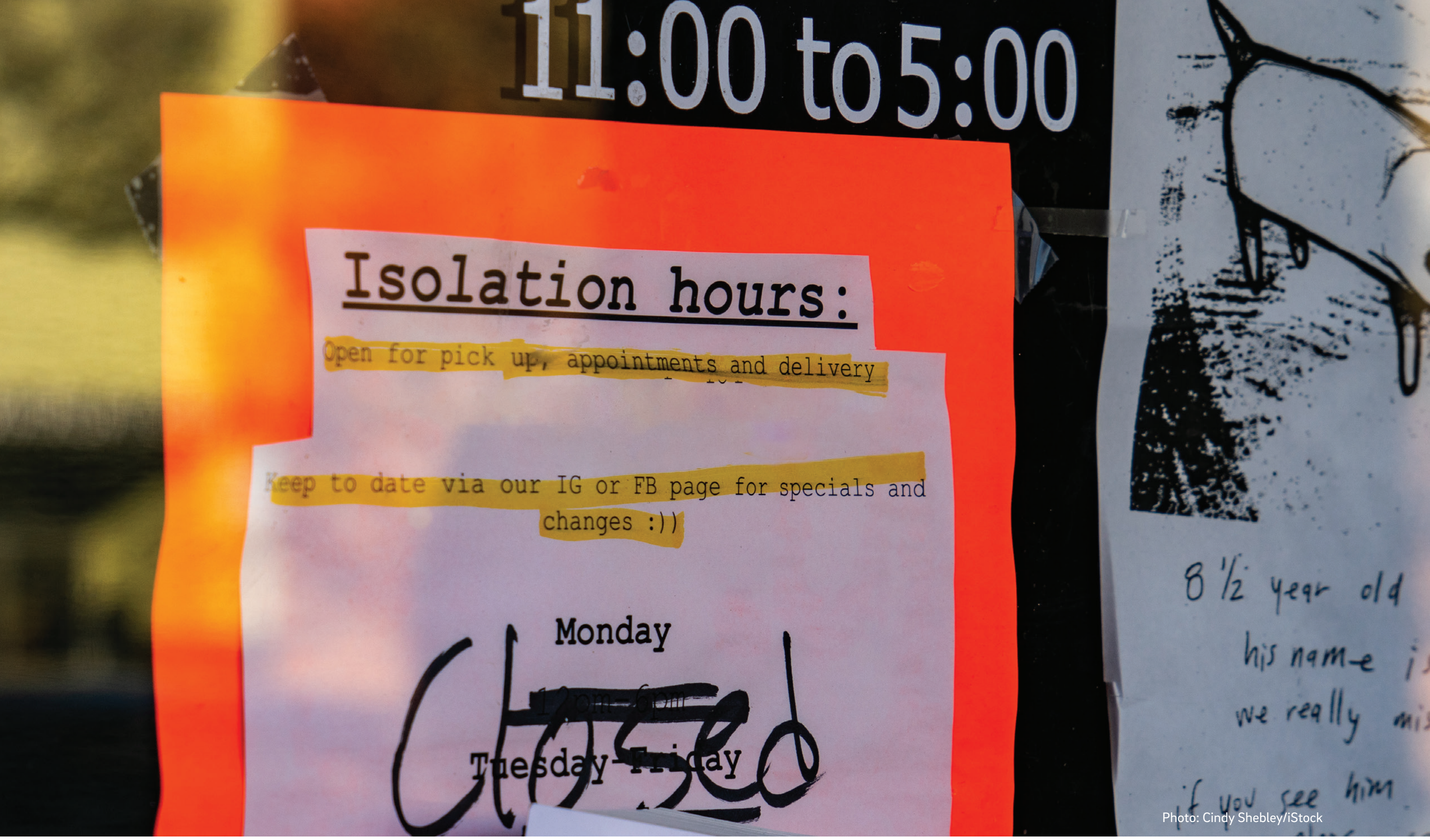

Photo: Cindy Shebley/iStock

Isolating the Effect of State Business Closure Orders on Employment

A closer look at the data reveals the extent to which state policies in response to COVID-19 may have increased unemployment.

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The views expressed in this article are not necessarily those of the Federal Reserve.

In late 2020, numerous states again imposed restrictions on business activity and personal travel in order to halt another wave of COVID-19 cases. These policies represented the most significant interventions since March and April of 2020, when almost all state governments substantially restricted, if not outright prohibited, the operation of businesses in several industries. The economic effects of the "shutdowns" last spring can potentially guide how we interpret the effects of more recent policies and how we shape mitigation efforts in future public health crises.

However, the effect of such orders on business activity, and in particular on employment, is unclear. Even before states intervened in March 2020, many fewer consumers were visiting establishments such as movie theaters, restaurants, and salons as anxious households limited their exposure to the coronavirus. Thus, even in the absence of a business closure order, it's likely that these establishments would have laid off workers. Can we isolate the exact effect of state business closure orders on employment?

A Taxonomy of Mitigation Policies

To mitigate the spread of the pandemic, state and local governments sought to restrict business activity in certain sectors. These restrictions took several forms, some more comprehensive than others.

In many states, the *initial closure orders* targeted only a few sectors in which social distancing was viewed as impractical. The affected sectors included amusement and recreation industries, which were subject to limitations on large gatherings. Casinos, museums, sports stadiums, and theaters typically had to shut down. Food service establishments were also nearly universally closed for dine-in. Personal care establishments, such as barbershops and salons, were often told to close, too.

