ALEXANDER W. BARTIK
University of Illinois at Urbana-Champaign

JESSE ROTHSTEIN
University of California, Berkeley

MARIANNE BERTRAND
University of Chicago

MATTHEW UNRATH
University of California, Berkeley

FENG LIN
University of Chicago

Measuring the Labor Market at the Onset of the COVID-19 Crisis

ABSTRACT We use traditional and nontraditional data to measure the collapse and partial recovery of the US labor market from March to early July, contrast this downturn to previous recessions, and provide preliminary evidence on the effects of the policy response. For hourly workers at both small and large businesses, nearly all of the decline in employment occurred between March 14 and 28. It was driven by low-wage services, particularly the retail and leisure and hospitality sectors. A large share of the job losses in small businesses reflected firms that closed entirely, though many subsequently reopened. Firms that were already unhealthy were more likely to close and less likely to reopen, and disadvantaged workers were more likely to be laid off and less likely to return. Most laid-off workers expected to be recalled, and this was predictive of rehiring. Shelter-in-place orders drove only a small share of job losses. Last, states that received more small business loans from the Paycheck Protection Program and states with more generous unemployment insurance benefits had milder declines and faster recoveries. We find no evidence that high unemployment insurance replacement rates drove job losses or slowed rehiring.

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The COVID-19 pandemic hit the US labor market with astonishing speed. The week ending March 14, 2020, there were over 250,000 initial unemployment insurance claims—about 25 percent more than the prior week, but still below January levels. Two weeks later, there were over 6 million claims. This shattered the pre-2020 record of 1.07 million, set in January 1982. Claims remained above 1 million for nineteen consecutive weeks, and over 60 million claims were filed by the end of October. The unemployment rate shot up from 3.5 percent in February to 14.7 percent in April, and the number of people at work fell by about 25 million.

The United States' labor market information systems are not set up to track changes this rapid. The primary official measures of the state of the labor market are two monthly surveys, the Current Population Survey (CPS) of households and the Current Employment Statistics (CES) survey of employers. Each collects data about the second week of the month. In 2020, an enormous amount changed between the second week of March and the second week of April.

In this paper, we attempt to describe the labor market in what may turn out to be the early part of the COVID-19 recession, compare the labor market downturn to previous recessions, and provide some evidence on the policies enacted in response to the downturn. We combine data from the traditional government surveys with nontraditional data sources, particularly daily work records compiled by Homebase, a private sector firm that provides time clocks and scheduling software to mostly small businesses. We link the Homebase work records to a survey answered by a subsample of Homebase employees. We supplement the Homebase data with data on firms with more than 100 employees from Kronos, another private sector firm providing time clock, scheduling, and other services. We use the Homebase and Kronos data to measure the high-frequency timing of the March-April contraction and the gradual April-early July recovery. We use CPS and Homebase data to characterize the workers and businesses most affected by the crisis. And we use Homebase data as well as data on physical mobility from SafeGraph, based on electronic tracking of mobile phones, to measure the effects of state shelter-in-place orders and other policies (in particular, the Paycheck Protection Program and unemployment insurance generosity) on employment patterns from March to early July.

We are not the only ones studying the labor market at this time. Allcott and others (2020), Alon and others (2020), Cajner, Crane, and others (2020), Cajner, Figura, and others (2020), Chetty and others (2020), Cortes and Forsythe (2020), Dey and others (2020), Forsythe and others (2020), Goolsbee and Syverson (2020), Gupta and others (2020), Kurmann, Lalé, and Ta (2020), Lin and Meissner (2020), and Mongey, Pilossoph, and Weinberg (2020) all conduct exercises that are related to ours. There are surely many others that we do not cite here. Our goal is neither to be definitive nor unique, but merely to establish basic stylized facts that can inform the policy response to, and future research on, the crisis.

The paper proceeds as follows. Section I describes our data sources. Section II provides an overview of the labor market collapse and subsequent partial recovery. In section III, we explore who was affected by the collapse, investigating characteristics of workers that predict being laid off in March and April, then being reemployed thereafter. Section IV uses event study models to examine the effects of non-pharmaceutical interventions (i.e., shelter-in-place and stay-at-home orders) on hours worked in the Homebase data and on physical mobility. Section V examines the impacts of the roll-out of unemployment insurance expansions at the state level and of the Paycheck Protection Program on Homebase hours. We conclude in section VI.

I. Data

We rely on three primary sources to measure the evolution of the labor market during the first half of 2020, supplementing with additional measures that provide context.

First, we use the CES survey of employers, the source of official employment counts, to track industry-level employment changes at a monthly frequency. Second, we use the CPS, a monthly survey of about 60,000 households that is the source of the official unemployment rate. Respondents are asked each month about their activities during the week containing the twelfth of the month. The most recent available data are from the June survey. By matching interviews with the same households in consecutive months, we identify workers who were employed in March but not in April, or who were out of work in April or May but reemployed in May or June.

We combine these official data sources with daily data from a private firm, Homebase, which provides scheduling and time clock software to tens of thousands of small businesses that employ hundreds of thousands of workers across the United States and Canada. The time clock component of the Homebase software measures the exact hours worked each day for each hourly employee at the client firms. Employers are identified by their industry and location.

Homebase's customers are primarily small firms in food and drink, retail, and other sectors that employ hourly workers (see online appendix A). The time clock data largely cover hourly workers within those firms. The Homebase subpopulation is highly relevant to the current moment, as the pandemic seems to have most affected the industries and small businesses that form the Homebase clientele. Indeed, we show that the employment collapse was much more dramatic in the Homebase sample than in the labor market as a whole.

When analyzing the Homebase data, we focus on US-based firms that were already Homebase clients before the onset of the pandemic. We define a base period as the two weeks from January 19 to February 1, and scale hours in subsequent weeks as a fraction of hours worked during this period.[1] We consider a firm to have shut down if in any week (Sunday to Saturday) it had zero hours reported by all of its hourly workers, and to have reopened if, following a shutdown, it again appears with positive hours.

We supplement the Homebase data with information from a survey of workers. Survey invitations were sent starting May 1 to everyone who had signed into the Homebase software as a user since February 2020. We use survey responses received by July 7, matched to the administrative records for the same workers, and we limit this group to workers with positive hours in the base period and only one Homebase client employer since January 19, 2020. Among the roughly 426,000 workers meeting this description, approximately 1,700 (0.4 percent) responded to our survey. Despite the low response rate, online appendix table B1 shows that the survey respondents are roughly representative of all Homebase workers on the (limited set of) dimensions on which we can compare them. However, survey respondents are somewhat positively selected on hours worked at the Homebase employer (online appendix figure B1) and hence may be more representative of the "regular" workforce at these employers. Online appendix table B2 summarizes demographic characteristics for survey respondents.

1. We exclude from all analyses any individual daily observations with more than twenty reported hours.

II. Overview of the Labor Market Collapse

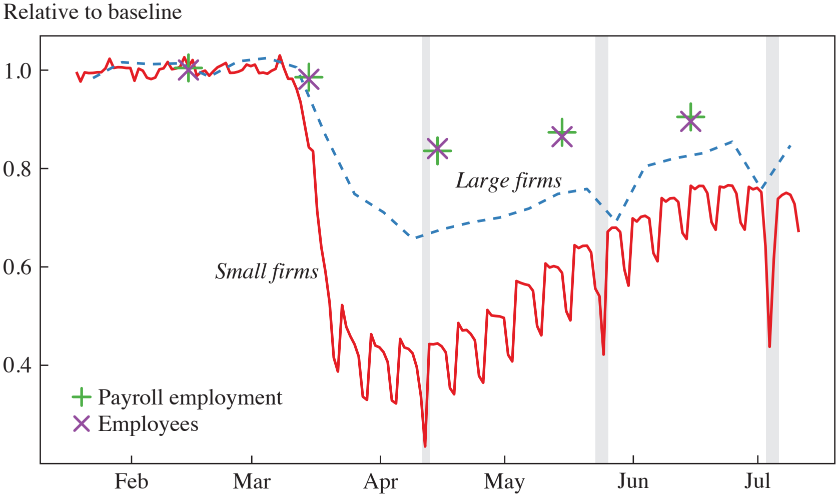
Between February and April 2020, the unemployment rate (not seasonally adjusted) spiked by 10.6 percentage points, reaching 14.4 percent, while the employment rate fell by over 9 percentage points. These two-month changes were roughly 50 percent larger than the cumulative changes over more than two years in the respective series in the Great Recession. In sharp contrast to past recessions, the February–April unemployment increase was entirely driven by increases in the share of workers who expected to be recalled to their former positions; the share who were looking for new jobs shrunk slightly. The temporary layoff share of the unemployed has never previously exceeded 30 percent, but rose to nearly 80 percent in April.[2] Employment and unemployment recovered a small amount in May, but remained in unprecedented territory.

The usual labor market categories are not well suited to pandemic conditions, and the official unemployment rate understated the amount of joblessness. The share who were employed but not at work grew by 3.3 percentage points from March to April.[3] The Bureau of Labor Statistics (BLS) believes much or all of this increase derives from misclassification of people who should have been counted as on temporary layoff; if they had been classified that way, the unemployment rate in April would have been 19.2 percent instead of 14.4 percent (BLS 2020, item 14). Similarly, labor force nonparticipation rose, with many of the new nonparticipants saying that they wanted jobs but were not actively looking for work or were not available to take jobs (BLS 2020, item 18). It seems likely that many of these were kept out of work by the pandemic and would otherwise have been counted as unemployed. If they had been included as well, the adjusted unemployment rate in April would have been well above 20 percent.

2. See https://fred.stlouisfed.org/graph/?g=x5O2 for a long time series using CPS data. Hedin, Schnorr, and von Wachter (2020) use administrative records from the California unemployment insurance system to explore the characteristics of unemployment insurance applicants. They find that over 90 percent of new claimants in late March reported that they expected to be recalled to their prior jobs, up from around 40 percent in February. The share expecting recalls gradually declined after late March, to around 70 percent at the end of May, but this nevertheless indicates that many of the job losses may not be permanent, and is consistent with the increase in temporary layoffs measured by the CPS.

3. See https://www.hamiltonproject.org/blog/who_are_the_potentially_misclassified_in_the_employment_report.

Figure 1. The Labor Market Collapse



Source: Homebase; Kronos; CES; CPS.

Notes: The small firms series shows daily total hours worked across all firms in Homebase data, as a fraction of average hours worked on the same day of the week in the January 19–February 1 base period. The sample includes firms (defined at the firm-industry-state-MSA level) that recorded at least eighty hours in the base period. The large firms series shows weekly punches (shifts) among hourly workers at firms with more than 100 employees, from Kronos data, as a share of the average during the January 20–February 2 base period. Payroll employment and employees series show monthly estimates from the official CES and CPS surveys, respectively, scaled as a share of their January levels. Vertical bars mark the weekends containing Easter, Memorial Day, and the Fourth of July.

Monthly statistics are inadequate to understanding the rapidity of the labor market collapse. Figure 1 plots daily total hours worked at Homebase's client firms. We also plot three lower-frequency comparisons: (1) weekly counts of shifts worked by hourly workers at larger firms (>100 employees) as measured in data collected by Kronos, another similar firm that serves larger employers; (2) payroll employment from the CES; and (3) monthly household employment from the CPS. In all four series, we report employment measures relative to a base period in late January.[4] Total hours worked at Homebase firms fell by approximately 60 percent between the beginning and end of March, with the bulk of this decline between March 13 and March 24. The nadir seems to have been around the second week of April. Hours then grew slowly and steadily through

4. The base period is January 19–February 1 in the Homebase data, January 20–February 2 in the Kronos data, and January in the CES and CPS data.

mid-June. They made up about half of the lost ground by the third week of June, but then fell back again slightly in late June and early July.[5]

The time pattern for larger firms in the Kronos data is more muted but quite similar in shape. The most rapid decline in employment occurred in the last two weeks of March, the nadir of employment occurred in the second week of April, and firms recovered about 50 percent of their employment losses by the third week of June. The lower-frequency CES and CPS data are also consistent with these patterns, with the employment trough in April in both series and a roughly 50 percent recovery by June. The most notable difference between the series is the magnitude of the overall employment decline, around 16 percent in the CES and CPS, 34 percent in the Kronos sample, and 60 percent in the Homebase data. As we discuss below, this likely reflects a combination of differences in industry coverage and firm size, with the smaller firms in food, drink, and retail that are the bulk of Homebase clients experiencing the most severe employment declines during this downturn.

III. Who Are the Unemployed? Who Are the Rehired?

In this section we explore the distribution of the job losses across industries, firms, and workers.

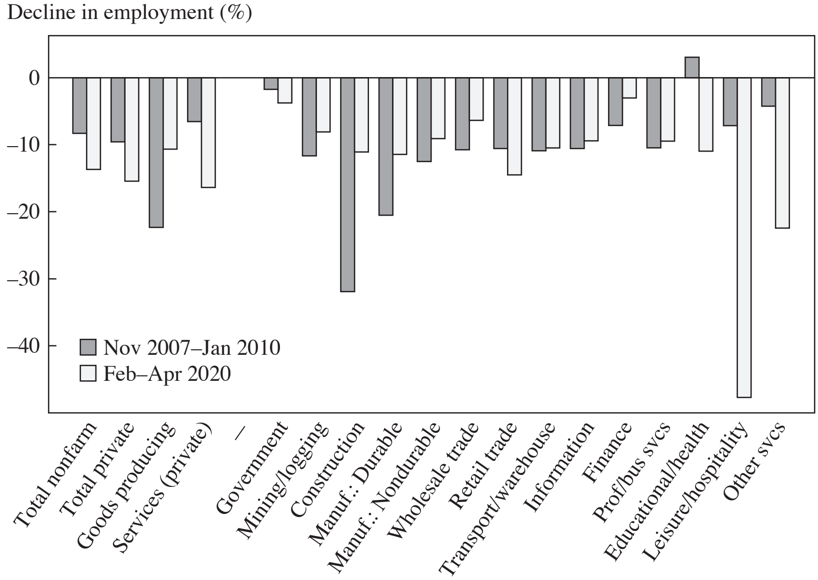
III.A. Industry

Figure 2 uses CES data to show the two-month decline in employment from February to April, by major industry. The service sector, and particularly its low-wage segment, experienced by far the largest drop in employment. In leisure and hospitality, which includes restaurants and hotels, employment fell by nearly half between February and April. Other services, which include repair and maintenance services, personal and laundry services, and services to private households, were the second most impacted, with more than 20 percent of employment lost by April. Workers employed in retail trade were also disproportionately exposed.[6]

5. There are clear day-of-week effects in the Homebase data as well: Homebase employment is lower on weekends than on weekdays since the onset of the crisis, relative to the day-of-week pattern in the base period. These reductions are largest in the holiday weeks of Easter, Memorial Day, and the Fourth of July.

6. Online appendix figure C1 shows monthly changes in 2020. Consistent with figure 1, employment recovered somewhat in May and June, with the recovery concentrated in the same sectors that saw the largest declines.

Figure 2. Employment Change in Great Recession and 2020, by Sector



Source: CES.

Notes: Payroll employment by industry or aggregate; the first four categories are aggregates that include many of the remaining series. Not seasonally adjusted.

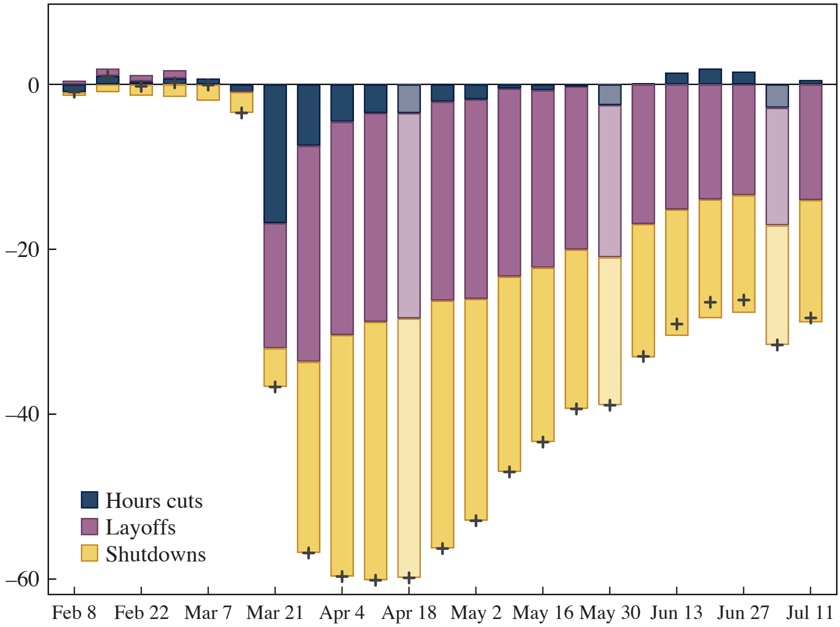
Figure 2 also shows the cumulative decline in employment between November 2007 and January 2010. Job loss in 2020 was about 60 percent larger than in the whole of the Great Recession, and the sectoral composition was quite different. Construction and durable goods manufacturing declined the most in the Great Recession, while low-wage services were relatively insulated.

