Restaurants Temporarily Closed (80% Threshold)



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

Figure B7: Potentially permanent closures: 80 percent threshold  
 Percent of Restaurants Permanently Closed (80% Threshold)



Source: Author calculations using SafeGraph microdata.  
 Note: Permanent closure defined in Appendix. Last 3 months are preliminary and may revise.