Nearly 40 states went further and issued a broad call to restrict business activity except in those sectors deemed essential. These states published detailed lists of essential-business exemptions; establishments in sectors not on the list had to cease on-site operations. (An order is treated as an "essential list" if it addresses a broad spectrum of industries. If an order only addresses, say, inessential retail, as in New Jersey, it is not classified as an essential list.) Telework was permitted, so a nonessential designation did not necessarily shut down all activity in a sector.

Following a burst of initial closure orders in mid-March, the issuance of essential-business lists stretched out over three weeks in March and April (Figure 1). Initial orders were adopted by most states over the course of just a handful of days: Over half of the states implemented such a policy on March 16 and March 17 alone. Among these same states, the adoption of essential-business lists was spread over the period of March 20 through March 30. In a few cases, though, the two orders coincided: The essential list was also the first appreciable prohibition on business activity.[1]

Although the initial closure orders likely weighed on employment, I focus on the essential-business lists to streamline the presentation. When I considered the effects of both orders on job loss, the essential lists, which affect a broader share of activity, proved to be the more significant intervention.

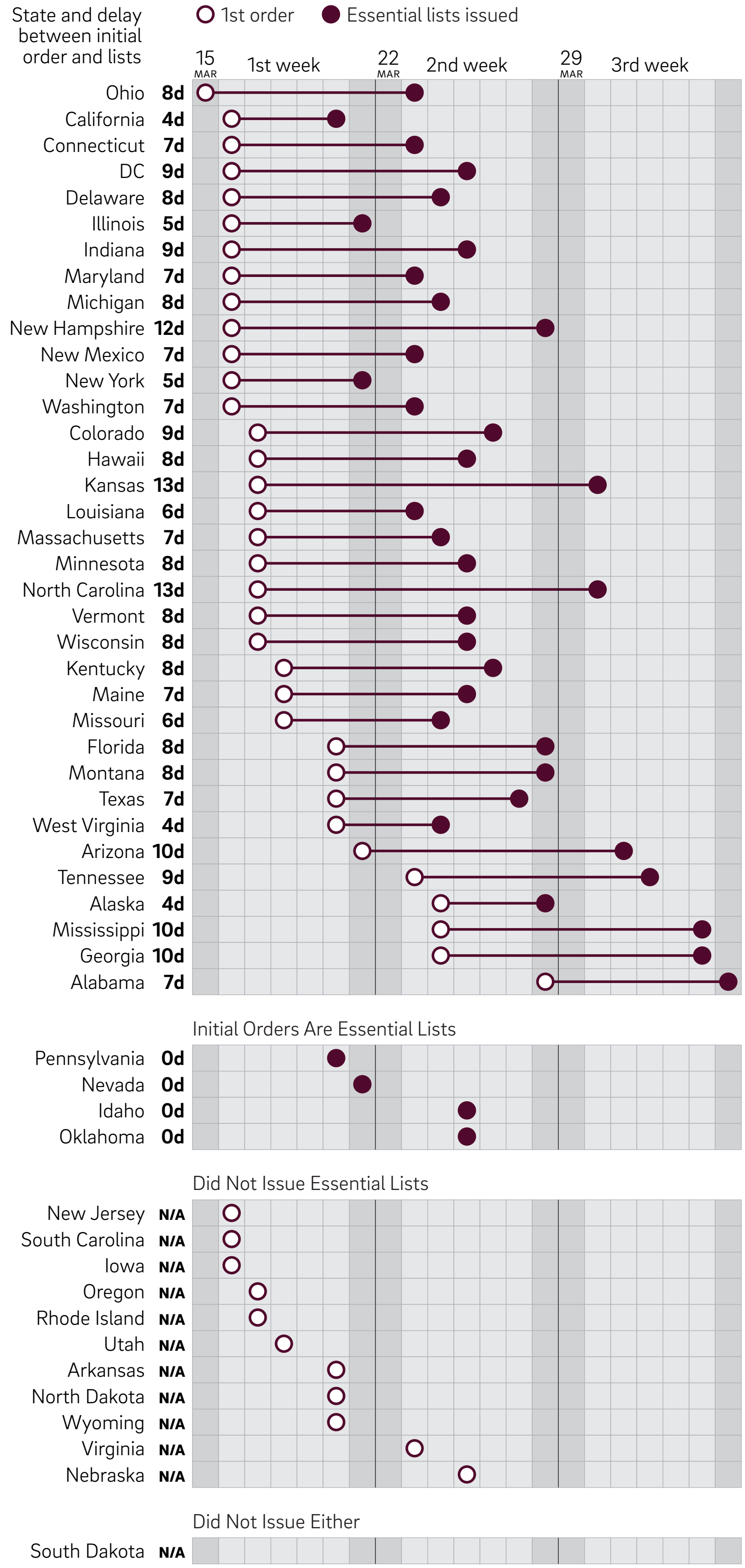
Academics and the media have also written extensively on stay-at-home (SAH) orders, which directed residents to shelter in place as much as possible. (It was understood that some travel, such as trips to the grocery store, was still necessary, and specific recreational activities, such as outdoor exercise, were permissible.) SAH orders were often issued in conjunction with essential-business lists, but the two did not always go hand in hand. In several states, business closure orders preceded SAH mandates. Pennsylvania, for example, closed "non-life-sustaining businesses" on March 19—one of the first orders of its kind in the U.S.—but its SAH order did not take

FIGURE 1

Most States Quickly Ordered at Least Some Businesses Closed

But it took longer for most to issue more-comprehensive essential lists.

