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Abstract

We use traditional and non-traditional data sources to measure the collapse and subsequent partial recovery of the U.S. labor market in Spring 2020. Using daily data on hourly workers in small businesses, we show that the collapse was extremely sudden -- nearly all of the decline in hours of work occurred between March 14 and March 28. Both traditional and non-traditional data show that, in contrast to past recessions, this recession was driven by low-wage services, particularly the retail and leisure and hospitality sectors. A large share of the job loss in small businesses reflected firms that closed entirely. Nevertheless, the vast majority of laid off workers expected, at least early in the crisis, to be recalled, and indeed many of the businesses have reopened and rehired their former employees. There was a reallocation component to the firm closures, with elevated risk of closure at firms that were already unhealthy, and more reopening of the healthier firms. At the worker-level, more disadvantaged workers (less educated, non-white) were more likely to be laid off and less likely to be rehired. Worker expectations were strongly predictive of rehiring probabilities. Turning to policies, shelter-in-place orders drove some job losses but only a small share: many of the losses had already occurred when the orders went into effect. Last, we find that 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 so far in support of the view that high UI replacement rates drove job losses or slowed rehiring substantially.

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I. Introduction

The COVID pandemic hit the U.S. labor market with astonishing speed. The week ending March 14, 2020, there were 250,000 initial unemployment insurance claims -- about 20% 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. At this writing, claims have been above one million for thirteen consecutive weeks, with a cumulative total of over 40 million. 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 25 million.

The United States labor market information systems are not set up to track changes this rapid.[1] Our primary official indicators about the state of the labor market come from 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, and is released early the following 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. We combine data from the traditional government surveys listed above with non-traditional 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 use the Homebase data to measure the high-frequency timing of the March-April contraction and the gradual April-May 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 from other sources (e.g., on physical mobility) 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 June.

[1] In response to the limitations of traditional data sources, the Census Bureau started Household Pulse and Business Pulse surveys to provide higher frequency data on changes in the labor market and for small businesses. These surveys provide very useful information going forward, but only started on April 23rd and 27th respectively, and so are limited in their ability to understand the start of the COVID-19 economic disruptions.

We are not the only ones studying the labor market at this time. Cajner et al. (2020; in this volume), Allcott et al. (2020), Chetty et al. (2020), Cortes and Forsythe (2020), Dey et al. (2020); Gupta et al. (2020), Khan et al. (2020), Kurmann et al. (2020), Mongey et al. (2020) and Lin and Meissner (2020) conduct exercises that are closely 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 future research on the crisis.

The paper proceeds as follows. Section II describes the data sources. Section III provides an overview of the labor market collapse and subsequent partial recovery. In Section IV, 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 V 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. To contextualize these effects, we also show impacts on Google search behavior, on visits to commercial establishments, and on COVID-19 case diagnoses. Section VI 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 VII.

II. Data

We rely on two primary sources to measure the evolution of the labor market during the first half of 2020, supplementing with a few labor and non-labor-market measures that provide useful context.

First, we use the Current Population Survey (CPS), the source of the official unemployment rate. This is a monthly survey of about 60,000 households conducted by the Census Bureau in collaboration with the Bureau of Labor Statistics. Respondents are surveyed during the week containing the 19th of the month, and asked about their activities during the prior week (the week containing the 12th). The most recent available data are from the May survey. The CPS sample has a panel structure, with the same households interviewed for several consecutive months. This allows us to identify workers who were employed in March but not in April, or who were out of work in April but re-employed in May.[2]

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. 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, individualize 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. As we discuss below, if the misclassified workers are counted as unemployed on temporary layoff, as the BLS commissioner suggests (BLS 2020a), the unemployment rate was notably higher in the spring than the official rate.

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.

[2] 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.

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 here are not seasonally adjusted.

We use two other traditional sources: the monthly Current Employment Statistics survey of employers, the source of official employment counts, and weekly unemployment insurance claims reports, compiled by the Department of Labor.

We supplement the 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 U.S. 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. Homebase has generously made these data available to interested academic researchers. We use them to construct measures of hours worked at the employer-worker-day level. Employers are identified by their industry and location.

Homebase's customer base is disproportionately composed of small firms in food and drink services, retail, and other sectors that disproportionately employ hourly workers. The data exclude most salaried employees, firms who do not require this type of time clock software for their operations, and larger firms who would use their own software for this purpose. Consequently, insights derived from the Homebase data should be viewed as relevant to hourly workers in small-sized businesses, primarily in food services and retail, rather than to the labor market at large. While not representative of the US labor market, the Homebase subpopulation is however highly relevant to the current moment as the pandemic seems to have disproportionately affected the industries that form the Homebase clientele.

When analyzing the Homebase data, we focus on U.S.-based firms that were already Homebase clients before the onset of the pandemic. We separate Homebase clients into separate units for each industry, state, and metropolitan statistical area (MSA) in which the client operates. We refer to these as "firms," and use them as the unit of analysis. We define a base period as the two weeks from January 19 to February 1, and we limit our attention to firms that had at least 80 total hours worked, across all hourly workers, in this base period.[3]

We aggregate hours across all firms and workers in the sample in each day or week, sometimes segmenting by state or industry or worker characteristics. We scale hours in each day or week as a fraction of total hours worked (sometimes by state or industry or worker characteristics) in the base period. In daily analyses, we divide by average hours for the same day of the week in the base period; in weekly analyses, we divide by average weekly hours in the base period.

We consider a firm to have shut down if in any week (Sunday-Saturday) it had zero hours reported by all of its workers. We do not observe work among salaried employees, so it is possible that some firms that appear to us to have shut down are continuing to operate without hourly workers. We consider a firm to have reopened if, following a shut down, it again appears with positive hours. At reopened firms, we consider a worker to be a pre-existing employee if he or she had hours with the firm before it shut down, and a new employee otherwise.

Appendix A presents summary statistics for the full Homebase sample. As noted earlier, Homebase clients are disproportionately small firms. The median firm has full-time-equivalent employment (of hourly workers) under 5; 80% have 10 or fewer (Appendix Figure A1). Nearly half of Homebase clients are in the food & drink industry, and another 15% are in retail (Appendix Figure A2). Homebase firms are also somewhat disproportionately concentrated in the West relative to the Northeast and Midwest (Appendix Figure A3).

We supplement the Homebase data with information from a survey that Homebase allowed us to conduct of the users of its software (i.e., of workers at client firms). Survey invitations were sent via e-mail, starting May 1, to all individuals who were users of the software from February 2020 onwards. We use survey responses received by May 26, matched to the administrative records for the same workers.

We limit the analysis of the survey data to those survey respondents that were represented in our primary Homebase sample. We further restrict the analysis to workers with positive hours worked

[3] We exclude from all analyses any individual observations with more than 20 reported hours in a single day. We also exclude firms in Vermont, which had fewer than 50 sample units among Homebase clients.

in our base period and who have worked for only one firm using the Homebase software since January 19, 2020. Among the roughly 430,000 workers meeting this description, approximately 1,500 (0.3%) responded to our survey.[4] Appendix Table B1 presents summaries of key survey measures.

Despite the very low response rate, Appendix Table B2 shows that the survey respondents are roughly representative of the population of Homebase workers that were active in our base period and associated with a firm in our sample on the (limited set of) dimensions we can compare them on. In particular, survey respondents broadly match the Homebase population in terms of their distribution across Census division, industry, and employer size. However, survey respondents are somewhat positively selected on hours worked at the Homebase employer (Appendix Figure B1) and hence may be more representative of the "regular" workforce at these employers. Whereas the average Homebase worker had about 10% fewer hours in early March than in the base period in January, survey respondents saw no drop off in hours by this point.

As seen in Appendix Table B1, about 67 percent of survey respondents are female. Survey respondents are young, with 37 percent of the sample between 18 and 25 years of age and another 27 percent between 26 and 37 years of age. A majority (55 percent) are single, and only 31 percent have children. Two-thirds of survey respondents are white, while only 9 percent are Black, 16 percent are Hispanic, and 6 percent are Asian. More than 40 percent of survey respondents report annual household income in 2019 below $25,000. The modal education in the sample is a two-year college degree or some college (34 percent); 24 percent of respondents have a bachelor's degree and another 29 percent are high school graduates. The modal self-reported hourly wage rate in this sample is $10-$12.50 (27 percent of survey respondents), with another 15 percent earning between $7.50 and $10 per hour and another 20 percent between $12.50 and $15.00 per hour; only 4 percent report hourly wages of $25 or more.

III. Overview of the labor market collapse

Panel A of Figure 1 shows the unemployment rate and the employment-population ratio, as reported in the official monthly employment reports from the Bureau of Labor Statistics. The labor

[4] We will update this analysis as more survey responses arrive.

market gradually strengthened from 2010 through early 2020. The unemployment rate has in recent years been below the pre-Great Recession level, though the employment-population ratio has not recovered its earlier peak, reflecting in part an aging population. However, things took a sharp turn in spring 2020: The unemployment rate (not seasonally adjusted) spiked by 10.6 percentage points between February and April, reaching 14.4%, while the employment rate fell by over 9 percentage points over the same period. These two-month changes were roughly 50% larger than the cumulative changes in the respective series in the Great Recession, which took over two years to unfold. Both unemployment and employment recovered a small amount in May, but remain in unprecedented territory.

Table 1 tabulates the CPS microdata for the population age 18-64 across various labor market states. The share employed at work fell by 14.4 percentage points between February and April. The ranks of the unemployed more than tripled, from 2.9% to 10.4% (expressed as a share of the population, rather than of the labor force), between February and April. Finally, the labor force non-participation rate grew by 3.6 percentage points. Table 1 also shows that all of the categories that grew in April shrunk somewhat in May, as the labor market recovered a bit.

As noted in Section II, the usual labor market categories are not well suited to measure a population under shelter-in-place, and the BLS believes that the official unemployment rate has understated the amount of joblessness. Additional rows in Table 1 show ways that the existing categories failed to capture the decline in work during the pandemic. The increase in measured unemployment was entirely driven by increases in the number of workers on layoff, who expected to be recalled to their former positions; the share who were looking for new jobs shrunk slightly. This is a stark contrast from past downturns. Prior to this year, temporary layoffs were thought to be a historical phenomenon, of much lower significance in recent recessions than in the past. The share of the unemployed who were on temporary layoff has never previously exceeded 30 percent, but rose to nearly 80 percent in April.

The share who were employed but not at work grew by 3.3 percentage points, all driven by the "other reasons" category. BLS believes much or all of the increase in this category is people who should have been counted as on temporary layoff; if they are re-classified that way, the unemployment rate in April rises from 14.4% to 19.2% (BLS 2020c). Similarly, a large share of the new nonparticipants said that they wanted jobs but were not actively looking for work or were not available to take jobs. 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 are included as well, the adjusted unemployment rate rises well above 20%.

Monthly statistics are inadequate to understanding the rapidity of the labor market collapse. Panel B of Figure 1 uses the Homebase data to show how employment at the subset of firms represented in these data evolved on a daily basis. Reported in the Figure are total daily hours worked across all firms in the Homebase sample, as a fraction of average total hours worked on the same day of the week in the January 19 to February 1 base period. The total hours worked at Homebase firms fell by approximately 60% between the beginning and end of March, with the bulk of this decline in the second and third weeks of the month -- largely after the CPS reference week. The largest single daily drop was on March 17, when hours, expressed as a percentage of baseline, fell by 12.9 percentage points from the previous day. The nadir seems to have been around the second week of April. Hours have grown slowly and steadily since then. They made up perhaps one-third of the lost ground by the May CPS reference week, and by the most recent data have recovered about halfway to their beginning-of-March level.

Also apparent in Figure 1.B are clear day-of-week effects: Homebase employment is lower on weekends than on weekdays since the onset of the crisis. Recall that the Homebase data are normalized relative to the same day of the week in the baseline period, so if hours fell proportionally on all days of the week we would not see this pattern. Evidently, Homebase firms have reduced hours by more on weekends than on weekdays. This could reflect businesses reducing to skeleton hours, all on weekdays, or perhaps disproportionate hours reductions or shutdowns of customer-facing businesses with high weekend shares.

Another source of near-real-time information about the labor market is unemployment insurance claims, shown in Figure 2. Initial claims spiked to unprecedented levels the week of March 21, continued to grow through early April, and have gradually fallen since, though they remain several multiples of pre-recession levels and even above the pre-2020 historical record. Over the twelve weeks since March 14, over 40 million initial claims were filed.

There were extensive reports of processing delays in the unemployment insurance system in March and April, and it was not clear whether the ongoing high claims reflected ongoing layoffs or backlogs of applicants who had been laid off in March but had been unable to file claims immediately due to system congestion. The Homebase data suggest that processing delays may not have badly distorted the time pattern; UI initial claims reached their peak just about the same time that Homebase hours reached their nadir, and have been falling since as hours have recovered.

Figure 2 also shows continuing claims. After March 27, when the CARES Act was passed, we add to claims under the regular UI program claims for new Pandemic Unemployment Assistance (PUA) benefits for independent contractors and others who do not qualify for regular benefits and claims for Pandemic Emergency Unemployment Compensation (PEUC), for those who have exhausted their regular benefits. Continuing claims rose steadily through April and have fallen only slowly since then, with nearly 30 million claims filed the last week of May. One puzzle of recent months is that the number of unemployment insurance claimants has exceeded the number of people counted as unemployed. This may be partially explained, however, by the prevalence of misclassification in the CPS; expanded measures of unemployment are closer to or larger than the number of UI claims. In the Homebase survey data, 41% of those who reported being furloughed, laid off, or absent from work said that they had applied for unemployment benefits.

Hedin, Schnorr, and von Wachter (2020) have used administrative records from the California unemployment insurance system to explore the characteristics of unemployment insurance applicants. They find that over 90% of new claimants in late March reported that they expected to be recalled to their prior jobs, up from around 40% in February. The share expecting recalls has gradually declined since late March, to around 70% 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.

IV. Who are the unemployed? Who are the rehired?

A. Industry

Figure 3 shows how uneven the labor market collapse was across industrial sectors. We use payroll employment data from the Current Employment Survey, conducted by BLS, to measure the total number of jobs in each primary industry by month. The CES counts paid workers, so may include workers who are not at work but continuing to draw paychecks.

Not surprisingly, the low-wage segment of the private service sector of the economy experienced the largest drop in employment. In the leisure and hospitality sector, which includes restaurants and hotels, employment fell by about half between March 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 April employment numbers less than 80% of where they stood in January. Workers employed in retail trade were also disproportionately exposed to the COVID-19 shock, with nearly 15% of retail jobs lost in April.

Appendix Figure C1 shows the change in the number of workers by industry in the CPS. Patterns are similar, with the largest job losses in the hospitality and other services industries. We also examine patterns for hourly workers, a proxy for the sectors represented in Homebase data. We find little evidence that the industry patterns of job loss differ dramatically for hourly and non-hourly workers.

This sector composition of the COVID-19 crisis stands in contrast with that of the Great Recession induced by the financial crisis in the late 2000s. Figure 4 compares the two-month decline in employment from February to April 2020 with the cumulative decline between November 2007 and January 2010. It shows that this year we lost about 50% more jobs in total than in the whole of the Great Recession, and that the industrial composition was quite different. The largest declines in employment between November 2007 and January 2010 were in construction and durable goods manufacturing; in contrast, the low-wage segment of the private service sector was relatively insulated from the Great Recession.

Figure 3 further shows that the partial recovery has also been uneven across industrial sectors. For example, the construction sector had nearly regained the April employment losses by May. Also, while manufacturing, wholesale trade, education and health and professional and business services experienced smaller job losses in April, little if any of these job losses had been recouped by May.

B. Firm closings and reopenings

An advantage of Homebase data over the CPS, beyond its daily availability, is that it enables us to link workers to their employers. We use this link to separate the observed change in total hours documented above (Panel B of Figure 1) into three channels: firm shutdowns, layoffs, and cuts in hours. To do this, we define a firm as having fully shut down in a given week if the Homebase data records zero employees clocking in at that firm during that week. Among firms that have not shut down, we count the proportional change in the number of workers with positive hours in a week, relative to the baseline, and attribute that share of baseline hours to layoffs. Last, we define hours cuts as the reduction in average hours, relative to the baseline period, among workers still employed at still operating firms.

One important caveat to this decomposition is what we refer to as a firm shutdown is a shutdown of Homebase-measured employment. If firms employ workers that do not schedule their time using Homebase and some of these workers remain employed, some of the hours losses that we attribute to shutdowns may instead be properly attributed to layoffs at continuing firms. Another caveat is that the "hours cut" category includes all workers with positive hours during a given week. Hours losses from workers who stop working in the middle of a week will be counted as hours reductions in that week and will then convert to layoffs or firm closures the following week.

With these caveats in mind, Figure 5 reports the percent change in hours each week since early February attributable to these three forms of hours reductions. As shown above, hours worked at Homebase firms fell by 60 percent between the baseline period of Jan 19-Feb 1 and the week of April 5-11 and have been slowly and steadily recovering since, reaching about 70 percent of baseline by the second week of June. Except for the very first week of the labor market collapse (week of March 15-21),[5] hours reductions as defined above have accounted for a very minor part of the change in total hours at Homebase businesses. Instead, the decline in total hours came primarily from firms that closed entirely and from reductions in the number of workers at continuing firms. The latter accounted for a larger share than the former in March, but thereafter

[5] We conjecture that the large role for hours reductions in this week is an artifact created by mid-week layoffs or firm closings. Consistent with this, Appendix Figure C4 shows that the distribution of hours per worker fell in that week but returned to normal the following week and has been quite stable through the year to date, other than transitory declines in holiday weeks.

the two have had about the same quantitative impact on "missing hours" throughout the rest of the sample period.[6]

The Homebase data also allows us to zoom in on the (partial) recovery period and assess the channels via which hours are being restored at businesses that had shutdown at the nadir of the labor market collapse. Of the roughly 42,000 unique firms in our baseline sample, approximately half shut down for at least one week by April 4. In Figure 6, we report the distribution of hours at these businesses from the week of April 6-11 to the week of June 7-13, as a share of total baseline hours at these businesses. The figure shows that about 60 percent of baseline hours are still missing at these businesses in the most recent week. Two-thirds (40 percentage points) of these missing hours are attributable to businesses that remain closed. Another third (20 percentage points) of missing hours can be attributed to businesses that have reopened but are doing so at reduced scale. Of the 40 percent of hours that have been collectively regained, a vast majority has been coming from rehiring of workers that were previously employed at these businesses (prior to the shutdown), with only a very small share of hours regained coming from new hires. However, the share attributable to new hires has been slowly trending up over time. New hires comprised only 5 to 7 percent of those re-employed at reopened firms between early April and early May; as of the week ending June 13, that share has risen to almost 18 percent. This suggests that while most firm-worker matches at these firms have been maintained through the crisis, the stability of these matches has become more and more precarious as the time between firm closure and re-opening increases.[7]

The Homebase data also allows 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. While Homebase does not collect much data from their employer clients, three employer characteristics can be tracked: size (defined as the employer's total number of unique employees in the Jan 19-Feb 1 base period), industry, and growth rate (which we define as the change in the number of

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

[7] Appendix Figure C3 shows the share of hours worked each day that came from workers who were present in the base period. This falls off over time due to normal turnover. The rate of decline is not as steep in 2020 as it was in 2018 and 2019, however, consistent with firms operating at reduced capacity but filling vacancies primarily from those who were laid off earlier.