III.B. Firm Closings and Reopenings

An advantage of Homebase data over the CPS, beyond their high frequency, is that they enable us to link workers to their employers. We use this link to separate the observed change in total hours into three channels: firm shutdowns, layoffs, and cuts in hours. We define a firm as having fully shut down in a given week if the Homebase data record zero employees clocking in at that firm during that week. Among firms that have not shut down, we count layoffs as the proportional change in the number of workers with positive hours in a week, relative to the

Figure 3. Hours Changes at Homebase Firms by Week

Percent change in hours attributable to each form of contraction



Source: Authors' analysis of Homebase data.

Notes: Changes in total hours worked at Homebase firms that were active between January 19 and February 1, relative to weekly hours during that period, decomposed into three sources: those due to firm closures, changes in the number of workers at continuing firms, and changes in average hours among remaining workers. The contribution of firm closure is measured by summing up baseline hours of firms that are shut down (with zero recorded hours) each week. The contribution of headcount changes (layoffs) is the proportionate change in the number of workers at continuing firms, multiplied by those firms' hours during the baseline period. The contribution of changes in average hours is the proportionate change in hours per worker at continuing firms, multiplied by baseline firm hours. Markers indicate the cumulative net effect, combining all three. Lightly shaded bars mark the weeks containing Easter, Memorial Day, and the Fourth of July.

baseline. Last, we define hours cuts as the reduction in average hours, relative to the baseline period, among workers remaining employed at still operating firms.[7]

Figure 3 reports the percent change in hours each week since early February attributable to these three channels. Except for the first week

7. Some firms that appear to us to have shut down may have retained some non-hourly workers who do not use the Homebase software to track their time, so should properly be classified as layoffs at continuing firms.

of the labor market collapse, reductions in hours per worker as defined above have accounted for a very minor part of the change in total hours at Homebase businesses.[8] Instead, the decline in total hours came primarily from firms that closed entirely and from reductions in the number of workers at continuing firms. Layoffs accounted for a larger share in March and shutdowns in April, but thereafter the two have had about the same quantitative impact on "missing hours."[9]

We next use the Homebase data to assess the role of firm reopenings in the (partial) recovery. Of the roughly 42,000 unique firms in our baseline sample, about 40 percent shut down for at least one week by April 4.[10] About 70 percent of these firms have reopened for at least a week after that date (though 10 percent have since closed again). In online appendix figure C2, we report the distribution of hours at ever-closed businesses through July 11, as a share of total baseline hours at these businesses. In the most recent week, total hours at these firms remain close to 55 percent below their baseline level. About two-thirds of these missing hours are attributable to businesses that remain closed; the remainder reflects businesses that have reopened at a reduced scale. Of the roughly 45 percent of hours that have been regained, a vast majority have come from rehiring of workers that had been employed by the business before the shutdown. However, the share attributable to new hires has been slowly trending up over time, reaching almost a quarter by the week of July 5–11. It is worth noting that Homebase firms have high turnover rates even in good times; in fact, the share of hours being worked by new hires is lower in 2020 than over similar periods in 2018 and 2019 (see online appendix figure C3).

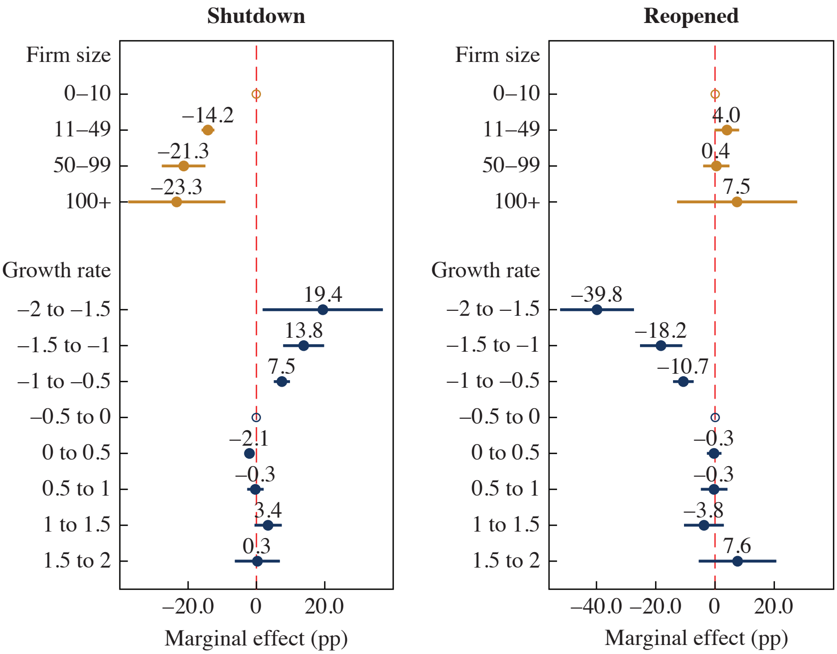
The Homebase data also allow us to investigate which businesses were more likely to shut down as well as take an early look into which firms are most likely to make it through the crisis. We consider two employer characteristics: size, defined as the employer's total number of unique employees

8. We conjecture that the large role for hours reductions in mid-March is an artifact created by mid-week layoffs or firm closings. When workers stop working in the middle of a week, our method counts that as a reduction in weekly hours that week and as a layoff or firm closing the following week. Consistent with this, online appendix figure C4 shows that the distribution of workers' hours fell the first week of the collapse but returned to normal the following week and has been quite stable through the year to date.

9. Online appendix figure C5 shows firm exits using a stricter definition that counts firms as exiting only if they do not return by mid-July. In 2018 and 2019, about 2 percent of firms exited Homebase each month. In March 2020, about 15 percent exited. After early April, the exit rate was similar to prior years.

10. Another 10 percent shut down for at least one week between April 5 and mid-July, mostly in mid-April.

Figure 4. Likelihood of Firm Closure and Reopening by Firm Size and Precrisis Growth Rate



Source: Authors' analysis of Homebase data.

Notes: Marginal effects and confidence intervals from two logit models, with state and industry fixed effects. Left panel: all firms in the Homebase data; the outcome is an indicator for the firm shutting down (recording zero hours) for at least one week between March 8 and April 4. Right panel: firms that shut down by April 4; the outcome is an indicator for subsequently reporting positive hours before July 11. Firm size is the number of unique employees in the base period (January 19–February 1). The growth rate is the change in the number of employees between January 2019 and January 2020, divided by the average of these two periods. Marginal effects are evaluated for a professional services firm in California with up to ten employees in the base period and a growth rate of –.5 to 0. Left panel: $N = 24{,}872$; right panel: $N = 9{,}594$.

in the January 19–February 1 base period, and growth rate, which we define as the change in the number of employees between January 2019 and January 2020 divided by the average of the beginning and end-point levels, a ratio that is bounded between –2 and 2.

Figure 4 reports marginal effects from logit models for the likelihood of the firm shutting down by April 4, and, for firms that did, for the likelihood of reopening by July 11, controlling for state and industry fixed effects. Larger firms were much less likely to shut down than smaller firms. Conditional on having shut down, larger firms are also somewhat more likely to have reopened by mid-July, though this is not statistically significant. Most interesting is how the likelihood of shutting down and reopening is predicted by employer growth between January 2019 and January 2020. Businesses that were struggling before COVID-19 have much increased odds of shutting down during the COVID-19 crisis and of remaining closed. Three non-mutually exclusive explanations are that these businesses might have been particularly low on cash and unable to withstand the shock (Bartik and others 2020); that they may have been de-prioritized by banks when they applied for Paycheck Protection Program (PPP) funding; or that the COVID-19 crisis sped up the pruning of some of the less productive businesses in the economy (Barrero, Bloom, and Davis 2020).

III.C. Worker-Level Job Loss and Rehiring

We next explore which workers are most likely to lose their jobs and subsequently be rehired, using both CPS and Homebase data. We estimate multivariate logit models that include a range of worker characteristics as predictors, along with fixed effects for states and major industry groupings. Our first model, in columns 1–2 of table 1, includes all CPS respondents who worked in March and takes as the outcome the absence of work in April, while the second, in columns 3–4, is estimated on those not working in April and May and takes work in the following month as the outcome of interest.

The analysis reveals systematic differences across socio-demographic groups in the likelihood of having stopped work in April. We see a strong U-shaped pattern in age for job loss. Workers who are over 65 years old (or 16 to 25, respectively) were 14.2 (7.8) percentage points more likely to exit work in April than otherwise similar workers aged 26–37. There is also a strong education gradient: Workers without high school degrees were 10.9 percentage points more likely to have stopped working in April than otherwise similar college graduates. Black, Asian, and Hispanic workers were, respectively, 4.8, 5.4, and 1.7 percentage points more likely to exit work in April than otherwise similar white workers. Finally, married individuals were less likely to lose jobs and women were more likely to do so. We do not observe systematic differences based on parental status, for either men or women.

These inequities in the distribution of job loss were for the most part not offset by rehiring in May or June. In particular, older workers, Black and Asian workers, single workers, and women, each more likely to lose their jobs in April, were also less likely to start work again in May or June. On the other hand, there is no clear education gradient in rehiring.

The remaining columns of table 1 repeat the analyses of job loss and rehiring, now using the Homebase data. We link the administrative records on hours worked to the worker survey, which provides demographic information. We define layoff and rehiring somewhat differently, thanks to the higher frequency data: a worker is counted as leaving work if he or she worked in the base period in January but had at least one week with zero hours between March 8 and April 25; then, for these workers, we classify as rehired those who returned to work and recorded positive hours at some point after April 18. Note that we do not distinguish in these definitions between firms that closed entirely and workers who were laid off from continuing firms, nor similarly between rehires at reopening versus continuing firms.

Perhaps unsurprisingly, given the small sample size, few of the estimated effects are statistically significant. However, a few patterns emerge. We see a much higher likelihood of layoff among those without a high school degree and much lower likelihood among those in managerial positions. We also see that workers with children were relatively spared from layoffs. In addition, while Hispanic workers are less likely to be laid off, we also see that, as in the CPS data, Black workers are notably less likely to be rehired.

The survey data we collected also allow us to understand more fully the experiences and expectations of the Homebase workers. Twenty-one percent of the sample reported having experienced a layoff because of COVID-19, while 31 percent report having been furloughed, and 21 percent report hours reductions. Less than 10 percent report having made a decision to not work or work less, with most of those saying it was to protect themselves or their family members from exposure to the virus. Less than 10 percent of the workers whose hours and employment status were negatively impacted by COVID-19 report being paid for any of the hours that they are not working. Among these negatively impacted workers, nearly 60 percent report that their employers encouraged them to file for unemployment insurance. This was notably higher (77 percent) among laid-off workers than among furloughed workers (66 percent) or workers who experienced reduced hours (35 percent). Fifty-two percent of workers that have been laid off report that their employer has expressed a desire to hire them back. Among workers that have been negatively impacted by COVID-19, only about a quarter report looking for work. The modal reason for not searching is an expectation of being rehired; only 7 percent attribute their lack of job search to financial disincentives to work. Among the people that expected to be rehired (when the surveys were conducted, largely in early May), the modal expected rehire date was June 1 (33 percent), followed by July 1 (26 percent).

Table 1. Worker Characteristics and Job Loss and Rehiring in Current Population Survey and Homebase Samples

	CPS				*Homebase*					
	Stopped work in April		*Started work in May or June*		*Stopped work mid-March to late April*		*Started work after stopping*			
	Marg. effect	*SE*	*Marg. effect*	*SE*	*Marg. effect*	*SE*	*Marg. effect*	*SE*	*Marg. effect*	*SE*
Age										
16–25	**0.078**	(0.011)	–0.002	(0.016)	–0.008	(0.043)	0.060	(0.042)	0.010	(0.029)
26–37	—		—		—		—			
38–49	–0.010	(0.007)	–0.009	(0.015)	–0.027	(0.047)	0.034	(0.051)	0.061	(0.054)
50–64	**0.019**	(0.008)	–0.026	(0.014)	–0.011	(0.049)	–0.045	(0.049)	–0.010	(0.034)
65 and over	**0.142**	(0.015)	**–0.066**	(0.018)	0.096	(0.078)	–0.041	(0.074)	0.000	(0.049)
Education Level										
Less than high school	**0.029**	(0.011)	–0.016	(0.017)	**0.178**	(0.064)	–0.007	(0.057)	–0.034	(0.038)
High school	—		—		—		—			
Some college	**–0.016**	(0.006)	–0.014	(0.012)	0.013	(0.037)	–0.001	(0.037)	–0.019	(0.026)
Bachelor degree or higher	**–0.080**	(0.009)	–0.025	(0.013)	–0.015	(0.041)	–0.070	(0.042)	–0.054	(0.042)
Race										
Black	**0.048**	(0.010)	**–0.074**	(0.015)	–0.024	(0.054)	**–0.196**	(0.059)	–0.054	(0.044)
Asian	**0.054**	(0.012)	**–0.053**	(0.018)	–0.059	(0.072)	0.081	(0.066)	0.038	(0.060)
Native American	0.029	(0.022)	–0.019	(0.038)	0.111	(0.145)	–0.043	(0.118)	–0.012	(0.081)
Hispanic	**0.017**	(0.008)	–0.015	(0.013)	**–0.096**	(0.045)	–0.021	(0.044)	0.006	(0.031)

Married	**–0.022**	(0.005)	**0.041**	(0.012)	0.033	(0.036)	0.050	(0.041)	0.034	(0.037)
Female	**0.034**	(0.007)	**–0.028**	(0.012)	–0.013	(0.042)	0.025	(0.039)	–0.024	(0.027)
Child | male	–0.005	(0.011)	0.019	(0.022)	**–0.158**	(0.066)	–0.002	(0.064)	–0.048	(0.042)
Child under 10 | male	0.002	(0.012)	0.002	(0.023)						
Child | female	0.008	(0.010)	–0.009	(0.019)	**–0.098**	(0.046)	0.040	(0.049)	0.010	(0.034)
Child under 10 | female	0.019	(0.012)	0.010	(0.022)						
Occupation										
Manager	**–0.072**	(0.008)	0.031	(0.020)	**–0.244**	(0.056)	0.071	(0.082)	0.037	(0.076)
Expected to be rehired										
Likely									**0.400**	(0.123)
N	31,692		11,387		1,620		1,217		650	

Source: CPS; Homebase.

Notes: Each pair of columns reports marginal effects and standard errors from a separate logit regression, controlling for two-digit industry and state effects (not reported here). In the CPS sample, the model for leaving work in April is limited to those who were at work in March; the model for starting work in May or June is limited to those who were not working the prior month. The models include gender by presence-of-children interactions; we report the marginal effects of children separately for males and females. In the Homebase sample, the model for stopping work is for an indicator for at least one week with zero hours between mid-March and late April, among those in our sample with positive hours in late January who responded to the worker survey. The models for starting work after stopping are for having positive hours in a subsequent week, among those with zero hours in a week; the final model limits to those who were not working at the time of the survey. Marginal effects are evaluated for an unmarried, childless, male, white, non-Hispanic individual age 26–37 with a high school diploma in a nonmanagerial occupation in the professional and technical services industry in California. Bold effects are significant at the 5 percent level.

Respondents were also asked if they would return to their employer if offered the opportunity. Three-quarters of respondents said they would go back. Job satisfaction is an important correlate of this response. For example, 80 percent of workers who strongly agreed with the statement “I liked my manager” would plan to go back if asked, compared with 67 percent who only somewhat agreed with this statement. Also, 89 percent of workers who strongly agreed with “I was satisfied with my wages” would plan to go back to their prior employer if asked, compared to 67 percent who only somewhat agreed with this statement.

In the final columns of table 1, we assess how expectations about rehiring relate to the likelihood of being rehired (defined as above). We reestimate the logit for rehiring, limiting the sample to those who were out of work at the time of the survey and adding an indicator for expecting to be rehired. Workers who believed it was likely they would be rehired were 40 percentage points more likely to be rehired subsequently than were otherwise similar workers in the same industry and state who believed a rehiring was unlikely. These results indicate that workers had access to predictive information about the odds of a maintained firm-worker match that may have helped at least some of them better manage through what was otherwise a period of massive disruption and uncertainty. The converse of this, though, is that the workers who have not yet been rehired disproportionately consist of those who never expected to be, making it less likely that further recovery will lead to additional rehiring.

IV. Evaluating Non-pharmaceutical Interventions

Many firm closures were closely coincident with state closure orders and other non-pharmaceutical interventions, and policy has generally proceeded on the assumption that many firms will reopen when these orders are lifted. It is not evident, however, that firms closed or remain closed only because of government policy. Closures reflected increased awareness about the threat posed by COVID-19, and consumers, workers, and firms might have responded to this information with or without government orders. Following closings, many businesses may have been permanently damaged and may not reopen even when conditions improve. Moreover, insofar as consumer behavior rather than state orders is the binding constraint on demand for firm services, the mere lifting of an order may not be enough to restore adequate demand.

In this section, we study the relationship between state labor market outcomes and so-called shelter-in-place and stay-at-home orders (which we refer to collectively as “shutdown orders”) that restrict the public and private facilities that people can visit to essential businesses and public services. This type of intervention is both the most prominent of the nonpharmaceutical interventions and the one that may have the largest direct effects on economic activity. We test the importance of these directives on firms’ hours choices, as captured by the Homebase data. We use event study models, using both contrasts between states that did and did not implement shutdown orders and variation in the timing of these orders to identify the effect of orders on hours worked. We also estimate event studies of the effect of the *lifting* of public health orders, which need not be symmetric to the effect of imposing them.