Dates of first state-level business-closure order and state-level essential-business list, 2020



Source: Author's tabulations based on published statements from Offices of the Governor and state health departments. County-level orders used in some states.

effect until April 1. Conversely, some states, such as Oregon and Virginia, issued SAH orders but never published an extensive essential list.

Nevertheless, I focus on essential lists rather than SAH orders because the lists more directly affect a broad base of employment. Indeed, SAH orders per se did not restrict travel *for employment* unless coupled with further prohibitions on nonessential businesses. My decision deviates somewhat from the research to date, which has tended to examine SAH orders. However, several key results that I report do not depend significantly on whether I consider SAH orders or essential lists.

In summary, many states substantially restricted business activity in March and April 2020 to mitigate the spread of COVID-19. However, state policy was far from uniform, especially in regard to essential-business lists. Twenty percent of states never issued such a list, and among the other states, the timing of their interventions varied. I will examine whether these differences in timing led to differences in employment outcomes. But first, it's instructive to briefly consider the content of essential lists.

The Content of Essential-Business Lists

Many states' essential lists are informed by federal guidelines issued by the Cybersecurity and Infrastructure Security Agency (CISA) of the Department of Homeland Security. I linked the textual descriptions in the CISA guidelines to standard industry classifications (NAICS).[2] I used the March 28 version of the guidelines, since this version was in force the longest before states started to "reopen" their economies at the end of April. I found that at least 69 percent of the U.S. workforce was classified as essential according to CISA guidance.

However, the essential share of employment varies starkly across economic sectors. In sectors such as utilities, banking, and health care, nearly the entire workforce was classified as essential. At the other end of the spectrum, essential shares were zero, or nearly zero, in the food service and amusement/recreation sectors. Finally, among other sectors the essential share varied, roughly, between 40 percent and 80 percent (Figure 2). In some cases, such as professional services and administrative support, the jobs can often be done from home, which illustrates why nonessential status does not necessarily imply job displacement. A nonessential designation is more likely to imply the stoppage of business activity in wholesale and retail trade; rental, leasing, and other services; and manufacturing.

Many states adopted federal guidelines, but their lists were far from uniform. Although I follow much of the research to date by focusing on differences in the timing of states' orders, the scope of the orders also varied.[3]

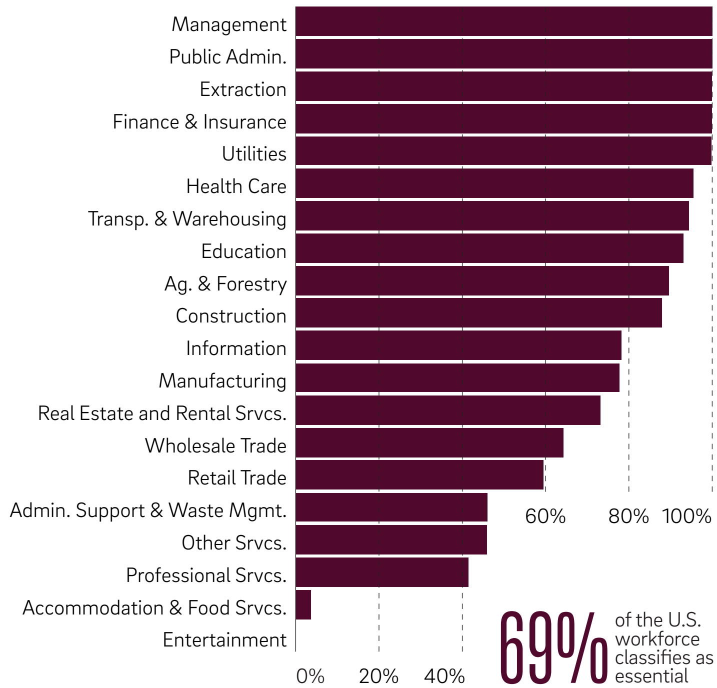
A handful of states published lists of essential sectors using a standard industry classification. These few states illustrate the variation in the scope of essential classifications. At one end, Vermont and Pennsylvania classified around 50 percent of their workforce as essential. By contrast, the essential share of the workforce in Oklahoma is closer to 95 percent. Delaware is in the middle, with an essential share of around 70 percent.

Essential shares could differ even among states whose essential lists consisted only of the sectors listed in the federal (CISA) guidelines. For instance, orders issued by Georgia and Michigan largely mirrored CISA guidelines, but Michigan's essential list was issued earlier and based on the first (March 19) edition of CISA guidance. After CISA substantially expanded the scope of essential activities on March 28, Georgia adopted its guidance, but Michigan did not incorporate CISA's revisions. At the end of March, the essential share of the workforce in Michigan was still around 60 percent, but it was slightly over 70 percent in Georgia.[4]

FIGURE 2

Essential Share of Employment Varies Across Sectors

Essential share of workers by sector, March 28, 2020



Source: Author's classification of the Cybersecurity and Infrastructure Security Agency's March 28, 2020, memorandum on essential critical infrastructure workers.

