EFFECTS OF SOCIAL DISTANCING POLICY ON LABOR MARKET OUTCOMES

Sumedha Gupta†
Department of Economics, IUPUI
Cavanaugh Hall, Room 523, 425 University Boulevard Indianapolis, IN 46032
Email: sugupta@iu.edu, Phone: (317) 278-7218

Laura Montenovo
O'Neill School of Public and Environmental Affairs, Indiana University
2451 East Tenth Street, Room 443 Bloomington, IN 47405
Email: lmonten@iu.edu

Thuy Nguyen
School of Public Health, University of Michigan
1415 Washington Heights M3234 SPH II Ann Arbor, Michigan 48109
Email: thuydn@umich.edu, Phone: (734) 936-1303

Felipe Lozano-Rojas
School of Public and International Affairs, University of Georgia
Baldwin Hall 203B, Athens, GA 30602
Email: flozano@uga.edu, Phone: (812) 929-6717

Ian Schmutte
Department of Economics, University of Georgia
Amos Hall B420 Athens, GA 30602
Email: schmutte@uga.edu, Phone: (706) 542-3000

Kosali Simon
O'Neill School of Public and Environmental Affairs, Indiana University
and NBER
1315 East Tenth Street, Room 357 Bloomington, IN 47405
Email: simonkos@iu.edu Phone: (812) 856-3850

Bruce A. Weinberg
Department of Economics, Ohio State University
446 Arps Hall, 1945 N High Street, Columnbus, OH 43210
Email: weinberg.27@osu.edu, Phone: (614) 292-(5652)

Coady Wing
O'Neill School of Public and Environmental Affairs, Indiana University
1315 East Tenth Street, Room 443 Bloomington, IN 47405
Email: cwing@iu.edu Phone: (812) 856-3850

†Corresponding author email: sugupta@iupui.edu
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Abbreviations: ABC, any business closures; SAH, stay-at-home orders; DID, difference-in-differences; CPS, Current Population Survey; UI, unemployment insurance; DHS, U.S. Department of Homeland Security; IHS, inverse hyperbolic sine; and NYT, New York Times.

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Abstract

U.S. workers receive unemployment benefits if they lose their job, but not for reduced working hours. In alignment with the benefits incentives, we find that the labor market responded to COVID-19 and related closure-policies mostly on the extensive (12 pp outright job loss) margin. Exploiting timing variation in state closure-policies, DiD estimates show, between March 12-April 12, 2020, employment rate fell by 1.7 pp for every 10 extra days of state stay-at-home orders, with little effect on hours worked/earnings among those employed. 40% of the unemployment was due to a nationwide shock, rest to social-distancing policies, particularly among "non-essential" workers.

1. Introduction

To slow the transmission of SARS-COV-2, state governments adopted social distancing policies that effectively shut down large sectors of the economy during Spring 2020. The combined effects of the COVID-19 pandemic and associated policy responses were massive and sudden. More jobs were lost within the first months of COVID-19 than during the entire Great Recession (Montenovo et al., 2020). Although studies have quantified the public health gains from social distancing policies (Courtemanche et al., 2020; Friedson et al., 2020), this paper is the first to assess whether the nature of labor market adjustments are consistent with the economic incentives present in U.S. social benefits programs. In this paper, we study the effects of state social distancing policies on labor market outcomes using data from several different sources, including cell phone data measuring work-related mobility, state-level data on initial unemployment insurance claims, unemployment-related internet searches, and person-level data from the US Census Bureau's monthly Current Population Surveys from January 2015 to April 2020.

Although state governments adopted various policies to encourage social distancing during March and April 2020 (Gupta et al., 2020), we focus on the two that most directly lead to the cessation of business activity. The first policy is restaurant and any other (non-essential) business closures ("any business closures", or ABC for short). These ABC policies were widespread, with 49 states having imposed such restrictions by April 7, 2020 (Fullman et al., 2020). These policies were adopted early in the pandemic before major changes occurred in consumer demand and labor markets. The second measure is stay-at-home (SAH) mandates, which occurred toward the end of a state's shutdown sequence and almost always at the same time as a state's closure of all non-essential businesses (Gupta et al., 2020). These orders were the strongest, implemented after large reductions in mobility but generally just before large-scale job losses. Even the eight states - Arkansas, Iowa, Nebraska, North Dakota, South Dakota, Oklahoma, Utah, and Wyoming – that did not issue SAH orders in any part of the state (Vervosh and Healy, 2020) took several other policy actions including SAH recommendations (but not mandates) and curfews and may have been generally impacted by nationwide changes in sentiments.[1] Both of these orders reduce economic activity in very direct and obvious ways. As Figure 1 shows, there is substantial variation in the timing of these two policies across states.

To study the effects of social distancing policies on labor market outcomes, we use difference-in-differences (DID) and event-study designs. Some of our data sources are at the day or week by state level and allow us to focus on the immediate period around policy events. However, these high-frequency data do not measure the conventional labor market outcomes that are of central interest to policy discussions. We use data from the monthly Current Population Survey (CPS) to study employment, work absence, earnings, and hours worked overall for essential and non-essential workers, allowing us to additionally investigate extensive vs. intensive margin labor market responses. We use a DID method that allows us to compare labor market outcomes in mid-April 2020 to those in mid-March 2020. This technique leverages differences in the amount of time that states were subject to social distancing policies, essentially comparing states that acted earlier to states that acted later. We include data from previous years to control for seasonality. By April, most states had adopted ABC and SAH mandates, but some states took these steps before others, so their economies were subject to these constraints for a longer period. Labor markets experienced large declines from January to April, with employment rates falling by about 12 percentage points nationally. We use our DID estimates to assess how much of this change is due to national forces that operate independently of each state's specific business closure and stay at home policies. By comparing the model-based predicted employment outcomes in the absence of the social distancing policies with estimates of realized employment outcomes during the Spring of 2020, we find that about 40% of the decline was driven by a nationwide shock and about 60% of the decline was driven by state social distancing policies. The negative employment effects of state policies were larger for workers in "non-essential" industries. State policies caused relatively modest changes in hours worked and earnings among those who remain employed. These results suggest that state social distancing policies have important economic effects on labor market outcomes.

The credibility of the DID analysis method revolves around common trend and non-anticipation assumptions. In the case of the CPS data, we examine a low-frequency (monthly) event study approach. We find no evidence of pre-trends in the CPS data. While that is reassuring, the CPS data are measured at monthly intervals, which makes it hard to rule out the possibility that the

[1] Gupta et al., 2020 show that changes in human mobility in response to these mandates were comparable to coding schemes that treated states with non-mandatory but strong SAH as equivalent to mandatory ones. Consequently, we coded states with non-mandatory but strong SAH as equivalent to SAH mandates in this study.

employment effects experienced in April happened before the social distancing policies were adopted but after the March CPS data were collected. When examining data on work related mobility, internet search activity, and initial unemployment claims, we use a high-frequency event history specification to explore pre-trends in key labor market outcomes and to trace out the timing of the policy effects. These analyses generally corroborate our finding of statistically insignificant pre-trends.

Data on unemployment insurance (UI) claims, work-related cell phone mobility measures, and Google Trends internet searches related to unemployment are all imperfect proxies for the conventional labor market outcomes of interest (i.e., employment, hours, and earnings). However, high-frequency data are critical in the fast-moving context of COVID-19. Our results show that UI claims, workplace mobility measures of cell data, and internet search behavior related to unemployment all suggest that the state policies have some causal effects.

While we focus mainly on the social distancing policies adopted to address the COVID-19 pandemic, our work fits into a broader literature on the role of public policy in supporting people during periods of high unemployment, sickness, and poverty (Bitler et al., 2017; Rothstein and Valletta, 2017; Rothstein, 2019; Scherpf and Cerf, 2019). It also connects to research on the economic and public policy implications of large scale disasters (Vigdor, 2008; Michel-Kerjan, 2010; Imberman et al., 2012). Large scale shocks that affect multiple sectors of the economy across many different regions of the country put substantial strain on many of the systems we use to help mitigate poverty in the U.S. The current crisis is one that has damaged population and individual health, created enormous economic losses, and led to rapid development of social distancing policies that have very little precedence in the policy analysis literature. Understanding how these policies affect different aspects of social and economic well-being, and how they interact with economic incentives built into existing social safety net programs (unemployment benefits), will remain crucial over the coming years, as the threat of the virus continues in a globally connected economy until the entire world population can be vaccinated.

2. Related Research

Institutions may play a vital role in how labor markets adjust during economic downturns. While there is considerable evidence that the policy environment in Europe, such as employment protection laws and collective bargaining mechanisms, increase intensive margin adjustments (changes in the hours worked/wages earned) during economic downturns (Merkl and Wesselbaum, 2011; Van Rens, 2012; Boeri et al., 2011), a large body of literature, including recent studies of the 2008-2009 Great Recession, agrees that the extensive margin adjustments dominate intensive margin adjustments during recessions or following natural disasters in the U.S. (Ohanian and Raffo, 2012; Hobijn,et. al 2010; Zissimopoulos and Karoly, 2010). The ability of workers to access unemployment insurance only in case of complete job-separation, and more generous unemployment benefits due to the CARES Act during the COVID-19 recession (Marinescu et al., 2020), are likely to encourage employers and workers to continue to opt for complete job-separation over reduced work hours in the pandemic downturn. Overall, any change in wages of those who remain full-time employed may be fully compensated by the decline in employment, leaving aggregate real wages largely unchanged (Daly and Hobijn, 2016).