Stay-at-home and reopen orders are sourced directly from government websites.[11] We define a stay-at-home order as any order that requires residents to stay at home or shelter in place. Orders that conveyed COVID-19-related guidelines but did not require residents to shelter in place are excluded.[12] In states that had stay-at-home orders, we define reopen orders as the first lifting of any of the shutdown-related restrictions on business activities, and time them to the effective date.[13] Online appendix figure C6 shows the number of states with active shutdown orders between the start of March and the present. California was the first state to impose a shutdown order, on March 19. The number of active orders then rose quickly, reaching 44 in early April. It was stable for about three weeks, then began to decline as some states started to reopen in late April and early May. By June 1, all states had reopened.

Stay-at-home orders can reduce employment simply by prohibiting nonessential workers from going to work. They can also have indirect effects operating through consumer demand, which may relate to public awareness of COVID-19, willingness of consumers to visit businesses, and COVID-19 caseloads. Consequently, we supplement our event study analysis of hours data from Homebase with data on mobility, which

11. They most commonly come from centralized lists of executive orders; see, for example, “Executive and Administrative Orders,” Illinois.gov, https://www2.illinois.gov/government/executive-orders. In some cases they come from centralized lists of public health and COVID-19-related orders; see, for example, “Public Health Orders and Executive Orders,” New Mexico Department of Health, https://cv.nmhealth.org/public-health-orders-and-executive-orders/.

12. See, for example, “Stay Safe, Stay Home Directive,” Coronavirus Utah.gov, https://coronavirus.utah.gov/stay-at-home/.

13. Results are similar when we define reopening as the lifting of the original shelter-in-place order.

captures in part the willingness of customers to visit businesses in person. We measure overall mobility using SafeGraph data on visits to public and private locations between January 19 and July 11, 2020, including only locations that recorded positive visits during our base period, January 19–February 1. We normalize the raw count of visits by the number of devices that SafeGraph sees on each day to control for the differences in the count of visits related to SafeGraph's ability to track devices, then rescale relative to the base period.

We estimate event-study models of the effect of shutdown and reopen orders (considered separately) on log hours worked from Homebase and log SafeGraph visits.[14] Each outcome is measured at the state-by-day level. The shutdown model is estimated on data from February 16 to April 19, while the reopen order model is estimated on data from April 6 through July 11. We regress each outcome on full sets of state and date fixed effects, state-specific trends, and a series of "event time" indicators for days relative to the date of the order ranging from −7 (corresponding to 7 days before the event) to the maximum observed in the data, either +31 (corresponding to 31 days after the event) for shutdown orders or +82 (for 82 days after the event) for reopening events.

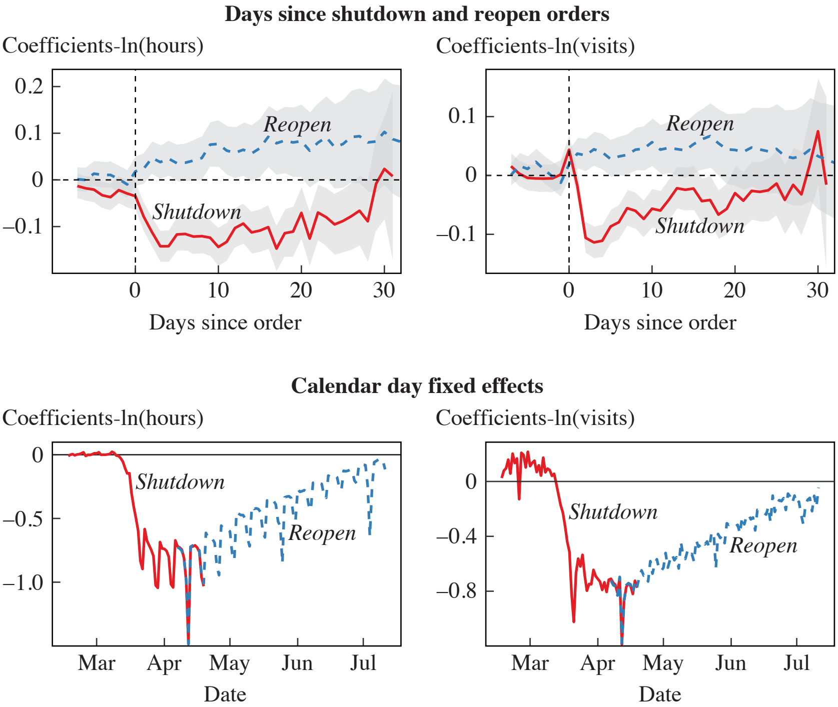
We report these results in figure 5.[15] The top panel reports the event study estimates for the shutdown (solid line) and reopen (dotted line) models, while the bottom panel reports the time effects from these specifications to aid interpretation of the magnitude of the event-study estimates in the top panel. Each panel includes two sub-panels, one for each of our outcomes.

Starting with the estimates for the relationship between shutdown orders and hours in the left side of the top panel of figure 5, we see that hours worked fell immediately following the orders, stabilizing at a decline of roughly 12 log points by the third day after the shutdown order.

14. We do not formally estimate the interaction of the different outcomes, but simply estimate reduced-form effects of orders on each. For examples of studies that do examine interactions among outcomes, see Chernozhukov, Kasahara, and Schrimpf (forthcoming) and Allcott and others (2020).

15. We have also reestimated the event study models without state-specific trends; see online appendix figure C7. An implicit assumption of event study models is that in the absence of orders any differences among states would have grown linearly with calendar time. We have also estimated weighted event studies (Ben-Michael, Feller, and Rothstein 2019) that rely on matching to identify control states with similar counterfactual trends. While traditional event study models can be poorly behaved in the presence of heterogeneous treatment effects (Goodman-Bacon and Marcus 2020; Callaway and Sant'Anna 2019), weighted event studies are not subject to this problem.

Figure 5. Event Study Estimates of the Effect of Imposition and Lifting of Shelter-in-Place Orders



Source: Authors' analysis of Homebase and SafeGraph data.

Notes: Samples for the shutdown event studies consist of state-by-day observations from February 16 to April 19. Samples for the reopening event studies consist of state-by-day observations from April 6 to July 11; states that never had shelter-in-place orders are excluded. Specifications include full sets of state and calendar date effects as well as state-specific trends. We exclude (normalize to zero) the effects for event times less than –7. The shutdown calendar time effects are normalized to zero on February 16. The reopening effects are normalized to align with the shutdown estimates on April 13. Shaded areas show 95 percent confidence intervals for the event time effects.

In our model for physical visits, we see an uptick in visits on the date of the shutdown announcement, possibly reflecting trips to buy groceries or other supplies, followed by a sharp, roughly 15 log-point decline after the shutdown orders are implemented.

Both hours and visits slowly recover after the shutdown order, returning to the level of non-shut-down states by about a month after the initial order. This may reflect adjustment of firms or workers to the restrictions, reduced compliance, or reduced enforcement of restrictions after they were put into place. The solid lines in the top panel of figure 5 report results from the corresponding specifications for reopen orders. We see that reopen orders have the opposite effect of shutdown orders, with hours and visits rising 6 to 8 log points in the first ten days after the orders and growing steadily thereafter. The estimates imply that the effects of shutdown orders, about 12 log points, are largely erased within about two weeks after the orders are lifted.

How should we interpret the magnitudes of the estimates in the top panel of figure 5? One way to think about them is to compare the estimates of the effects of shelter-in-place orders to the calendar date effects from the same specifications, which reflect other determinants of the outcomes that are common to all states. The bottom panel of figure 5 reports the calendar date effects from the specifications reported in the top panel. The sample windows for the two models overlap for the period April 6–19, and we show both, normalizing the reopen order estimates to align with the layoff estimates on April 13.

As expected, given the results in section I above, the calendar date effects show extremely large reductions in hours (about 100 log points at the weekend trough and 60–75 log points on weekdays) and visits in late March. These are much larger than the effects of the orders reported in the top panel. The estimated effect of shutdown orders on log hours (log visits) is about one-sixth (one-seventh) as large as the pure calendar time effects.[16] These results imply that, at least in the short run, shutdown and reopen orders account for only a modest portion of the changes in labor markets and economic activity during the crisis; the overall patterns have more to do with broader health and economic concerns affecting product demand and labor supply rather than with shutdown or reopen orders themselves.[17]

Two caveats are important to keep in mind when interpreting our finding that shutdown and reopen orders play only a modest role in the labor market effects of COVID-19. First, shutdown orders may have

16. For example, for hours, the event time effect of shutdown orders is about −12 log points, as discussed above. By contrast, the calendar time effects for late March and early April are around −75 log points on weekdays, and even larger on weekends.

17. Consistent with this interpretation, when we estimate event studies models that also include effects of school closing events, which should not have had direct effects on small businesses but may have had a larger signaling value about the importance of reducing contact, we find larger effects of these events (online appendix figure C8). Nevertheless, even the combined effect of shelter-in-place and school closing orders is no more than half as large as the pure calendar time effects, and only about a third as large during the labor market trough in the second week of April.

spillover effects on other states not captured in our model. In particular, the first shutdown orders may have played a role in signaling the seriousness and potential risk associated with COVID-19, even if subsequent shutdown orders had more muted effects. Second, over longer time horizons, if shutdown orders reduce caseloads, this may result in labor market improvements that counteract to some extent the negative effects that we estimate here. Explorations of these more complicated medium- and long-run interactions of shutdown orders, labor market activity, social distancing, and caseloads are beyond the scope of our analysis here. Several papers, including Chernozhukov, Kasahara, and Schrimpf (forthcoming) and Allcott and others (2020), have investigated these interactions by combining treatment effect estimates like those here with epidemiological and economic models that specify the relationships among our outcomes to estimate how the full system responds over time to shutdown orders.

V. Evaluating Economic Policy Responses

The Coronavirus Aid, Relief, and Economic Security (CARES) Act was signed on March 27, with over $2 trillion allocated to a range of provisions aimed at supporting the labor market and economy through the early stages of the crisis. In this section, we present descriptive evidence regarding the relationship between two components of CARES—its enhancement of unemployment benefits and the Paycheck Protection Program (PPP) loans to small businesses—and labor market outcomes. While our analyses do not have strong causal designs, they are suggestive about the likely short-run impacts.

The CARES Act included many provisions aimed at expanding and enhancing unemployment insurance benefits. Pandemic Unemployment Assistance (PUA) extended unemployment benefits to independent contractors and others who did not have enough earnings history to qualify for regular unemployment insurance, and Pandemic Emergency Unemployment Compensation (PEUC) provided additional weeks of benefits for those whose regular benefits have run out. A third major component is Federal Pandemic Unemployment Compensation (FPUC), which added $600 to every weekly unemployment benefit payment.

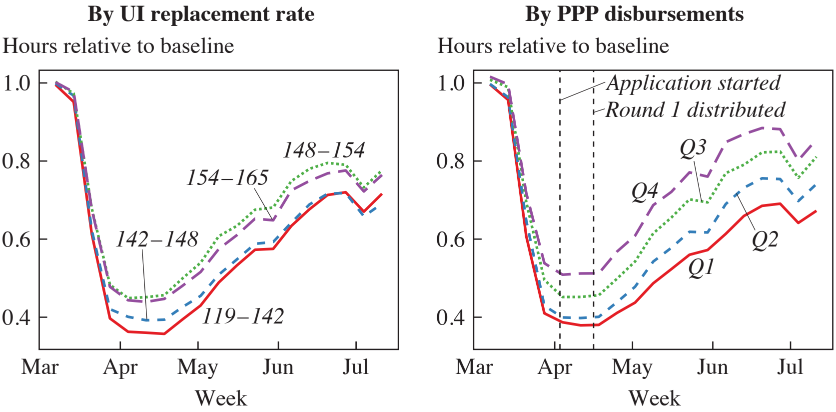
The primary goal of these expansions was to aid workers who had been thrown out of their jobs by the pandemic and the associated public health measures. By all accounts, they were successful: average personal income rose by an unprecedented amount in April, though this likely masks important heterogeneity. But they also affect the labor market in two offsetting ways. First, unemployment insurance plays a broadly stimulative effect, supporting consumption of displaced workers (Ganong and Noel 2019; Rothstein and Valletta 2017) and thus demand for goods and services. Second, enhancements and extensions of unemployment benefits may reduce the incentive for displaced workers to search for work. This may slow rehiring, and could even lead to more job loss; although workers who quit their jobs are not eligible for unemployment insurance (UI), workers who would prefer to receive unemployment benefits instead of remaining on the job might persuade their employers to implement layoffs rather than going into debt to keep the business open.

These moral hazard concerns have focused on FPUC, which was controversial from the start. The $600 amount was chosen to raise the UI replacement rate to around 100 percent for the average US worker. Because many workers, particularly those displaced in March and April, earn less than the average, and because the FPUC payment did not vary with prior earnings, many workers faced replacement rates well in excess of 100 percent. Ganong, Noel, and Vavra (2020) find that the median replacement rate was 145 percent and that 76 percent of workers unemployed in the past would have qualified for replacement rates greater than 100 percent under FPUC. Anecdotally, some employers reported that laid-off workers were unwilling to return to work, even when businesses reopened, because this would mean a loss in income (Morath 2020).

We take two strategies for evaluating the effects of the expansions of UI under the CARES Act. One uses across-state variation, and the other uses variation in the timing of the rollout of two components of the CARES unemployment insurance expansions.

We begin with the across-state comparison. Ganong, Noel, and Vavra (2020) document wide variation across states in unemployment insurance replacement rates under CARES, with a low median replacement rate of 119 percent in Arizona and a high of 165 percent in Oklahoma. We divide states into four groups by the median replacement rate, following Ganong, Noel, and Vavra (2020, appendix table 1), and investigate whether either the employment collapse or rehires vary across these groups. Variation in the replacement rate comes from two sources: differences in state wage distributions, and differences in the generosity of states' preexisting unemployment insurance benefit formulas. Neither is random, so differences across states may capture other state characteristics that correlate with these factors. We also explore estimates that control for census division fixed effects, which may capture some of the most important differences among states.

Figure 6. Hours Trends by State Median Unemployment Insurance Benefit Replacement Rate and Round 1 PPP Amount



Source: Authors' analysis of Homebase data.

Notes: Unemployment Insurance replacement rates, expressed as percentages of weekly earnings, are from Ganong, Noel, and Vavra (2020, table A-1) and include CARES Act supplements to benefits. Washington, DC, is excluded, as Ganong, Noel, and Vavra (2020) do not report UI data for it. For PPP graph, states are ranked by the amount of PPP loans under $150,000 to firms in NAICS industries 44 and 72 (food and drink and retail) approved on or before April 16, divided by the total payroll (in dollars) of establishments under size fifty in these industries in 2018:Q1, from the County Business Patterns data. The first quartile has the smallest amount.

The left panel of figure 6 shows the time series of Homebase hours, relative to the late January base period, for each of the four groups. The states with the lowest replacement rates saw the steepest collapse of hours in March, and recovered no more quickly thereafter. This is not the pattern one would expect if either were driven by labor supply responses to UI generosity, although as noted, other differences across states may confound this estimate.

We can use a similar strategy to develop descriptive evidence about the forgivable small business loans provided under the PPP. Like the UI programs, PPP was rolled out very quickly and somewhat haphazardly. It relied on banks to disburse loans to their existing customers, and banks varied in their preparedness to process applications quickly. Moreover, the program was initially under-funded: loan applications opened on April 3, and the initial appropriation was exhausted by April 16. (Additional loans from a second round of PPP funding started being provided on April 27.) There was substantial variability across areas in the amount of loans processed during the short initial application window. We classify states into four quartiles by the amount of PPP loans by April 16 for small firms (loans under $150,000) in the retail and food and drink industries, divided by total payroll in small businesses in these industries in March 2018. This ratio is over 160 percent larger in the top quartile of states than in the bottom. Again, this variation is not random, as greater small business distress may have led to higher take-up of PPP loans. But the very short, chaotic period between the opening of applications and the exhaustion of funds suggests that some of the variation likely reflects idiosyncratic factors related to existing banking relationships and bank preparation (and willingness) to handle the loans rather than any response to pandemic conditions.

The right panel of figure 6 shows hours worked by the four PPP quartiles. The trough in hours is lower in the states that received the least PPP money (as a share of potentially covered payroll), and these states also saw slower recoveries than states that received more funds. This is consistent with a protective effect of PPP loans. However, a substantial gap is already apparent at the beginning of April, before the PPP loan window opened, suggesting that other factors may confound this comparison.

One factor that could confound the comparison is differences in the industry or worker mix across states. To explore this, we turn again to logit models for job loss and rehiring, akin to those reported earlier. Online appendix table C1 reports several estimates in both the CPS and the Homebase data. Each model includes the controls listed in table 1 as well as industry fixed effects, but we replace the state fixed effects from those specifications with indicators for three of the four quartiles of states by PPP volumes and by UI replacement rates. In even-numbered columns, we also add fixed effects for the nine census divisions, so that comparisons are only among nearby states. Patterns are generally similar to what was seen in figure 6. Higher PPP volumes and higher UI replacement rates are associated with fewer layoffs; higher PPP volumes are also associated with faster rehiring. The states with the lowest UI replacement rates did have somewhat faster rehiring in the CPS data, but this merely matches their greater layoffs, and in any case is not replicated in the Homebase data.