Closure Orders and Job Losses

In March and April 2020, weekly unemployment insurance (UI) claims reached previously unimagined heights. During just the two weeks ending March 28, nearly 9 million workers filed an initial UI claim. This figure represents 5.5 percent of the prepandemic labor force. Remarkably, another 11 million filed claims in the succeeding two weeks.[5]

Importantly, the national data mask considerable differences across states. Looking again at the two weeks ending March 28, the UI claims rate—the number of initial claims relative to the state's prepandemic labor force—varied by a factor of five during this period, ranging from over 11 percent in Pennsylvania and Rhode Island to as low as 2 percent in South Dakota and West Virginia. Might differences in state mitigation policies account for some of this variation in initial claims?

Much of the research on this question applies a simple event study framework, which attempts to uncover the effect of a policy, or "event," by comparing outcomes when the policy is observed to outcomes when no policy is adopted. More exactly, the event study is implemented using a barebones statistical (regression) model of, for example, initial UI claims. The model relates the change in a state's initial claims rate in any given week to (i) the state's own policy (in that week) as well as (ii) a common "time effect," which captures the average claims rate across all states (in that week).

If a policy is to have an effect in this framework, it must lead to higher initial claims upon its adoption (i) relative to the state's own claims rates at other dates and (ii) relative to the typical change in claims observed across all states at that time (as captured by the time effects). In our context, the driving force behind these common time effects is the evolving public health risk posed by COVID-19.

Perhaps surprisingly, this event study model omits any mention of a state's own recent growth in COVID-19 cases. I considered the role of caseloads but found its effect to be almost negligible, which is consistent with the results found by University of California economists Zhixian Lin and Christopher M. Meissner. Variation in the timing of the orders appears to reflect differences in states' responses to a given caseload rather than big differences in caseloads themselves. To illustrate this point, consider that when California issued the nation's first SAH order on March 19, it had registered roughly the same number of cases per 100,000 residents as Arkansas—yet Arkansas never issued an SAH order (or any order like it).

Following recent research, I used this event study framework to examine the effect of a specific policy, essential-business lists, on job loss in March and April 2020. I considered three separate indicators of job loss, starting with initial UI claims.

Weekly Initial Claims

I used weekly data on initial claims over a three-month window around mid-March, when the first essential lists were introduced. Each observation in the data refers to the number of initial claims filed between a Sunday and the subsequent Saturday. The sample includes all 50 states plus the District of Columbia (Figure 3). Thus, the sample consists of states that issued essential lists at different times as well as states that never issued a list.[6]

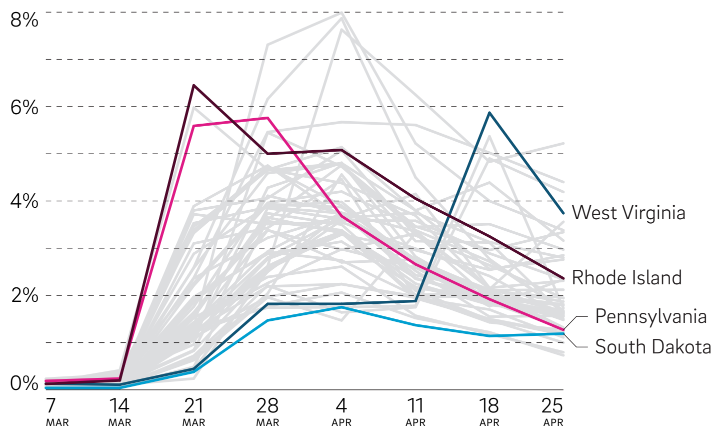
Note that since we measure weekly claims, the date of a new policy corresponds to the week in which it was introduced.[7] Thus, the immediate impact of a policy will partly reflect when in the week states enact it, since the effect is likely to be larger if the policy is in force for more of the period. With the exception of the week of March 15, when a handful of states introduced an essential list, the dates of enactment were distributed roughly evenly throughout a week. On average, an essential list was implemented on the third day of the week.

Based on the event study analysis, I calculated paths for the initial claims under two scenarios (Figure 4). One estimate (burgundy line) is the claims rate that would have been observed if states had not enacted the essential list. The other estimate (pink line) accounts for the policy. Thus, the difference between the two paths indicates the effect of the essential list. Lastly, the pink shaded area represents a "confidence band": Every estimate is uncertain, but there is a 90 percent probability that the "true" path of claims implied by the essential list lies within this band.[8]

FIGURE 3

Weekly UI Claims Rates Varied Substantially Across U.S. States

Number of initial unemployment insurance claims relative to the state's prepandemic labor force, 2020

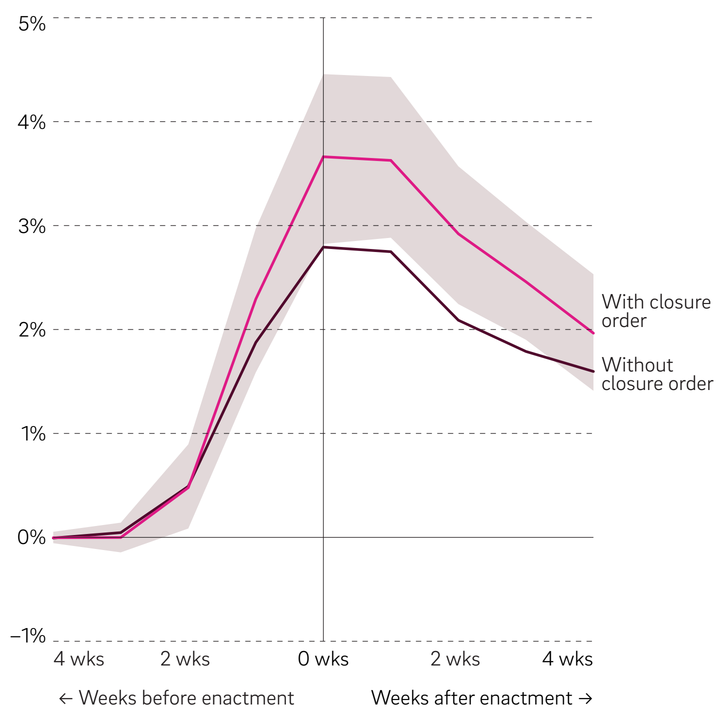


Source: Harvard's Opportunity Insights institute based on the Employment and Training Administration's release of weekly initial unemployment claims and the Bureau of Labor Statistics' estimates of 2019 state labor force levels.