Specifically, for the COVID-19 induced recession, the social science literature continues to evolve, but this paper relates to several themes that have already emerged. One line of research examines how the pandemic and social distancing policy responses have affected labor market outcomes overall. There were 20.5 million job losses and rapid increases in unemployment insurance applications in April 2020 alone. The unemployment rate rose from 4.5 percent in March to 14.7 percent in April 2020. Considering data until March 2020, Lozano-Rojas et al. (2020) show that the historically unprecedented increase in initial unemployment claims in March 2020 was largely across the board, occurring in all states regardless of local epidemiological conditions or policy responses. Baek et al. (2020) come to a broadly similar conclusion with UI records, examining a longer time period.

Campello et al. (2020) provide evidence on labor demand using job postings data from Linkup, although they do not investigate the role of state policy. They find that job postings declined about 2 weeks before the large rise in UI claims. Kahn et al. (2020) show a large drop in job vacancy postings in the second half of March 2020. They report that, by early April, there were 30% fewer job postings than at the beginning of the year. These declines also largely happened across states, regardless of state policies or infection rates.

Our analysis of CPS data in this paper through April 12, 2020, first notes a strong connection between labor market outcomes and state policies. It is not surprising that analysis using March 2020 CPS data (Lozano-Rojas et al., 2020) did not find such a result, as very few closure policies had gone into effect by the CPS reference week that month (March 12th). However, even with data through mid-April, we find that there is a large across-the-board reduction in labor market outcomes including in states that did not institute strong SAH policies. While their primary focus is on expectations and consumer spending, Coibion et al. (2020b) use custom data to show that lockdowns are related to worse labor markets, controlling for COVID-19 cases. More recent literature has also noted a modest 2-8% increase in UI claims due to state policies, with business closures having a larger effect than SAH orders (Kahn et al., 2020; Kong and Prinz, 2020; Lozano-Rojas et al., 2020). Similar work analyzes the economic effects of the pandemic in other countries (Adams-Prassl et al., 2020; Dasgupta and Murali, 2020; Rothwell and Van Drie, 2020).

Recent work studies the effects of the pandemic (but not social distancing policy specifically) on particular sub-populations, with emphasis on the role of job characteristics. Montenovo et al. (2020) study early labor market outcomes during the pandemic using CPS data from March 2020. They find high rates of recent unemployment among workers in jobs that are harder to perform remotely, workers in jobs that require more face-to-face contact, and industries that were deemed essential. Dingel and Neiman (2020) and Mongey and Weinberg (2020) also study high work-from-home occupations. Leibovici et al. (2020) take a similar approach to measure occupations with high interpersonal contact. Aaronson et al. (2020) build a forecasting model that uses Google search activity for unemployment-related terms to predict weekly unemployment insurance claims and find that unemployment insurance claims and Google searches for unemployment insurance both peak prior to SAH orders. In this spirit, we draw on UI claims data as well as cell phone mobility to workplaces to provide high-frequency information to augment our CPS analyses. However, note that Coibion et al. (2020a) use data from an early-April household survey and find that unemployment rate may greatly exceed unemployment insurance claims.

A last line of related work examines the effects of state and local social distancing policies on measures of mobility and social interaction. Using cell phone data, Gupta et al. (2020) document a massive, nationwide decline in multiple measures of mobility outside the home. They also find evidence that early and information-focused state policies did lead to larger reductions in mobility than policies that mandated sheltering but were imposed later. These reductions in time spent outside the home suggest that many people are experiencing work disruptions, and that those who can work remotely may be more able to maintain employment during the crisis. Relative to this work, we focus on mobility related to the workplace in particular and use such analysis to validate results from the CPS data. We also connect our work directly to a range of labor market outcomes for essential versus non-essential workers.

3. Data

3.1. Current Population Survey

We use data from the Basic Monthly CPS from January 2015 to April 2020, including all individuals aged 21 and above. There are between 76,000 and 97,000 observations per month, and our total sample contains approximately 5.9 million observations. These surveys ask respondents about their labor market activities during a reference week that includes the 12th of the month (U.S. Census Bureau, 2019), allowing us to measure both extensive and intensive margin measures. Our primary measure of employment status is the share of the population that the CPS codes as being employed and at work. This measure excludes people who have a job but were temporarily absent[2]. Lozano-

[2] The CPS defines as "absent from job" all workers who were "temporarily absent from their regular jobs because of

Rojas et al. (2020), Bogage (2020), and Borden (2020) highlight the importance of properly coding people who are employed but absent for measuring employment status during the COVID-19 pandemic[3]. When we construct our outcome measure of employment, we include only those who are employed and at work. Given the importance that absence from work has gained during the pandemic, we also consider the outcome "Absent - Employed," which includes only those workers classified as absent from work but still employed during the Basic Monthly CPS.

To examine hours worked, to characterize changes in employment along the intensive margin, our measure is actual hours worked during the week before the survey. In parts of our analysis, we include individuals who are not employed by assigning them zero hours, which provides a comprehensive measure of hours of work and combines changes along the intensive and extensive margins. We also show estimates that treat people who are not at work as having missing hours. This measure isolates the intensive margin for those who remain employed. We acknowledge that changes in the composition of those who are working may separately affect our measure of hours.

We also study COVID-19 policy effects on earnings as a second intensive margin measure. On the one hand, reduced demand for many commercial activities, including overtime, may lead to reductions in hourly wages, including overtime payments, and thus reduce earnings. On the other hand, the health risks (COVID-19 exposure) increased, and theory leads us to expect higher wages. A number of high-exposure jobs provided workers with additional compensation for the added risk incurred by COVID-19, and some industries experienced increases in demand. Thus, it is possible that, for some, earnings may have increased rather than decreased as infection rates and state policies changed in response to the pandemic. Moreover, it seems likely that the composition of people who are employed (and hence report earnings) will have changed. As with hours of work, we report results including people with zero earnings as "zeros". These estimates are comprehensive, combining the intensive and extensive margins. Given that there has been a large reduction in employment, we also provide estimates that consider outcomes only among those who continue to be employed, acknowledging that these will be affected by changes in the composition of people working. These estimates isolate changes along the intensive margin for people who remain employed. When we use earnings as the outcome variable, our sample is limited to people in the

illness, vacation, bad weather, labor dispute, or various personal reasons, whether or not they were paid for the time off" (U.S. Census Bureau, 2019).

[3] First, some employers released workers intending to rehire them. Second, some workers may have requested leave from their schedule to provide dependent care or to care for a sick household member. Third, there was a misclassification problem during the data collection of the March and April 2020 CPS. Specifically, the BLS instructed surveyors to code those out of work due to the epidemic as recently laid off or unemployed, but U.S. Bureau of Labor Statistics (2020a) and U.S. Bureau of Labor Statistics (2020b) explain that surveyors appeared to code at least some of them as employed-but-absent. These factors contribute to the massive increase in the share of workers coded as employed but absent from work between February and April. In our sample, the employed-but-absent share group rose by almost 150% from February to April 2020.

outgoing rotation groups of the CPS sample because only these individuals are asked questions about earnings.

3.2 Homeland Security Data on Essential Work

The U.S. Department of Homeland Security (DHS) issued guidance about critical infrastructure workers during the COVID-19 pandemic[4]. The DHS guidance outlines 14 categories that are defined as essential critical infrastructure sectors. We follow Blau et al. (2020)'s definition of essential industries, which matches the text descriptions to the NAICS 2017 four-digit industry classification from the U.S. Census Bureau[5], and to the CPS industry classification system. Of the 287 industry categories at the four-digit level, in our CPS sample 194 are identified as essential in 17 out of 20 NAICS sectors.

3.3 Weekly Initial Unemployment Insurance Claims

In addition to the monthly CPS, we also study the number of initial UI claims in each U.S. state[6], including Washington, DC and Puerto Rico, from the first week of 2019 to the week ending in May 16, 2020. We focus on the number of new UI claims per covered worker, using the number of covered workers in January 2020 as a fixed denominator to avoid changes in rates driven by changes in covered employment.

3.4 Social Distancing Policy Data

We use data on state social distancing policies previously reported in Gupta et al. (2020). Basic information about the timing of state policy actions was originally collected by Washington University researchers (Fullman et al., 2020) and Boston University researchers (Raifman and Raifman, 2020).

3.5 Work-Related Mobility Data

We extract work-related mobility from a cell signal aggregator, Google Mobility, which has made its data available for researchers during the pandemic[7]. We use a day-by-state-level index of activity detected in work locations. The advantage of these data is that they are available at the daily level and provide a way for us to investigate whether employment followed a different trend in states with early social distancing policies, a challenge in the CPS data given its monthly schedule. However, prior to the pandemic, cellphone mobility data had not been widely used in labor economics research

[4] The list of critical infrastructure jobs is available at: https://www.cisa.gov/
[5] North American Industry Classification System. Available at https://www.census.gov/
[6] Data available from the Department of Labor at: oui.doleta.gov
[7] Data Available at https://www.google.com/covid19/mobility/

and their properties are not well understood. We view them as a proxy for time spent at a person's typical work location. These measures will not capture remote work, which has become more common during the pandemic. In the CPS, our concept of employment does not depend on whether it is done physically at a work location. Thus, we view the mobility data as supplementary to the CPS data.