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).

Reported in Figure 7 are marginal effects from logit models on the likelihood of the firm shutting down by April 4, and, for firms that did, on the likelihood of re-opening by mid-June, controlling for state and industry fixed effects. We find that larger firms are much less likely to shut down than smaller firms. Larger firms are also somewhat more likely to have re-opened by the second week of June conditional on having shut down, though this is not statistically significant. Across industries, the shutdown rate was highest in the beauty and personal care industry, while employers in the retail and health care and fitness sectors, which were not particularly likely to shut down, had somewhat larger re-opening odds.[8] Most interesting though is how the likelihood of shutting down and re-opening is predicted by employer growth rate in the January 2019 to January 2020 period. There is a close to monotonic relationship, with more vibrant businesses (as proxied for by higher employment growth in the pre-COVID period) having both a lower likelihood of shutting down and a higher likelihood of re-opening conditional on shutting-down. That is, it is the businesses that were already struggling pre-COVID that have the highest odds of shutting down during the peak of the COVID crisis and of remaining closed. Three possible explanations are that businesses that were already struggling prior to COVID might have been particularly low on cash and unable to withstand the shock (Bartik et al., 2020); that those businesses may also have been de-prioritized by banks when they applied for PPP funding; or that the COVID crisis sped up the pruning of some of less productive businesses in the economy (Barrera, Bloom, and Davis, 2020).

Worker-level job loss and re-hiring

We now turn to examine the relationship between worker characteristics and job loss within sectors. The first three columns of Table 2 report characteristics of three groups of workers: those who were employed in both March and April, those who were employed in March but not in April, and those who were out of work in April but started work in May. The second group, who we refer to as “job losers” or as “laid off” workers though we also include voluntary departures and those who have jobs but are not at work, has higher shares of very young (under 25) and old (over 65)

[8] Figure C2 compares job loss by industry in Homebase and CPS data, using a crosswalk created by Etienne Lalé to assign CPS employment to Homebase industries. The alignment is not particularly close, which we attribute to the difficulty of matching the specific industry composition of Homebase clients within broader sectors.

workers than does the group of continuously employed workers. Job losers are notably less educated and more likely to be non-white. They are also more female, less likely to be married, and less likely to have managerial positions. The third group of job returners broadly resembles the job losers, with the notable exception that it includes a smaller share of Black workers and somewhat fewer women.

To further understand the determinants of leaving work in April and of starting work in May, we estimate multivariate logit models that include all of these characteristics as predictors, along with fixed effects for states and major industry groupings. Our first logit model includes all CPS respondents who worked in March and takes as the outcome the absence of work in April, while the second is estimated on those not working in April and takes work in May as the outcome. Marginal effects are shown in the rightmost columns of Table 2, and shown graphically in Figure 8.

The analysis reveals systematic differences across socio-demographic groups in the likelihood of having stopped work in April that mostly mirror the unconditional patterns. We see a strong U-shaped pattern in age for job loss. Workers that are 65 years of age or more (16 to 24 years of age) were 14 (8) percentage points more likely to exit work in April compared to otherwise similar workers in the 25 to 34 age group. The education gradient also remains quite strong. Workers without a high school degree were 10 percentage points more likely to have stopped working in April than similar workers with college degrees. There are also systematic racial differences. Black, Asian, and Hispanic workers were, respectively, 4.6, 5.2, and 1.6 percentage points more likely to exit work in April relative to otherwise similar white workers. Finally, married individuals were less likely to lose jobs and women were more likely to do so. Perhaps surprisingly, 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 re-hiring in May. In particular, older workers, Black and Asian workers, and single workers were more likely to lose their jobs in April and, having done so, less likely to start work again in May. However, education gradients in re-hiring were comparatively weak, with both more- and less-educated workers less likely to return to work than high school graduates.

In Appendix Table C1, we replicate the logit models for the subset of workers that were employed in hospitality and retail trade in March, to more closely match the industrial composition of the Homebase dataset.[9] Many of the patterns uncovered in the full CPS sample remain. The biggest differences are that in these sectors Black and Hispanic workers are not disproportionately affected by job loss (though Black workers are less likely to be rehired in May), and both male and female workers with young children are less likely to have returned to work in May than their peers with older children.

To conduct similar analyses in Homebase data, we link the administrative records on hours worked to our worker survey, which provides demographic information. We also collected additional worker characteristics that are not available in the CPS, such as the hourly wage rate, workers' expectations about the crisis (such as expectation to be recalled if laid off or furloughed), information about the nature of communication between employer and worker at the time of job separation (such as whether the employer told the worker he or she would be rehired), and more general information about how workers, especially those that have lost employment, have been financially coping through the crisis.

Figure 9 presents time series for hours worked across education (panel A) and wage (panel B) groups. Panel A shows that the lowest education group (those without a high school degree) was most negatively impacted by COVID-19, with hours as low as 10 percent of baseline at the trough, but also experienced a somewhat larger recovery, back to nearly 50 percent of baseline hours by early June. In contrast, the COVID-19 shock was less extreme in the highest education group (master's degree or more) but recovery since the trough has also been much more muted for that group. Moreover, while the magnitudes differed across education groups, the timing of hours loss and recovery was nearly identical. Patterns of impact by hourly wage broadly match those by education: workers with hourly wages below $15 saw their hours decline much more steeply than those with hourly wages above $15, but they also experienced a faster recovery.

Table 3 repeats the analyses of job loss and rehiring from Table 2 in the Homebase data. We again present the marginal effects from our logit analyses graphically in Figure 10. We define layoff and

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[9] The CPS allows us to identify hourly workers only in their last month in the sample, making it impossible to conduct our longitudinal analyses of job loss and hiring on the subsample of hourly workers.

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 re-hired those who returned to work and recorded positive hours at some point between April 18 and the end of our sample. 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 re-hires at reopening vs. continuing firms. We define explanatory variables as similarly as possible to the CPS.

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 men with children were relatively spared from layoffs. In addition, as in the CPS data, Black workers are notably less likely to be rehired.

The Homebase data also allows us to study how the likelihoods of layoff and rehiring (conditional on layoff) differ across workers at different wage levels, tenure lengths, and prior full-time status.[10] We augment the logit models from Table 3 with these predictors, and present marginal effects in Figure 11. Full-time workers are 21 percentage points less likely to be laid off than otherwise similar part-time workers; full-time workers are also (marginally significantly) more likely to be rehired. Controlling for other worker characteristics, we do not see statistically significant differences by hourly wage rate or by length of tenure at the firm, except that the lowest wage workers (who are likely to be tipped workers) are marginally significantly less likely to be rehired. In addition, though it is not statistically significant, those who have been with their employers for over a year were somewhat less likely to be laid off.

The survey data we collected also allow us to understand more fully the experiences and expectations of the Homebase workers. Twenty-two percent of the sample reported having experienced a layoff as because of COVID, while 34 percent report having been furloughed and 20 percent report hours reductions. Less than 10 percent report having made the decision to not

[10] We classify workers who worked more than 20 hours per week during our base period in late January as full-time. We use wages as recorded in the Homebase administrative data, to maximize the sample size.

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 has been negatively impacted by COVID 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 (78%) among laid-off workers than among furloughed workers (66%) or workers who experienced reduced hours (35%). Fifty-four 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, only about a quarter report looking for work, with the modal reason for not being looking for work being an expectation to being rehired; only 8 percent attribute their lack of job search to financial disincentives to work. Among the people that expect to be rehired, the modal expectation about rehire date is June 1 (32 percent), with the second most common expectation being July 1 (27 percent).

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 with this employer is an important correlate of the decision to go back if asked. For example, 80% of workers who said that they strongly agreed with the statement “I liked my manager” would plan to go back if asked, compared with 67% who only somewhat agreed with this statement. Also, 91% 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% who only somewhat agree with this statement.

In Figure 12, we assess how answers to some of these questions relate to the likelihood of being rehired (defined as above). The marginal effects presented in Figure 12 are from three separate logit models where the only additional controls are state and industry fixed effects. Workers who believed it was likely they would be rehired were 17 percentage points more likely to have been rehired relative to other workers in the same industry and state who believed a rehiring was unlikely. Similarly, workers who had been told by their employers that they would rehired were 26 percentage points more likely to be rehired than those who had been told they would not be rehired. Similarly, we also see that workers that were encouraged by their employer to file for unemployment insurance were slightly less likely to be rehired, but the difference here is not statistically significant. Together, 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.

V. Evaluating non-pharmaceutical interventions

Many of the firm closures observed to date were closely coincident with state closure orders and other non-pharmaceutical interventions, and policy has generally proceeded on the assumption that many of these firms will reopen when these orders are lifted. It's 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. As for reopening, many businesses have been permanently damaged by their closure and may not reopen. 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 "shut-down orders") that restrict the public and private facilities that people can visit to essential businesses and public services. We focus on this type of intervention because it is both the most prominent of the non-pharmaceutical interventions and the one that may have the largest direct effects on economic activity. We test the importance of these government 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 shut-down 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. They most commonly come from centralized lists of executive orders (see Illinois.gov 2020, for example), but in some cases come from centralized lists of public health and COVID-related orders (e.g., New Mexico Department of Health 2020). We define a stay-at-home order as any order that requires residents to stay at home or shelter in place (timed to the announcement date). Orders that include COVID-related guidelines, but do not require residents to shelter in place (e.g., coronavirus.utah.gov 2020), are not included. In states that had stay-at-home orders, we define reopen orders as the first lifting of these restrictions (timed to the effective date). Figure 13 shows the number of states with active shut-down orders between the start of March and the present. California was the first state to impose a shut-down order, on March 19. The number of active orders then rose quickly, reaching 44 in early April. It was stable for about three weeks, but then began to decline as some states removed their orders in late April and early May. By June 1, only Wisconsin still had an order in place.

Stay-at-home orders can reduce employment simply by prohibiting non-essential workers from going to work. But 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 these three outcomes. We measure public awareness using Google trends data on state-level relative search intensity for "coronavirus" from January to early June 2020. We normalize these to set the maximal value in California at 100, relying on Google's normalization of other states relative to California.[11] Overall mobility is measured using SafeGraph data on visits to public and private locations between January 19 and June 13, 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. State-level daily COVID-19 case data comes from the database maintained by USAFacts, a source cited by CDC, and is divided by 2019 state population.

We estimate event-study models of the effect of shut-down and re-opening orders on the four outcomes described above: log hours worked (from Homebase), log SafeGraph visits, an index of

[11] This follows the approach used by Goldsmith-Pinkham, Pancotti, and Sojourner (2020) in normalizing Google search trends when predicting unemployment claims.

COVID-19 related Google searches, and log COVID-19 cases.[12],[13] Each outcome is measured at the state-by-day level. We regress each on full sets of state and date fixed effects and a series of indicators for "event time" relative to the imposition or lifting of a shut-down order (estimated separately). We normalize the event time effect to zero fourteen days before a shut-down order is put in place and seven days before a reopening order. The shut-down model is estimated on data from February 16 to April 19, while the order lifting model is estimated on data from April 6 through June 13.

We report these results in Figure 14 (for shut-down orders) and 15 (for re-opening orders). In each figure, Panel A reports results for our base specification, while Panel B reports results for an additional specification that includes state-specific time trends. Each panel includes four sub-panels, one for each of our outcomes. Starting with the estimates for the relationship between shut-down orders and hours in Panel A of Figure 14, we see that hours worked began trending downward a few days before shut-down orders began, but the trend accelerated in the days immediately after the orders. The cumulative effect of the post-shutdown acceleration is to reduce hours worked by about 10 to 15 log points two weeks after the order takes effect.

Turning to our measure of visits from SafeGraph, we see that there appears to be a very slight pre-trend in visits, but a sharp, roughly 15 log point decline in visits after the shut-down orders are implemented. Perhaps unsurprisingly given the lag in diagnoses and likely feedback from diagnoses to orders, there is a strong pre-trend of cases with respect to orders, but little sign of an immediate change when the order takes effect. Finally, turning to the last sub-panel on the relationship between shut-down orders and Google searches about the coronavirus, we see a spike in Google searches on the day of the implementation of the shut-down order, and then a decline afterwards, possibly reflecting a decreased need to search for COVID-19 information once information has initially been connected or decreased searches due to changing caseloads.

Panel B of Figure 14 reports event-study models that incorporate state-specific time-trends. These models may be more robust to unobserved state heterogeneity that is correlated with treatment

[12] 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 (2020) and Allcott et al. (2020).
[13] We use log((cases+1)/population) for to allow for zeroes.

timing, but must be interpreted cautiously, given that they rely on the assumption that linear state-specific trends are a good approximation for time trends in the absence of shut-down orders.[14] That being said, two interesting patterns present themselves in Panel B. First, both hours and visits decline immediately after the shut-down orders and then slowly recover afterwards, 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. Second, once state-specific trends are allowed for, there does appear to be a gradual decline in caseloads per capita after shelter-in-place orders. Given the lagged response of caseloads to changes in behavior, and the possibility that states implementing shut-down orders also implemented other public interventions at the same time, more work is necessary to fully understand the relationship between shut-down orders and caseloads. However, these results are consistent with shut-down orders reducing cases per capita relative to states without shut-down orders.

Figure 15 reports results from the corresponding specifications for re-opening orders. Focusing on Panel B, which reports results with state-specific trends, we see that the effects of re-opening orders broadly look like the mirror image of shut-down orders; hours and visits immediately rise, while cases per capita slowly rise. The magnitudes are similar, although slightly smaller than those for shut-down orders.

How should we interpret the magnitudes of the estimates in Figures 14 and 15? One way to think about them is to compare the estimates of the effects of shelter-in-place to the calendar date effects from the same specifications, which reflect other determinants of the outcomes that are common to all states. Figure 16 reports the calendar date effects for each of our four outcomes from the shut-down and reopening specifications reported in Figures 14A and 15A, respectively. The red line reports the date effects from the shut-down model, covering February 16 to April 19, and the blue line reports date effects from the re-opening model, which uses data from April 6 to June 13.

[14] 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 difference-in-differences and 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.

We include both lines for the period from April 6 to 19 where the periods overlap, and normalize the reopen estimates to align with the layoff estimates on April 13.

As expected given the results in Section 1 above, the calendar date effects show extremely large reductions in hours (about 75 log points at the weekend trough and about 60 log points on weekdays) and visits in mid-March, and rising cases per capita over time. These large changes contrast with the comparatively modest effects of shut-down and re-opening orders that we estimated in Figures 14 and 15. For example, the estimated effect of shut-down orders on log hours is about one-sixth as large as the pure calendar time effects, while the estimated effect of shut-down orders on log visits is about one-eighth of the calendar time decline in visits. Combined, these results imply that, at least in the short-run, shut-down and re-opening orders are 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 shut-down or re-opening orders themselves. This is consistent with the relatively large effects we see on visits to businesses in Figures 15 and 16.

Two caveats are important to keep in mind when interpreting our finding that shut-down and re-opening orders play only a modest role in the labor market effects of COVID-19. First, shut-down orders may have spillover effects on other states not captured in our model. In particular, the first shut-down orders may have played a role in signalling the seriousness and potential risk associated with COVID-19, even if subsequent shut-down orders had more muted effects.

Second, over longer time horizons, the effects of shut-down orders on social distancing and caseloads may result in larger labor market effects than we estimate here. For example, the slow rebound of hours worked in Figure 14, Panel B may in part be due to reduced caseloads making people more comfortable engaging in economic activity. Explorations of these more complicated medium- and long-run interactions of shut-down orders, labor market activity, social distancing, and caseloads is beyond the scope of our analysis here. Several papers, including Chernozhukov, Kasahara, and Schrimpf (2020) and Allcott et al. (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 shut-down orders.

VI. 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 for the pandemic. We have mentioned two already: 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 Pandemic Unemployment Compensation (PUC), which adds $600 to every weekly unemployment benefit payment. As the average weekly benefit prior to CARES was around $300, this is a dramatic increase in the generosity of unemployment benefits. With PUC, many low wage workers would receive higher total UI payments than they would have earned at work (though for many this would be offset by the loss of employer-provided benefits).

The primary goal of these expansions was to aid workers who, through no fault of their own, 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 supporting 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 re-hiring, and could even lead to more job loss -- although workers who quit their jobs are not eligible for UI, workers who would prefer to receive unemployment benefits than to remain on the job might persuade their employers to pursue a layoff-based strategy 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% for the average U.S. worker. Because many workers, particularly those displaced in March and April, earn less than the average, and because the PUC payment did not vary with prior earnings, many workers faced replacement rates well in excess of 100%. Ganong, Noel, and Vavra (2020) find that the median replacement rate is 134% and that 68% of workers unemployed in the past would have qualified for replacement rates greater than 100% under PUC. Anecdotally (e.g. Morath 2020), some employers have reported that laid off workers have been unwilling to return to work, even when businesses reopen, because this would mean a loss in income.

We take two strategies for evaluating the effects of the expansions of UI and business loans 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 et al. (2020) document wide variation across states in unemployment insurance replacement rates under CARES, with a low median replacement rate of 129% in Maryland and a high of 177% in New Mexico. We divide states into four groups by the median replacement rate, following Ganong et al. (Figure 5 in their paper), 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' pre-existing 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 17 shows the time series of hours worked, 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 have seen the slowest recovery since then. This is the opposite of the pattern one would expect if either were importantly driven by labor supply responses to UI generosity, though as noted there may be other differences across states that confound this.

We can use a similar strategy to develop descriptive evidence about the forgivable small business loans provided under the Paycheck Protection Program (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 accept and 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 total amount of PPP loans processed, divided by non-farm payroll in April 2019. The amount of PPP money disbursed to firms in states in the top quartile was nearly 70% larger than the amount going to firms in the bottom quartile. 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 suggest that much 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.

Figure 18 shows hours worked by the four PPP quartiles. The trough in hours is lower in the states that received the least PPP money, and even at the end of our sample these states have lower hours relative to baseline 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 possible 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. Table 4 reports several estimates in both the CPS and the Homebase data. Each model includes all of the controls listed in Table 2 (CPS) or Table 3 (Homebase) as well as industry fixed effects, but we replace the state fixed effects from those earlier estimates with sets of 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 Figures 17 and 18. Both higher PPP volumes and higher UI replacement rates are associated with fewer layoffs and faster rehiring. Effects are notably larger in the Homebase data than in the CPS sample. Many of these effects cease to be statistically significant when we include division fixed effects, but the directional pattern generally remains.