FIGURE 4

The Closure of Nonessential Businesses Is Associated with an Increase in the Initial Claims Rate

Estimated change in initial unemployment insurance claim rate (percent) before and after state enacts an essential-business list, two scenarios



Source: Author's estimates of event study model using weekly initial unemployment insurance claims from Harvard's Opportunity Insights institute.

Note: There is a 90 percent probability that the "true" path of claims implied by the essential list lies within the shaded band.

The closure of nonessential businesses is associated with an increase in the initial claims rate. The claims rate in week 0–that is, the week when the essential list was enacted–is predicted to be 3.7 percent (pink line in Figure 4), whereas it would have been roughly 2.8 percent in the absence of a policy (burgundy line). This difference of nearly 1 percentage point between the two estimates measures the effect of the policy. The impact of the policy persists but diminishes in subsequent weeks. The cumulative effect of the policy across all five weeks (that is, weeks 0-4) is just over 3.5 percentage points.[9] However, the overall claims rate rose by 14.5 percentage points over this period. By this measure, essential lists account for no more than 25 percent of total claims.[10]

The data also show, however, that initial claims generally started to rise even before states issued their essential lists. Importantly, this increase appears to reflect the common "time effects" in the event study framework, which capture the average claims rate across states independent of mitigation policy. That is, this increase is predicted to occur even if a state did not enact an essential list (burgundy line in Figure 4). This rise in the average claims rate before week 0 presumably reflects concerns about the spread of the coronavirus, which prompted households across all states to curtail their commercial and social activities. The estimated effect of the policy (the difference between the pink and burgundy lines) remains small prior to week 0 and cannot be distinguished from zero with any confidence. This is an important observation: If job loss accelerated more in policy-adopting states before essential lists took effect, one might worry that policy merely coincided with a relative decline in employment that was ongoing in those states and would have continued in any case. According to these estimates, though, this pattern, known as a pre-event trend, is not clearly evident in the policy-adopting states.

Redoing this analysis using SAH orders yields broadly similar findings, with two qualifications. First, the effects of SAH orders in weeks 0-4 are even somewhat larger than I find when using essential lists. However, and secondly, I also find more significant pre-event trends, consistent with the fact that, in several states, SAH orders were issued later than essential lists and after substantial job losses.[11]

Differences in policies contribute to, but are not the key driver of, the increase in initial claims. Much, though not all, of the earlier research into mitigation policies also concluded that they were a secondary factor behind job loss. For example, University of Illinois economist Eliza Forsythe and her coauthors conclude that the most striking aspect of the data is the broad-based decline in employment across states and sectors "regardless of the timing of stay-at-home policies." Lin and Meissner report that "there is no evidence that stay-at-home policies led to stronger rises in jobless claims," an even starker conclusion than my own.[12] Indiana University economist Sumedha Gupta and her coauthors consider SAH orders as well as interventions akin to what I have termed initial closures, which often applied narrowly to certain retail and recreational establishments. They find that initial closures did increase claims in the week in which the policy was adopted, but the estimated effect of the policy amounted to 15-20 percent of the overall increase in claims.[13]

Weekly Private Sector Employment

Aside from initial UI claims, the labor market indicators published by the U.S. government are available, at best, on a monthly basis. Monthly data are even less suitable than weekly data for an event study of the COVID-19 crisis, which evolved rapidly in March and April.

Fortunately, Harvard's Opportunity Insights institute has made available state-level employment data at a higher frequency. The institute culled the data from payroll-processing firms, time-tracking software, and paycheck deposits. The employment records cover a reasonably representative cross section of the nonfarm private sector.[14]

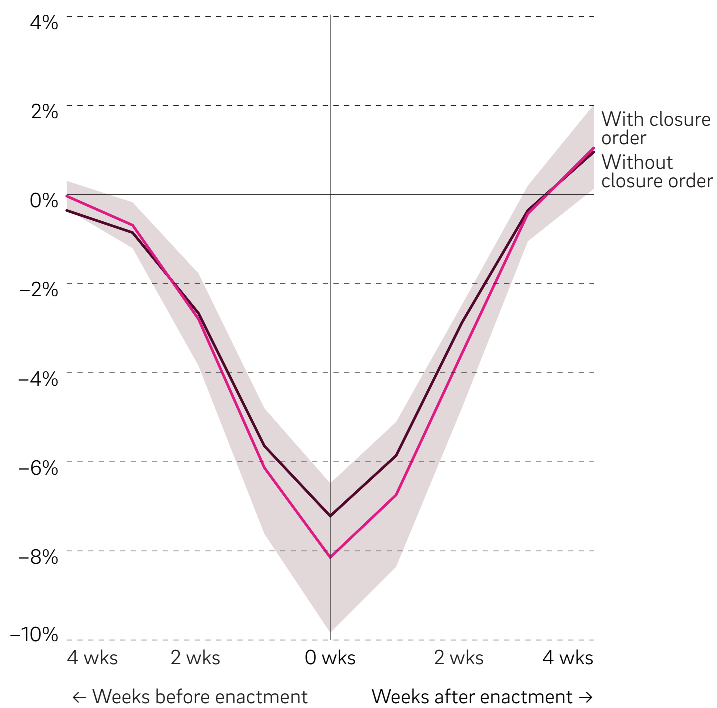
In principle, the employment series is daily. However, the data are reported as a seven-day moving average, making it akin to a weekly series. Indeed, we can extract from the moving average a measure of weekly employment growth between each Sunday and Saturday. This weekly format matches the structure of the initial claims data. Also, the seven-day decline in employment is close in concept to initial claims, which is a measure of the number of newly unemployed.[15]