A second strategy for assessing the impact of UI benefits, though not PPP, is to exploit differences in the rollout of benefit enhancements across states. While most of the current benefit enhancements were authorized as part of the CARES Act and workers across the country became eligible for them at the same time, the actual rollout of FPUC and PUA was staggered: states took several weeks to reprogram computer systems to make the additional FPUC payments, and longer to set up whole new application and eligibility determination processes for PUA. Claimants should have received benefits that were retroactive to the beginning of the programs, but the liquidity benefits would not arrive until the payments were actually made, and it is plausible that any labor supply response, which would have depended on knowledge of the program, did not fully manifest until the payments actually appeared.

Online appendix figure C11 shows event study plots for the two treatments' effects on hours worked. Both are estimated using a balanced sample of states and calendar dates, running from February 16 to July 11, and include full sets of state, calendar time, and event time indicators. We also control for the presence of an active stay-at-home order. We see little sign that hours changed following the initiation of payments under either program. If anything, PUA might have had a very small positive effect, the opposite of the decline in labor supply that concerned critics.

VI. Conclusion

We are only in the very early stages of the economic recession induced by the COVID-19 pandemic, and much of its story remains to be written. Yet, data accumulated over the last four months already illustrate some important facts and lay out important questions for future research and suggest directions for policy responses.

The labor market collapse triggered by the COVID-19 pandemic was unprecedented in its speed, with the bulk of the job losses happening in a matter of just two weeks. As we show above, there is little evidence that shutdown orders or school closures promulgated by states by themselves played a major role in this collapse. Instead, crescendoing public health concerns in the middle of March, and their subsequent implications for product demand in the "in-person" sectors, appear to be the principal drivers.

The labor market recovered quickly from mid-April through mid-June before plateauing as the virus surged. The recovery, though very partial and interrupted, allowed many workers to return to their prior places of employment within a few months' time. Nevertheless, many firms remain closed and many workers have not returned. It is likely, and the data we report already suggest, that the displaced workers that were left out from this very early stage of the recovery will face a steeper challenge reentering the labor market. Firm-worker matches are going stale, and many of the former employers appear unlikely to reopen. A potential second wave of closings only elevates these concerns.

The speed of the recession underscores the limitations of ad hoc policy responses, and the importance of automatic programs. By the time the

CARES Act passed on March 27, millions of workers had already been displaced, and tens of thousands of firms had already shuttered. It then took several more weeks to implement the various CARES support provisions. Moreover, when CARES was passed, many anticipated that the economic crisis would be short. The FPUC program (the $600 supplement to UI benefits) was set to expire at the end of July, while PPP loans were meant to support firms for only eight weeks. As of this writing, it appears that the period of economic weakness will last much longer, particularly as the COVID-19 public health crisis proved not to be as short-lived as initially anticipated. Policy responses with built-in triggers tied to economic conditions could adjust flexibly and automatically to the evolving situation.

The COVID-19-induced labor market collapse has also been unique in its sectoral composition, hitting mainly (at least in this early stage) the low-wage services and retail sectors of the economy. This is a sharp contrast with the recessions of the recent past, which have hit the higher-paid construction and manufacturing sectors hardest. Furthermore, our data show that within these already low-wage sectors the least advantaged workers have been most negatively affected. Both access to formal credit and the informal safety net (assets and savings, borrowing from family and friends) are likely to be particularly weak for the young, less educated, disproportionally nonwhite workers that have lost work since the pandemic hit. There is a high risk that many in this group will experience deep distress, absent additional policy responses to strengthen the formal safety net before labor demand recovers. In this regard, our evidence above does not suggest any adverse effects of higher unemployment insurance replacement rates on employment in early summer. This suggests that (as in the Great Recession; see Rothstein 2011) concerns about moral hazard effects may be overstated, and that labor demand is the more important determinant of employment outcomes thus far. Whether or not this pattern will hold when the public-health risks of COVID-19 recede is also an important topic for future work.

A central policy concern and question for future research is whether the long-term economic losses associated with mass layoffs in the service and retail sectors, where turnover is generally higher and workers may have less firm-specific human capital, will be as large as those caused by mass layoffs in sectors such as manufacturing, where turnover is generally lower and workers may have more firm-specific human capital.

Another topic for future study concerns the concentration of job losses in businesses that shut down entirely. An important fact that emerges from our early analysis is that firms that were struggling before COVID-19 were much more likely to shut down at the peak of the (first wave) of the pandemic and also much less likely to reopen during the recovery. This suggests a cleansing effect of the recession, but the causes and consequences of this pattern remain to be determined. It is possible that the delayed government response to expand support to small businesses played a role, making it impossible for businesses that were already low on cash before COVID-19 to build a financial bridge until the PPP money became available. It is also possible that banks prioritized healthier firms in their decision to extend PPP loans. The loan-level data that were recently released mask the identity of small borrowers, but future research with identified data about loans to small businesses may help in sorting out these hypotheses.

Altogether, our findings show that this recession has differed sharply from other recent downturns in its speed, the types of firms and workers it affected, workers' beliefs about its longevity and their likelihood of recall, as well as in the nature and size of the policy response. Combining nontraditional sources with traditional labor market data has been key in understanding and responding to the downturn so far, and will remain so as circumstances continue to change rapidly going forward.

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Note: Comments and discussion cover two papers presented at the Session 4 of Summer 2020 BPEA conference on labor markets and the economics of non-pharmaceutical interventions.

Comments and Discussion

SUMMARY OF COMMENT

VICTOR CHERNOZHUKOV provided oral comments. He congratulated the authors on providing such rapid and innovative data on economic activity early in the pandemic.

His comments focused on some of the challenges of estimating the effect of non-pharmaceutical interventions (NPIs) on economic activity. He presented weekly correlations between seven distinct NPIs (state-level data, March through May 2020). Nearly all the correlations exceeded 0.8, and several exceeded 0.9, indicating scope for omitted variable bias in regressions by Gupta, Simon, and Wing and by Bartik and colleagues, which considered only a subset of NPIs. Another econometric challenge is that the policies considered in these data were "hard" policies that took effect at a specific known date, while policies that changed behavior more gradually were excluded. Policies that induce gradual behavioral change, if not measured and included, would induce patterns that these regressions could misattribute as endogenous self-protection. As an example, Chernozhukov turned to some of his own research with Hiro Kasahara and Paul Schrimpf on use of masks.[1] They found a large effect of masking orders on cases, deaths, and mobility, both through a direct channel and through a behavioral channel. These and other econometric considerations led him to speculate that both papers—by Gupta, Simon, and Wing and Bartik and colleagues—could underestimate the effect of policies on economic activity.

1. Victor Chernozhukov, Hiroyuki Kasahara, and Paul Schrimpf, "Causal Impact of Masks, Policies, Behavior on Early COVID-19 Pandemic in the U.S.," *Journal of Econometrics* 220, no. 1 (2021): 23–62.

COMMENT BY

CAROLINE BUCKEE The deadly COVID-19 pandemic emerged in early 2020 and, in the absence of effective treatments or a vaccine, led to the unprecedented implementation of socially and economically disruptive non-pharmaceutical interventions around the world. In the two papers by Bartik and colleagues and by Gupta, Simon, and Wing the impact of these interventions on employment and human behavior, respectively, are examined, and in both papers, the authors use data streams from mobile phones to measure social and economic activity in relation to the dynamics of the labor force and public health policies around the United States. The comments below reflect my background as an infectious disease epidemiologist and as a researcher who has been using mobile phone data to monitor movement patterns in the context of disease modeling for nearly a decade. I have focused on two aspects that are relevant to both studies: the importance of spatially heterogeneous disease burden and the use of mobile phone data as a proxy for human behavior.

THE IMPORTANCE OF SPATIAL HETEROGENEITIES IN THE BURDEN OF COVID-19 Both studies examine economic and behavioral time series data in relation to policies that were implemented to slow the transmission of SARS-CoV-2. As they find, and as others have observed (Badr and others 2020), people across the country reacted strongly to the declaration of a national emergency on March 13 regardless of local policies. Almost any measure of mobility or other behavior is likely to show this rapid countrywide decline in activity in response to the threat of the pandemic. Most analyses, including these two, have concluded that the synchronization of behavior may have resulted from individuals acting based on national and global information about the pandemic rather than local policies. Indeed, Bartik and colleagues note that their results with respect to labor markets and economic activity "have more to do with broader health and economic concerns affecting product demand and labor supply" than with the timing of specific policies.

However, the trajectory of the epidemic in the United States has been characterized by distinct geographic heterogeneities within and between individual states, among different demographics, and even within cities (Kissler and others 2020). These heterogeneities reflect the spatial progression of the epidemic across the country, starting in Seattle and New York before moving into the south and middle of the country over the summer, as well as remarkable local heterogeneities resulting from income and racial inequalities. Both of these types of heterogeneity have implications for the interpretation of economic and mobility data because decision making by individuals generating the data reflect very different experiences of the disease itself.

Although people's behaviors in response to the national lockdowns were relatively synchronized across the country, their perceptions of the risks posed from COVID-19 are likely to have been strongly dependent on their personal, local experiences. People in New York may have experienced illness or death among friends and loved ones or witnessed the fatigue and desperation of health workers in their communities. In contrast, recent sero-prevalence estimates suggest that even by June, much of the Midwest had not yet experienced any significant SARS-CoV-2 transmission (Anand and others 2020). Not only would this have an impact on individuals' real and perceived risks from COVID-19 but also on their sense that the economic and social hardships experienced as a result of interventions were justified. To the extent that compliance and reaction to non-pharmaceutical interventions will depend on perceived risks, as we have seen in the context of Ebola in West Africa (Peak and others 2018), many of the nationwide metrics analyzed in these studies may mask significant regional heterogeneity. In particular, the speed and behavioral response to reopening, including consumer behavior, people leaving home and mixing socially, and the likelihood that individuals look for work and re-open their businesses, may have shown significant regional variation.

The second important spatial heterogeneity in disease incidence and burden is highly local and reflects structural disparities between neighborhoods that fall along socioeconomic and racial lines. Indeed, Bartik and colleagues find significant differences in employment and rehiring between different racial groups and income levels. Just as regional differences in disease burden may have had an impact on state-level economic and behavioral metrics, local differences in the experience of disease and death from COVID-19 are likely to have been pronounced among these economic categories. Consistent with nationwide racial disparities in mortality due to COVID-19 (Bassett, Chen, and Krieger 2020), analyses of COVID-19 deaths in Cook County, Illinois, found startling mortality rate differences due to COVID-19 between neighborhoods depending on poverty and race, varying from 14.1 per 100,000 in wealthy neighborhoods among white people, to 135.1 per 100,000 in poor neighborhoods among Hispanic and Latinx people (Feldman and Bassett 2020; Acosta and Irizarry 2020). A seroprevalence study among pregnant women in New York City in April showed a cumulative incidence of 11 percent in Manhattan versus 26 percent in South Queens, for example (Kissler and others 2020). In that study we showed that local differences in commuting behavior, measured using mobility data from Facebook users, was strongly correlated with seroprevalence. Thus, both mobility behavior related to employment and COVID-19-related illness and death have had an impact even on people living in the same city differently.

Studies aiming to understand social and economic decisions made by individuals in relation to public health and other policies—as both studies presented here seek to do in different ways—may therefore gain important insights if they account for the dramatic differences between individuals in their local experience of the epidemic when interventions were imposed or lifted.

THE USE OF MOBILITY DATA FROM PRIVATE COMPANIES AS A PROXY FOR HUMAN ACTIVITY Both Bartik and colleagues and Gupta, Simon, and Wing derive quantitative behavioral estimates from SafeGraph data, and Gupta, Simon, and Wing go further and use multiple different sources of activity data (for example, from Google and Apple) from mobile phones. Gupta, Simon, and Wing note that while mobility data from mobile phones have become relatively routine among infectious disease epidemiologists, they are still quite rare in other fields. While mobile phone data are a useful nearly real-time proxy for human behaviors, including for monitoring human behavior during this pandemic, there are a number of important issues that—in my opinion—make it challenging to directly use derived metrics in a quantitative, statistical analysis.

Gupta, Simon, and Wing discuss some of these caveats, including the representativeness of the data with respect to demographic structure, but it is important to outline some of the other systematic biases that may have an impact on analyses. These have been reviewed in the context of COVID-19 in Grantz and others (2020) and Oliver and others (2020), and a standardization of mobility metrics of this kind has been called for (Kishore and others 2020).

So-called ad tech data, such as the data from SafeGraph, can be distinguished from other data sources, including Google, Apple, Facebook, or data from mobile operators. Ad tech data derive from advertisements associated with the use of particular apps on smart phones, and the data from individuals are processed and packaged by multiple companies before they are analyzed. This creates opacity around the biases and details of individual data sets, including missingness, and data imputation or inference is often performed prior to release of the data. Therefore, even an investigation of the biases in the data becomes impossible for research groups using the data. Indeed, unlike data from Facebook, for example, where data quality or missingness is sometimes reported, this imputation step means that uncertainty in the SafeGraph estimates is impossible to ascertain.

Demographic biases are clearly an issue, because most mobility data from mobile phones reflect smart phone users only, who skew young and wealthy (mobile operator data are an exception because they include “dumb phone” subscribers, which is why operator data are often more appropriate in low-income settings). With respect to representativeness, unlike Google or Facebook, ad tech data providers often report their “monthly active users” (MAU), but this can be misleading. For example, 1 million monthly active users is not the same as a longitudinal sample of 1 million individuals because a user may appear infrequently or only once in the data set, and the number of users can vary dramatically from day to day. This high turnover is rarely reported, making it difficult to quantify uncertainty associated with any particular day and location. There are, in addition, geographic variations in representativeness that cannot be accounted for. For example, by comparing Facebook data to SafeGraph data across the United States, we find that while Facebook reports missingness in rural counties, SafeGraph imputes data and reports no missingness (personal communication).

Demographic and geographic representativeness aside, mobility metrics derived from these data sets—such as the mixing index used by Gupta, Simon, and Wing—are difficult to interpret. Standardized analytical frameworks, particularly validated ones, are still absent for this kind of data (Kishore and others 2020). Interpreting mixing indexes and other metrics of mobility is also complicated by the fact that in a large, geographically diverse country, the same movement patterns may represent very different behaviors in urban versus rural locations. Out-of-county travel, for example, is hard to interpret in the absence of spatial context, even when compared to a baseline, because it may depend on the spatial layout of grocery stores and so on. Gupta, Simon, and Wing include multiple metrics and data sources as a way to confirm their findings, which makes sense, but since all the metrics are likely to be biased in the same ways (reflecting smart phone users) there may still be bias unaccounted for. Taken together, although the qualitative findings are important and useful, these issues with uncertainty about data quality and representativeness and the rigor of particular derived metrics mean that making sense of effect sizes from time series and statistical analyses is challenging.

CONCLUSIONS Both studies track the behavioral and economic impacts of the unprecedented public health interventions that were put in place due to COVID-19 earlier this year. As we move into autumn and face a long winter with possible renewal of various behavioral interventions, understanding how people and the economy will respond is critical. Mobile phone data are a valuable source of information about human activity, although they are a loose proxy for the contacts that spread the virus and likely to be increasingly difficult to interpret epidemiologically against the backdrop of layered interventions such as masking. I don't necessarily expect the reaction to future lockdowns to recapitulate the behavioral dynamics we saw in the spring, not only because the economic and political situation is different now, but also, crucially, because now there are hardly any US communities that have not suffered significant illness and death due to COVID-19, and this will change the social and political acceptability of interventions.

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GENERAL DISCUSSION Jason Furman inquired about the nature of job loss over time. Furman remarked that it is possible that if weekly unemployment insurance (UI) claims remain high throughout the summer, then those unemployment spells may be different in nature. For example, he posited that some initial job losses could be primary, direct effects of the COVID-19 pandemic but that it is possible subsequent job losses could be the result of more traditional recession forces. Furman speculated that by determining this distinction between types of job loss, policymakers may be able to gain insights into how and when those jobs might be recovered.

Hilary Hoynes speculated whether it would be possible to link the private sector Homebase data used in the paper with recently published data from the Treasury Department on the Paycheck Protection Program (PPP). Hoynes suggested it would be interesting to see if there could be a way to see to what extent the PPP affected labor market outcomes for workers in the Homebase data. More specifically, she wondered whether such a linking could shed light on whether PPP loans accomplished certain goals policymakers had for it (e.g., keeping workers connected to their employers).

Adding to this conversation, Marianne Bertrand pointed out that the Treasury Department plans to release detailed data on the name of firms, location, firm size, and so on, for the larger loans (above $150,000). She pointed out that when these data become available, it could be possible to link the firms in the Treasury Department's PPP loan data with the firms in the Homebase data set.

Simon Mongey shared a resource from the Philadelphia Federal Reserve Bank on the PPP loans.[1]

Stephen Goss asked the discussant Caroline Buckee about the effects of seasonality and weather on the spread of the coronavirus. He mentioned that some observers have pointed to Brazil, which, being in the Southern Hemisphere and currently in the midst of winter, has still seen a surge in cases. Goss inquired whether Brazil's experience might provide insights into what sort of experience the United States and the European Union (EU) may have with the virus as our seasons begin to change. He speculated whether the EU's current relative success in controlling the virus may be short-lived as the weather begins to change.

Henry Aaron asked whether improved treatment methods are being incorporated into models. He remarked that it seems much of the conversation has surrounded spread and deaths but not much on changes in treatment.