A second strategy for assessing the impact of unemployment insurance 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 due to delays in state implementation: 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. While claimants should have received benefits that were retroactive to the beginning of the programs, 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.

Figure 20 shows the distribution of initiation dates of payments under the PUA and FPUC programs. Figure 21 shows event study plots for the two treatments' effects on Homebase hours. Both are estimated using a balanced sample of states and calendar dates, running from February 16 to June 13, 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 worked 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.

VII. 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 three months already illustrates some important facts and lays out important questions for future research and 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 shut-down orders promulgated by states 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..

It is now clear that the labor market began to recover quickly, in mid-April. This recovery, though very partial to date, has 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 very likely, and the data we report already suggest, that the displaced workers that have been left out from this very early stage of the recovery will face a much steeper challenge reentering the labor market. Firm-worker matches are going stale, and many of the former employers appear unlikely to reopen.

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, it appeared that the economic crisis would be short. The PEUC 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. It now appears that the period of economic weakness will last much longer, and that additional supports will be needed. 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 shows 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 non-white, 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 the speed of rehiring.

One interesting 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.

This recession has already been labelled by some as a "She-cession."[15] Indeed, the sectors most disrupted by the pandemic disproportionately employ women, where the sectors that have led past recessions are disproportionately male. Besides this compositional aspect, we also find some evidence within industrial sectors that women left work at the peak of the downturn at higher rates than men. On the other hand, there is limited evidence in our analysis that the job recovery has been uneven by gender (within sector), at least so far. It will be important to track how this continues to unfold in months to come, with a particular focus not solely on the employed/unemployed margin, but also the labor force participation margin. A lot of factors that will affect these developments, such as impacts of the continuing public health crisis on the provision of childcare and regular schooling, remain unknown.

Another topic for future study concerns the concentration of job losses, at least from small firms, in businesses that shut down entirely. It will be important for future research to further investigate the full picture of economic activity reallocation induced by the pandemic and the role of public policy in this reallocation. An important fact that emerges from our early analysis is that firms that were struggling prior to COVID were much more likely to shut down at the peak of the (first wave) of the pandemic and also much less likely to re-open during the recovery. This suggests a cleansing effect of the recession, but the causes and consequences of this pattern remains 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 prior to COVID 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. While the data that the Small Business

[15] https://www.nytimes.com/2020/05/09/us/unemployment-coronavirus-women.html.

Administration is currently making available is too aggregated to address any of these questions, the recently promised future availability of loan-level data may help in sorting them out.

A final take-away from our analysis is that there is no evidence, at least to date, that high unemployment insurance replacement rates encouraged layoffs or discouraged re-hires. If anything, we find that more generous replacement rates are associated with shallower declines and more rapid recoveries, though these effects are not entirely robust. 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.

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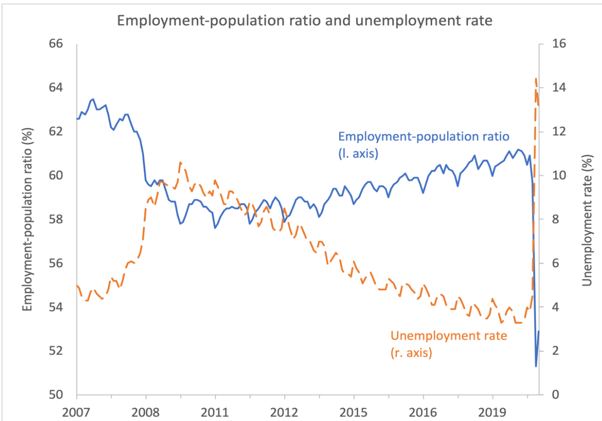
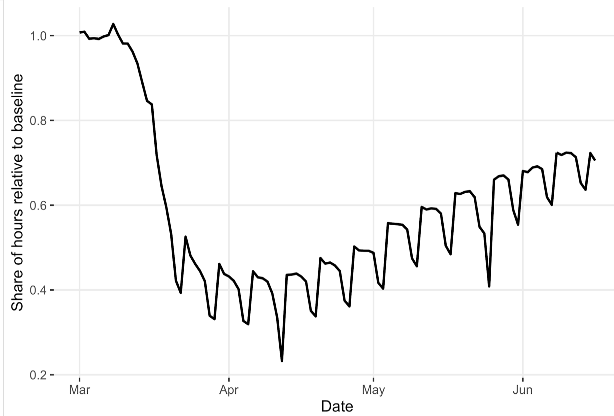
Figure 1: The labor market collapse

A. Monthly official statistics, 2007-2020



B. Daily data from Homebase, March - June 2020



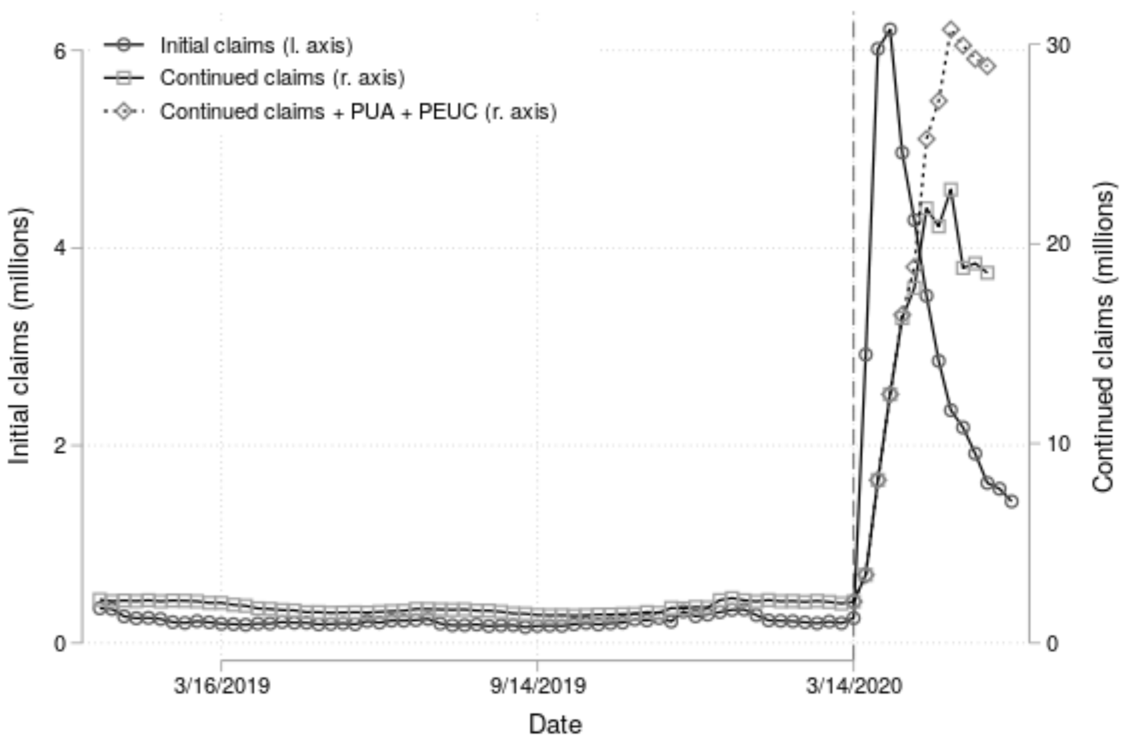
Notes: Panel A shows monthly unemployment rate and employment-population rate statistics, not seasonally adjusted. Panel B shows daily total hours worked across all firms in Homebase data, as a fraction of average hours worked on the same day of the week in the January 19-February 1 base period. The sample includes firms (defined at the firm-industry-state-MSA level) that recorded at least 80 hours in the base period and excludes Vermont.

Table 1: Employment status of the population, February - May 2020

	Levels (%)				Changes (percentage points)			
	February	March	April	May	Feb. - March	March-April	April-May	Feb.-May
Employed at work	72.0%	69.8%	57.6%	60.9%	-2.1	-12.3	3.4	-11.0
Employed but not at work	1.8%	2.9%	5.1%	3.7%	1.0	2.2	-1.4	1.9
All enumerated reasons	1.6%	2.0%	1.5%	1.8%	0.4	-0.4	0.3	0.2
Other	0.2%	0.9%	3.6%	3.3%	0.7	2.6	-0.3	3.0
Unemployed	2.9%	3.4%	10.4%	9.4%	0.5	7.0	-1.0	6.5
On layoff	0.5%	1.0%	8.2%	6.9%	0.5	7.2	-1.3	6.4
Looking	2.4%	2.4%	2.2%	2.6%	0.0	-0.2	0.3	0.2
Not in the labor force	23.3%	23.9%	26.9%	25.9%	0.6	3.0	-1.0	2.6

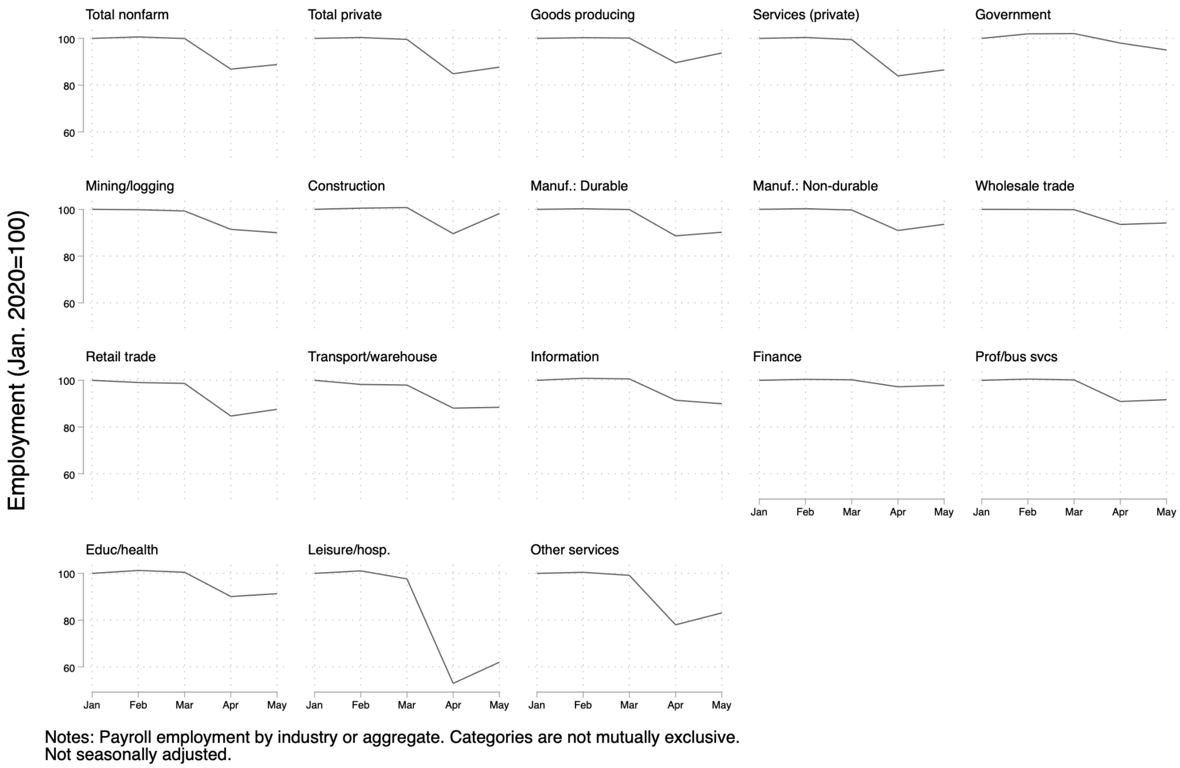
Notes: Computed from Current Population Data public use samples, for the population aged 18-64. Not seasonally adjusted.

Figure 2: New claims and continuing claims for unemployment insurance



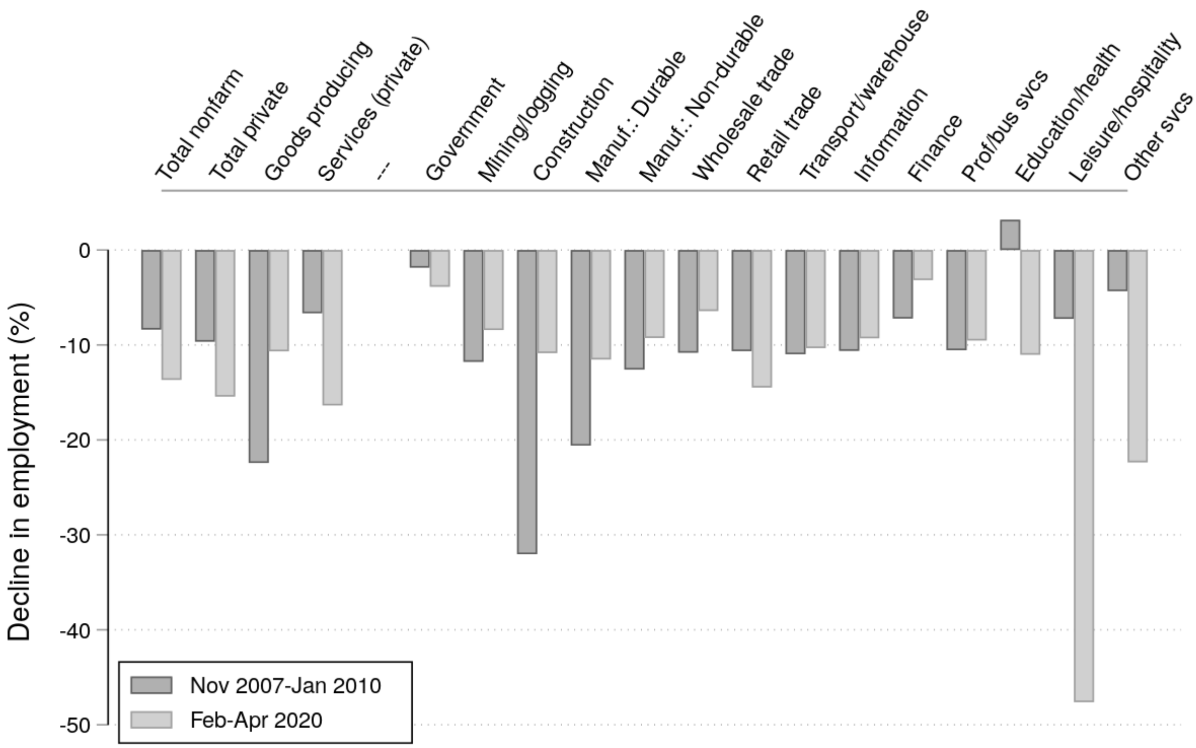
Note: Not seasonally adjusted data as of June 18, 2020. Dashed continuing claims series includes claims under the new Pandemic Unemployment Assistance program for those not eligible for regular benefits and the new Pandemic Emergency Unemployment Compensation program for those who have exhausted their regular benefits.

Figure 3: Payroll employment by sector and month, 2020



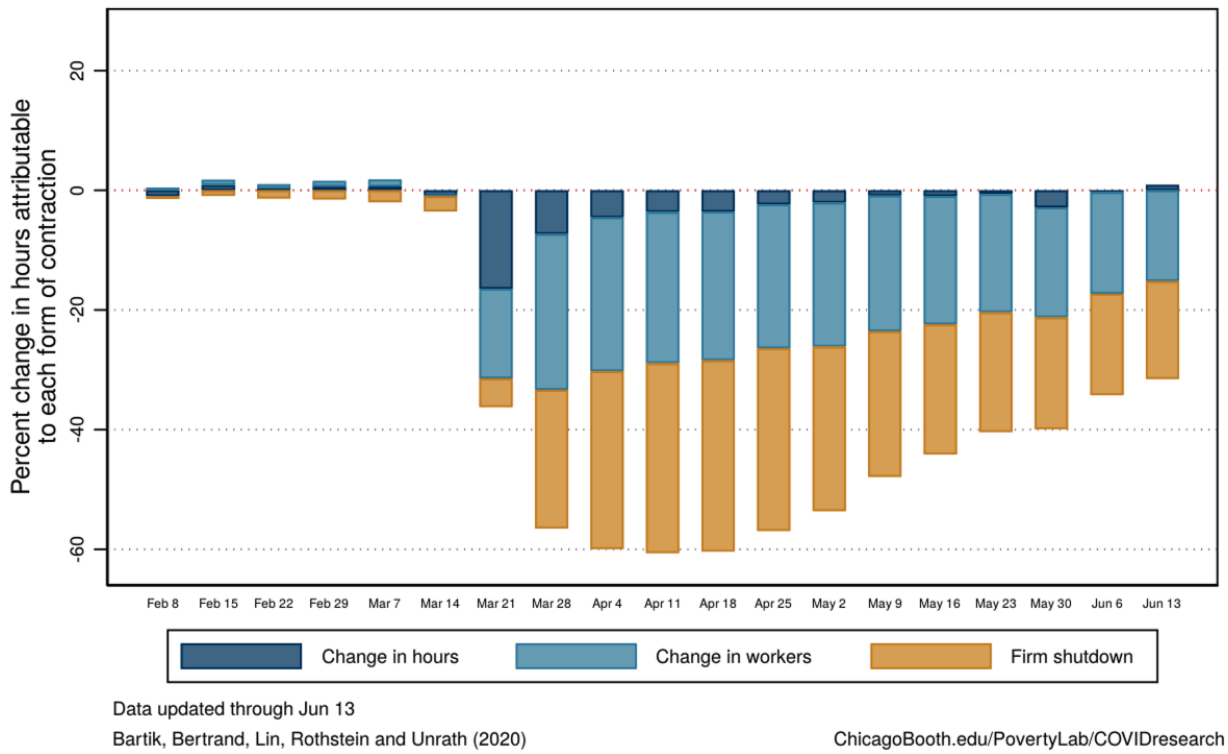
Notes: Payroll employment by industry or aggregate, scaled relative to January 2020, from the official Current Employment Statistics release. The first four panels are aggregates that include many of the remaining series. Not seasonally adjusted.