3.6 Google Trends Data

We obtain information on internet search behavior by day by state through the Google Health API, which allows us to follow internet search queries across different terms, topics, and geographies, in a way that allows comparisons across time and place[8]. Using data pulled from queries related to unemployment and unemployment benefits as suggested on the Google Trend webpage, we construct a measure that encompasses several terms (see Appendix A.1) related to unemployment queries. We present the series of the measure in Fig 6 Panel (a), and, in the Appendix, Fig B.1 shows the series of the individual terms used to construct the measure. For the event study graph plotting the Google search data, Figure 7, we aggregate all these individual unemployment-related terms to a state-level search index as the outcome.

4. Econometric Methods

We conduct three broad empirical analyses. First, we examine the connection between state social distancing policies and both cell-phone-based measure of work-related physical mobility and Google Trends data on work-related internet search activity. The cell-phone-based data provide information at the day-by-state level; we use an event study model to analyze the immediate changes in work related mobility following ABC and SAH orders. Second, we examine the relationship between initial unemployment claims and state policies using an event study model at the week-by-state level. These first two sets of analysis provide relatively high-frequency measures of labor market-related activity, and they allow us to assess pre-trends and anticipation effects in considerable detail. These tests are particularly important for our study as during the early days of COVID-19 the pandemic and the response policies were rapidly evolving together, raising concerns about policy endogeneity (Farboodi, Jarosch, and Shimer, 2021). However, the mobility data and the initial unemployment insurance claims data are both aggregate analyses, providing little opportunity to assess effects across sub-populations, and whether adjustments were on intensive and extensive margins of labor force participation. Moreover, they are not the conventional measures of labor market performance:

[8] We access this information using the apiclient.discovery package for Python and its function getTimelinesForHealth. For a thorough explanation of the different information available with Google Trends, see Baker and Fradkin (2017) and www.medium.com.

mobility measures are fairly new to the literature and their properties are not fully understood. Google search behavior reflects only the extent to which job changes altered internet search patterns; and UI claims are known to substantially underestimate the extent of job losses.[9] To address these concerns, we turn to the CPS and use a generalized DID strategy and a low-frequency event study based on monthly data.

4.1 Analyses of High-Frequency Data: Work-Related Mobility, Google Trends, and Unemployment Insurance Claims

Throughout this paper, we focus on SAH mandates and ABC mandates. States adopted these measures at different times, and this creates variation across states in how long the mandates have been in place. Let E_{Ps} be the adoption date of policy $P \in \{SAH, ABC\}$ in state s. $TSE_{Ps} = t - E_{Ps}$ measures the elapsed time between the period t and the policy adoption date. In the analysis of work-related mobility and internet search data, the data are measured at the daily level: the elapsed time is measured as the number of days. The initial unemployment insurance (UI) claims are weekly: we consider weeks since adoption in those data. We set lower (l) and upper limits (u) for the event time coefficients following the availability of periods. For the daily analyses of Google Mobility data and Google Trends data, we allow for a window of 21 days before and after policy as lower and upper limits. In the weekly analyses for UI claims, we follow up to 10 weeks prior to the policy change and 7 weeks after. We fit event study regression models that allow for concurrent effects of both policies with the following structure:

$$y_{st} = \sum_{P \in \{SAH, ABC\}} \left(\sum_{a=-l}^{-2} \alpha_{P_a} 1\left(TSE_{P_{st}} = a\right) + \sum_{b=0}^{u} \beta_{P_b} 1\left(TSE_{P_{st}} = b\right) \right) + \theta_s + \gamma_t + \varepsilon_{st} \quad (1)$$

In the model, θ_s is a set of state fixed effects, which are meant to capture fixed differences in the level of outcomes across states that are stable over the study period. γ_t is a set of daily or weekly time fixed effects, which capture trends in the outcome that are common across all states. ε_{st} is a residual error term. α_{P_a} and β_{P_b} are event study coefficients that trace out deviations from the common trends that states experience in the days leading up to and following the SAH orders and business closures. Specifically, α_{P_a} traces out differential pre-event trends in the outcome that are associated with states that go on to experience policy $P \in \{SAH, ABC\}$ examined in the model. β_{P_b}

[9] Weekly UI claims may also differ from the other high-frequency data we examine as there may have been UI processing delays during closures, and the largest increases in UI claims may sometimes be observed in the following week. But weekly UI data relate more closely to direct measures of employment than work-related mobility or Google Trend searches, while still enabling us to use high-frequency event studies to explore pre-trends in key labor market outcomes and to trace out the timing of the policy effects, which may otherwise be missed in the monthly CPS data. Despite possible limitations of the individual datasets, examination of CPS data along with high-frequency event studies of several different employment-related outcomes, from multiple data sources, provides a more comprehensive look at the labor market responses to the social distancing policies.

traces out differential post-event trends in the outcome that occur after a state adopts policy $P \in \{SAH, ABC\}$. In addition to the state-level event study analysis, we show a separate event study graph generated by blocking the sample into states with longer and shorter SAH orders and ABC, expecting that early adopting states may have larger effects on work-related mobility, unemployment-related Google searches and UI claims. Longer SAH orders are defined as those that were in effect for at least 18 days (the median implementation period) at the end of our observation window. Similarly, longer business closures are defined as those that were in effect for at least 26 days on April 12, 2020, the April CPS focal date.

4.2 Monthly CPS Analysis

We analyze the CPS data at the individual level using monthly files from January 2015 to April 2020. We examine a dichotomous variable for being employed at work, employed but absent from work, weekly earnings, and hours worked last week. We present two versions of the weekly earnings and hours worked variables. First, we examine intensive margin responses using the sample of people who are employed and therefore have positive earnings and positive hours worked. Second, we examine earnings and hours measures that are set to zero for people who are not employed. In the regression models, we apply an inverse hyperbolic sine (IHS) transformation to the earnings variable; a regression of $IHS(Earnings_{ismt})$ on covariates is comparable to a conventional log-linear regression specification, but the IHS transformation is defined for people who have zero earnings as well as for people who have positive earnings. Let Y_{ismt} be a labor market outcome associated with person i in state s in month m and year t. X_{ismt} is a vector of individual demographic and human capital characteristics. Following the notation above, let E_{SAH_s} and E_{ABC_s} be the adoption dates of the SAH and ABC mandates in state s, and let t^*= April 12, 2020, be the focal date of the April CPS. Then $SAH_s = t^* - E_{SAH_s}$ be the number of days that the SAH policy had been in place by the April CPS focal date. Likewise, $ABC_s = t^* - E_{ABC_s}$ is the number of days that ABC orders had been in place in a state as of the April CPS focal date. Finally, let $April_{mt}$ be an indicator variable equal to 1 if the observation is from the April 2020 CPS and set to 0 otherwise. We use a generalized DID model to study the effects of the policies on labor market outcomes:

$$y_{ismt} = \delta_1(SAH_S \times April_{mt}) + \delta_2(ABC_s \times April_{mt}) + X_{ismt}\beta + \theta_s + \gamma_{mt} + \epsilon_{ismt} \quad (2)$$

In the model, θ_s is a state fixed effect that captures time invariant differences across states, and γ_{mt} is a month × year fixed effect that captures time trends that are common across states. ϵ_{ismt} is an error term that we assume is strictly exogenous of the policy variables and the covariates. The interaction terms $SAH_S \times April_{mt}$ and $BC_s \times April_{mt}$ are analogous to the Treat × Post terms in a conventional DID framework, except that the treat variable here is a continuous (dosage) measure of how long a given social distancing policy has been in place. δ_1 and δ_2 represent the effects of one additional day of exposure to the SAH and ABC policies. The main effects associated with SAH_s , ABC_s, $April_{mt}$ are absorbed by the fixed effects. We estimate the model using OLS regressions with fixed effects, and we compute standard errors using a cluster robust variance matrix that allows for heteroskedasticity and for dependence between observations from the same state.