An event-study analysis of these employment data indicates that closure orders added about 1 percentage point to the decline in employment in the week they took effect (week 0).

FIGURE 5

Closure Orders Added About 1 Percentage Point to the Decline in Employment in the Week They Took Effect

Estimated change in employment (percent) before and after state enacts essential-business list, two scenarios



Source: Author's estimates of event study model using Harvard's Opportunity Insights institute's reports of state-level employment growth.

Note: There is a 90 percent probability that the "true" path of claims implied by the essential list lies within the shaded band.

This estimate (Figure 5) is nearly identical to what we observed when we considered the effect of essential lists on initial UI claims (Figure 4).

However, the cumulative effect of the closure orders over subsequent weeks is somewhat smaller than what was implied by our analysis of UI claims. In total, closures contributed a 2.5 percentage point decline in employment, which represents just 15 percent of the job loss over this period.

Importantly, the effects of the closure orders are also estimated less precisely than in the case of UI claims. This result is illustrated by the width of the confidence band, which now indicates that there is no significant difference between the path implied by the closure orders and the path employment would have followed in the absence of any mitigation policy.

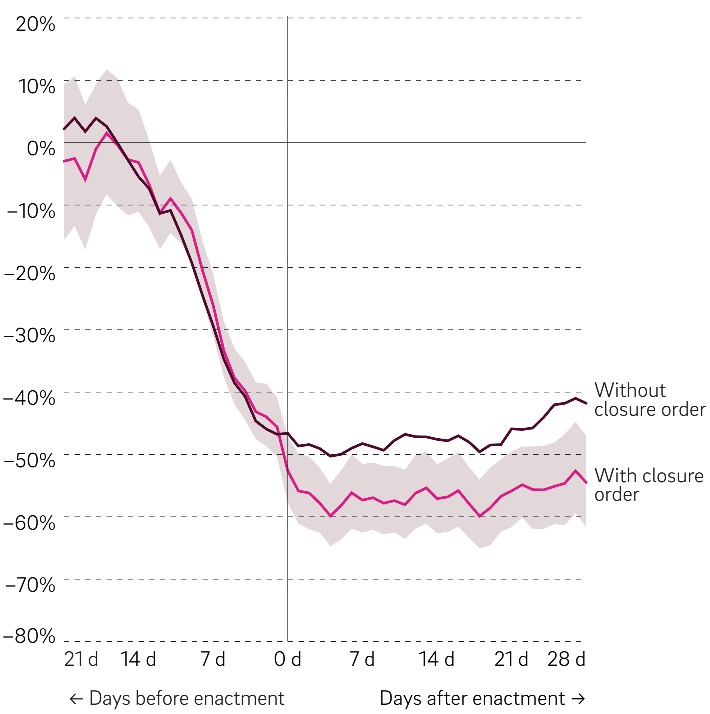
Small-Business Employment

The COVID-19 crisis has taken a particularly large toll on small firms in the U.S. For businesses with fewer than 50 workers, employment fell over 25 percent in March and April—almost twice the rate observed for larger employers.[16] The causes of job loss in smaller businesses is thus of special interest.

FIGURE 6

Employment in Small Businesses Fell More and Earlier Than in the Corporate Sector as a Whole

Estimated cumulative change in employment (percent) in small businesses before and after state enacts essential-business list, two scenarios



Source: Author's estimate of event study model using daily employment data from Homebase (https://joinhomebase.com).

Note: There is a 90 percent probability that the "true" path of claims implied by the essential list lies within the shaded band.

To measure job loss in smaller businesses, I drew on data from the software company Homebase, whose scheduling app is used by clients to track employees' hours worked.[17] Homebase covered some 60,000 small firms at the onset of the pandemic and provides daily data on employees' hours worked, which allows us to more precisely relate employment outcomes to the timing of state policies. A drawback of the data is that Homebase clients represent only a segment of the broader small-business community: Homebase clients are disproportionately drawn from the food service sector and are relatively small (even for small businesses), averaging only five employees prior to the pandemic.