Figure 4: Employment change in Great Recession and 2020, by sector



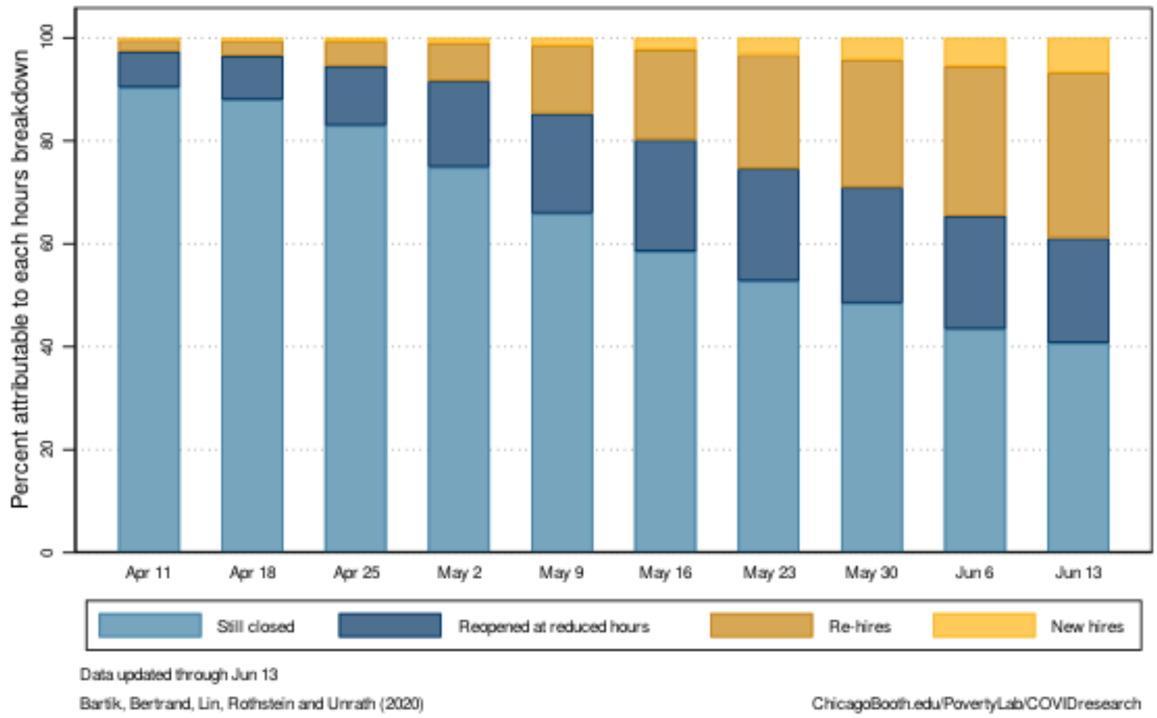
Notes: Payroll employment by industry or aggregate, from the official Current Employment Statistics release. The first four categories are aggregates that include many of the remaining series. Not seasonally adjusted.

Figure 5: Hours changes at Homebase firms each week, relative to Jan 19-Feb 1, decomposed into firm shutdowns, layoffs and hours reductions



Note: We decompose changes in total hours worked at Homebase firms that were active in Jan. 19-Feb. 2 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. We identify the contribution of firm closure 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.

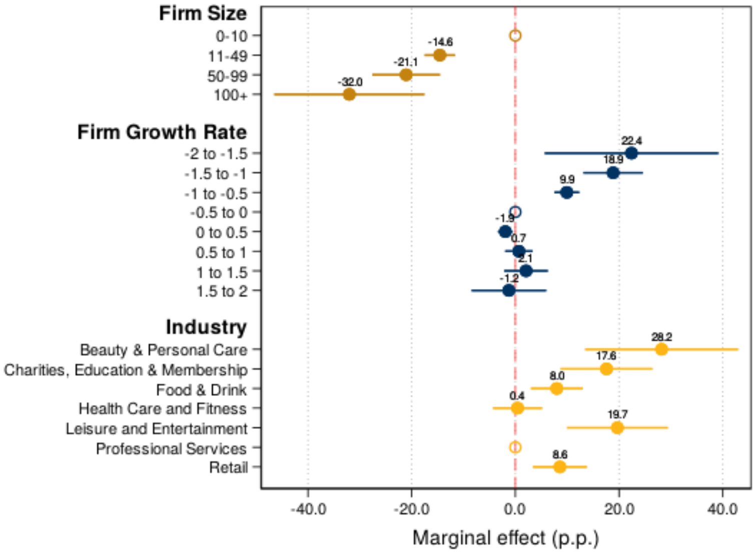
Figure 6: Distribution of hours, relative to Jan 19-Feb 1, among firms that shut down by Apr 4



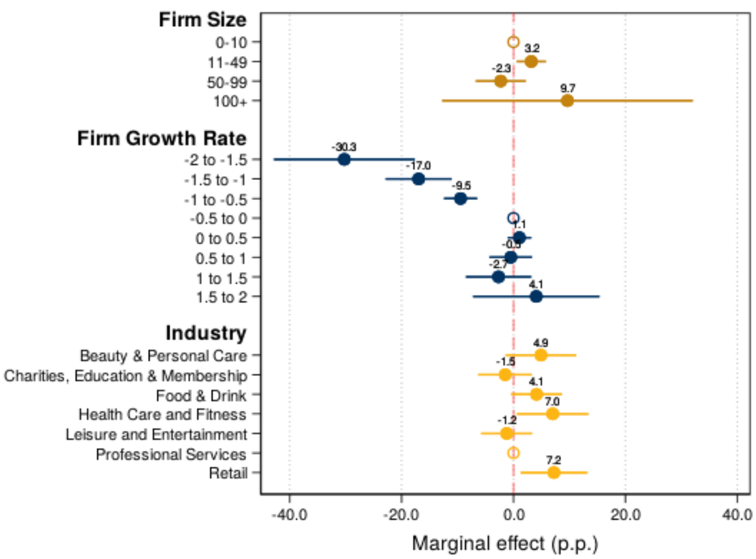
Notes: The sample consists of firms in our baseline Homebase sample that had at least one week of zero recorded hours by April 4. Across these firms, we identify the share that remain closed through each subsequent week and sum their baseline hours (light blue). Among reopened firms, we distinguish reductions 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 7: Likelihood of firm closure and reopening by firm size, growth rate and industry

A: Shutdown



B: Reopen



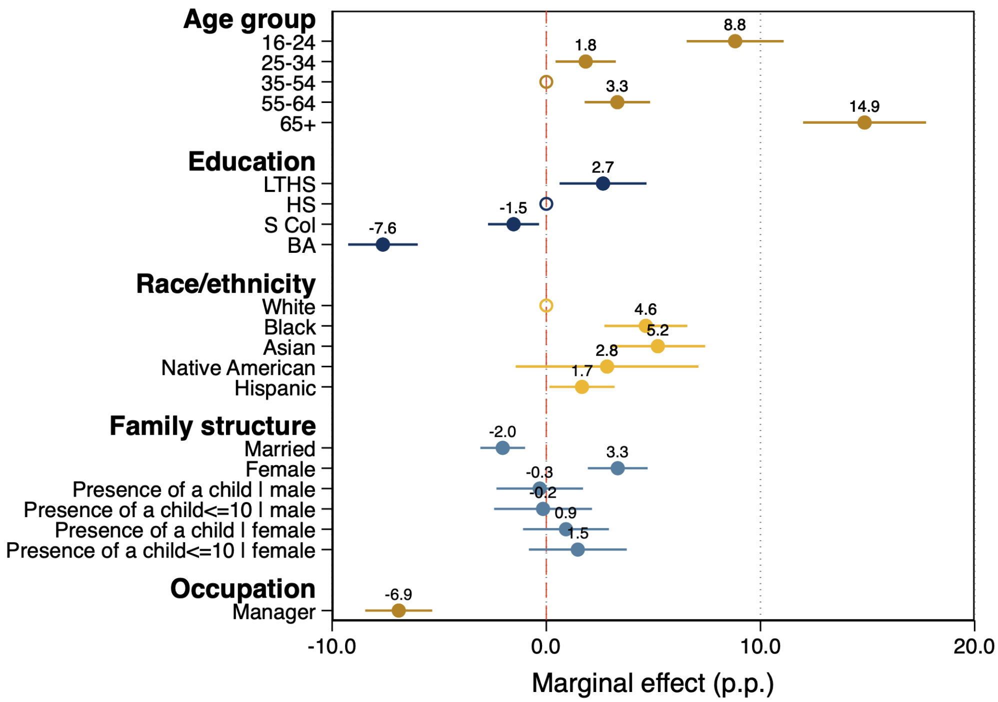
Notes: Figure reports marginal effects and confidence intervals from two logit models, with state fixed effects. In Panel A, the sample is all firms in the Homebase data, and the outcome is an indicator for the firm shutting down (recording zero hours) for at least one week between March 8 and April 4. In Panel B, the sample is firms that shut down by April 4, and the outcome is an indicator for subsequently reporting positive hours before June 13. Firm size is the number of unique employees in the base period (Jan 19-Feb 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. Four industries -- “Transportation”, “Home Repair”, “Other” and “Unknown” -- for which we have small sample sizes are not reported but are included in our analysis. Marginal effects are evaluated for a professional services firm in California with 0-10 employees in the base period and a growth rate of -.5 to 0. N=25,044 for Panel A and N=12,075 for Panel B.

Table 2: Characteristics of job-leavers and returners in the Current Population Survey

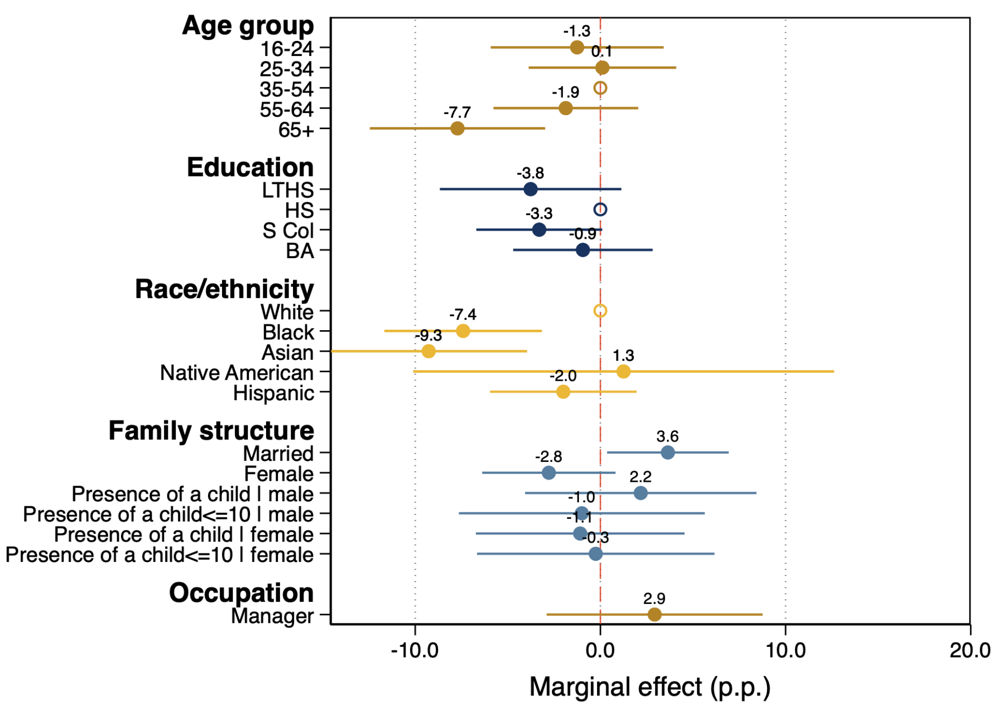
	Employed in March and April	Left work in April	Started work in May	Logit: Stopped work in April		Logit: Started work in May	
	Mean	Mean	Mean	Marg. effect	SE	Marg. effect	SE
Age							
16-25	0.11	0.23	0.24	**0.078**	(0.011)	-0.007	(0.024)
26-37	0.28	0.23	0.23	--		--	
38-49	0.27	0.20	0.20	-0.010	(0.007)	0.015	(0.022)
50-64	0.29	0.25	0.24	**0.019**	(0.008)	-0.025	(0.021)
65 and over	0.06	0.09	0.08	**0.142**	(0.015)	**-0.079**	(0.026)
Education Level :							
Less than high school	0.06	0.12	0.12	**0.027**	(0.010)	-0.038	(0.026)
High school	0.22	0.31	0.32	--		--	
Some college	0.26	0.31	0.31	**-0.015**	(0.006)	-0.034	(0.018)
BA or more	0.46	0.26	0.25	**-0.076**	(0.008)	-0.010	(0.020)
Race :							
Black	0.12	0.16	0.13	**0.046**	(0.010)	**-0.077**	(0.022)
Asian	0.07	0.08	0.06	**0.052**	(0.011)	**-0.096**	(0.028)
Native American	0.01	0.02	0.02	0.028	(0.022)	0.014	(0.059)
Hispanic	0.16	0.20	0.21	**0.016**	(0.008)	-0.021	(0.021)
Demographics							
Married	0.57	0.44	0.47	**-0.021**	(0.005)	**0.040**	(0.017)
Female	0.45	0.52	0.49	**0.033**	(0.007)	-0.028	(0.019)
Presence of a child | male	0.40	0.37	0.38	-0.005	(0.010)	0.019	(0.032)
Presence of a child under 10 | male	0.27	0.23	0.23	0.002	(0.012)	-0.013	(0.035)
Presence of a child | female	0.40	0.41	0.43	0.008	(0.010)	-0.017	(0.029)
Presence of a child under 10 | female	0.25	0.26	0.26	0.018	(0.012)	-0.001	(0.034)
Occupation							
Manager	0.14	0.07	0.07	**-0.068**	(0.008)	0.030	(0.030)
N	25,003	6,689	2,839	31,692		6,419	

Notes: The first three columns present means from matched March-April (columns 1-2) and April-May (column 3) CPS samples, based on employment status in the two months. Regressions in right columns control for 2-digit industry and state effects (not reported here). The model for leaving work in April is limited to those who were at work in March; the model for starting work in May is limited to those who were not working in April. The model includes gender-by-presence-of-children interactions; we report the marginal effects of children separately for males and females. Marginal effects are otherwise evaluated for an unmarried, childless, male, white, non-Hispanic individual age 25-54 with a high school diploma in a non-managerial occupation in the professional and technical services industry in California. Bold effects are significant at the 5% level.

Figure 8: Predictors of layoffs and rehires, Current Population Survey data
A. Layoffs

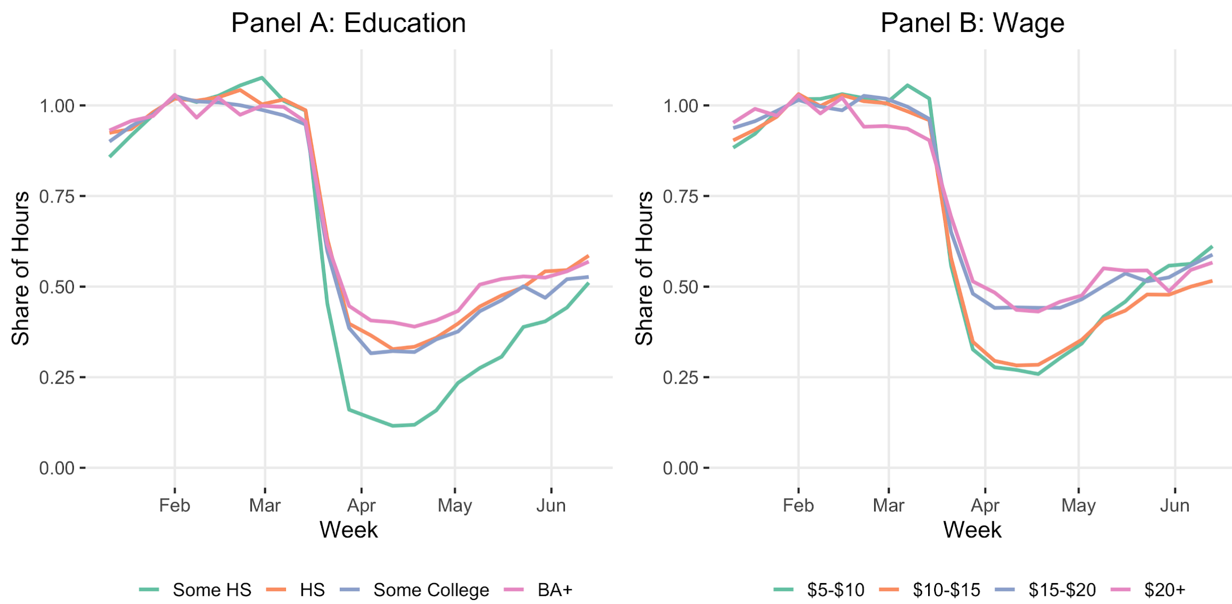


B. Rehires



Note: Figure displays marginal effects from final columns of Table 2.

Figure 9: Hours trends by demographic group, Homebase data



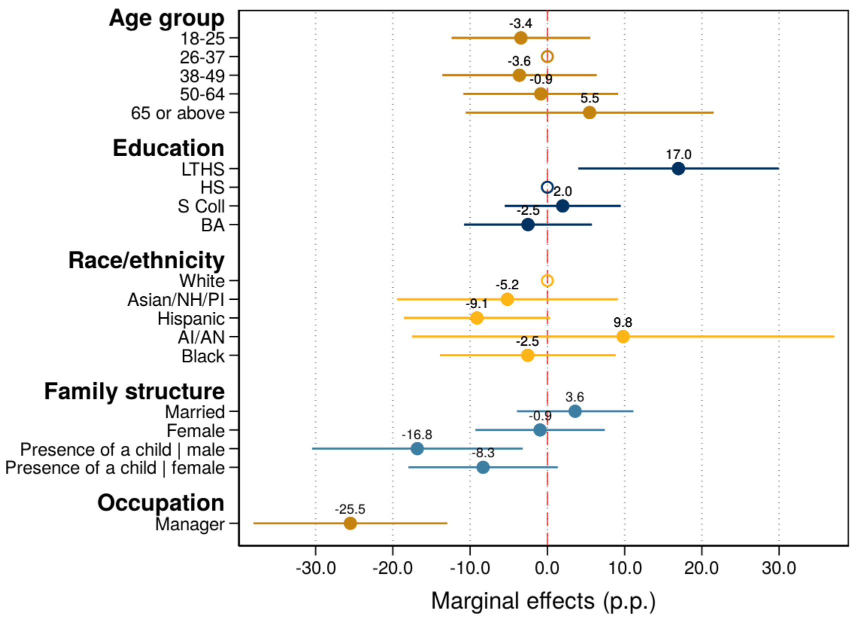
Note: Hours for workers of each group are presented as a share of hours for that group in the base period (Jan. 19-Feb. 1). Estimates are computed over Homebase workers who responded to our worker survey (N=1,523).