This version of the DID model relies on the common trends and strict exogeneity assumptions (Wing et al., 2018). The common trend assumption implies that, after adjusting for covariates and state fixed effects, average labor market outcomes in a state would have followed a common time trend in the absence of state social distancing policies. The strict exogeneity assumption implies that state policy decisions in one time period are not associated with labor market outcomes in previous time periods. This assumption might fail if patterns of employment, compensation, or hours worked change in anticipation of downstream policy changes, or, alternatively stated, if higher early pandemic severity would imply early policy adoption but also confound anticipatory employment-related changes due to increases in precautionary savings and associated reductions in consumer demand and overall economic activity. These are strong assumptions that are not easy to test. We descriptively examine whether early pandemic severity is associated with early adoption of ABC and SAH policies, by using COVID-19 related cases and deaths rates data, collected since the start of the pandemic by the *New York Times* (NYT)[10], and ranking states by their cumulative number of COVID-19 cases and deaths per 100,000 state population as of March 12, 2020, as measures of their early pandemic severity. Appendix Table A1.1 summarizes the number of days each policy (ABC and SAH) had been in effect by April 12, 2020 (the CPS focal date for the April CPS) by the quartile of the early pandemic severity measures.[11] From Appendix Table A1.1 we see that days since ABC policy adoption ranged from 0-58 days across all states, irrespective of quartile of early pandemic severity, with comparable means and standard deviations. Considering adoption of SAH orders, on average, days since SAH policy adoption ranged from 0-52 days across all states, with states with lower early pandemic severity adopting the policy just a few days earlier than states with higher early pandemic severity, but again with comparable means and standard deviations. The only exception is the states in the third quartile of early cumulative COVID-19 case rates, reflecting relatively low early pandemic severity, where the SAHs had, on average, been in effect about 3 days longer than the states in all other quartiles. Overall, we take this descriptive examination as evidence that there was in fact considerable and similar variation in the timing of policy adoption across all states regardless of their early pandemic severity.

[10] *NYT* data have been extensively used in the large body of literature that has emerged during the pandemic to capture pandemic intensity by state. These are publicly available from: https://github.com/nytimes/covid-19-data
[11] Since ninety percent of the states had not yet had their first confirmed COVID-19 death by mid-March, we only consider the top 10 percent or below in case of death rates.

While our analysis of pre-trends in high-frequency data provides supportive evidence, we investigate pre-trends in our monthly CPS data, estimating an event study model using multiple waves of the CPS.

$$\begin{aligned} y_{ismt} = {} & \delta_1(SAH_S \times April_{mt}) + \delta_2(ABC_s \times April_{mt}) + \sigma_1(SAH_S \times March_{mt}) \\ & + \tau_1(ABC_s \times March_{mt}) + \sigma_2(SAH_S \times February_{mt}) \\ & + \tau_2(ABC_s \times February_{mt}) + \sigma_3(SAH_S \times January_{mt}) \\ & + \tau_3(ABC_s \times January_{mt}) + X_{ismt}\beta + \theta_s + \gamma_{mt} + \epsilon_{ismt} \qquad (3) \end{aligned}$$

In this model, δ_1 and δ_2 coefficients continue to represent the effect of days of policy exposure in April 2020. However, this time, the model includes interaction terms between the (time invariant) days of SAH and ABC policy exposure and dummy variables for each of the three months preceding the adoption of the policy. σ_1, σ_2, and σ_3 provide estimates of the difference in labor market outcomes between states that will go on to have more vs. fewer days of SAH exposure in March, February, and January 2020. Since the SAH policies had not been implemented in these earlier months, a significant coefficient on these SAH policy leads would cast doubt on the strict exogeneity assumption due to differential pre-trends. τ_1, τ_2, and τ_3 have a similar interpretation for the ABC mandates. These tests are one way to assess the empirical credibility of the DID research design at the core of our CPS analysis.

Although this kind of event study analysis is the recommended approach to probing the validity of some key DID assumptions, it is unclear how well the method applies in the context of the COVID-19 pandemic. The unprecedented speed of the pandemic and subsequent changes in labor market conditions means that a gap of one month between labor market outcome measures could actually be too long to assess assumptions about pre-trends in the period leading up to state social distancing policy changes. The specific concern is that much of the large decline in employment observed in the April CPS could have taken place in a narrow interval of time after the March CPS but before the adoption of state social distancing policies. In that case, the monthly event study analysis would not detect evidence of pre-trends, and the DID estimator could deliver biased estimates of the causal effects of the social distancing policies. As indicated, to do our best to alleviate this concern, we examine the CPS data in conjunction with several high-frequency proxy measures of labor market activity: work-related mobility, employment-related internet search activity, and initial unemployment claims.

4.2.1 Interactions Between Social Distancing Policies and Essential Work

Recent work suggests that a large fraction of workers are involved in the delivery of essential services and that, during the pandemic, workers in essential industries entered unemployment at lower rates than non-essential workers (Montenovo et al., 2020). It is plausible that the economic effects of social distancing policies may have had a different effect on essential and non-essential workers. To estimate different effects for people employed in essential and non-essential industries, we estimate models that include an indicator for whether a person is employed in an essential industry and interactions between that indicator and the social distancing policy variables. Formally, we estimate

$$
\begin{aligned}
y_{ismt} = {} & \delta_1(SAH_S \times April_{mt}) + \delta_2(ABC_s \times April_{mt}) \\
& + \pi_1(Essential_{ismt} \times SAH_S \times April_{mt}) \\
& + \pi_2(Essential_{ismt} \times ABC_S \times April_{mt}) + X_{ismt}\beta + \theta_s + \gamma_{mt} + \epsilon_{ismt} \qquad (4)
\end{aligned}
$$

In these models, δ_1 and δ_2 represent DID effects of additional days of policy exposure for non-essential workers, and π_1 and π_2 represent differential policy effects for essential workers. In most cases, we expect the policy effects to generate larger reductions in employment, earnings, and hours worked for workers employed in non-essential industries compared to those in essential industries.

5. Results

5.1 Trends in Labor Market Outcomes

In Figure 2, we examine the pattern of our focal CPS labor market outcome variables from January to April, in each of the years 2015-2020. The top left panel of the figure plots the employment rate. The red line shows that employment rates from January through March 2020 are similar to the pattern observed over the same months in other years. The 2020 line begins declining slightly between February and March, and then falls sharply from March to April 2020. The employment rate in April 2020 is only 50%, far lower than the rate in the same month in earlier years. The temporarily absent from work rate also rose substantially during the early months of 2020, which may indicate a combination of measurement error challenges in the those waves of the CPS and genuine increases in work absenteeism (Montenovo et al., 2020).

The middle panel reports earnings, which are measured only for the CPS outgoing rotation groups. The earnings graph on the left displays an apparently counterintuitive result: average weekly earnings among employed workers increased in April 2020 (left panel). The rise in earnings likely reflects a composition change in the employed population. That is, it may be that workers who remained employed during the very first months of the pandemic were disproportionately those with higher earnings. However, it is also possible that earnings rose among employed workers because of wage increases that reflect new job risks and demand for scarce labor or increases in hours worked and overtime pay for some workers who remained employed. In the middle-right panel, we plot earnings over time, setting the earnings of the non-employed to zero in order to combine extensive and intensive margin changes in earnings. The graph now shows a large fall in weekly earnings of close to $300 a week between March and April 2020, indicating that job losses have, in aggregate, translated into substantial declines in labor market earnings.

The bottom panel shows that average hours worked last week also decreased from February to March in 2020 relative to other years, and then they experienced a sharp downturn in April. Among people who are employed, the fall in hours is only about 1 hour a week. Like our analysis of weekly earnings, in the panel to the bottom right we set hours worked last week for the non-employed to zero, rather than missing. Now, the change in hours worked during the week before the survey represents a drop of close to 6 hours between March 2020 and April 2020. This also makes it clear that job losses were the key driver of overall labor market outcomes in Spring 2020. While we do not address changes in the composition of workers, intensive margin responses were much smaller in comparison.

5.2 Work-Related Mobility Patterns

We next turn to our high-frequency Google Mobility data series, starting with Figure 3 showing the basic time series of work-related mobility by state. The study window runs from February 15, 2020, to April 12, 2020, which keeps the end date of the study period the same as in the CPS analysis. In the left panel, the grey lines turn red when each state issues a SAH mandate. In the right panel, the grey lines turn red when the state adopts an ABC ordinance. ABC policies tend to happen earlier than SAH policies.

Work related mobility falls about at the same time in all states with an ABC policy, although some of the change in slope seems to happen a few days before the policy effective date. SAH orders appear to go into place later in the month, after a lot of the decline in workplace mobility already happened. From the figure, it is also clear that decreases happen after the SAH orders, but these reductions in mobility also occur in the states that did not implement SAH orders.

To examine parallel trends assumptions and effect size magnitudes, we next turn to Figure 4, which shows event study estimates from models that examine both SAH and ABC policies simultaneously for work related mobility. The effects for SAH mandates are shown in the left panel. The right panel reports the effects of the ABC closures. The notes in each figure show the mean of the Google Mobility's index of work transport at baseline (February 15, 2020, in all graphs).

The left panel suggests a slight downward pre-trend prior to the implementation of a SAH order, followed by a sizeable decline at the point of a SAH order and then the continuation of moderate downward trends. The right panel of the figure exhibits the timing of changes around ABC policies. The estimates in the right panel are striking, trending slightly upward prior to the implementation of ABCs, but then showing a small drop followed by a steep, sustained downward trend. Thus, the mobility estimates show rather clear adverse effects on workplace mobility. Note, of course, that the mobility measures can pick up work behavior only as defined by physical travel to locations. We believe these are reliable during the very first days of the social distancing policies, which we consider in the high-frequency event studies, to the extent that remote work arrangements were relatively uncommon. Also, our analysis of mobility does not shed light on more specific job-related outcomes. For example, they do not reveal information about job losses, earnings changes, or work disruptions. The CPS data will fill this gap.