In response to Goss's comments, Buckee says that because other coronaviruses do exhibit seasonal effects, it is likely that this strand may be affected by seasonality, but to a limited degree. The much more relevant way that seasonality will play a role is in the gathering of people indoors as a result of the colder weather in the fall and winter months. Buckee worried about the potential surge in cases that may result if many of the social interactions that have occurred outdoors during the summer continue indoors in the fall. In particular, she was concerned about schools reopening in the fall without the proper precautions being taken. As for the comparison between the United States and the EU, Buckee argued that the difference in success with dealing with the virus has largely been an effect of policy choices: lack of increased testing capacity, issues surrounding social response and messaging, and so on.

Addressing Aaron's question, Buckee replied that changes in treatment methods have not shown through in the data, largely because there have not been many significant breakthroughs in treatments. In addition to the many ongoing trials, Buckee referred specifically to a recent trial of dexamethasone that showed a 30 percent reduction in deaths among people on ventilators. However, she pointed out that many of those trial results haven't been

1. "SBA_PPP," public data tables on the Payroll Protection Program by the US Small Business Administration, GitHub, https://github.com/RocArm/SBA_PPP?fbclid=IwAR0hHw_lJObIzRoroYWLhWU7RcpiXDszdIkdsMCRz3VLKNMQ4GSsngUwBw.

rolled out widely yet, which is why she didn't think that these trials were having a major impact on treatment and the death rate. A related point that Buckee made in this conversation was that a large share of deaths early on in the pandemic occurred in nursing homes and assisted care facilities. More recently, as states have begun reopening, the largest surge in cases has been among young people, who have a lower mortality rate anyway.[2] In light of these two trends, Buckee commented that it's difficult to disentangle whether that change reflects a demographic shift, differences in social distancing behavior, or household structure differences in different geographic areas as the epidemic spreads across the country. Buckee concluded that while it can be hard to discern exactly what's happening, these trends will be important moving forward.

Austan Goolsbee highlighted a recent paper that he and Chad Syverson have put out that uses county-level lockdown policies (rather than state-level policies).[3] Goolsbee claimed that their paper finds that looking at county-level policies as opposed to state-level ones seems to matter a fair amount: many of the hardest-hit counties implemented policies well before their states did. Goolsbee mentioned that by doing a horse race on the two levels of policy, they find that the local level appears to be far more influential. He concluded by saying that he and his coauthor have posted the data publicly for anyone to use.

Alessandro Rebucci pointed out he has a paper where he and his coauthors analyze the relationship between partisanship and state-level heterogeneity in compliance with non-pharmaceutical interventions (NPIs).[4] Their empirical evidence shows that preferences and attitudes toward "free" interactions are an additional factor in the decision problem. Responding to this point, Bertrand commented that it would be interesting to think about heterogeneity of order effects between Democratic versus Republican states. She speculated that one can imagine that a truly enforced order in a Republican state may matter more than in a Democratic state if people in

2. Julie Bosman and Sarah Mervosh, "As Virus Surges, Younger People Account for 'Disturbing' Number of Cases," *New York Times*, June 25, 2020, https://www.nytimes.com/2020/06/25/us/coronavirus-cases-young-people.html.

3. Austan Goolsbee and Chad Syverson, "Fear, Lockdown, and Diversion: Comparing Drivers of Pandemic Economic Decline 2020," working paper 27432 (Cambridge, Mass.: National Bureau of Economic Research, 2020).

4. Alexander Chudik, M. Hashem Pesaran, and Alessandro Rebucci, "Voluntary and Mandatory Social Distancing: Evidence on COVID-19 Exposure Rates from Chinese Provinces and Selected Countries," research paper 20-03 (Baltimore: Johns Hopkins Carey Business School, 2020), https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3576703.

Democratic states take the disease more seriously and are adjusting their behavior even absent an order to do so.

Jesse Rothstein thanked both of the discussants and the participants for their helpful comments. Responding to Furman's comment, Rothstein mentioned that the data used in the paper did not allow for that distinction to be drawn, but he pointed to Till von Watcher's recent paper analyzing California UI claims data.[5] Rothstein mentioned that Hedin, Schnorr, and von Wachter find that the first waves of UI claims were concentrated among workers with less educational attainment and workers in specific industries and that subsequent waves of UI claims tended to be more representative of the broader labor force, potentially supporting Furman's hypothesis.

Alexander Bartik echoed Rothstein's thanks and responded to a few participants' points in particular. Building on Rothstein's response to Furman, Bartik highlighted the figure in their paper that shows payroll employment by month and industry. He emphasized that this figure showed that in the early weeks of the pandemic, the leisure and hospitality industry in particular was hard hit; the data at that time had not shown a spread to certain industries (e.g., durable goods, manufacturing, construction, etc.). However, Bartik acknowledged that the data may change in the coming months.

Bartik responded to Hoynes by stating that currently that sort of linking is not yet possible, but that he and his coauthors are working with scholars at Harvard to conduct a survey of the firms in the Homebase data to see if they can use quasi-experimental methods to accomplish a similar goal with regards the PPP loan data. He also pointed to work being done by Granja and others, who have looked into PPP disbursement and employment effects.[6]

Bartik acknowledged that several participants raised the issue of the paper's focus being only on the shutdown orders. He said that they did this for a variety of reasons but that they plan to incorporate the fuller set of policies into future analysis. Given the nature of Homebase data, he pointed out that they should be able to analyze relatively fine measures of timing. He wanted to clarify that they are not taking a strong stance on

5. Thomas J. Hedin, Geoffrey Schnorr, and Till von Wachter, "California Unemployment Insurance Claims during the COVID-19 Pandemic," policy brief (Los Angeles: California Policy Lab, 2020).

6. Joao Granja, Christos Makridis, Constantine Yannelis, and Eric Zwick, "Did the Paycheck Protection Program Hit the Target?," working paper, April 26, 2020, https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3585258.

the time effects of information per se, but that their interpretation of those effects was that they reflected reduced consumer demands for in-person services. Bartik pointed out that this reduced consumer demand could, in part, be a function of schools being closed, since school closings change how parents consume in-person services.

Lastly, Bartik commented that although they had not done it yet, it is possible for them to look at their Homebase sample for 2018 and 2019, which could bolster their analysis.

Sumedha Gupta also thanked all of the participants and said that she greatly appreciated their feedback. Responding to Buckee's comments, Gupta acknowledged that she agreed with many of her points, especially regarding the heterogeneity of the data sources. She pointed out that their paper addresses many of these differences in the data sets, which is why they chose to look at all of them in an effort to capture the whole story and to see if that story is consistent. Since, thus far, much of the data they have looked at have been consistent, Gupta felt confident in claiming the direction (even if not the magnitude) of the effect. Gupta also pointed out that some of their analysis did look at some local (rural versus urban) differences. She also highlighted that their analysis found interesting differences when looking at indoor versus outdoor activity.

Responding to Victor Chernozhukov's comments, Gupta expressed interest in learning more about his bias correction approach and stated that she intended to look into some of the papers he recommended to see if they can implement it.

Gupta also acknowledged that there is difficulty in parsing out the timing differences between the state of emergency declarations versus stay-at-home orders, especially since it all happened in about a three-week period. Furthermore, Gupta posited that although it can be quite difficult to disentangle the effects of each of the public policies, she and her coauthors think of the emergency declaration as a sort of "reduced form" effect for several of the other policies; in other words, it is almost as if the emergency declarations triggered the start of many of the other policies. However, Gupta still recognized the importance of doing estimations by including controls for the different policies as well as the need to have linearized, event time studies to see the effects for all of the policies simultaneously.

Gupta concluded by stating that their main takeaway is that while there has clearly been a policy response (regardless of how wide-ranging the policies one chooses to include), their data seem to suggest the larger effect has been a private response to this pandemic.

Appendix to "Measuring the labor market at the onset of the COVID-19 crisis"

Alexander W. Bartik, Marianne Bertrand, Feng Lin, Jesse Rothstein, Matthew Unrath

Data

CPS

We use data from the Current Population Survey public use microdata files for January through June 2020.

To ensure comparability over time, the labor force status questions in the CPS are maintained unchanged from month to month. However, these questions were not designed for a pandemic.[1] In ordinary times, people without jobs are counted as unemployed only if they are available for work and actively engaged in job search, so someone who would like a job but is not actively looking due to shelter-in-place rules would be counted as out of the labor force. Similarly, the coding structure is not designed to measure workers who are sheltering at home due to public health orders, individualized quarantines, or school closures. Consequently, beginning in March, CPS surveyors were given special instructions (Bureau of Labor Statistics 2020c): people who had jobs but did not work at all during the reference week as a result of quarantine or self-isolation were to be coded as out of work due to "own illness, injury, or medical problem," while those who said that they had not worked "because of the coronavirus" were to be coded as unemployed on layoff. Interviewers were also instructed to code as on temporary layoff people without jobs who expected to be recalled but did not know when, a break from ordinary rules that limit the category to those who expect to be recalled within six months. Despite this guidance, many interviewers seem not to have followed these rules, and unusually large shares of workers were classified as employed but not at work for "other reasons," while the share coded as out of the labor force also rose.

[1] The CPS is conducted via a combination of telephone and in-person interviews. In-person interviews were suspended and two call centers were closed mid-way through data collection for the March survey, to avoid virus transmission. Although the Census Bureau attempted to conduct the surveys by telephone, with surveyors working from home, the response rate in March was about ten percentage points lower than in preceding months, and continued to fall in subsequent months. While this may have impacted the accuracy of the survey, BLS's internal controls indicate that data quality is up to the agency's standards.

BLS added several new questions to the May CPS to better probe job loss due to the pandemic (BLS 2020b). At this writing, results from these questions are not yet available.

Our analyses focus on the distinction between employed at work and all other statuses, and do not rely on the classification of those not working as furloughed, on leave, unemployed, or out of the labor force.

A last issue with the CPS concerns seasonal adjustment. Neither multiplicative nor additive seasonal adjustment procedures are appropriate to an unprecedented situation. All CPS statistics that we report are not seasonally adjusted.

Homebase

In our analyses of Homebase data, we focus on Homebase's clients as the unit of analysis. In a few cases, a single client stretches across multiple geographic areas. We separate clients into separate units for each industry, state, and metropolitan statistical area (MSA) in which the client operates, and treat these units as "firms."

All of our analyses of Homebase data consider hours worked as a fraction of hours worked in the base period, January 19-February 1. We construct aggregate hours indexes at the state or industry level, separately for daily and weekly analyses. For daily analyses, we divide hours each day after February 1 by average hours for the same day of the week in the base period. For weekly analyses, we divide by average weekly hours during the base period.

Figures A1-A3 present descriptive statistics for the Homebase data.

We also rely on a survey we conducted of Homebase workers. Table B1 compares the characteristics of survey responses with those of other Homebase workers who did not respond to the survey invitation. Table B2 presents summary statistics for the survey responses. We restrict the sample for this table and all analyses of the survey data to workers who were active users of the Homebase platform during our late January base period and who have not worked at more than one Homebase firm (as defined above) since January 19, 2020. Figure B1 shows the distribution of hours worked during the base period for survey respondents and non-respondents, while Figure B2 shows the time series of daily hours worked for the two groups.

Kronos

We obtained a tabulation of "punches" (daily sign-ins) from Kronos, a firm that offers time-clock and payroll services similar to those provided by Homebase but serves larger businesses. The tabulation reports the total number of shifts worked at firms with more than 100 employees who use the Kronos time-and-attendance service.

Additional analyses

We conducted a number of additional analyses that were not presented in the main paper.

Figure C1 presents monthly employment by industrial sector or aggregate from the Current Employment Statistics firm survey.

Figure C2 shows the distribution of hours worked at Homebase firms, by week, limiting attention to firms that shut down for at least one week by April 4. We divide the total base period hours for each firm into four groups for each subsequent week *w*: Hours worked in the base period by firms that are closed in week *w*, the difference between base-period and week-w hours for firms that have positive hours in week *w*, hours worked in week *w* by workers who had worked with the firm before it shut down, and hours worked in week *w* by workers who were new to the firm after it shut down. These categories are mutually exclusive, and sum to the total base period hours.

Figure C3 provides another look at turnover at Homebase firms. Here, we do not restrict to firms that shut down, but instead measure the share of hours worked on each day by workers who were with the firm in our late January base period. We also compute similar statistics for 2018 and 2019, defining similar base periods in those years.

Figure C4 presents selected percentiles of the distribution of weekly hours among Homebase workers with positive hours in each week.

Figure C5 shows two views of firm survival in the Homebase data. The left panel shows the share of firms that were active in the base period that also showed positive hours in each subsequent week, both for 2020 and for comparable periods in 2018 and 2019. The right panel shows the share of firms that were active in the base period and that had positive hours in *or after* each subsequent week. A firm that shut down in week *w* but subsequently reopened would be counted as surviving in week *w* in the right panel but not in the left.

Figure C6 shows the number of states that were under a shelter-in-place order at each date. We use the same definitions used in the text, counting a reopen order as ending the original shelter-in-place order.

Figure C7 presents an alternative specification for our event study estimates from Figure 5. Figure 5 included state-specific trends; we exclude them here. Note that the time effects presented in the lower panel of Figure 5 were the sum of the pure calendar time effects and the average of the state time trends; there is no such complication in Figure C7.

Figure C8 presents a second alternative to the event study specification. Here, we reincorporate the controls for state-specific trends, but also add a set of event time indicators where the event is a school shutdown.

Figure C9 presents an alternative specification for our analysis of PPP in Figure 6. There, we classified states into quartiles by the amount of small-dollar loans awarded to businesses in Homebase's primary sectors of retail and food services. In C9, we instead rank states by the total volume of PPP loans awarded, divided by total state payroll.

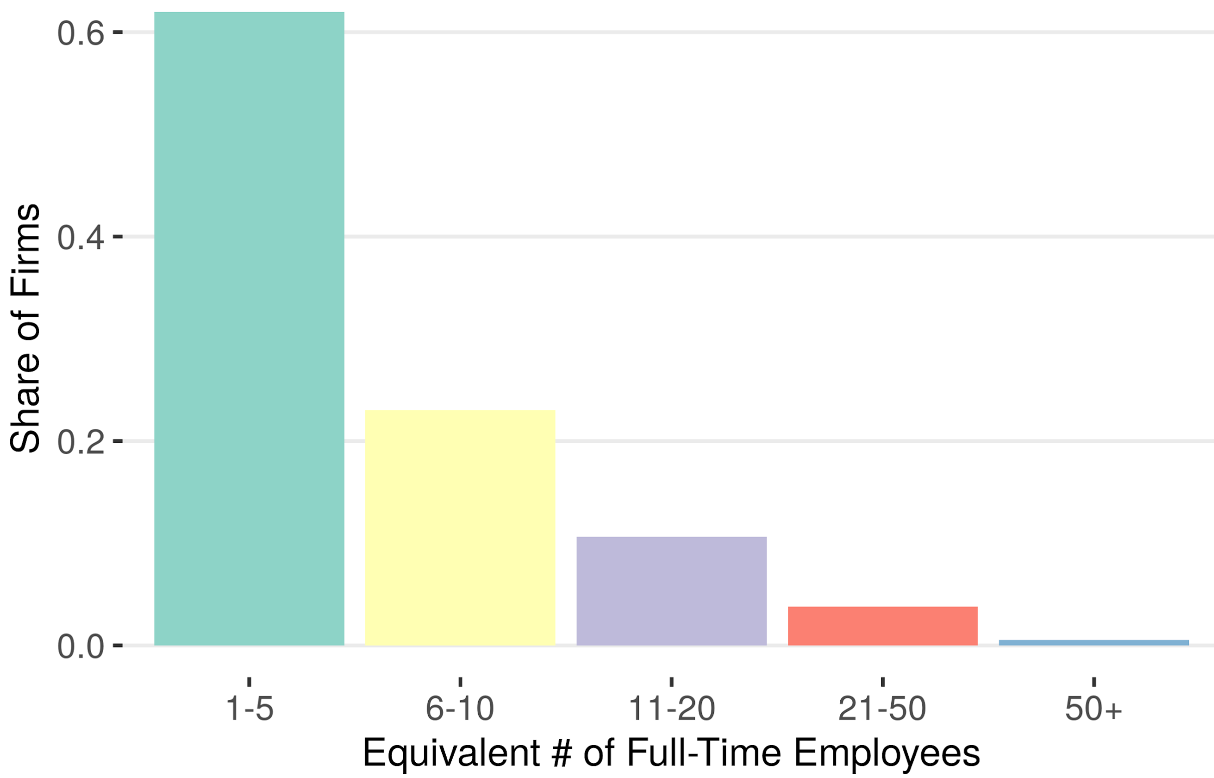
Table C1 presents regression estimates from logit specifications like in Table 1. The state fixed effects from those specifications are removed and replaced by sets of indicators for the state's PPP and UI replacement rate quartiles and, in columns 2 and 4, by census division fixed effects.

Figure C10 illustrates the distribution across states of the timing of initial payments under FPUC and the Pandemic Unemployment Assistance (PUA) program.

Finally, Figure C11 presents event study estimates of the effect of each of those two events.