Table 3: Characteristics of job-leavers and returners in the Homebase data

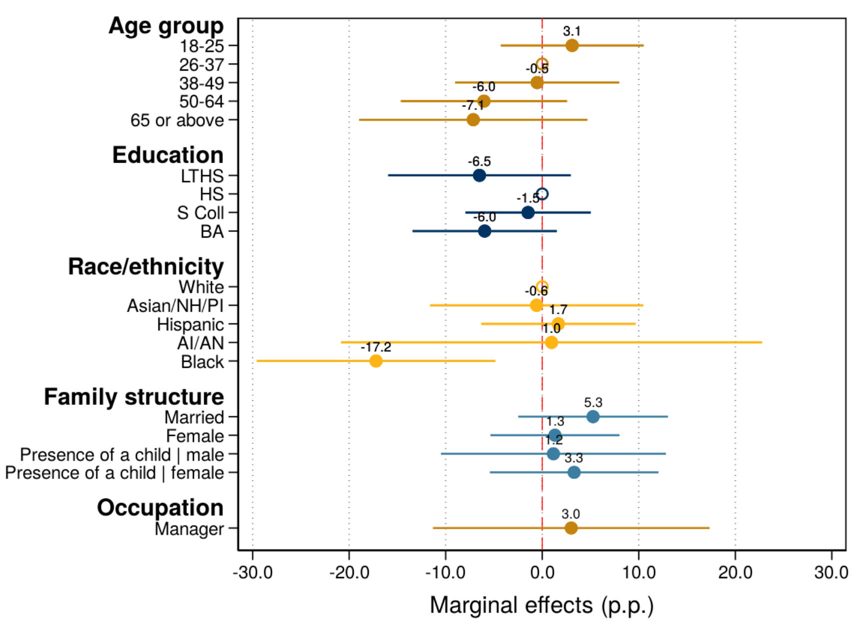
	Remain employed in March and April	Left work in March or April	Restarted work in May or June	Logit: Likelihood of being laid-off		Logit: Likelihood of being rehired	
	Mean	Mean	Mean	Marg effect	SE	Marg effect	SE
Age:							
18-25	0.32	0.39	0.40	-0.034	(0.046)	0.031	(0.038)
26-37	0.25	0.27	0.26	--		--	
38-49	0.20	0.15	0.15	-0.036	(0.051)	-0.005	(0.043)
50-64	0.19	0.14	0.14	-0.009	(0.051)	-0.060	(0.044)
65+	0.04	0.05	0.05	0.055	(0.082)	-0.071	(0.060)
Education level:							
Less than high school	0.03	0.08	0.07	**0.170**	(0.066)	-0.065	(0.048)
High school	0.30	0.28	0.30	--		--	
Some coll	0.32	0.34	0.35	0.020	(0.038)	-0.015	(0.033)
BA or more	0.34	0.30	0.27	-0.025	(0.042)	-0.060	(0.038)
Race:							
White	0.66	0.68	0.73	--		--	
Black	0.10	0.08	0.05	-0.025	(0.058)	**-0.172**	(0.063)
Asian/NH/PI	0.06	0.07	0.06	-0.052	(0.073)	-0.006	(0.056)
AI/AN	0.17	0.15	0.15	0.098	(0.139)	0.010	(0.111)
Hispanic	0.01	0.01	0.01	-0.091	(0.048)	0.017	(0.041)
Demographics:							
Married	0.29	0.26	0.29	0.036	(0.038)	0.053	(0.040)
Female	0.68	0.66	0.71	-0.009	(0.043)	0.013	(0.034)
Presence of a child | male	0.12	0.08	0.06	**-0.168**	(0.070)	0.012	(0.059)
Presence of a child | female	0.27	0.20	0.23	-0.083	(0.049)	0.033	(0.045)
Occupation:							
Manager	0.12	0.06	0.08	**-0.255**	(0.064)	0.030	(0.073)
N	362	1,161	518	1,469		1,103	

Notes: The first three columns summarize the demographic composition of three groups of Homebase workers: Those who were employed all weeks in March and April (column 1), those who reported zero hours for at least one week between mid-March and late April (column 2), and the subset of those laid off workers who subsequently recorded positive hours by June 13. The next four columns report marginal effects and standard errors for logit models with industry and state fixed effects (not reported here). The layoff model is estimated on the full sample and the outcome is an indicator for at least one week with zero hours; the rehire model uses only those who were previously laid off and the outcome is an indicator for subsequent positive hours. Each model includes a gender-by-presence-of-children interaction; we report the marginal effect of children separately for males and females. Marginal effects are otherwise evaluated for an unmarried, childless, male, white, non-Hispanic individual age 26-37 with a high school diploma in a non-managerial occupation in the professional and technical services industry in California. Bold effects are significant at the 5% level.

Figure 10: Predictors of layoffs and rehires, Homebase data
Panel A: Layoffs



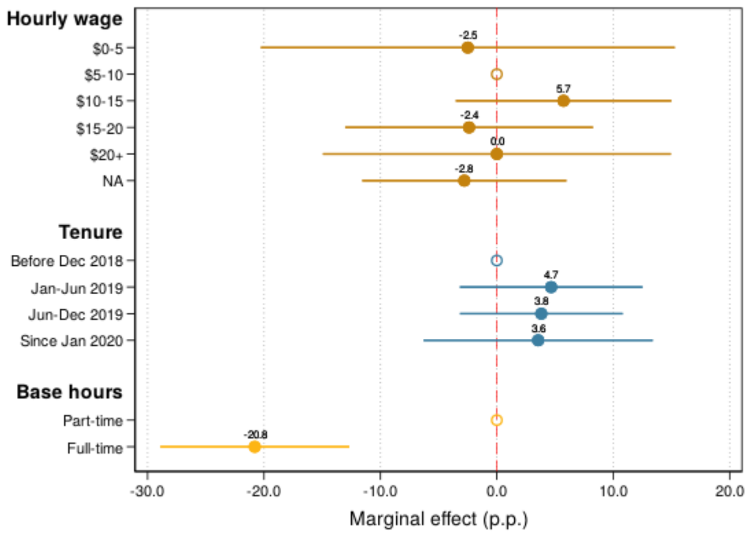
Panel B: Rehires



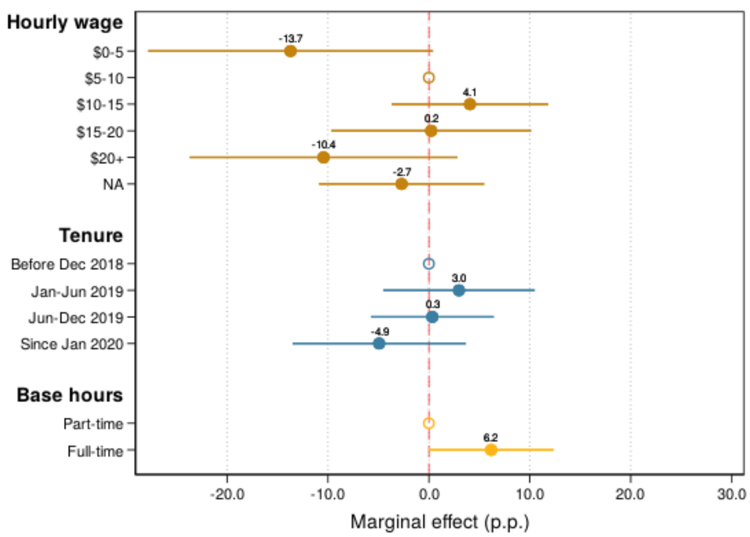
Note: Figure displays marginal effects from models reported in the rightmost columns of Table 3.

Figure 11: Likelihood of being laid-off and rehired by workers' wage, tenure and work status observed in Homebase records

A. Layoffs:

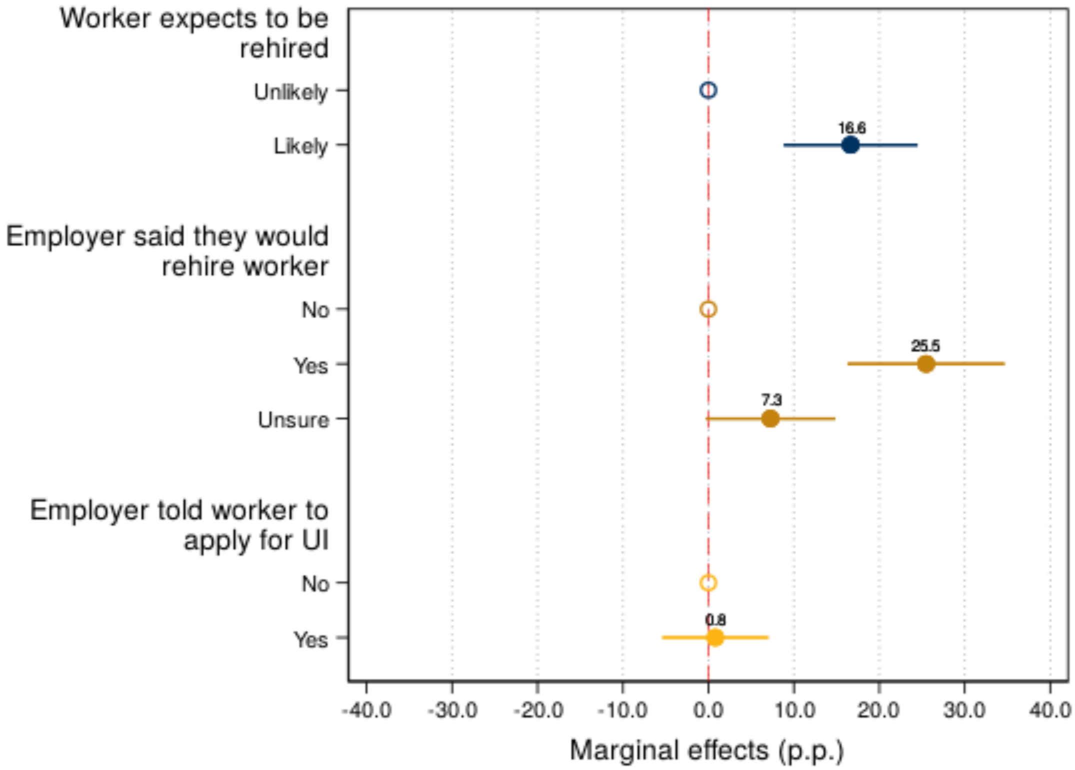


B. Rehires



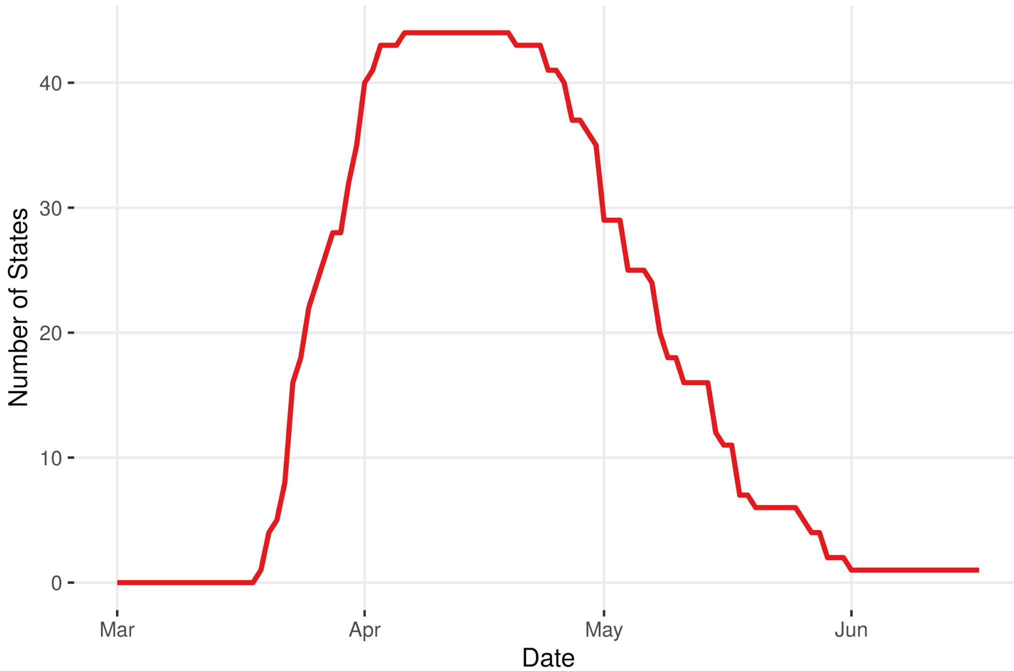
Notes: Figures show marginal effects of worker characteristics on the likelihood of being laid off in March or April (Panel A) and the conditional likelihood, given layoff, of being rehired before June (Panel B), from logit models with state and industry fixed effects. Models include and all demographic characteristics from Table 3. Full time workers are those who worked more than 40 hours (20 hours per week) in the base period. Marginal effects are evaluated for an unmarried, childless, male, white, non-Hispanic individual age 26-37 with a high school diploma in a non-managerial occupation in the professional and technical services industry, who reports earnings between \$0-8 an hour, worked part-time in the base period, and started his job before May 2018. N=1,483 in Panel A and 1,104 in Panel B.

Figure 12: Likelihood of being rehired, by worker expectations



Notes: Marginal effects from logit models for the likelihood of being rehired before June, estimated on a sample of laid off workers. We exclude from the sample workers who had already been rehired before responding to our survey. Separate models are estimated for each of the indicated characteristics, controlling for state and industry fixed effects. Marginal effects are evaluated for an individual who works in the professional services industry in California. N=576.

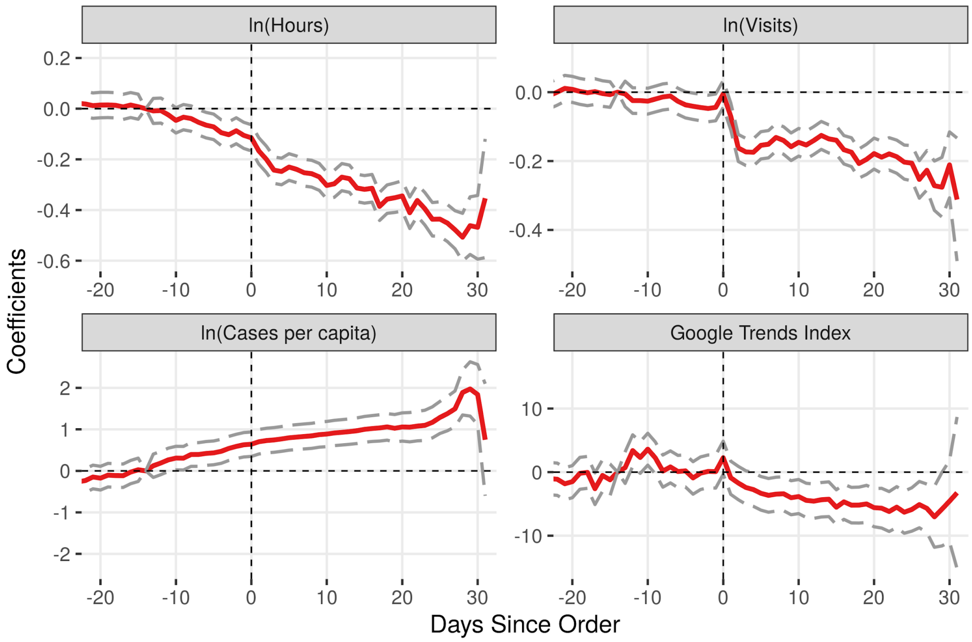
Figure 13: Timing of shelter-in-place and stay-at-home orders



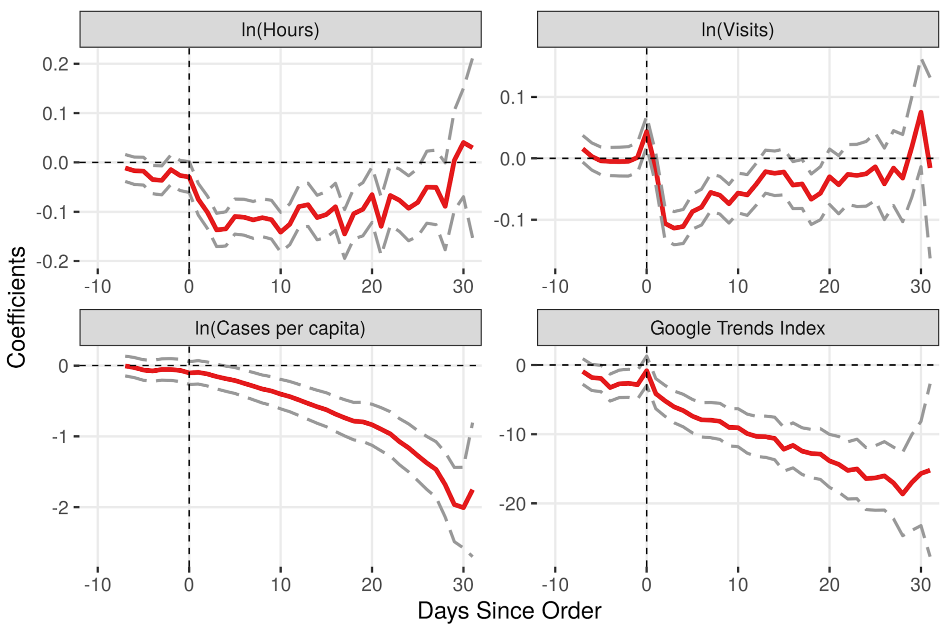
This plot shows the number of states with active shelter-in-place or stay-at-home orders between March 1st and mid-June 2020.