Figure 5 shows event study analysis of the work-related mobility when the data are stratified into early adopting states and late adopting states (based on above and below median days since adoption), as early adopting states might have acted before the potential impact of the policy was lessened by nationwide sentiment and sheltering responses. In these graphs, the left panel shows the event study for states that implemented SAH and ABC mandates early, and the right panel shows event studies for states that adopted the policies later. The results again show that the workplace mobility measure did seem to respond to the social distancing policies, with effects that are larger in states that adopted the policies earlier.

5.3 Google Search Trends for Unemployment Related Terms

Another high-frequency measure of job-market-related behavior is Google search trends for unemployment topics (not related to the Google Mobility workplace measure above). We next turn to this measure for further data to examine whether the changes in employment patterns happened in the days prior to implementation of the state policies or after their implementation. Unlike the mobility data, the search queries data are available for multiple years. Choi and Varian (2012) show that Google searches for unemployment-related terms queries are predictive of downstream unemployment insurance claims, Baker and Fradkin (2017) construct an unemployment index based on Google search terms and Aaronson et al. (2020) apply the idea to the COVID-19 pandemic. Figure 6 Panel (a) shows the national time series of Google searches for aggregated search terms for the first 150 days of the calendar year in each year from 2015 through 2019. The 2020 data are shown in orange. There is a large and sudden increase in the volume of unemployment-related searches starting in the first half of March, which corresponds to the beginning of the pandemic in the U.S. No such changes in searches are observed for the previous years, indicating no confounding seasonality issues in seeking for resources available for unemployment.

Figure 7 Panel (a) shows estimates from event study regressions related to SAH and ABC policies based on state level versions of the Google Trends data. The outcome variable is an aggregate measure of searches for multiple unemployment related terms combined.

Interestingly, there is some evidence of a pre-trend in the share of Google searches on unemployment topics before the SAH ordinance, highlighting that at least some of the decline in employment occurred prior to state mandates and was associated with growing employment related Google search activity. In fact, after the implementation of SAH mandates, searches for unemployment-related terms seem to stabilize after the implementation of the order. This may indicate that people reduced job search efforts during the lockdown, or that job losses grew rapidly in the days leading up to SAH ordinances in most states and then stabilized at a new level over the next 20 days. The evidence is different when we consider ABC mandates. There is less indication of a strong pre-trend, and there is a substantial increase in the volume of unemployment-related search activity in the days following the ABC mandates.

A possible explanation for the difference in the Google search trends we observe (as with the other high-frequency data) in SAH vs. ABC is the timing of the policies' implementation. While SAH orders occurred well into the trajectory of movement slowdowns, ABCs occurred relatively early: they were fairly unexpected and more likely to have occurred before large-scale labor market changes. Event study estimates presented in Appendix Figure C.1, using samples stratified by early vs. late adopting states (based on above and below median), provide some support for this possibility. We find flat pre-trends before the implementation of ABC's for both early and late adopters of ABC's with no evidence of anticipatory changes. In comparison, we find a significant pre-trend in the share of Google searches on unemployment topics before the SAH ordinance in early adopting states affirming potential anticipatory effects for the later policies.

5.4 Unemployment Insurance Claims

The last of our high-frequency job-market series is week-by-state unemployment insurance (UI) claims. Figure 6 panel (b) plots the log number of UI claims nationally up to the second week of April for the years 2015-2019 in dashed lines. The orange line shows the same figures for 2020. During the first ten weeks of 2020, the average level of UI claims across the country was the lowest in the last 6 years. From that week onward, the level of the UI claims was higher, often an order of magnitude higher, than in previous years. Week Ten ended on March 7, 2020, and the number of initial UI claims reached its highest spike two weeks later. This is – essentially – the time when the pandemic exploded, and states began to implement social distancing policies.

Figure 7, Panel (b) presents results from an event study analysis of the effects of SAH and ABC mandates using state-by-week-level data on initial UI claims per covered worker. Prior to the adoption of social distancing policies, there is no clear difference in trends for SAH, but an insignificant decline in the case of ABCs. The initial UI claims rates increase in the days following SAH mandates. There is also an increase following the ABC mandates, but the effects are noisier and not statistically significant. Starting from a baseline in the first week of March, the average state had 1.37 UI claims per 1000 workers. The estimated event study coefficient corresponding to the week that ABC policies were adopted (week 1) is 8.06 (SE=4.113), which implies that the policies are associated with a six-fold increase in new UI claims, which is statistically significant at the 10%

level. The short-term coefficient for SAH order is high as well (4.21 in week zero); however, it is not statistically significant. Moreover, the estimate for week 2 is even higher for both SAH and ABC policies, although neither estimate is statistically significant.

Event study estimates presented in Appendix Figure C.2, using samples stratified by early (top panel) vs. late (bottom panel) adopting states (based on above and below median), show statistically insignificant pre-trends in weekly UI claims per covered worker prior to the implementation of ABC policies for both early and late adopting states and for early SAH adopting states. These results reinforce that early policies may have been implemented relatively abruptly and that UI claims responded to the social distancing policies, with effects that are larger in states that adopted the policies earlier.

Taken together, these high-frequency data on labor market outcomes provide evidence that labor market outcomes start to change slightly before policy changes, with large changes in level and slope that occur after the policy date, suggesting that the policies do have some causal effects. For most outcomes, the policy effects of ABCs, which preceded SAHs, appear larger than those of SAHs. Next, we turn our attention to more conventional and direct measures of labor market activity, such as employment, earnings, and hours worked.

5.5 Effects of Social Distancing on Employment, Earnings, and Hours Worked

We turn to the CPS data to study the effects of state social distancing policies on a range of labor market outcomes and to compare the policy effects across sub-populations defined by essential work designation. We focus on a set of six labor market outcomes: (i) employment; (ii) absent but employed; (iii) earnings among the employed; (iv) earnings in the full sample, including people with zero reported earnings; (v) hours worked among the employed; and (vi) hours worked in the full sample, including people with zero hours of work. The earnings analysis is limited to people in the outgoing rotation groups of the CPS sample because only these groups are asked questions about earnings. All regressions are weighted using the appropriate CPS sampling weights.[12] Table 1 shows that our largest sample (i.e. when considering “Employed” as labor market outcome) consists of observations on 5,851,310 CPS responses from individuals ages 21 and older, including all observations in the monthly samples from January 2015 to April 2020. 60% of respondents are employed. Earnings are reported only for outgoing rotation groups; thus, the sample size is smaller for those outcomes. The share of all individuals who are deemed essential workers is 70.4%.

5.5.1 Difference in Differences Models

[12] We use the earnings study weights for analysis based on the earnings outcome, and the final CPS sampling weight for all other analyses.

Table 1 Panel A reports estimates from two-way fixed effects regressions of CPS labor market outcomes on the DID policy interactions, individual covariates, state fixed effects, month fixed effects, and month-by-year fixed effects. The SAH measure gives the number of days that a SAH order was in place as of April 12, 2020, and the ABC measure gives the number of days that restaurants or other businesses closure mandates were in place as of April 12, 2020. The DID estimate is the coefficient on the interaction of these policy variables with a dummy variable for April 2020.

The first column of Table 1 suggests that both SAH policies and ABC policies are associated with reduced employment levels. An additional 10 days of the SAH mandate is associated with a 1.7 percentage point decline in the employment rate, which is statistically significant at the p=0.05 level. The employment rate in the United States averaged 60% over the study period (see Table 1). Thus, adopting a SAH order for an extra 10 days reduced employment levels by about 2.83% relative to the mean. For ABCs, the effect on the employment rate is a 1.8 percentage point decline for every 10 days that state ABC orders were in effect. The demographics variables have reasonably sized and signed coefficients (not presented, available upon request): for example, employment peaks in the (excluded) 41-50 age group and is monotonically increasing in education.

As there is concern that those coded as absent but employed actually reflects a form of unemployment, the second column tests whether this measure increases due to state policy. We do not find statistically significant effects here: the coefficients have the expected sign but are small.

The third and fourth columns show estimates of the effects of social distancing policies on earnings. The point estimates in column (4), which include zero earnings for people who are not employed, are negative and not small. They indicate that an 10 extra days under a SAH policy is associated with 3% lower earnings, and an 10 extra days of ABC is associated with 5% lower earnings. At the same time, given the substantially smaller sample when studying earnings, neither estimate is statistically significant. Column (3) reports estimates for earnings that are restricted to people with positive earnings. These estimates differ markedly from the ones that include people with zero earnings. They actually show a small increase in earnings for those who are employed while social distancing. Though these estimates do not account for selection on the basis of unobservable characteristics, they suggest that there may not be large reductions in earnings for those who remain employed. Based on these point estimates, it is impossible to rule out the possibility that compensation is increasing due to supplementary pay for people who continue to work and experience risk of infection during the pandemic.

The fifth and sixth columns report estimates of the effects of the policies on measures of hours worked. In column (6), which includes people who are employed and people who are not employed (zero hours worked), the results indicate that SAH orders are associated with fewer hours of work.

Thus, an 10 additional days of a SAH order is associated with about a 0.5 hour reduction in hours worked. The estimate for ABCs is similar, but not statistically significant. Column (5) reports estimates that are restricted to people with positive hours. These estimates indicate that both policies are associated with more hours of work among those who remain employed, but the point estimates are not statistically significant at conventional levels. Overall, the estimates suggest that there may not have been large change in hours for those who retained their jobs.