Employment among Homebase clients also fell far more, and much earlier, than in the corporate sector as a whole: It fell 45 percent *prior* to the enactment of any essential lists (burgundy line in Figure 6). This collapse in employment among small food service and retail firms, which occurred in the first three weeks of March, is likely due to the steep decline in consumer traffic observed in all states as households sought to limit their exposure to the virus. Indeed, reports of consumer traffic at retail and recreational establishments show declines of 40 percent during this period.[18] Small businesses have relatively little cash on hand to meet expenses when revenues fall so steeply, triggering job losses.[19]

Still, when an essential list is introduced, its impact on Homebase clients is immediate and significant: Employment falls 6 percent and then declines further in the next several days. On average, the essential list depresses employment by almost 10 percent over the subsequent month (the difference between the pink and burgundy lines in Figure 6). However, even in the absence of a policy, the pandemic would have reduced employment by 55 percent on average over this same period (the burgundy line in Figure 6). Thus, the essential list accounts for 15-20 percent of the overall decline. The estimated share of job losses due to the orders is consistent with the earlier results reported in Figure 5 based on employment for a broader set of firms.[20]

These results largely confirm estimates in earlier research. For their 2020 Brookings paper, University of Illinois economist Alexander W. Bartik and his coauthors conducted virtually the same event study analysis of Homebase data but used SAH orders. They found that the effects of SAH orders were just as persistent but somewhat larger than were the effects implied by my analysis. However, they caution that such persistent effects of a mitigation policy may be difficult to disentangle from other trends in the state's response to COVID-19. If such trends are in force, the authors show, the effect of the policy after 10 days is less than half as large and then largely dissipates over the next two weeks.

New York University economist Hunt Allcott and his coauthors assessed mitigation policies on COVID-19 case rates, consumer traffic, and employment outcomes, though I focus on their analysis of Homebase data. These authors collected SAH orders for all counties, which tightens the link between the governing policy in an area and the area's economic outcomes. Still, the results of my analysis of Homebase data are largely consistent with their estimated effects and with the implied contribution of SAH orders to the overall decline in employment.[21]

Final Thoughts

In this article, I have reviewed the effect of states' COVID-19 mitigation policies that targeted business activity. I considered in particular the degree to which essential-business lists contributed to the historic rates of job loss observed in March and April 2020. I conducted this analysis within a popular event study framework featured in numerous research papers on COVID-19. I found that the effects of the policies vary somewhat across employment indicators, but on balance the results suggest that they increased job losses by 15-25 percent.

15–25%

Estimated increase of essential business shutdown orders on job losses

This article has merely scratched the surface of the burgeoning research on the economic effects of COVID-19 mitigation policy. Indeed, this review, which has focused on the labor market, has had to largely bypass related analyses of consumer activity.[22] Clearly, a more integrated analysis of employment and expenditures would be worthwhile. In the meantime, I close with a few remarks on related labor market research I did not have the space to cover in detail.

Job Loss

The evidence on job loss is still not settled. Whereas I have examined daily and weekly data, two studies report larger effects of SAH orders using two prominent sources of monthly employment data. Gupta and her coauthors examine the Current Population Survey, which is the official source of the unemployment rate. They find that if a state had been under an SAH order for 20 days as of mid-April, its employment rate was 3.5 percentage points lower. This estimate represents more than 40 percent of the decline in employment between March and April. Forsythe and her coauthors also find relatively large effects in the monthly Current Employment Statistics survey, from which official nonfarm payroll numbers are derived. It is not immediately clear how to reconcile these results with those based on other employment indicators. Daily and weekly employment data have generally been preferred in prior research, because it's possible to draw a tighter link between the enactment of policies and employment outcomes. Still, these results based on monthly data merit further attention.[23]

Reopenings

A more complete evaluation of mitigation policies must also consider the effect of lifting such mandates. Economist Raj Chetty and his coauthors at Harvard's Opportunity Insights institute estimate a 1.5 percent gain in employment within two weeks of lifting an SAH order. Interestingly, the absolute size of this effect is smaller than, but not much different from, what I find when looking at the effect of imposing a closure order. Estimates from Bartik and Allcott (and their respective coauthors) also suggest that, on balance, the effects of lifting closure policies were somewhat smaller than the effects of imposing the policies.

Recent Policy Actions

The findings reported here and elsewhere can help interpret recent labor market activity. There has been a deceleration in employment growth in recent months, during which many localities reimposed restrictions on entertainment, recreation, and food services establishments. Research to date would suggest that these restrictions contributed to the slowdown, though recent policies were more targeted than the business closure orders in March and April. However, a lesson from prior work is that the key driver of labor market activity is likely the substantial escalation in the spread of COVID-19 itself. Still, further research is needed on these recent policy actions.

Notes

1 Consider the case of Pennsylvania. Prior to publishing its essential list, the state's only restriction on business activity was a prohibition on indoor dining. By contrast, initial orders in many other states effectively shuttered the amusement and recreation sectors through limits on gatherings and closed personal care services. In order to enforce a degree of consistency in coding initial closures, I did not classify closing indoor dining *alone* as an initial order. Accordingly, Pennsylvania's initial closure order is also its essential list.

2 See also Tomer and Kane (2020a, 2020b).

3 The data set underlying Atalay et al. (2020) attempts to capture much of the variation across states and counties in the scope of their closure and reopening orders.

4 The "exposure" of workers to a mitigation policy can also differ across states even if the policy is the *same*. For example, a given policy can have disparate effects based on the feasibility of telework. This cross-state variation will be considered in future research. For more on telework, see Blau et al. (2020), who combine CISA guidance with Dingel and Neiman's (2020) estimates of the feasibility of telework to identify *frontline* workers, the subset of essential workers who are most likely to have to work on site.