Figure 14: Event study estimates of the effect of shelter-in-place orders, with 95% confidence intervals

A. Basic event study



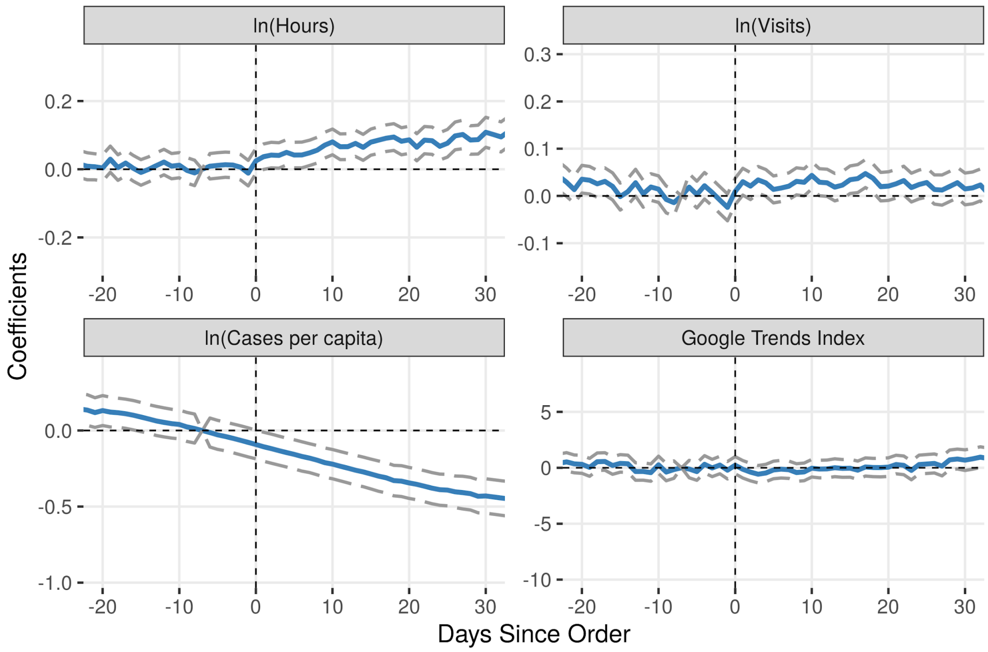
B. With state-specific trends



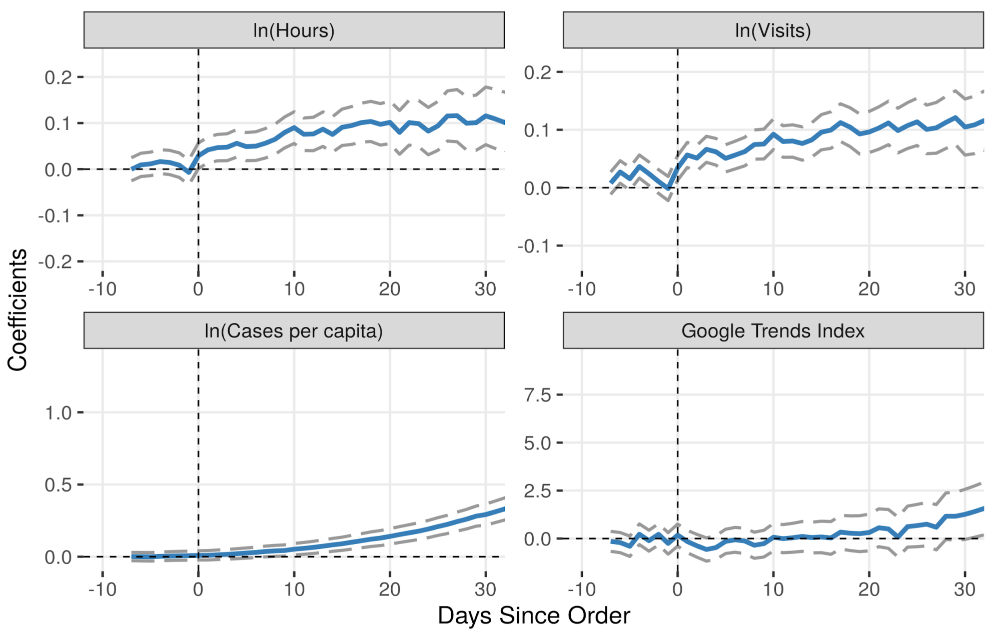
Notes: Samples consist of state-by-day observations from 2/16-4/19. Specifications include full sets of state and calendar date effects. Panel A includes all estimable event time effects, with event time -14 normalized to 0. In Panel B, we include state-specific trends, and exclude effects for event time less than -7.

Figure 15: Event study estimates of the effect of lifting of shelter-in-place orders, with 95% confidence intervals

A. Basic event study

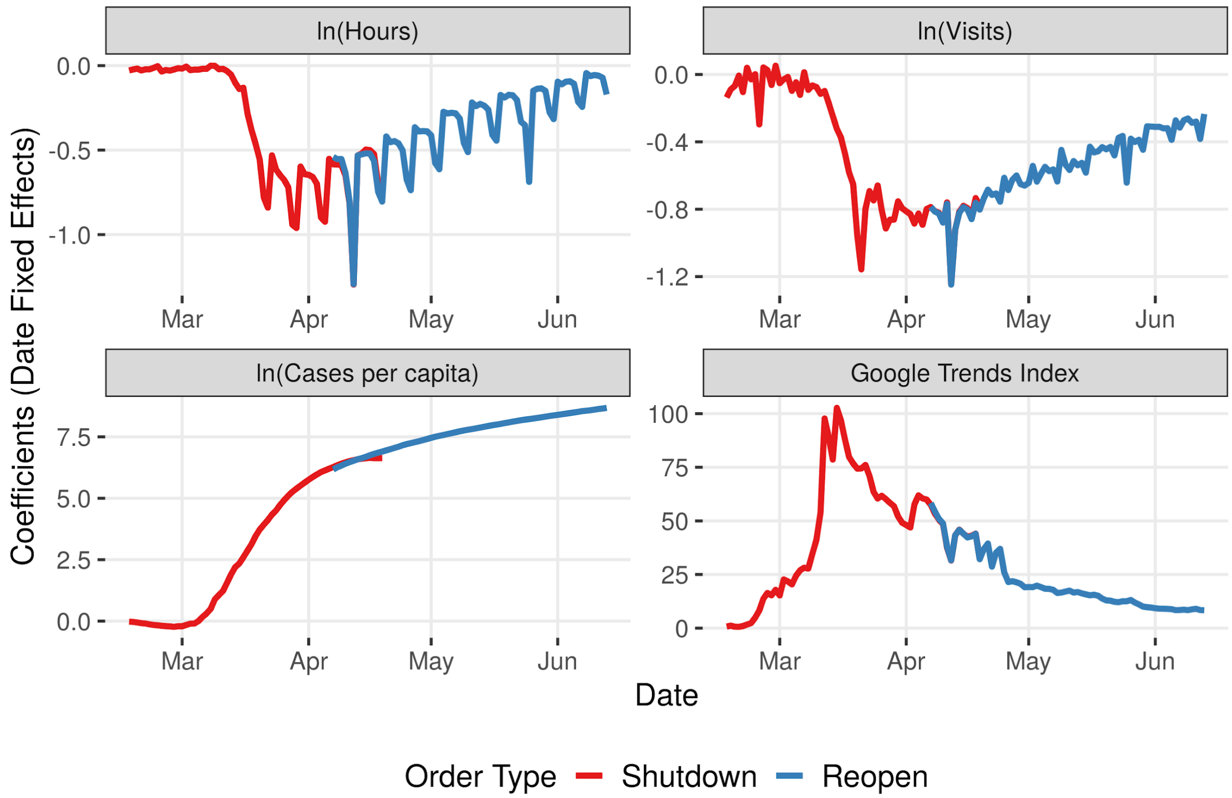


B. With state-specific trends



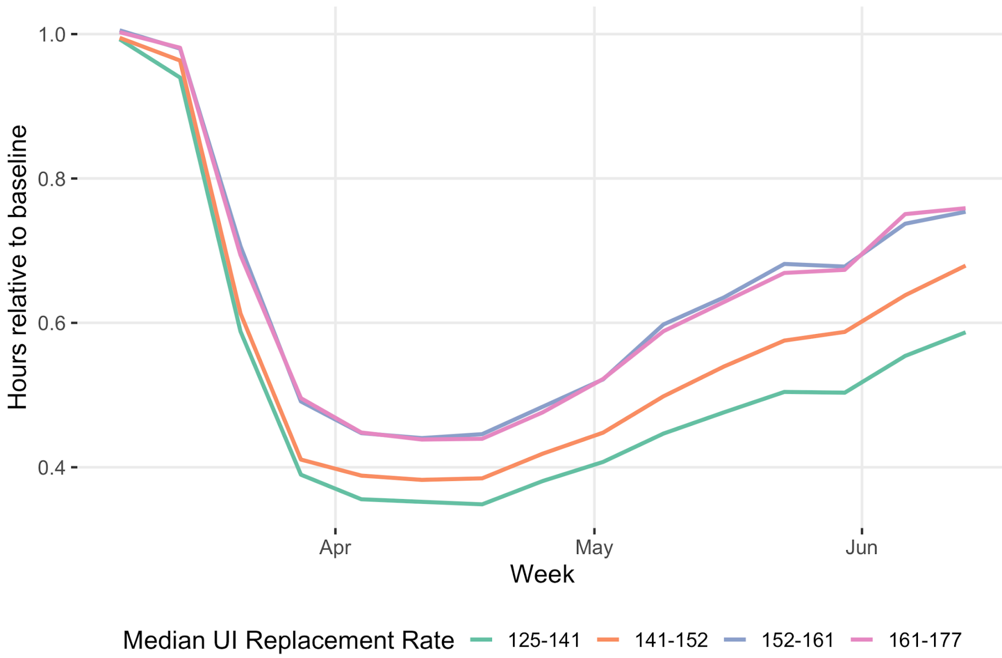
Notes: Samples consist of state-by-day observations from 4/6-6/13; states that never had shelter-in-place orders are excluded. Specifications include full sets of state and calendar date effects. Panel A includes all estimable event time effects, with event time -7 normalized to 0. In Panel B, we include state-specific trends, and exclude effects for event time less than -7.

Figure 16: Calendar time effects from event study models



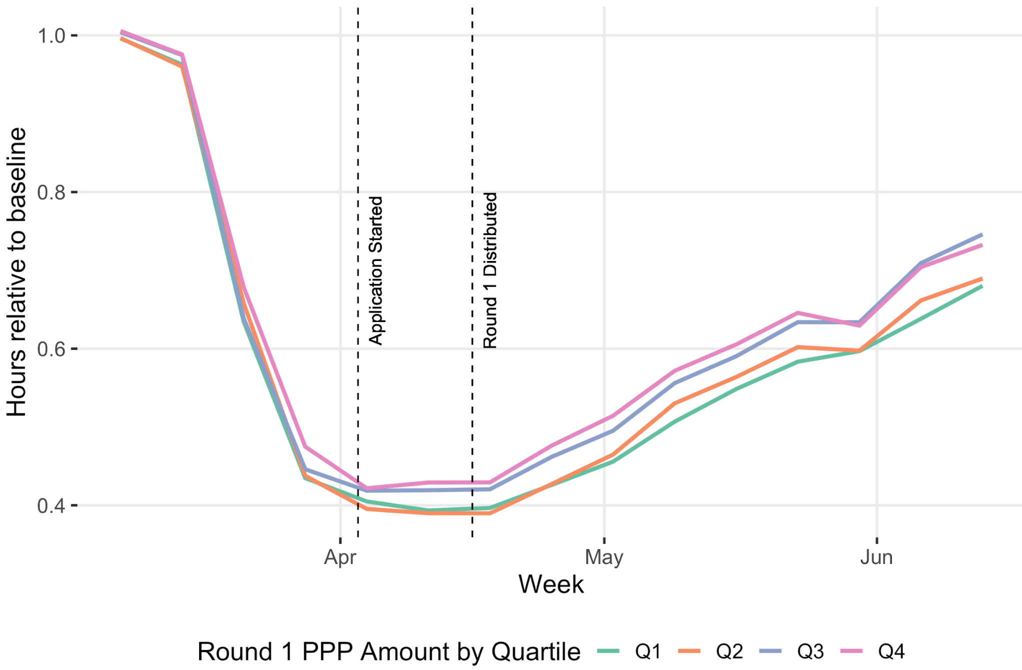
Notes: Figures show calendar time effects from the shutdown and reopening event studies in Figures 14A and 15A. The shutdown calendar time effects are normalized to zero on February 16. The reopening effects are normalized to align with the shutdown estimates on April 13.

Figure 17: Hours trends by median UI benefit replacement rate, Homebase data



Notes: UI replacement rates, expressed as percentages of weekly earnings, are from Ganong, Noel, and Vavra (2020), Figure 5, and include CARES Act supplements to benefits.

Figure 18: Hours trends by round 1 PPP amount, Homebase data



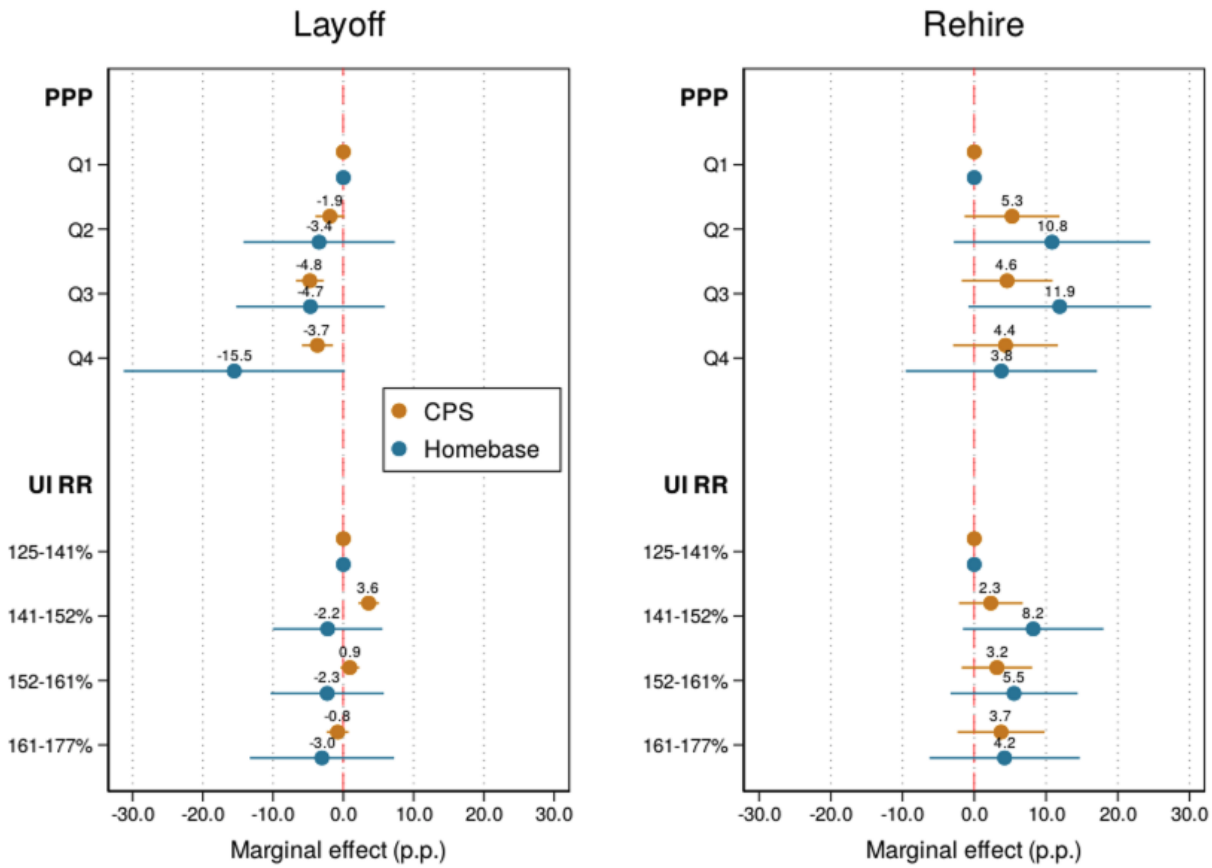
Note: States are ranked by the amount of PPP received by April 16 divided by non-farm payroll in April 2019. The first quartile has the smallest amount.

Table 4: Variation in layoff and rehire probabilities with PPP payouts and UI replacement rates

	Logit: Stopped work in April				Logit: Rehired in May			
	CPS	CPS	Homebase	Homebase	CPS	CPS	Homebase	Homebase
PPP volumes								
Quartile 2	**0.016**	-0.019	-0.011	-0.034	**0.050**	0.053	**0.096**	0.108
	(0.006)	(0.011)	(0.042)	(0.055)	(0.020)	(0.034)	(0.042)	(0.070)
Quartile 3	**-0.014**	**-0.048**	-0.007	-0.047	0.034	0.046	**0.089**	0.119
	(0.006)	(0.010)	(0.042)	(0.054)	(0.020)	(0.032)	(0.041)	(0.065)
Quartile 4	**-0.023**	**-0.037**	**-0.126**	-0.155	0.037	0.044	-0.008	0.038
	(0.007)	(0.011)	(0.060)	(0.080)	(0.025)	(0.037)	(0.059)	(0.068)
UI replacement rates								
Quartile 2	**0.026**	**0.036**	-0.039	-0.022	**0.042**	0.023	**0.104**	0.082
	(0.007)	(0.007)	(0.044)	(0.040)	(0.020)	(0.023)	(0.042)	(0.050)
Quartile 3	**-0.013**	0.009	**-0.104**	-0.023	**0.075**	0.032	**0.137**	0.055
	(0.006)	(0.007)	(0.044)	(0.041)	(0.022)	(0.025)	(0.045)	(0.045)
Quartile 4	**-0.027**	-0.008	**-0.113**	-0.030	**0.081**	0.037	**0.156**	0.042
	(0.007)	(0.008)	(0.055)	(0.052)	(0.025)	(0.031)	(0.059)	(0.053)
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 and specifications are identical to those in Tables 2 and 3, respectively, 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.

Figure 19: illustration of PPP and UI replacement rate coefficients from Table 4



Notes: Figures plot estimates reported in Table 4 (columns 2, 4, 6 and 8) -- the marginal effects of PPP funds and median UI replacement rates at the state level, controlling for worker-level demographic characteristics and fixed effects for industry and Census division, using the CPS and Homebase data.