Panel B of Table 1 separates effects of policies for essential and non-essential workers. The results indicate that, all else equal, people employed in essential jobs had substantially higher employment rates, lower rates of absence from work, higher earnings, and hours worked. In the case of employment, 10 days of SAH mandates is associated with a 1.9 percentage point fall in employment rates among non-essential workers. In contrast, among essential workers, a period of 10 additional days of state-at-home mandates is associated with a 1.2 percentage point increase in employment rates (-1.9 plus 3.1). Thus, SAH orders had a positive rather than a negative effect on the employment of essential workers compared to non-essential workers.

ABCs appear to reduce employment for non-essential workers; the interaction term with ABC is small and statistically insignificant, suggesting that business closures had similar effects on essential and non-essential workers. The estimates for absent from work (in column (2)) continue to be small.

As in the base specification, we find little evidence that social distancing policies affect earnings among people who continue to be employed, regardless of whether they were working in an essential industry. In contrast, we do find that, when we code earnings as zero for non-employed people, the adoption of ABC mandates reduces earnings substantially among non-essential workers and the effect is not offset for essential workers. In contrast, SAH mandates have little effect on earnings among non-essential workers, but the coefficient on the Essential × SAH × April interaction term is positive. This finding suggests that SAH mandates were actually associated with increases in earnings among essential workers, although these estimates do not account for selection on unobservables. Column (5) shows some evidence that SAH mandates increased hours worked among non-essential workers who remain employed. Column (6), shows an overall increase in hours among workers in essential industries.

One concern with a causal interpretation of our estimates may be that higher initial pandemic severity may have led to early social distancing policy adoption and to changes in employment-related outcomes.[13] Our descriptive examination of the variation in the number of days each policy (ABC and

[13] Another concern may be heterogeneous implementation of the policies across states, in which case our estimates are capturing an average effect of these differentially implemented policies. One source of heterogeneous policy implementation may be state political affiliation – Republican or Democrat – which has been noted to play an

SAH) had been in effect by April 12, 2020 (the CPS focal date for the April CPS), by the quartile of the early pandemic severity proxy measures does not support the policy endogeneity possibility that states with higher initial pandemic severity adopted ABC and SAH policies considerably earlier. We further investigated whether there were differential monthly trends in employment outcomes among states with early versus late adoption of ABC and SAH orders to test for any violation of the strict exogeneity assumption and common trend assumption of the basic DID model. Figure 8 shows the event study coefficients for each of employment, earnings, and hours worked last week, to more directly test for anticipatory effects in outcomes prior to policy adoption. Across all models, we generally find that the pseudo-DID pre- trend interaction terms are small and statistically insignificantly different from zero. These results again provide some support to the core assumptions of the DID framework we use throughout our analysis.

5.6 Role of Policy vs. Secular Changes?

Our DID estimates suggest that state social distancing policies did have important effects on employment outcomes. To put our DID estimates in context and estimate how much the state social distancing policies altered the trajectory of employment outcomes across the country in the Spring of 2020 beyond secular nationwide changes due to the shock of the pandemic, we used our generalized DID specification to compare realized employment rates with estimates of employment rates in April in the absence of state social distancing policies. Specifically, let $\hat{y}_{\mathrm{ismt}}$ be the fitted value for a labor market outcome for person i in state s in month m and year t from estimating equation (2). The fitted value includes the exposure specific impact of the social distancing policies in state s if state s had adopted the policies SAH_s and ABC_S days ago, as of the April CPS focal date t^*= April 12, 2020,[14] and provides a model-based estimate of what actually happened in the state. Next, let $y^*_{ismt} = \hat{y}_{\mathrm{ismt}} - \hat{\delta}_1(SAH_S \times April_{mt}) - \hat{\delta}_2(ABC_s \times April_{mt})$ be the estimated counterfactual outcome for person i in state s in month m and year t. The counterfactual outcome is simply the realized fitted value net of the state's policy effects. The counterfactual analysis is graphically summarized in Figure 9. The green line shows our estimates of realized national employment rates from January

important role in determining voluntary and mandated social distancing during the pandemic (Alcott et al., 2020). Throughout, our analyses include state fixed effects, which control for time-invariant differences across states. Political affiliation of the governor for 37 out of the 50 states and Washington DC did not change - i.e., 37 stated consistently had a Republican or a Democrat governor - throughout our study period. Appendix Table C.1 presents our CPS DID estimates with state fixed effects, excluding the fourteen states – AK, HI, IL, KS, KY, LA, ME, MI, NH, NJ, NM, NV, VT, and WI – where the political affiliation of the governor was not time constant during the study period, effectively controlling for strictness of NPI implementation across both major parties. From results presented in new, we see that these estimates are similar in sign and magnitude to our estimates with all 50 states and DC but are somewhat more noisily estimated using the smaller subset of states.

[14] $SAH_s = t^* - E_{SAH_s}$ is the number of days that the SAH policy had been in place as of the April CPS focal date and $ABC_s = t^* - E_{ABC_s}$ is the number of days that ABC laws had been in place in a state as of the April CPS focal date.

2019 to April 2020 ($\text{i.e.}, \hat{y}_{\text{ismt}}$), from which we note that from January 2020 to April 2020, the employment to population ratio for people over age 20 fell from 61% to 49%, a drop of 12 percentage points. The orange line in the graph shows our estimates of the employment rate in the absence of state SAH and business closure mandates (i.e., y^*_{ismt}). The two lines are identical until the social distancing policies are implemented in April 2020. The counterfactual line shows that if state social distancing policies were not in place, employment rates would have "only" fallen from 61% to 56% from January to April. This implies that state social distancing policies explain about 60% of the realized 12 percentage point decline in employment from January to April. The remaining 40% of the drop in employment comes from a secular shock that was shared across all states. These estimates are contingent on assumptions about common trends and the absence of pre-trends in labor market activity. Monthly event study analyses of the CPS data provide support for these assumptions, but monthly data cannot rule out the possibility of very rapid differential pre-trends that could have occurred after the March CPS but before state policy actions. High frequency data on several other work-related outcomes provide additional evidence that supports the absence of large pre-trends in employment outcomes.

6. Conclusion

Although the initial unemployment insurance claims showed steep increases from mid- March 2020 onward, questions remain regarding how much of the employment changes were due to state policy as opposed to federal policy (such as the CARES Act - Humphries et al. (2020); Faria-e Castro (2020)) or personal responses to the perceived risks. This article is the first study to provide a comprehensive assessment of whether the response was primarily in job loss rather than hours worked and earnings. Personal responses to protect oneself from virus spread could occur on the part of cautious employers and employees, due to state shut-down policies that prohibit businesses from conducting business in person, or from reductions in consumer demand due to perceived risks. Employment changes are also partially a result of economic activities that are difficult to translate into an online or otherwise modified format that avoid high risks of disease transmission.

The main aim of this paper is to look at the link between state social distancing policies and employment, hours, and earnings. We considered two policies - ABC and SAH mandates - that were widely adopted to curb the transmission of the virus and most directly disrupted economic activity. The US Census Bureau's Current Population Surveys are arguably the best large-scale, fast-release, public data for such analyses. However, the CPS survey frequency is only monthly, and the onset of COVID-19 led to extremely sudden changes in both labor market activity and state level public policy. Consequently, we started by examining several proxy indicators of labor market activity and related them to social distancing policies around closings.

We looked first at what could be learned from work activities using cell signal data. Here, we used data from Google Mobility that pertained to work. The Google Mobility index on movement in workplaces showed clearly that there was a decrease in levels and trends in work activity after states adopted stay at home mandates and business closures. ABC policies occurred at a time when consumer demand and labor markets were unexpectedly disrupted, early in the pandemic period. SAH policies, although the strongest in mandating closures, occurred towards the end of a state's shutdown sequence, when nationwide economic activity had already slowed down. Despite slight pre-policy trends in the Mobility data, these were mostly not statistically significant, and the break in trend clearly suggested that policies exerted some causal effect on outcomes. We see larger effects on mobility measures in states that adopted closures earlier. This could be because the later adopters were the more reluctant adopters or because activity had already slowed considerably before the late adoptions (i.e., the orders did not bind). To the extent that work was conducted remotely, it would not be picked up as employment that involved travelling to a work location. We also examined measures of unemployment insurance claims and a leading, high-frequency proxy for unemployment insurance claims: Google Trends data on searches related to unemployment (see Aaronson et al. (2020)). These estimates for work-related mobility, UI claims, and Google Trends search data on unemployment generally suggest that on top of nationwide disruption of employment, state social distancing policies themselves added to these effects.

Our main analysis is built around the Current Population Survey because it allows us to analyze a range of outcomes and specific groups. To study the effects of state policies, we leveraged differences in the time at which social distancing policies occurred and, hence, the amount of time that states were subject to closures between March 12 and April 12, 2020. Our DID estimates suggested that social distancing policies had clear employment effects: being under social distancing policies longer leads to lower employment. We assessed pre-trends using a month-by-month event study framework and did not find much evidence that social distancing policies were anticipated by differential labor market outcomes at the monthly scale. We also used the CPS to examine the effects of state policies on rates of absence from work, hours worked, and earnings. For the most part, we found effects on hours and earnings were driven by extensive margin changes associated with employment losses. We saw considerably smaller effects along the intensive margin among those who remain employed. When we look at subgroups, we saw that changes in employment were concentrated in non- essential jobs. These findings are intuitive given that closings (especially of businesses) targeted non-essential industries.