5 In March 2020, Congress temporarily extended UI eligibility to many more workers, such as independent contractors, and increased UI compensation. This decision surely contributed to the eightfold increase in weekly initial claims relative to the Great Recession. However, much of this increase reflected heightened job loss rather than a greater propensity among the laid off to apply for, and receive, UI. The Current

Population Survey shows, for instance, that the number of newly unemployed rose sixfold relative to the Great Recession.

6 I determined the timing of an essential list according to county policies for six states where at least half of the population was under county orders by the time the state-wide policy was enacted. The six states were California, Florida, Kansas, Missouri, Texas, and Utah. For detailed analyses of the effects of county and city SAH orders on consumer activity, see Alexander and Karger (2020) and Goolsbee and Syverson (2020).

7 More specifically, I assume an essential list applies to a week as long as it is enacted before the final day of the week (i.e., by Friday). This approach recognizes that essential lists can take effect near the end of a day, so it may be infeasible to apply for UI that week if the policy is implemented on Saturday. Alternatively, one could assume a policy applies to a given week only if it was introduced nearer to the start of the week, as in Gupta et al. (2020). When I do this, I find that the immediate effect of an essential list is larger, as anticipated. However, the effect of the list is also estimated to be significant even *before* it is introduced, which makes sense: The list was indeed in effect before the week marked as the date of enactment.

8 Figure 4, and related figures in this article, are computed as follows. I draw a vector of parameter values based on the covariance matrix of the regression estimates, and then calculate a predicted path of the claims rate for each policy-adopting state. The path underlying the burgundy line is computed using only the time effects, whereas the path underlying the pink line also accounts for the policy effects. The calculation of each path (burgundy and pink) takes account of the timing of the state's order and is then expressed in terms of weeks from the date of the order. I compute an unweighted average of each path across states, draw another parameter vector, and repeat. The figure illustrates the typical path across 500 draws, and the confidence band encompasses 90 percent of the simulated observations.

9 If I extend the horizon beyond four weeks, the sample will overlap with the period of the first "reopening" orders. I wish to focus here on job loss and so avoid any interaction with the reopening period.

10 These results persist, and indeed strengthen somewhat, if I drop from the sample the 10 states that never issued an essential list. Thus, the variation in the timing of orders among essential-list-issuing states is sufficient to identify an effect of the list.

11 Importantly, Alexander and Karger (2020) do *not* find pretrends when they examine the effect of *county*-level SAHs on consumer traffic and expenditure. Initial UI claims by county can be collected from each state, and Alexander and Karger's results suggest that a broader county-level analysis may be worthwhile.

12 This difference in emphasis likely stems from various discrepancies in the statistical models we used. One difference is that Lin and Meissner examine changes in the natural logarithm of initial claims, whereas I consider changes in the claims rate, or claims as a share of the labor force. The natural log function can compress changes in claims relative to the claims rate. For example, the log of claims in North Dakota and Pennsylvania increased equally in the latter half of March even though the change in the claims rate in Pennsylvania was *twice* as large as in North Dakota. The effect of Pennsylvania's early business-closure policy is more evident in the claims rate.

13 Both Forsythe et al. and Gupta et al. find larger effects of SAH orders when examining monthly data. I return to this point a little later.

14 See Chetty et al. (2020).

15 Let n_t be the number of workers at a firm on day t; $m_t = (1/7)\sum_{i=0}^{6} n_{t-i}$ the 7-day moving average; and $\underline{m}$ the January average. In the data, we see $g_t \equiv m_t/\underline{m} - 1$. Dividing g_t by g_{t-1} and making a few manipulations shows that $\frac{m_t - m_{t-1}}{(1/2)(m_t + m_{t-1})} = \frac{g_t - g_{t-1}}{1 + (1/2)(g_t + g_{t-1})}$. We observe the right side of this equation. Recalling the definition of m_t, the left side is equivalent to $\frac{(1/7)\, n_t - n_{t-7}}{(1/2)(m_t + m_{t-1})}$. Multiplying by 7 yields a measure of employment growth between day $t-7$ and day t.

16 See Cajner et al. (2020). Bartik et al. (2020b) estimate an even faster rate of decline, although entertainment and recreation establishments are overrepresented in their survey.

17 In using Homebase (https://joinhomebase.com) data to chart the effect of the pandemic on small businesses, I'm following the example set by other researchers, including Bartik et al. (2020b), Allcott et al. (2020), and Kurman et al. (2020).

18 This estimate is based on the Mobility Reports published by Google and derived from the Location History data of Google users. Analyses of similar data from different vendors (e.g., SafeGraph) have the same basic message. See Goolsbee and Syverson (2020).

19 See Bartik et al. (2020a).

20 The initial closure orders, which typically targeted food service and recreational establishments, do not appear to have had a significant, immediate effect on the employment of Homebase clients. In separate event study estimates, the impact of the initial orders is not clear until seven days or so after their enactment, by which point states had begun to issue essential lists. A clear and immediate effect of the initial orders may be difficult to detect using only differences in the timing of the orders; many states issued such orders on very nearly the same day.

21 These authors also look at closure orders. But again, the closure orders in this case—restrictions on "gathering venues for in-person services"—are probably akin to what I call the initial closures rather than to the broader essential-business lists.

22 See, among others, Alexander and Karger (2020), Baker et al. (2020), Coibion et al. (2020), and Goolsbee and Syverson (2020).

23 See also Coibion et al. (2020), who report relatively large labor market and consumer expenditure effects based on a series of customized surveys.

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