Figure 20: Initiation of Pandemic Unemployment Assistance (PUA) and Federal Pandemic Unemployment Compensation (FPUC) payments

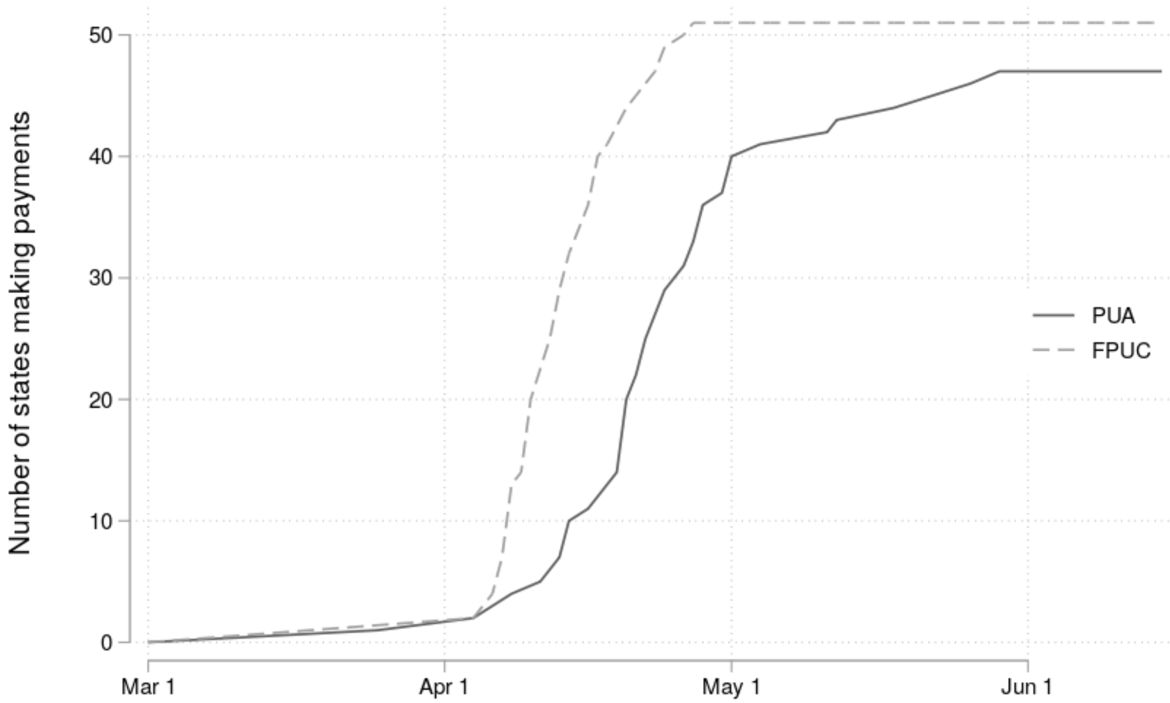
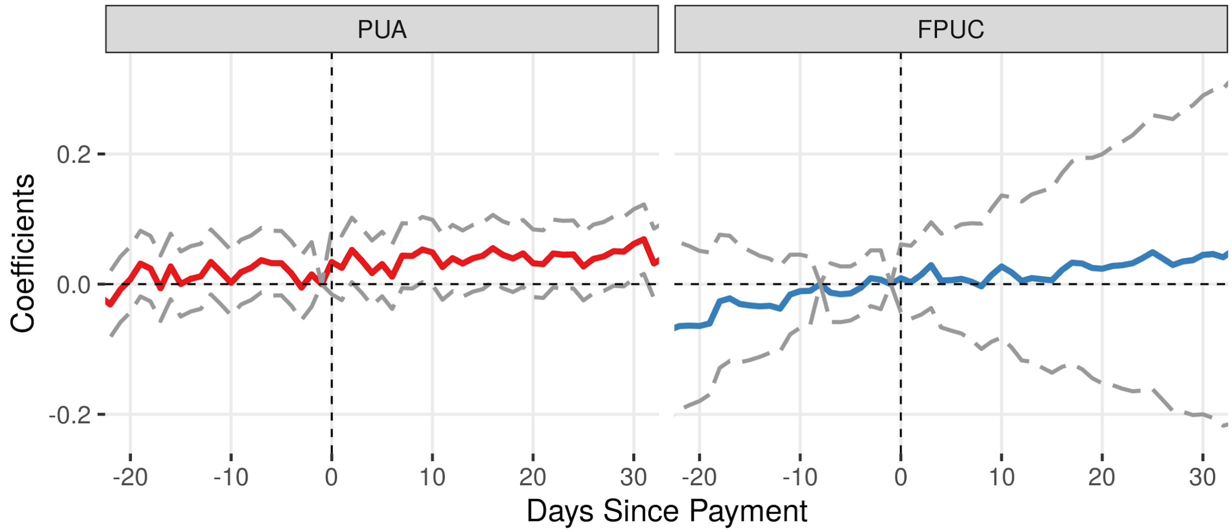


Figure 21: Event-study estimates of effects of PUA and FPUC payment starts on hours worked

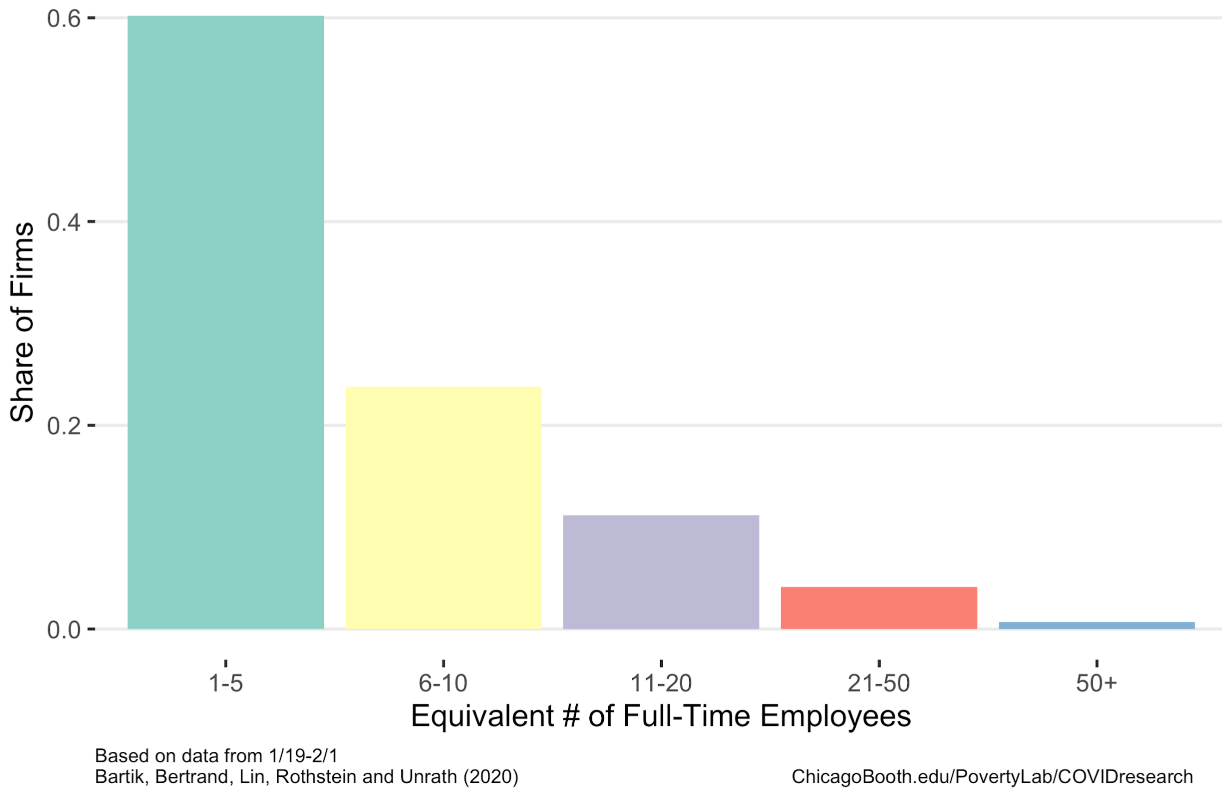


Note: Panel PUA (respectively, FPUC) shows estimates from an event study models where the event is the beginning of Pandemic Unemployment Assistance (respectively, Federal Pandemic Unemployment Compensation) payments in the state. The sample is state-by-day observations from 2/16-6/6. Each specification 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.

Appendices

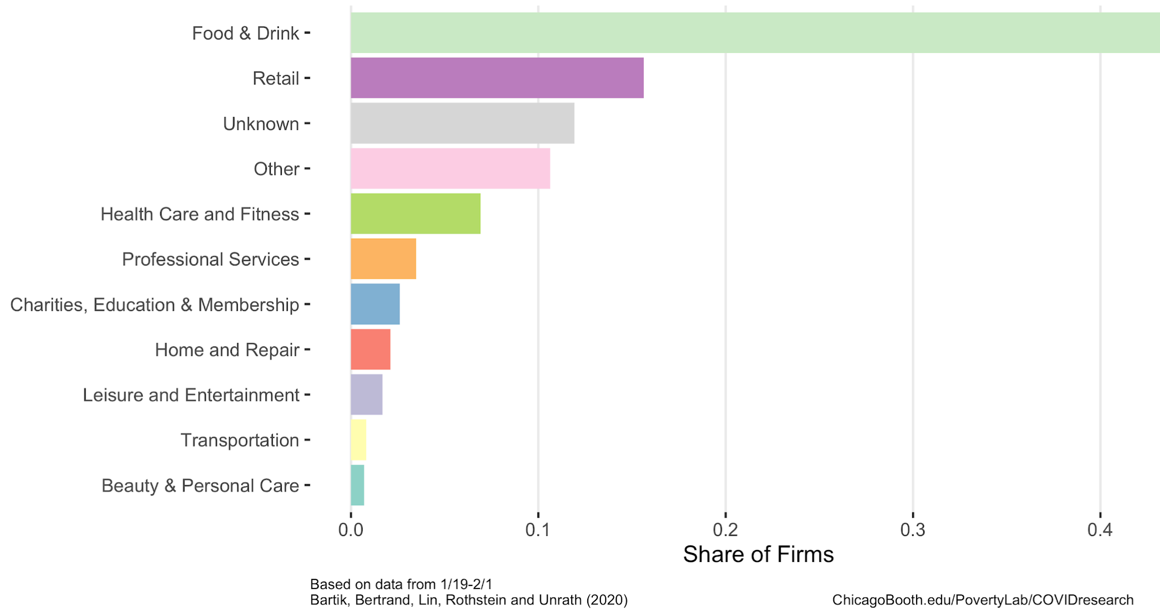
Appendix A. Representativeness of Homebase data

Figure A1: Firm Size Distribution of Homebase Firms



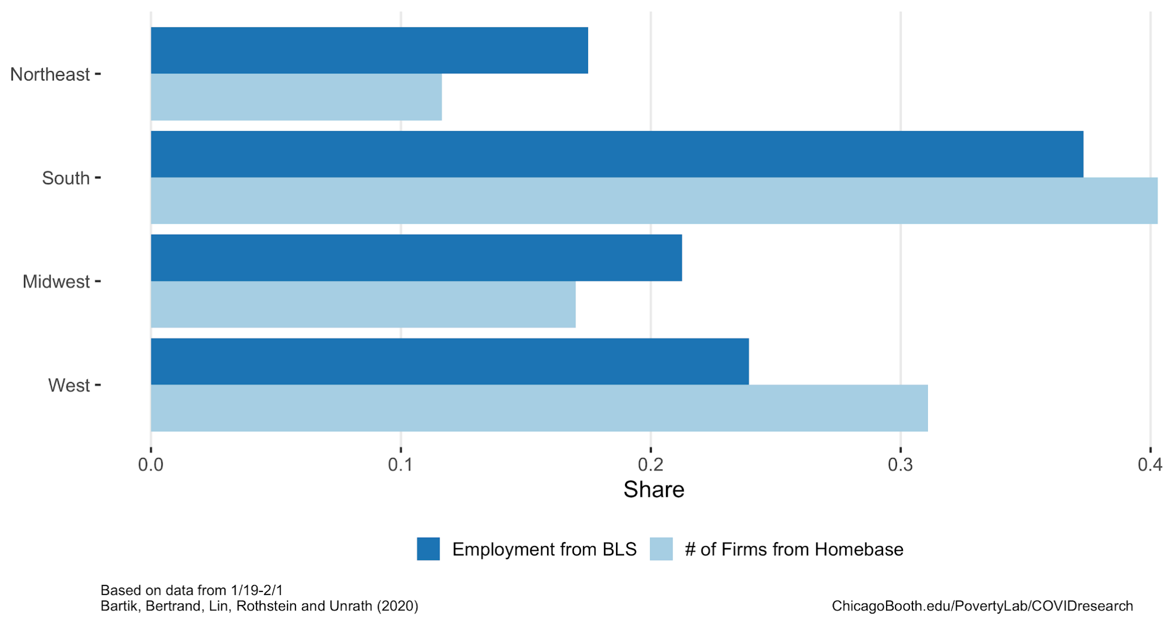
Notes: The full-time equivalent firm size is calculated by dividing total hours worked at the firm in the two-week base period by 80.

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: Demographics of Matched Homebase Survey Respondents

Demographics	Share	Demographics	Share
Gender		**Education**	
Female	67.0%	Some high school	6.9%
Male	30.9%	High school graduate	28.6%
Non-binary	2.2%	Two-year degree/some college	33.5%
Age		Bachelor's degree	23.8%
18-25	37.1%	Master's degree or more	7.2%
26-37	26.7%	**Household Income**	
38-49	16.2%	Less than $15,000	21.1%
50-64	15.4%	$15,000-$24,999	21.3%
65 or above	4.7%	$25,000-$34,999	15.0%
Race		$35,000-$44,999	8.4%
White	68.0%	$45,000-$54,999	7.4%
Black	8.8%	$55,000-$64,999	6.2%
Hispanic	15.8%	$65,000-$74,999	5.0%
Asian	5.9%	$75,000-$84,999	2.8%
Native American	0.9%	More than $85,000	12.8%
Pacific Islander	0.5%	**Wage in Jan 2020**	
Marital Status		$5-$7.49	7.3%
Single	55.2%	$7.50-$9.99	15.3%
Married	26.7%	$10-$12.49	27.0%
Living with partner	9.6%	$12.50-$14.99	20.3%
Separated	1.4%	$15-$17.49	15.5%
Divorced	5.8%	$17.50-$19.99	4.9%
Widowed	1.3%	$20 -$22.49	4.0%
Having Children		$22.50-$24.99	1.8%
Yes	30.9%	$25 or higher	4.0%
No	69.1%	**Job Title**	
Number of Children under 18		Employee	94.7%
1	48.7%	Manager	4.0%
2	30.1%	General Manager	1.2%
3	14.4%		
4	5.1%		
More than 4	1.6%		
N	1523	N	1523

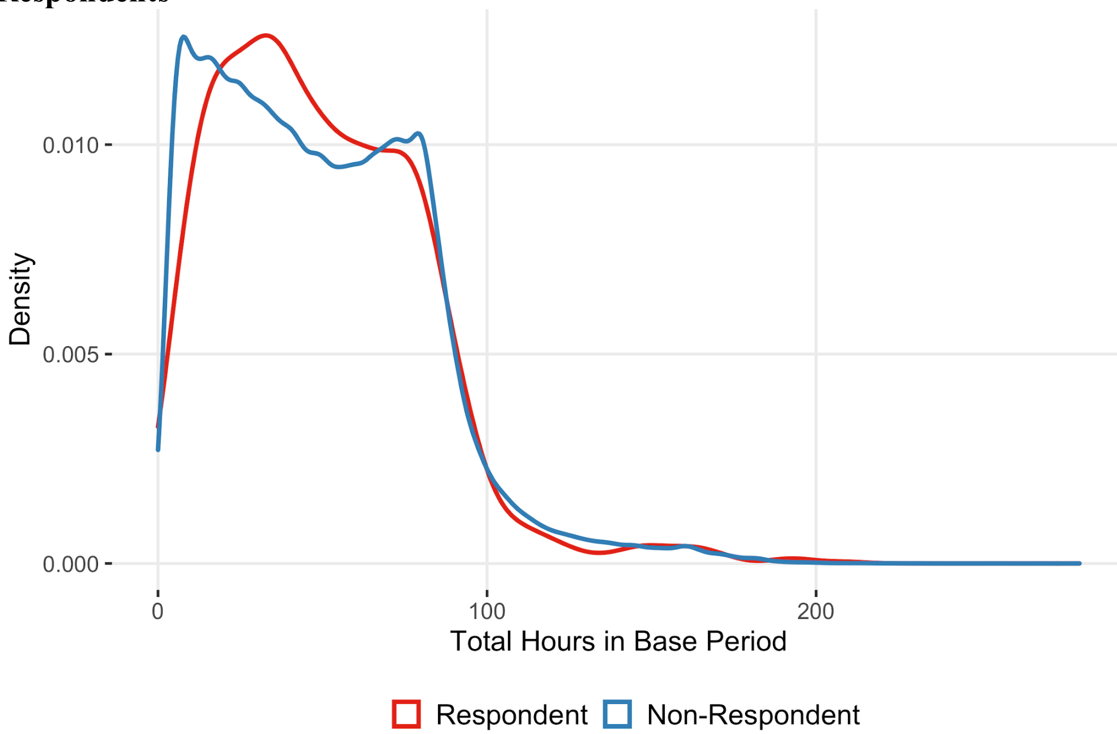
Note: The table reports the demographics of survey respondents who 1) are active workers in our base period and associated with firms in our sample and 2) have worked for only one firm since January 19, 2020.

Table B2: Distribution of Survey Respondents and Non-Respondents

Characteristic	Respondent	Non-Respondent
Industry		
Beauty & Personal Care	0.3%	0.4%
Charities, Education & Membership	3.9%	3.5%
Food & Drink	47%	51%
Health Care and Fitness	5.3%	5.2%
Home and Repair	0.5%	1.3%
Leisure and Entertainment	3.4%	2.0%
Professional Services	3.2%	2.5%
Retail	11%	11%
Transportation	0.8%	0.8%
Other	12%	11%
Unknown	12%	11%
Census Division		
New England	3.0%	3.0%
Middle Atlantic	11%	9.1%
South Atlantic	19%	21%
East South Central	4.1%	4.6%
West South Central	8.3%	12%
East North Central	13%	11%
West North Central	7.5%	6.2%
Mountain	9.2%	9.2%
Pacific	25%	23%
Firm Size		
$[1, 5]$	29%	31%
$(5, 10]$	26%	26%
$(10, 20]$	20%	22%
$(20, 50]$	18%	15%
$(50, Inf]$	7.6%	6.2%
N	1523	428095

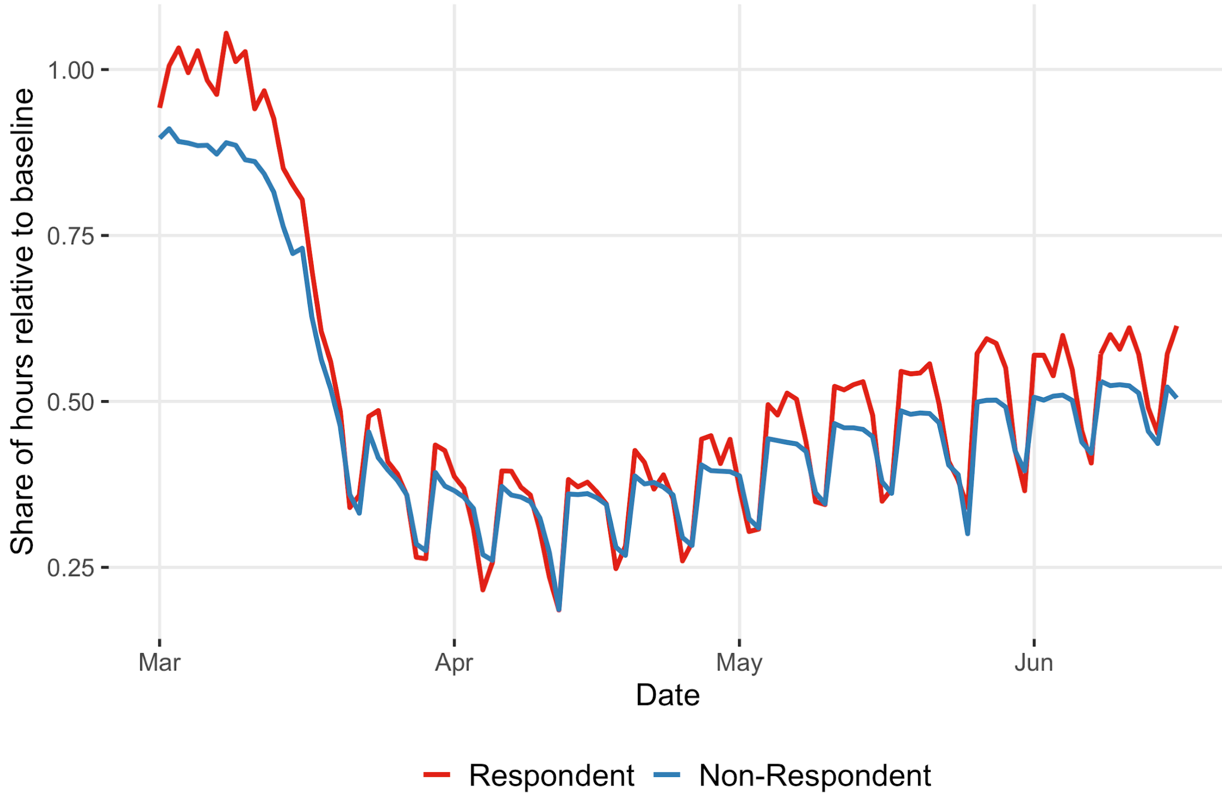
Note: The full sample is all workers who 1) worked at firms in our sample in our late January base period and 2) have worked for only one firm since January 19, 2020. Respondents are the subset of workers who responded to our survey.