The COVID-19 pandemic has had enormous consequences for the level of economic activity in the United States and other countries around the world. It seems clear that at least a large share of the decline in employment and the decline in economic activity was caused by the public health shock itself. However, the social distancing policies adopted by state governments trying to control the outbreak have had large consequences. SAH and business closure mandates almost certainly affected the level of economic activity at some point and on some margin. A basic question is how much of the economic disruption from the pandemic comes from individual and group responses to the public health threat posed by the virus, and how much comes from the public policies governments are using to control the pandemic? Analysis of cross-state variation in new unemployment insurance claims in early March suggested that the spike in job losses was nationwide and that differences in state school closure policies and in the severity of state pandemics had a comparatively small effect (Lozano-Rojas et al., 2020).

In this work, we examined labor market outcomes using richer data with a longer follow up time. Our DID estimates suggest that state social distancing policies were associated with important changes in employment outcomes, explaining 60% of the 12-percentage point decline in employment rates between January and April 2020, with the remaining 40% being driven by nationwide shock.

The results of this study can be considered in the context of several subsequent studies that have examined the relationship between state social distancing policies and labor market outcomes. Since the publication of our initial working paper, other studies have corroborated our conclusions (refer to Gupta, Simon and Wing (2020), and references therein for an early review of the related literature). On average, the literature notes a modest 2–8 percent increase in UI claims and net hours worked due to state policies, with business closures having a larger effect than SAH orders (Kahn et al., 2020; Kong and Prinz, 2020; Lozano-Rojas et al., 2020). Further in line with our study, subsequent literature has noted larger declines in employment in states that adopted closure policies earlier (Crucini and O'Flaherty, Crucini and O'Flaherty), with workers deemed non-essential being disproportionately affected (Buera et al., 2021).

Although these studies, including the current study, have tried to pin down the effects of the different closure policies on employment and other relevant outcomes, it is important to bear in mind that multiple closely timed and overlapping social distancing policies were implemented at the onset of the COVID-19 pandemic in the Spring of 2020.[15] Despite efforts to understand the order and timing of the sequence of policies and the implementation of multiple policy event-study designs to separate out their effects, it may be infeasible to fully disentangle the effects of the ABC and SAH policies. Given that the ABC orders were adopted early, the larger impact we find of these policies may indicate that initial policy changes convey greater information regarding the pandemic to employers or workers, or that employers or workers may simply react more to the earliest policies, whereas more restrictive policies like SAH orders happened relatively late. Alternatively, larger

[15] Mean days between state SAH and ABC policies was 9.3 days. Median days between state SAH and ABC policies was 9 days.

estimated effects of the earlier orders on employment outcomes could be interpreted as reduced-form impacts of the sequence of state closure policies. In either case, our DID estimates indicate that social distancing policies contributed substantially to recent job losses in addition to the economic slowdown caused by the threat of the virus itself, and it is now clear that state reopenings have not fully reversed economic losses associated with the Spring 2020 shutdowns. Studies find that official state reopenings at the end of April-early May 2020 have contributed a modest 0-4% increase in employment and slowed down further job losses among those employed (Cheng et al., 2020; Chetty et al., 2020; Hall and Kudlyak, 2020). Moreover, many of those who were reemployed appear to have returned to their previous employment, with the rate of reemployment decreasing with time since job loss. In the meantime, our finding that loss of employment was concentrated at the extensive margin allows displaced workers to continue seeking unemployment benefits. Despite the economic hardship, research shows that social distancing reduced disease transmission and deaths, and since the rollout of state vaccination campaigns, it is now important to understand the ways that states can work towards recovering labor markets and economies while continuing to balance public health risks that remain.

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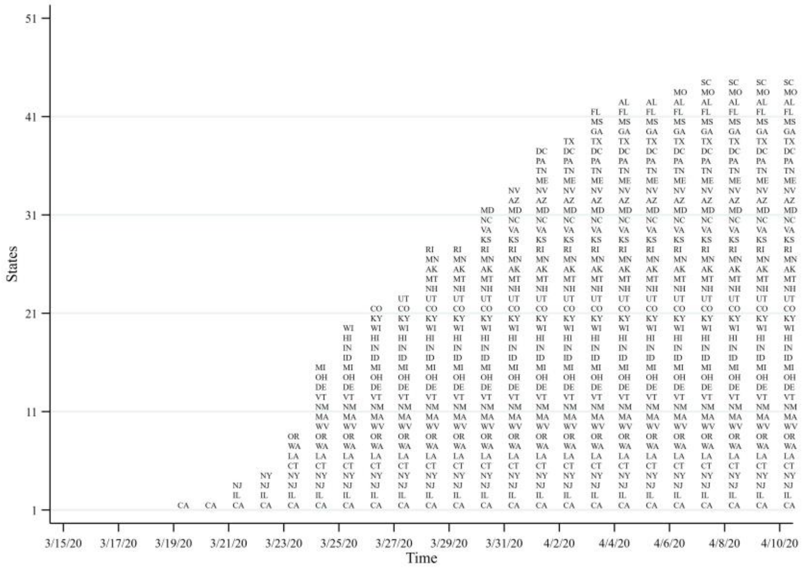
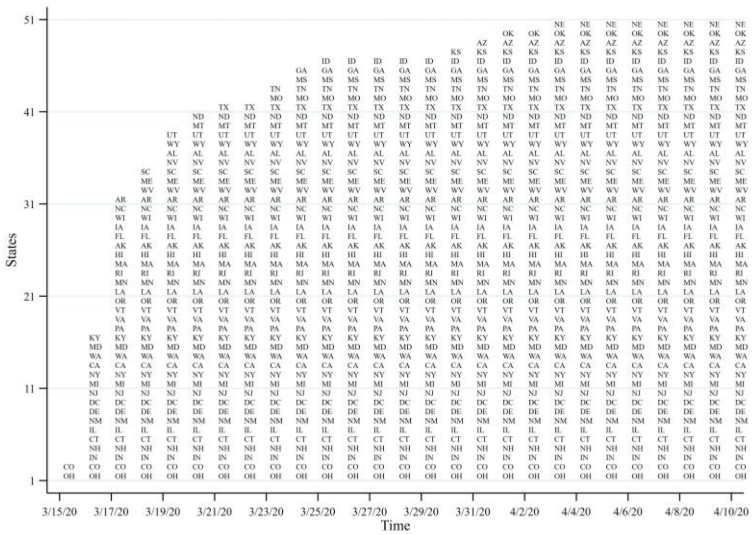
Tables and Figures

Figure 1: Timing of any business closures (ABC) and stay-at-home (SAH)

(a) Mandatory or recommended SAH

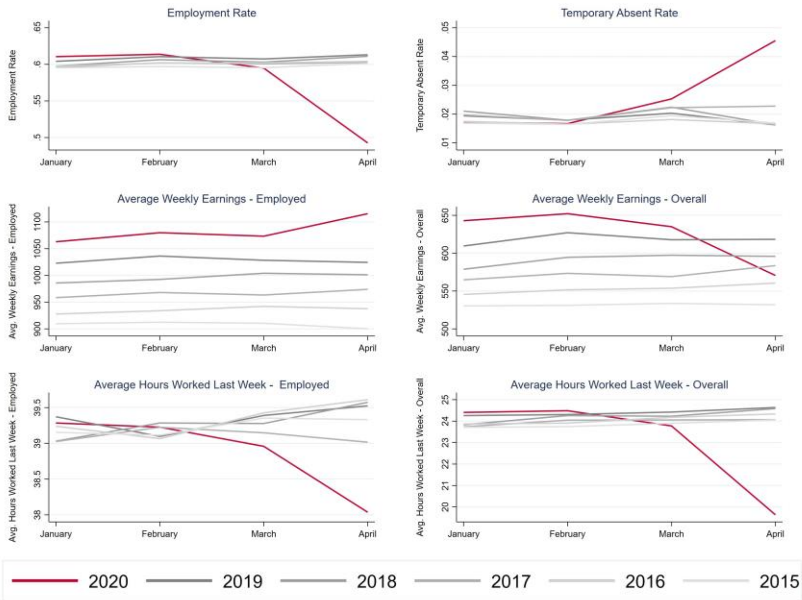


(b) Mandatory or recommended ABC



Notes: Authors' compilations based on Fullman et al. (2020).