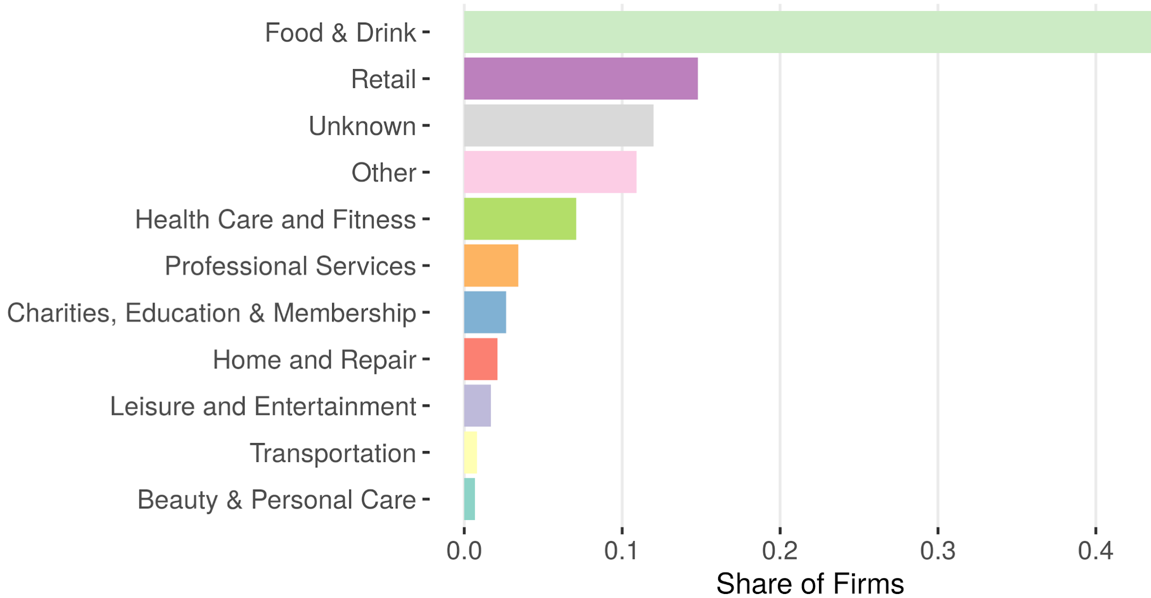
Appendix A. Representativeness of Homebase data

Figure A1: Firm Size Distribution of Homebase Firms



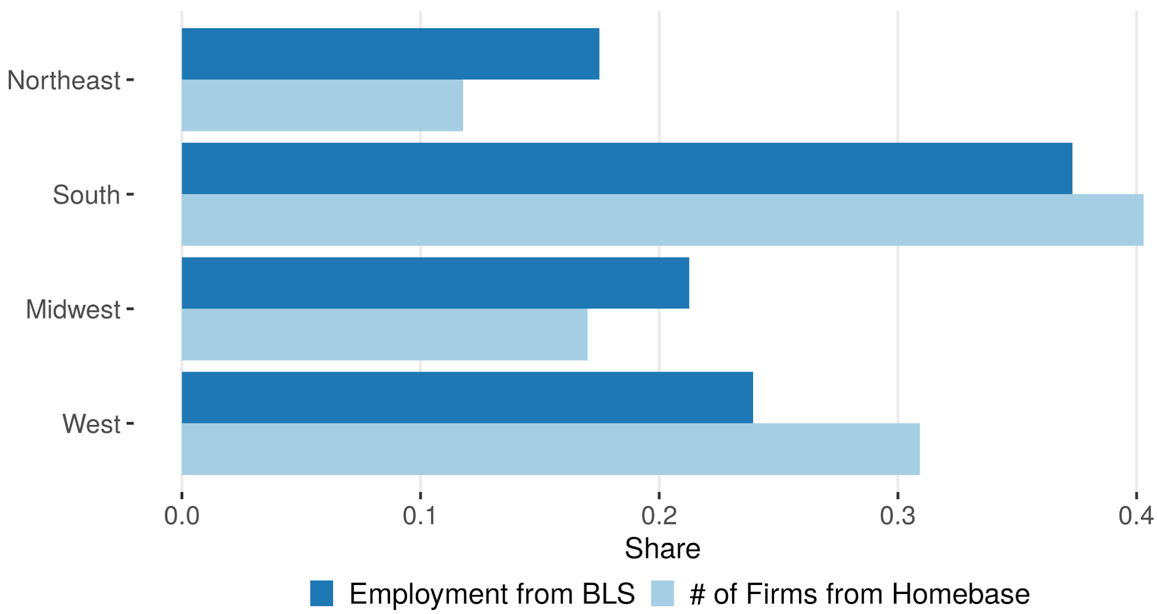
Notes: The full-time equivalent firm size is calculated by dividing total hours worked at the firm in the two-week base period by 80.

Figure A2: Industry Distribution of Homebase Firms



Notes: Industry coding is based on firm self reports.

Figure A3: Comparison of Homebase and BLS Data by Census Region



Notes: BLS data is employment counts by region from the Current Employment Statistics payroll survey, and pertains to January 2020.

Appendix B. Homebase worker survey data

Table B1: Characteristics of Homebase survey respondents

Characteristic	Respondent	Non-Respondent
Industry		
Beauty & Personal Care	0.2%	0.4%
Charities, Education & Membership	3.9%	3.5%
Food & Drink	46%	51%
Health Care and Fitness	5.5%	5.2%
Home and Repair	0.5%	1.3%
Leisure and Entertainment	3.1%	1.9%
Professional Services	3.1%	2.5%
Retail	12%	11%
Transportation	0.8%	0.8%
Other	13%	11%
Unknown	13%	11%
Census Division		
New England	3.2%	3.2%
Middle Atlantic	10%	9.1%
South Atlantic	19%	21%
East South Central	4.3%	4.6%
West South Central	8.6%	12%
East North Central	13%	11%
West North Central	7.3%	6.1%
Mountain	9.1%	9.2%
Pacific	25%	23%
Firm Size		
$[1, 5]$	30%	32%
$(5, 10]$	25%	26%
$(10, 20]$	21%	21%
$(20, 50]$	17%	15%
$(50, Inf]$	6.6%	5.4%
N	1,688	426,043

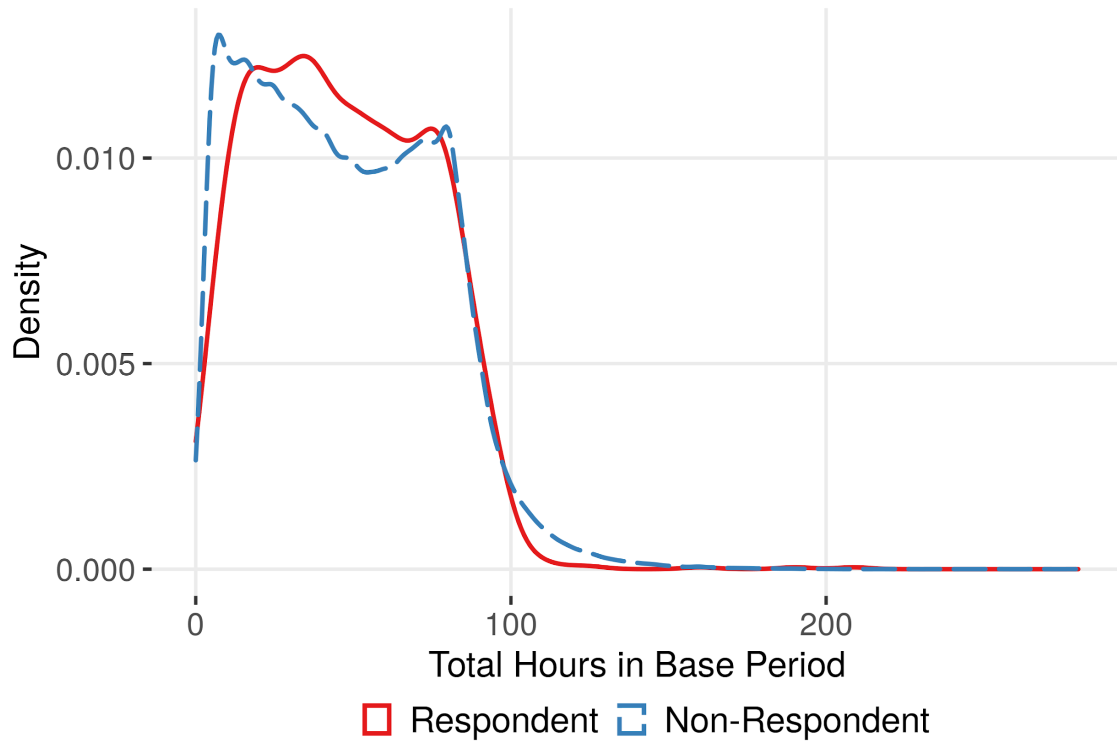
Note: The full sample is all workers who 1) worked at firms in our sample in our late January base period and 2) have worked for only one firm since January 19, 2020. All were invited to participate in the survey. Respondents are the subset of workers who responded to our survey.

Table B2: Demographics of Matched Homebase Survey Respondents

Demographics	Share	Demographics	Share
Gender		**Education**	
Female	67.0%	Some high school	6.9%
Male	30.6%	High school graduate	28.7%
Non-binary	2.4%	Two-year degree/some college	33.8%
Age		Bachelor's degree	23.5%
18-25	36.4%	Master's degree or more	7.1%
26-37	27.3%	**Household Income**	
38-49	16.3%	Less than $15,000	21.3%
50-64	15.4%	$15,000-$24,999	21.9%
65 or above	4.6%	$25,000-$34,999	15.1%
Race		$35,000-$44,999	8.7%
White	80.4%	$45,000-$54,999	7.1%
Black	10.7%	$55,000-$64,999	6.2%
Asian or Pacific Islander	7.7%	$65,000-$74,999	4.6%
American Indian or Alaskan Native	1.1%	$75,000-$84,999	2.7%
Ethnicity		More than $85,000	12.4%
Hispanic	16.4%	**Wage in Jan 2020**	
Non-Hispanic	83.6%	$5-$7.49	7.0%
Marital Status		$7.50-$9.99	15.0%
Single	55.1%	$10-$12.49	26.7%
Married	26.4%	$12.50-$14.99	20.8%
Living with partner	10.0%	$15-$17.49	15.8%
Separated	1.5%	$17.50-$19.99	4.8%
Divorced	5.8%	$20 -$22.49	4.0%
Widowed	1.2%	$22.50-$24.99	1.8%
Have Children		$25 or higher	4.1%
Yes	31.6%	**Job Title**	
No	68.4%	Employee	94.1%
Number of Children (if any)		Manager	5.9%
1	49.3%		
2	28.9%		
3	15.1%		
4	5.3%		
More than 4	1.4%		
N	1,688	N	1,688

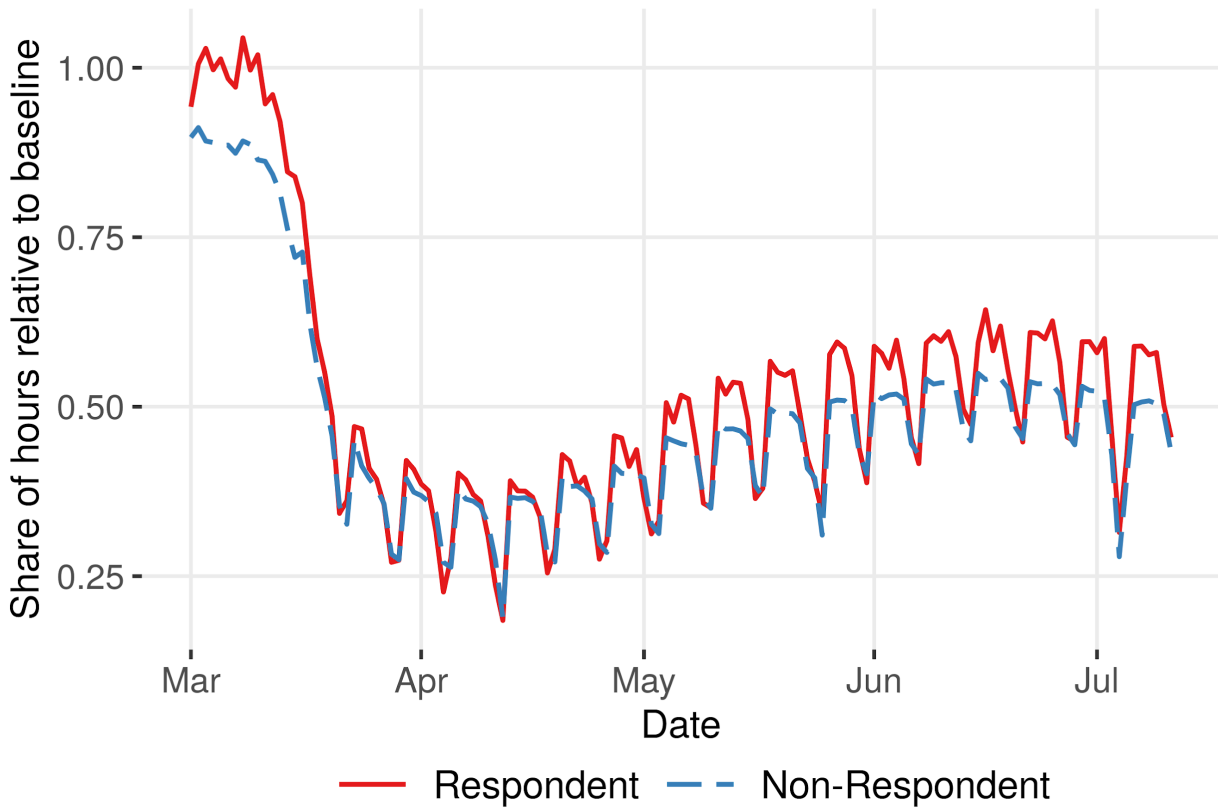
Note: The table reports the demographics of survey respondents who 1) are active workers in our base period and associated with firms in our sample and 2) have worked for only one firm since January 19, 2020.

Figure B1: Distribution of Base Period Hours for All Homebase Workers and for Survey Respondents



Note: The full sample is all workers who 1) worked at firms in our sample in our late January base period and 2) have worked for only one firm since January 19, 2020. Respondents are the subset of workers who responded to our survey.

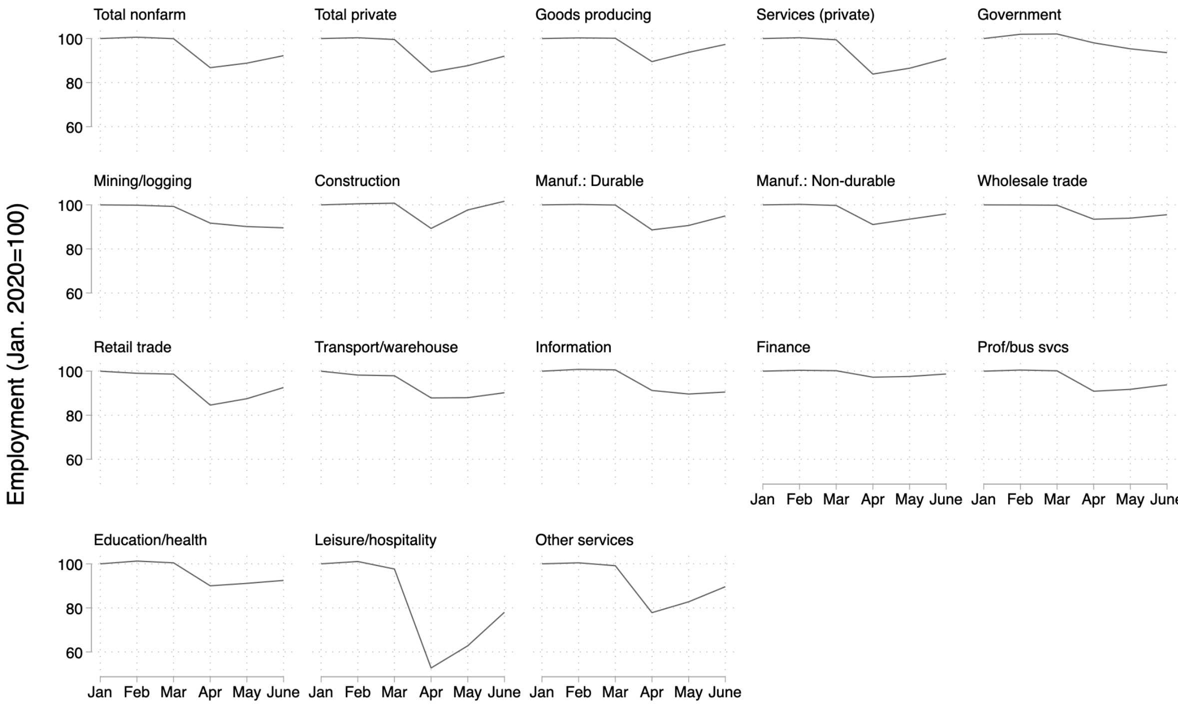
Figure B2: Trends in Hours for Survey Respondents and Non-Respondents



Note: The full sample is all workers who 1) worked at firms in our sample in our late January base period and 2) have worked for only one firm since January 19, 2020. Respondents are the subset of workers who responded to our survey.