Figure 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.

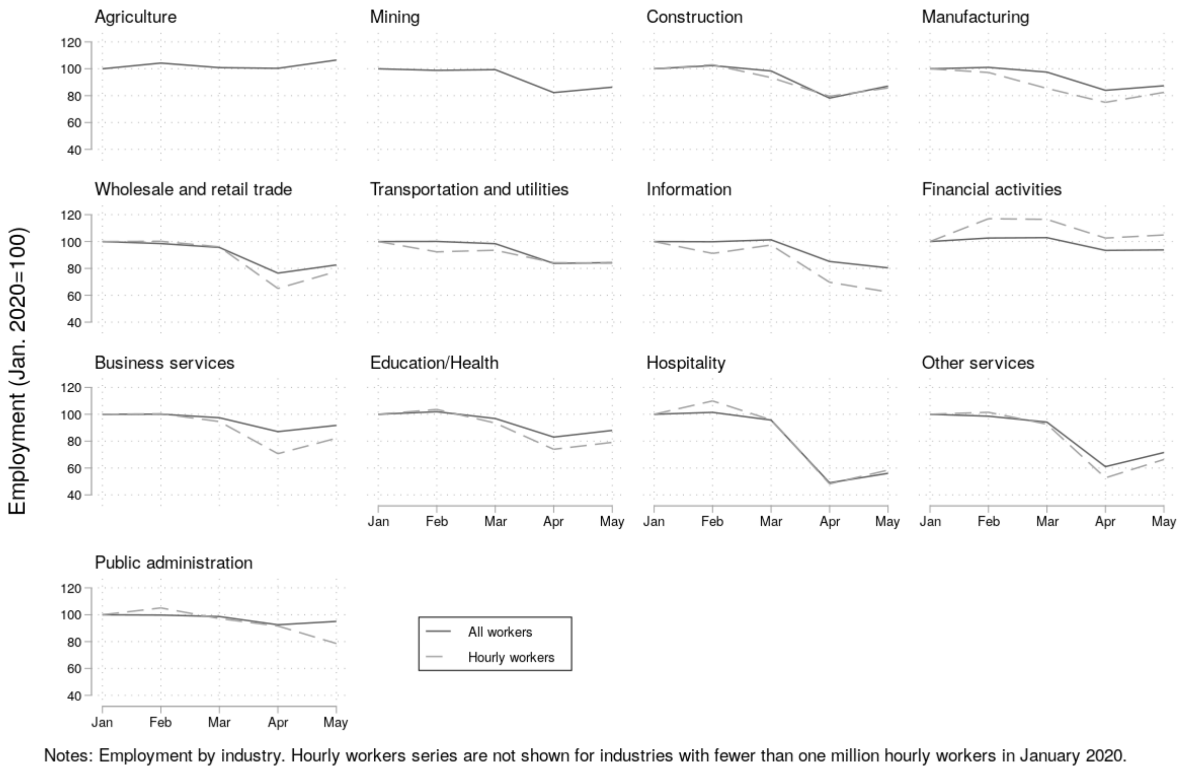
Appendix C. Additional results

Table C1: Characteristics of job-leavers and returners in the retail trade and accommodation and food services sectors in the Current Population Survey

	Employed in March and April	Left work in April	Started work in May	Logit: Stopped work in April		Logit: Started work in May	
	Mean	Mean	Mean	Marg. effect	SE	Marg. effect	SE
Age							
16-25	0.26	0.37	0.33	**0.055**	(0.027)	-0.020	(0.033)
26-37	0.26	0.22	0.25	--		--	
38-49	0.21	0.14	0.17	-0.048	(0.028)	0.000	(0.038)
50-64	0.22	0.19	0.18	0.032	(0.028)	-0.035	(0.033)
65 and over	0.05	0.07	0.07	**0.200**	(0.036)	-0.030	(0.043)
Education Level :							
Less than high school	0.12	0.20	0.13	**0.078**	(0.029)	-0.048	(0.032)
High school	0.33	0.32	0.39	--		--	
Some college	0.34	0.31	0.31	-0.004	(0.022)	-0.021	(0.026)
BA or more	0.21	0.17	0.17	0.001	(0.026)	-0.043	(0.030)
Race :							
Black	0.13	0.14	0.13	0.014	(0.031)	**-0.075**	(0.032)
Asian	0.07	0.10	0.06	**0.107**	(0.034)	-0.061	(0.039)
Native American	0.02	0.02	0.03	0.041	(0.065)	0.125	(0.118)
Hispanic	0.20	0.22	0.21	0.006	(0.025)	-0.023	(0.030)
Demographics							
Married	0.41	0.31	0.35	**-0.056**	(0.020)	0.033	(0.030)
Female	0.47	0.53	0.52	**0.049**	(0.022)	-0.044	(0.027)
Presence of a child | male	0.37	0.36	0.33	-0.046	(0.036)	0.095	(0.065)
Presence of a child under 10 | male	0.21	0.22	0.17	0.078	(0.045)	**-0.099**	(0.040)
Presence of a child | female	0.42	0.43	0.46	0.005	(0.035)	0.111	(0.059)
Presence of a child under 10 | female	0.25	0.25	0.25	0.011	(0.039)	**-0.087**	(0.036)
Occupation							
Manager	0.110	0.053	0.054	**-0.197**	(0.031)	0.052	(0.058)
N	25,003	6,689	2,839	5,155		1,693	

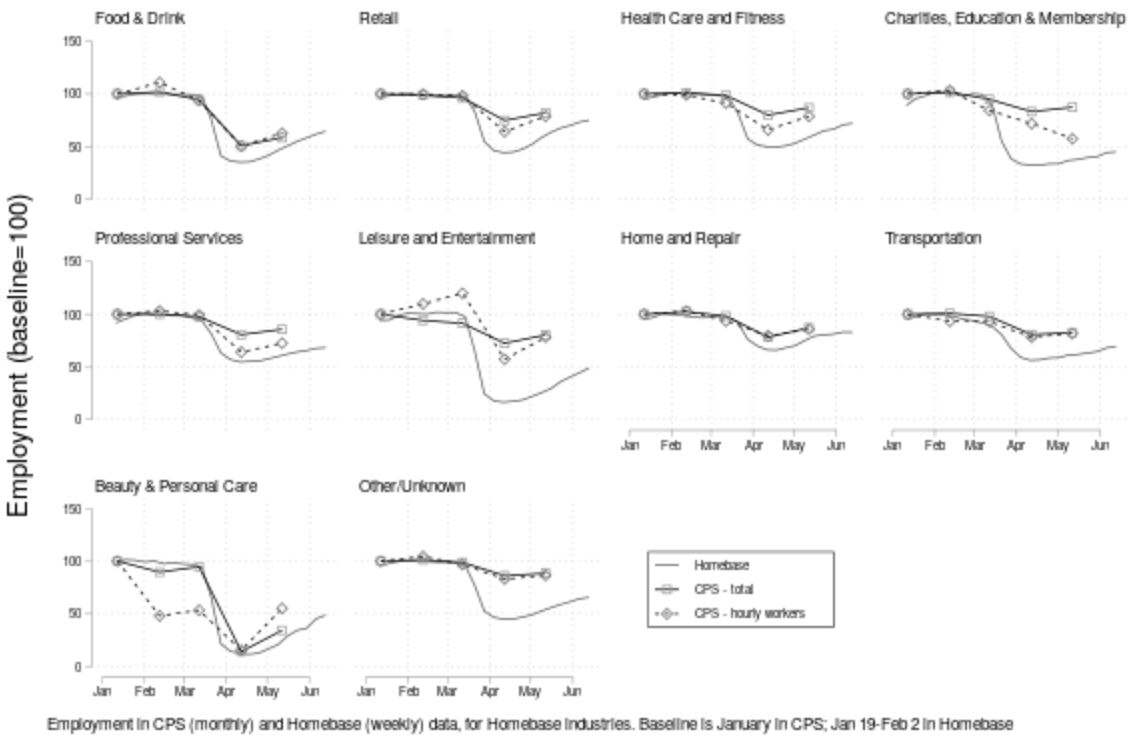
Notes: The first three columns present means from matched March-April (columns 1-2) and April-May (column 3) CPS samples, based on employment status in the two months and reported industry in the first month. Regressions in right columns control for 2-digit industry and state effects (not reported here). The model for leaving work in April is limited to those who were at work in March; the model for starting work in May is limited to those who were not working in April. The model includes gender-by-presence-of-children interactions; we report the marginal effect of children separately for males and females. Marginal effects are otherwise evaluated for an unmarried, childless, male, white, non-Hispanic individual age 25-54 with a high school diploma in a non-managerial occupation in the retail trade industry in California. Bold effects are significant at the 5% level.

Figure C1: CPS employment by sector and month, 2020



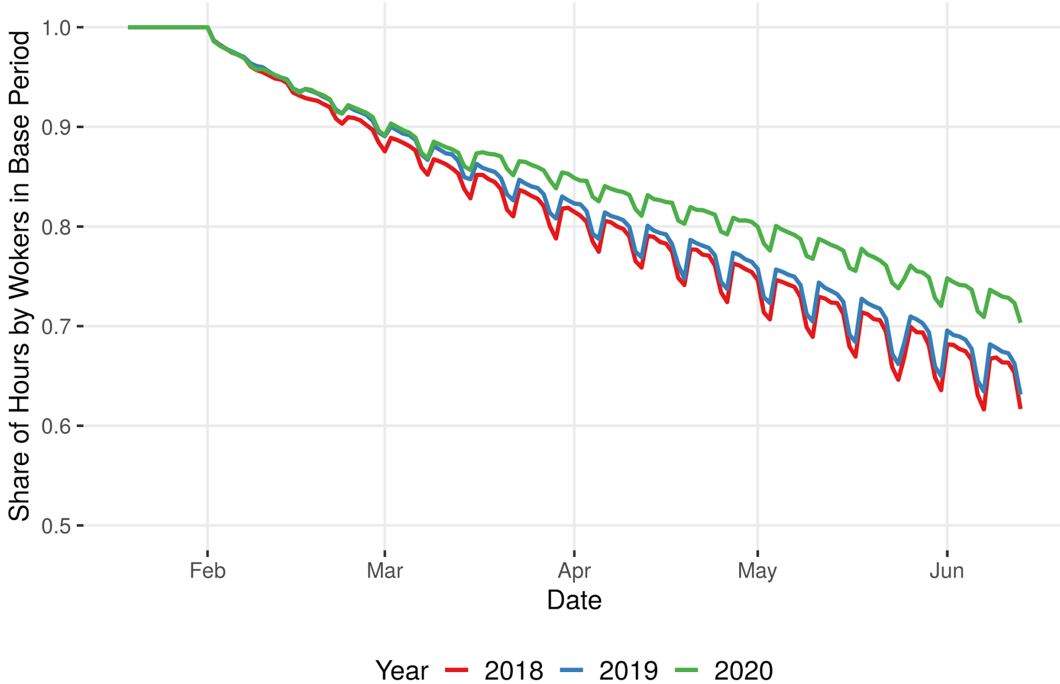
Notes: Computed from CPS microdata files. Hourly workers series are not shown for agriculture and mining, which had fewer than one million hourly workers each in January 2020.

Figure C2: Industry employment comparison, Homebase vs. CPS



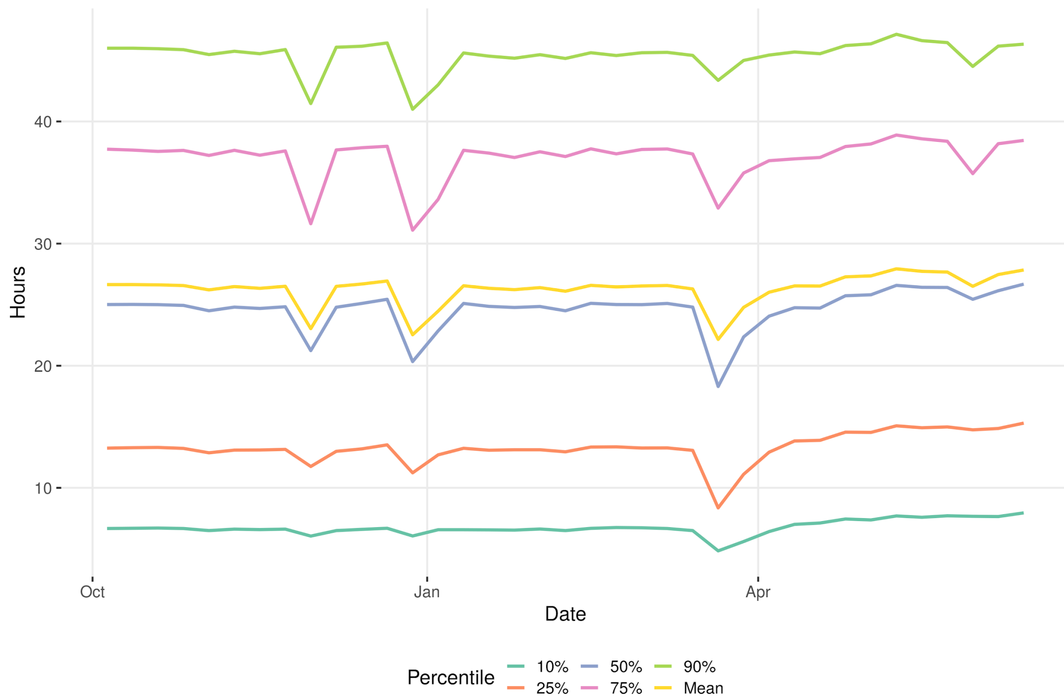
Notes: Homebase industries do not align perfectly with those reported in the CPS. We use a crosswalk created by Etienne Lalé to convert CPS employment to Homebase industries.

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



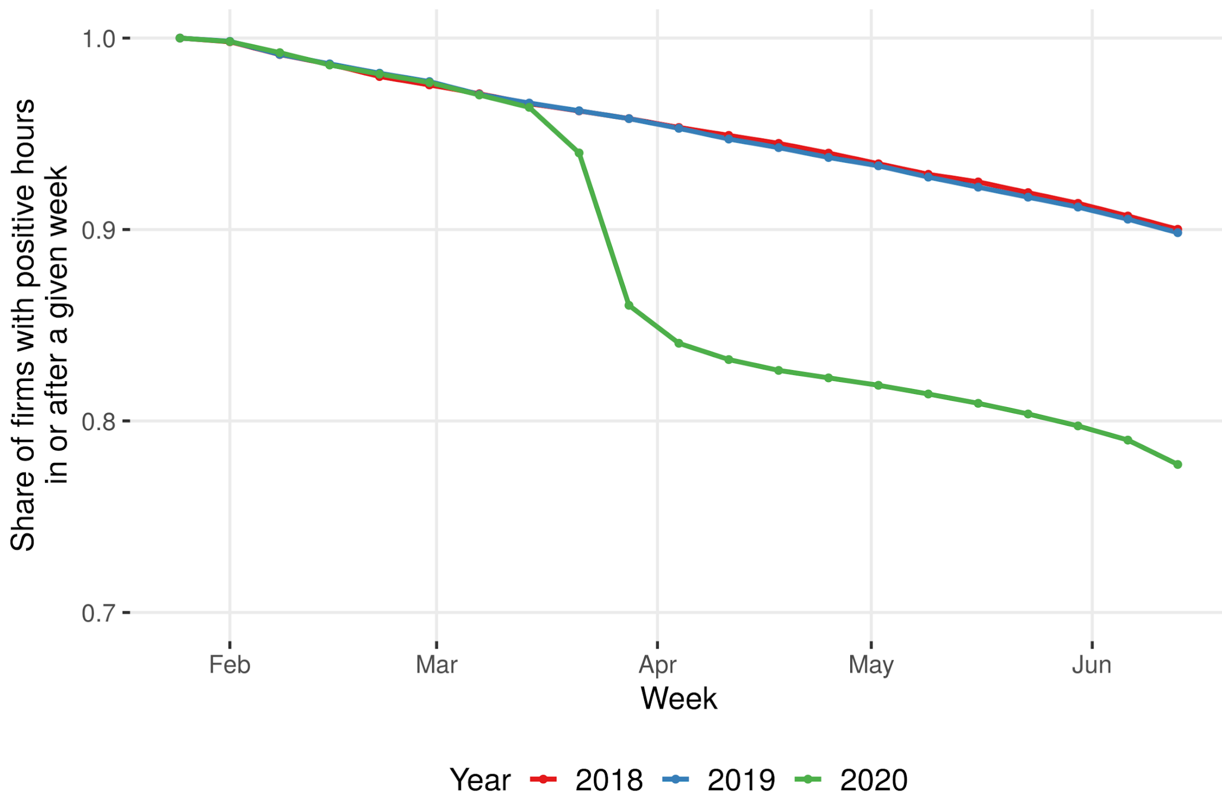
Note: The share shown in the figure is the share of hours worked on a given day coming from workers who appear in the Homebase data in the base period. The base period is two weeks at the end of January; for 2018 it is 1/21-2/3, for 2019 it is 1/20-2/2, and for 2020 it is 1/19-2/1. Firms included in 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 the coronavirus; also visible are the weeks containing Thanksgiving, Christmas and New Years, and Memorial Day.

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



Note: The share shown in the figure is the share of firms that we observe positive hours in or after a given week. We only use observations from weeks between the base period and 20 weeks after the start of 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 the end of weeks with 2020.