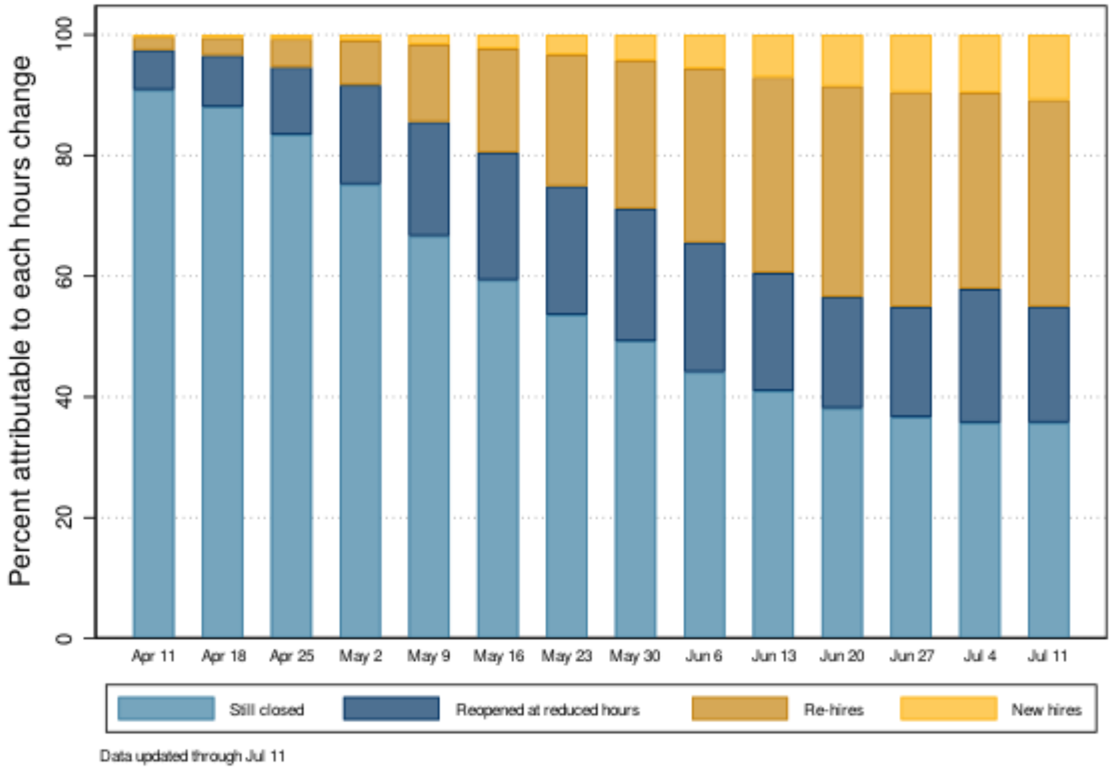
Appendix C. Additional results

Figure C1: Payroll employment by sector and month, 2020



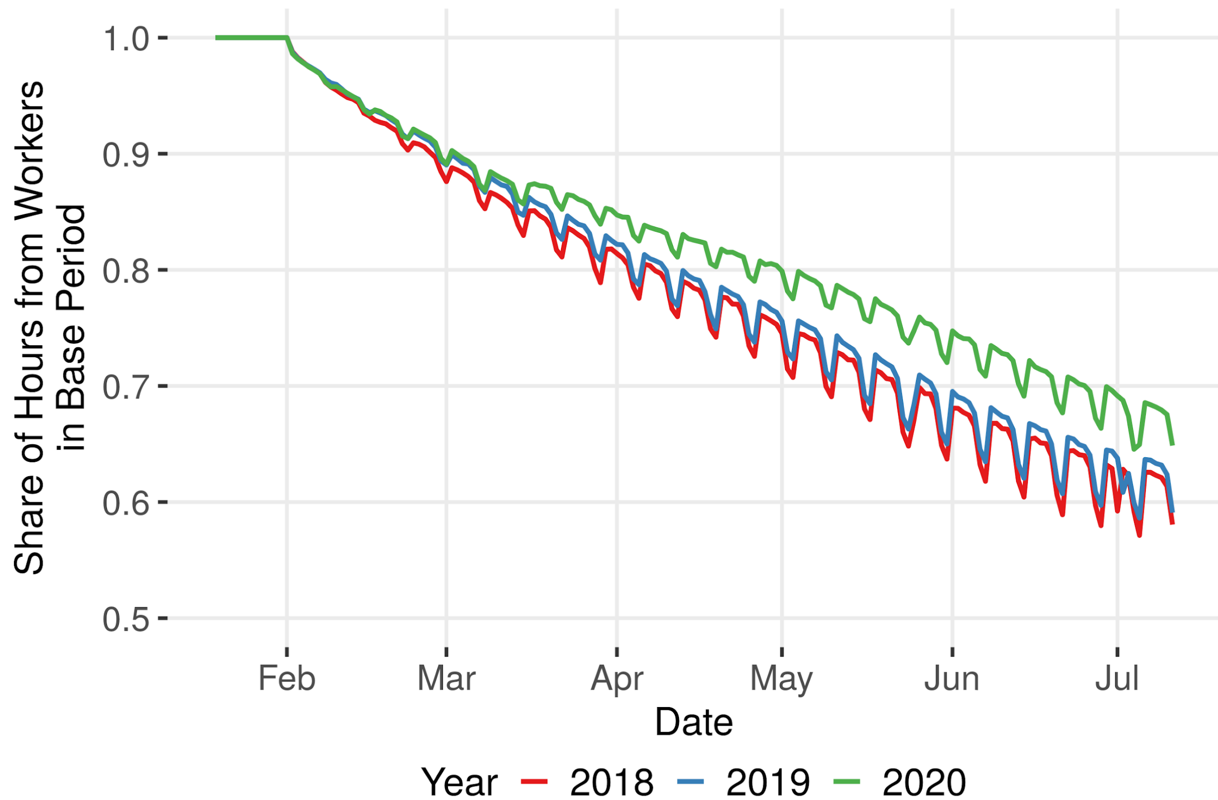
Notes: Payroll employment by industry or aggregate, scaled relative to January 2020, from the official Current Employment Statistics June 2020 release. The first four panels are aggregates that include many of the remaining series. Not seasonally adjusted.

Figure C2: Distribution of hours, relative to Jan 19-Feb 1, among firms that shut down by Apr 4



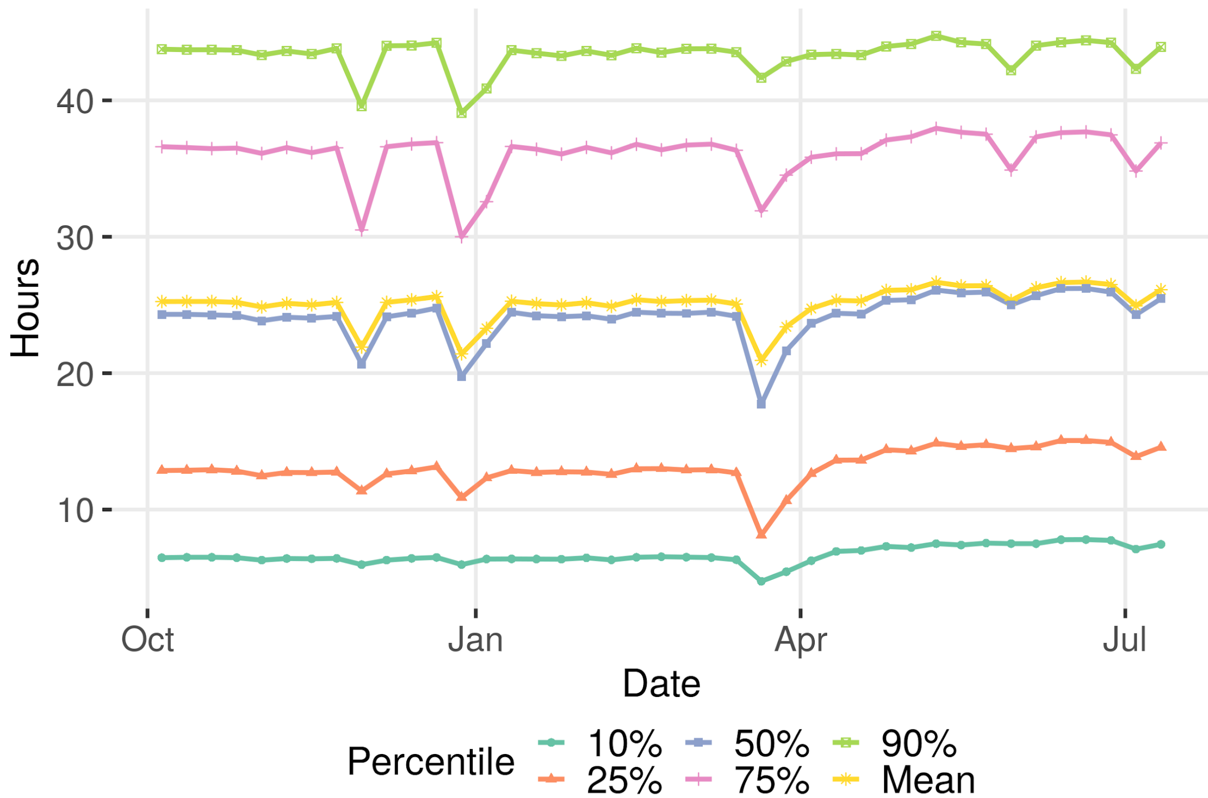
Notes: The sample consists of firms in our baseline Homebase sample that had at least one week of zero recorded hours by April 4. We identify the firms that remain closed through each subsequent week and sum their baseline hours (light blue). Among reopened firms, we distinguish reductions in total hours relative to baseline (dark blue), hours worked by workers who were employed at the firm before the firm shut down (golden) and hours worked by workers who had not previously been seen at the firm (yellow).

Figure C3: Share of hours by workers active in base period



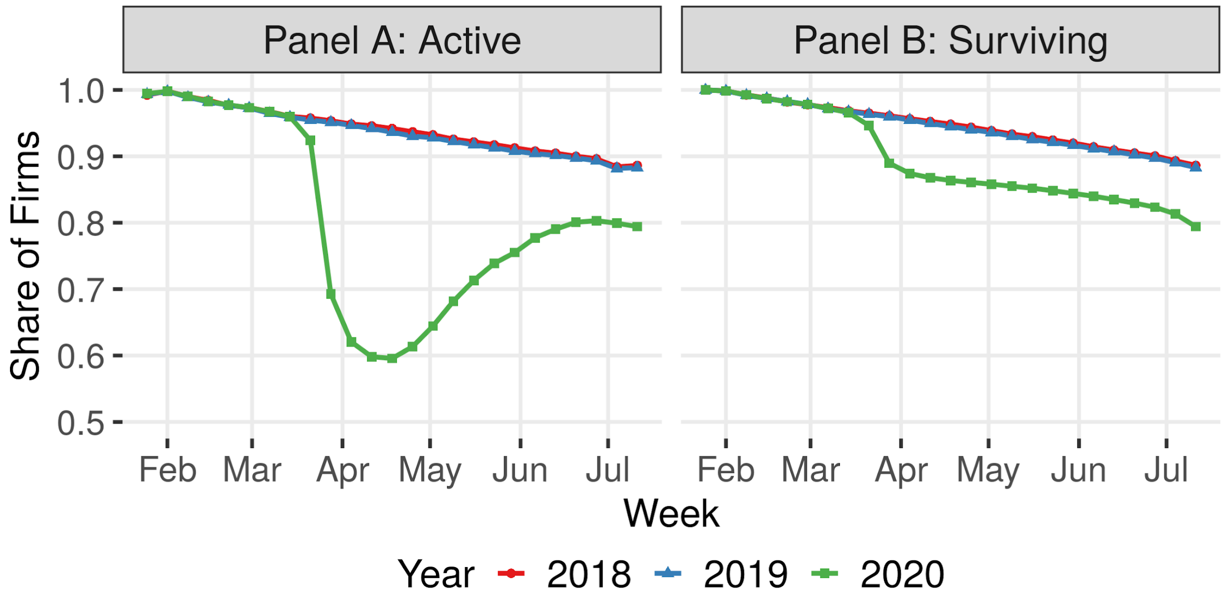
Note: The share shown in the figure is the share of hours worked on a given day coming from workers who appear in the Homebase data in the base period. The base period is two weeks at the end of January; for 2018 it is 1/21-2/3, for 2019 it is 1/20-2/2, and for 2020 it is 1/19-2/1. Firms included in the samples are those with at least 80 hours in the respective base period. The lines for 2018 and 2019 are shifted leftward (by two days and one day, respectively) to align days of the week with 2020.

Figure C4: Distribution of hours worked by active workers in the week



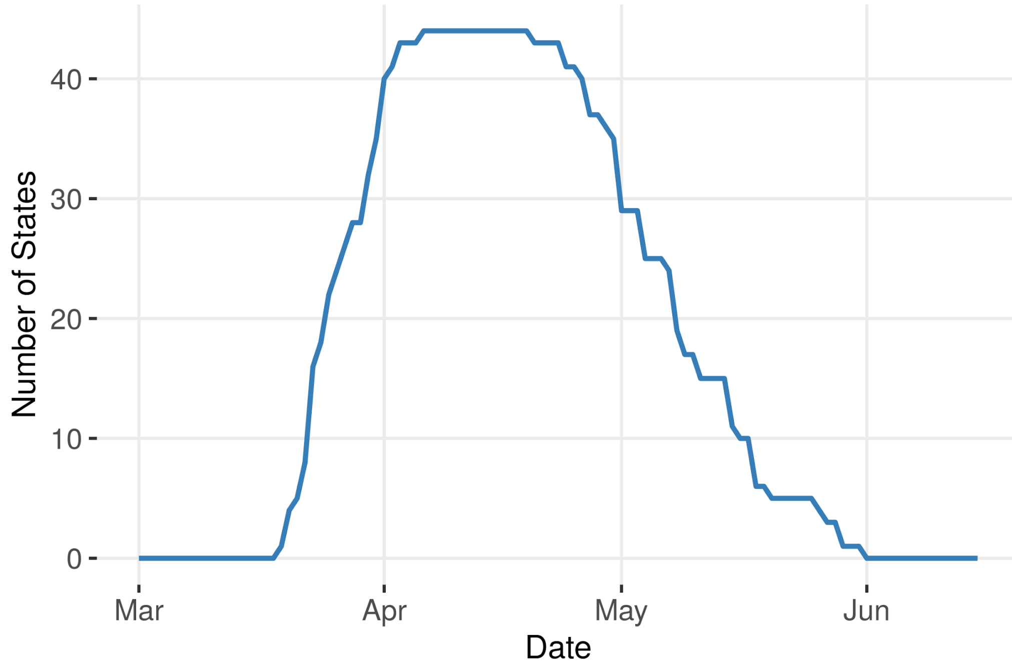
Note: Series show percentiles of weekly hours in October 2019-June 2020, among workers with positive hours that week. Hours are computed at the job level; a worker associated with multiple firms creates multiple observations. The late March blip reflects onset of the coronavirus crisis; also visible are the weeks containing Thanksgiving, Christmas and New Years, Memorial Day, and July 4.

Figure C5: Share of firms with positive hours in or after a given week



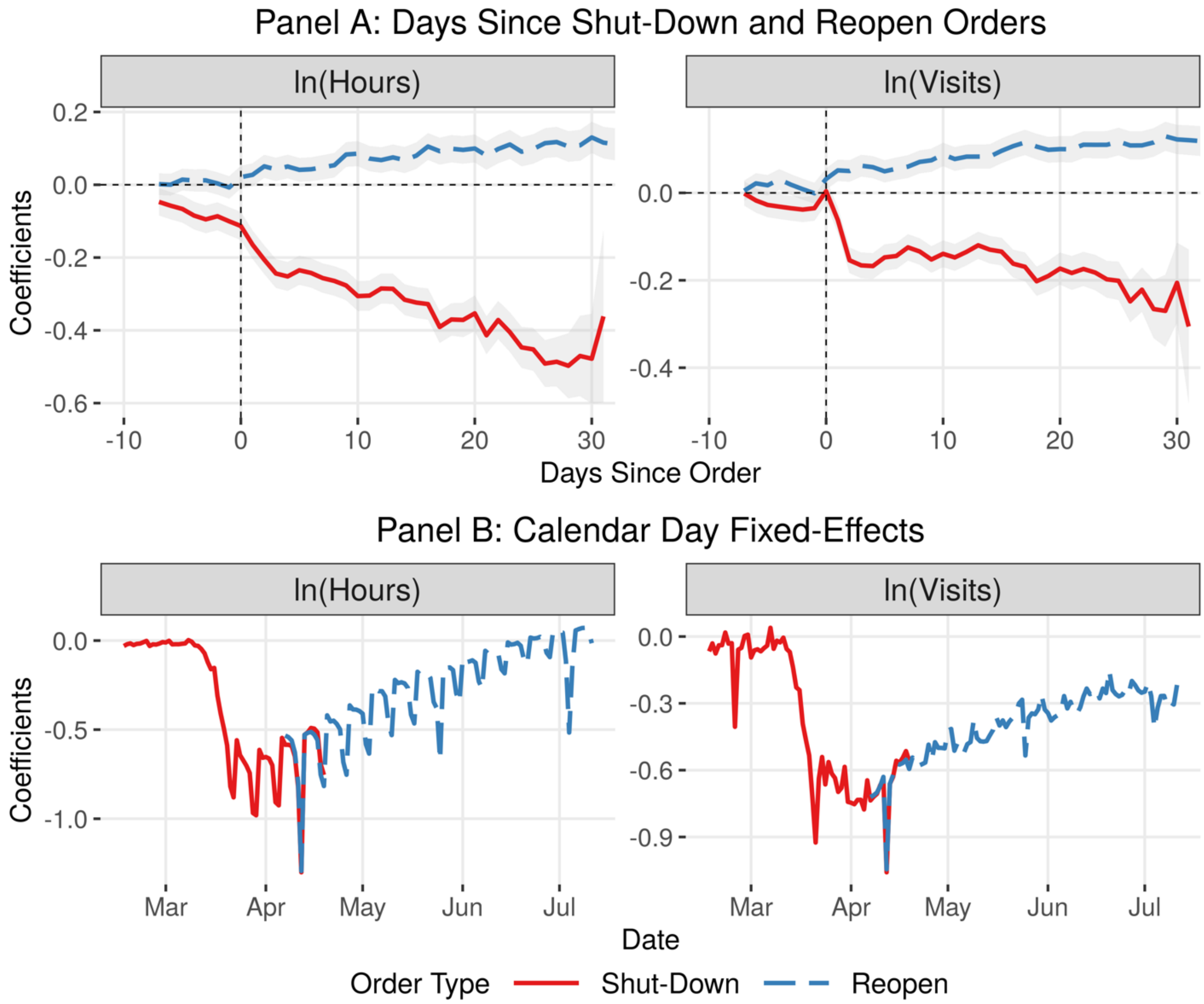
Note: Panel A shows the share of firms present in the base period that show positive hours in each week, while Panel B shows the share of firms with positive hours between the indicated week and the week containing July 11. The base period for each series is two weeks at the end of January; for 2018 it is 1/21-2/3, for 2019 it is 1/20-2/2, and for 2020 it is 1/19-2/1. Firms included in the samples are those with at least 80 hours in the respective base period. The lines for 2018 and 2019 are shifted leftward (by two days and one day, respectively) to align the end of weeks with 2020.

Figure C6: Timing of shelter-in-place and stay-at-home orders



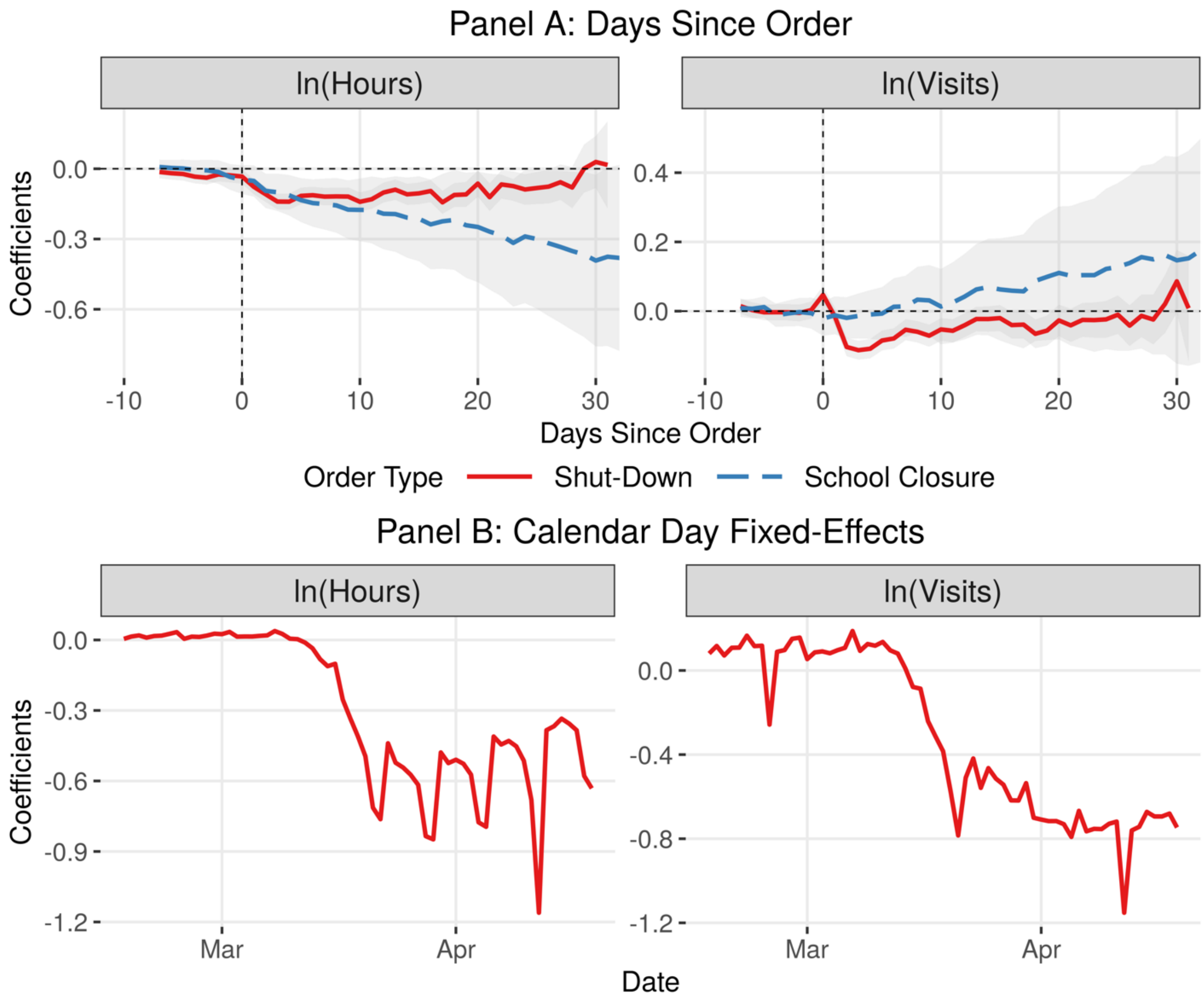
This plot shows the number of states with active shelter-in-place or stay-at-home orders between March 1st and mid-June 2020. We define orders as ceasing to be active on the first date that any business activity restriction is lifted.

Figure C7: Event study estimates of the effect of imposition and lifting of shelter-in-place orders without state-specific trends, with 95% confidence intervals



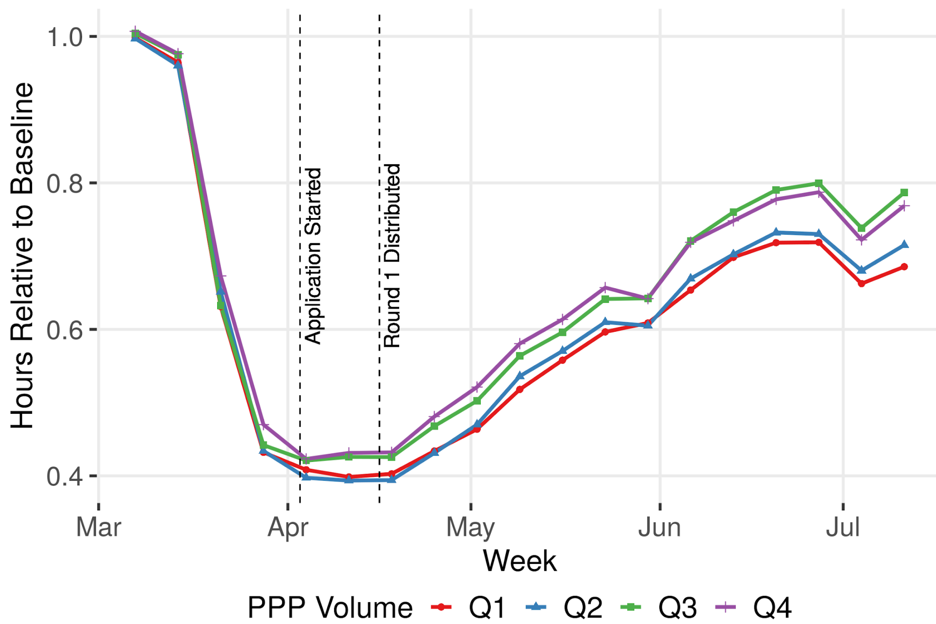
Notes: Samples for shutdown event studies consist of state-by-day observations from February 16 to April 19. Samples for the reopening event studies consist of state-by-day observations from April 6 to July 11; states that never had shelter-in-place orders are excluded. Specifications include full sets of state and calendar date effects. We exclude (normalize to zero) the effects for event times less than -7. The shutdown calendar time effects are normalized to zero on February 16. The reopening effects are normalized to align with the shutdown estimates on April 13. Shaded areas show 95% confidence intervals for the event time effects.

Figure C8: Event study estimates of the effect of shelter-in-place and school closing orders



Notes: We report estimates for a single event study model with two sets of event time indicators, for days relative to shut-down orders and days relative to school closings, along with calendar date effects, state fixed effects, and state-specific trends. Sample consists of state-by-day observations from February 16 to April 19. We exclude (normalize to zero) the effects for event times less than -7. The calendar time effects are normalized to zero on February 16. Shaded areas show 95% confidence intervals for the event time effects. Dates of school closure are from Education Week.

Figure C9: Hours trends by alternative measure of round 1 PPP amount, Homebase data



Notes: Figure reproduces Figure 6, using an alternative measure of PPP volumes. Here, states are ranked by the total volume of PPP loans received by April 16, divided by non-farm payroll in April 2019.

Figure C10: Initiation of Pandemic Unemployment Assistance (PUA) and Federal Pandemic Unemployment Compensation (FPUC) payments

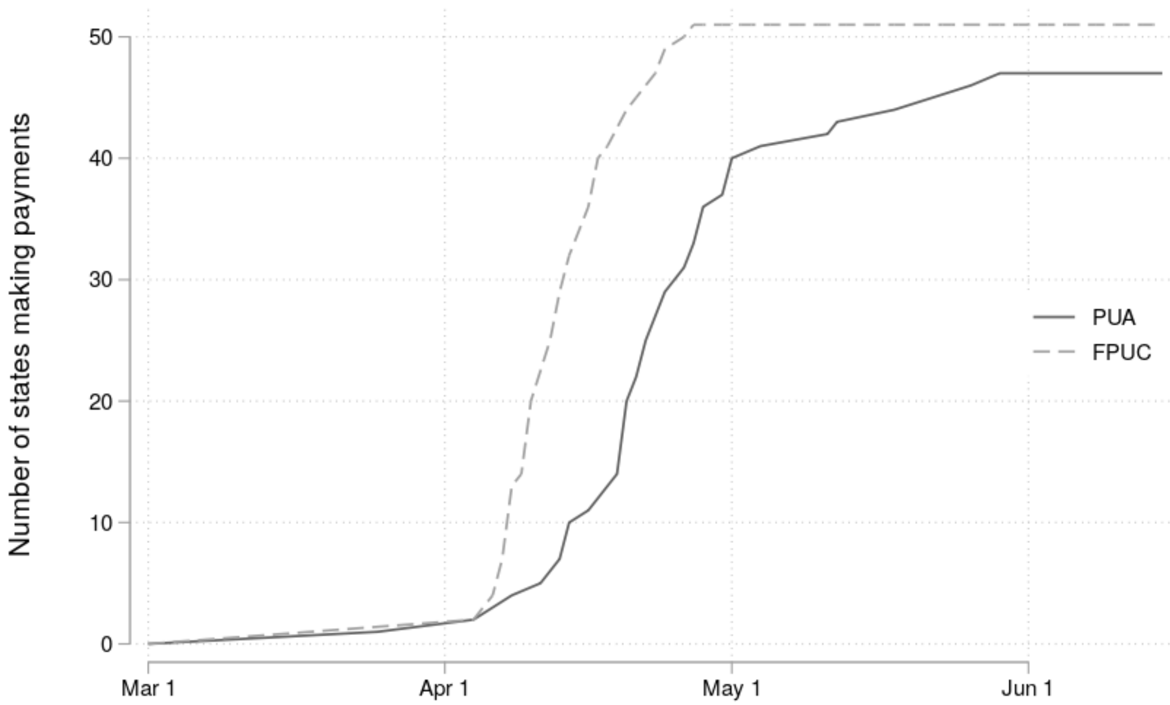
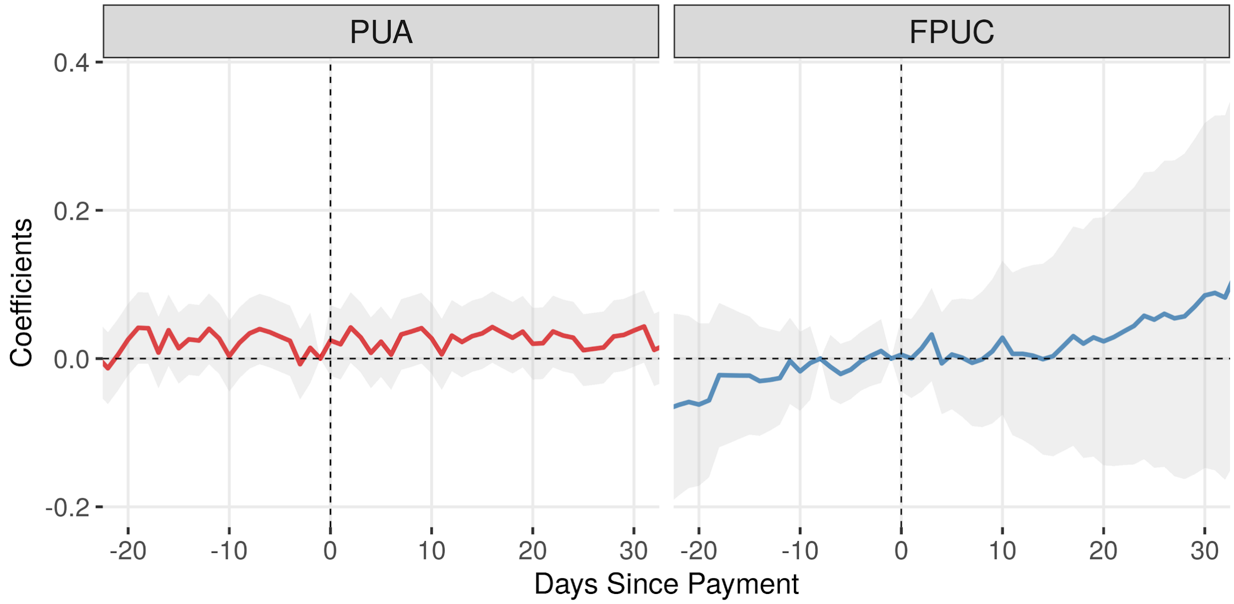


Figure C11: Event-study estimates of effects of PUA and FPUC payment starts on hours worked



Note: Panel PUA (respectively, FPUC) shows estimates from an event study model where the event is the beginning of Pandemic Unemployment Assistance (respectively, Federal Pandemic Unemployment Compensation) payments in the state. The sample is state-by-day observations from 2/16-7/11. Each specification includes an indicator of active stay-at-home order, a full set of state and calendar date effects, and all estimable event time effects. Event time -1 is normalized to 0 for both types of UI, and event time -8 is also normalized to 0 for FPUC.

Table C1: Variation in layoff and rehire probabilities with PPP payouts and UI replacement rates

	Logit: Stopped work in April				Logit: Started work in May or June			
	CPS		Homebase		CPS		Homebase	
PPP volumes								
Quartile 2	**-0.012**	**-0.038**	-0.005	-0.027	**0.041**	**0.044**	**0.124**	**0.137**
	(0.006)	(0.008)	(0.037)	(0.046)	(0.012)	(0.018)	(0.036)	(0.055)
Quartile 3	**-0.029**	**-0.027**	-0.019	-0.026	**0.043**	0.021	**0.154**	**0.189**
	(0.007)	(0.010)	(0.048)	(0.059)	(0.015)	(0.020)	(0.047)	(0.074)
Quartile 4	**-0.053**	**-0.067**	**-0.128**	**-0.193**	**0.044**	0.041	**0.152**	**0.246**
	(0.008)	(0.011)	(0.060)	(0.085)	(0.019)	(0.027)	(0.071)	(0.096)
UI replacement rates								
Quartile 2	**0.026**	**0.043**	-0.019	-0.000	**0.028**	0.018	0.008	0.031
	(0.006)	(0.008)	(0.041)	(0.043)	(0.013)	(0.015)	(0.039)	(0.047)
Quartile 3	-0.003	**0.020**	**-0.093**	-0.006	**0.040**	0.014	0.030	0.002
	(0.007)	(0.007)	(0.045)	(0.047)	(0.015)	(0.017)	(0.046)	(0.056)
Quartile 4	-0.012	0.015	-0.082	0.035	**0.043**	0.012	0.038	-0.048
	(0.007)	(0.010)	(0.056)	(0.057)	(0.018)	(0.023)	(0.057)	(0.074)
N	31,151	31,151	1,629	1,629	11,235	11,235	1,226	1,226
Division FEs	N	Y	N	Y	N	Y	N	Y

Notes: Table reports marginal effects from logit specifications for job leaving and beginning of work, using CPS (columns 1, 2, 5, 6) and Homebase data (3, 4, 7, 8). Samples, specifications and controls are identical to those in Table 1 (first and second specifications), except that we replace state fixed effects with indicators for three quartiles of the volume of PPP loans in a state, as a share of state non-farm payroll in April 2019, three quartiles of the median unemployment insurance replacement rate in the state (from Ganong, Noel, and Vavra, 2020), and, in even-numbered columns, division fixed effects. Washington, DC is excluded, as Ganong et al. (2020) do not report UI data for it.