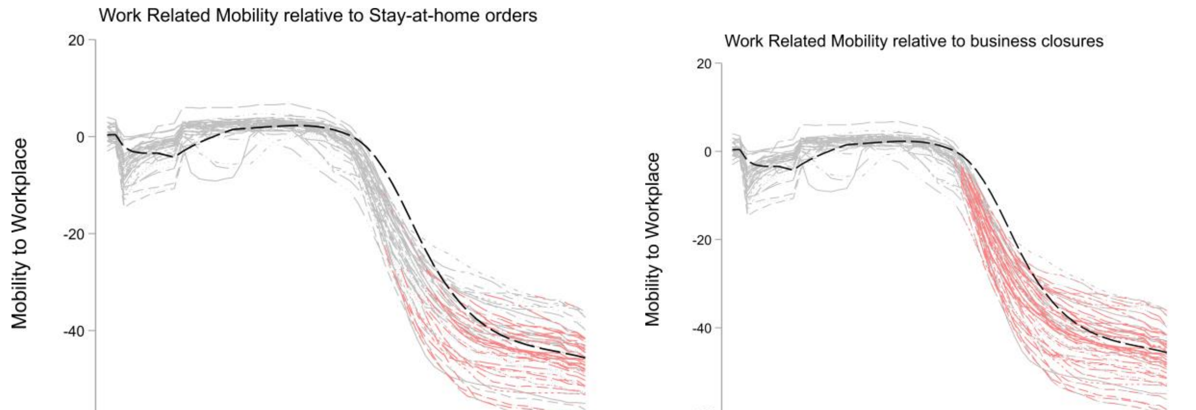
Figure 2: Deviation from Historical Trends: Labor market outcomes series, January-April, 2015-2020.



Notes: Authors' calculation based on the Current Population Survey January – April, 2015-2018.

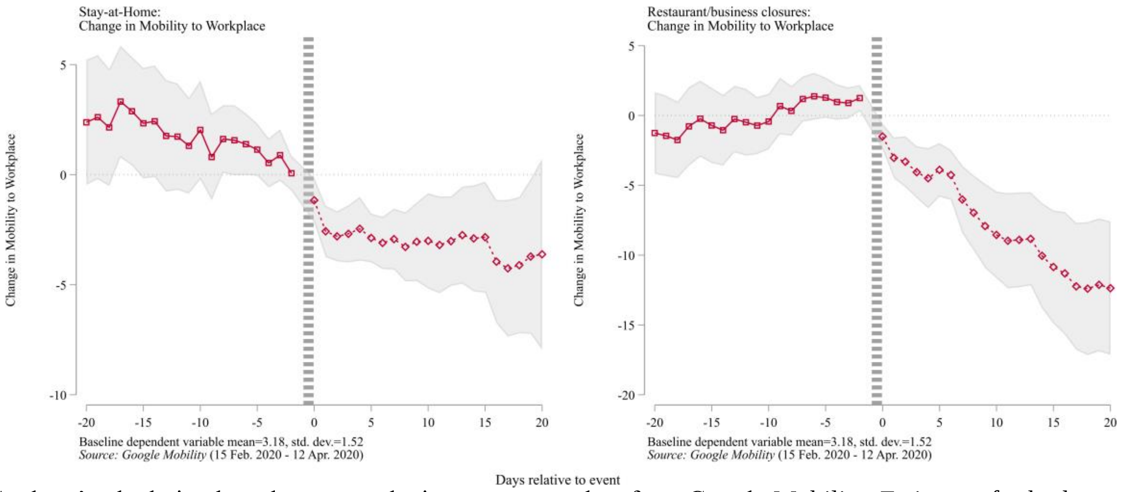
Figure 3: Trends in work-related mobility changes

(a) Mobility to workplace following SAH (b) Mobility to workplace following ABC



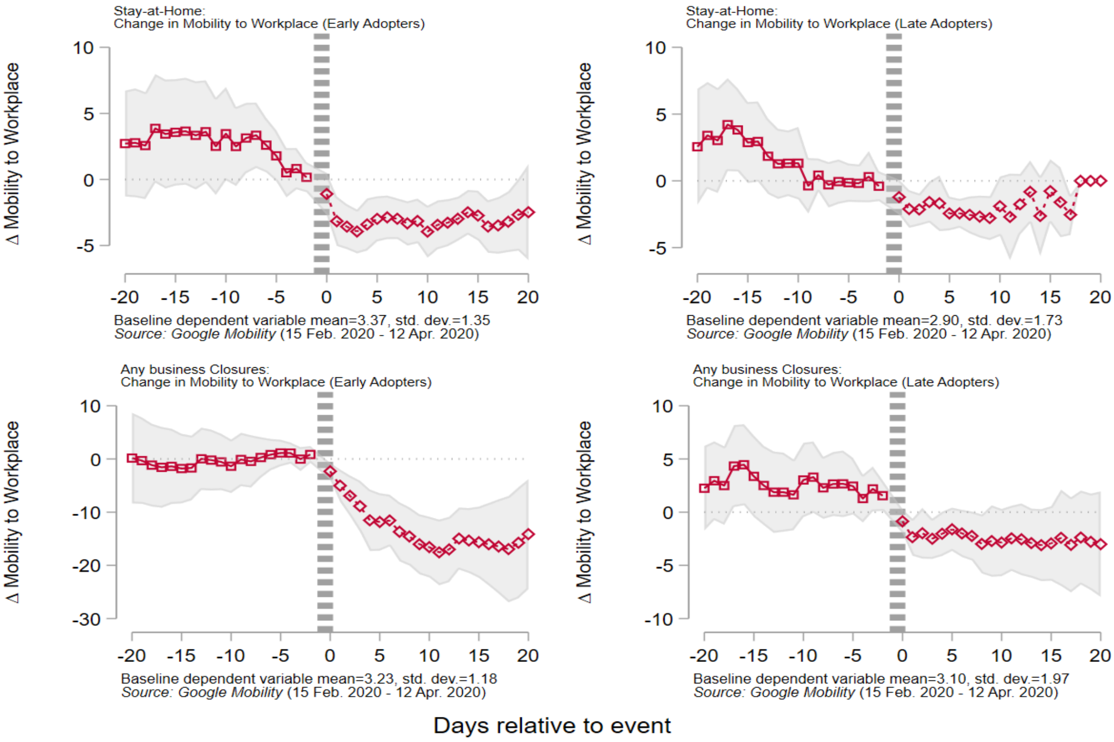
Notes: Author's calculation based on Google Mobility index smart device data. Each grey line represents a state. Grey lines turn red once SAH/ABC orders turn on in the state. The thick black line represents a "smoothed" 7-day moving average of the states.

Figure 4: Effects of ABC and SAH orders on workplace related mobility



Notes: Authors' calculation based on smart device movement data from Google Mobility. Estimates for both panels are from a single regression, which estimates event studies for both policies simultaneously. Estimation sample window is February 15, 2020 - April 12, 2020 for Google Mobility cellphone aggregate data. Baseline means as of February 15, 2020.

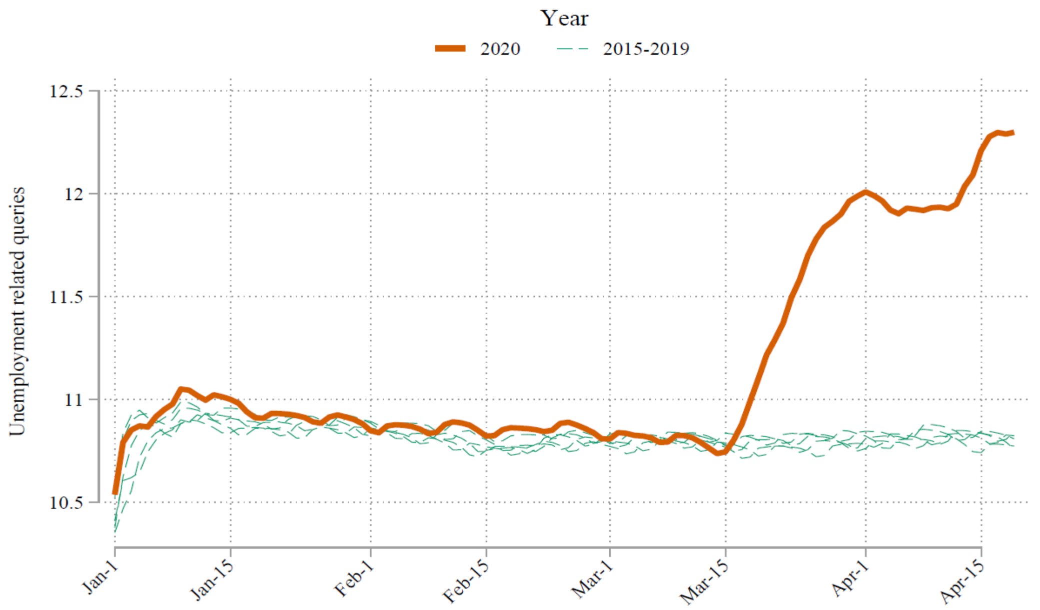
Figure 5: Effects of restaurant/business closures and stay-at-home orders on workplace related mobility. State-level heterogeneity analysis by duration of policy.



Notes: Authors' calculation based on smart device movement data from Google Mobility. Each panel is a separate regression. Early/late Stay-at-home order adopters are defined as those that implemented these orders more/less than the 18 days (national median) as of April 12, 2020, the focal April CPS date. Early/late ABC adopters are defined as those that implemented these orders more/less than the 26 days (national median) as of April 12, 2020. Estimation sample window is February 15, 2020 - April 12, 2020 for Google Mobility. Baseline means as of February 15, 2020.

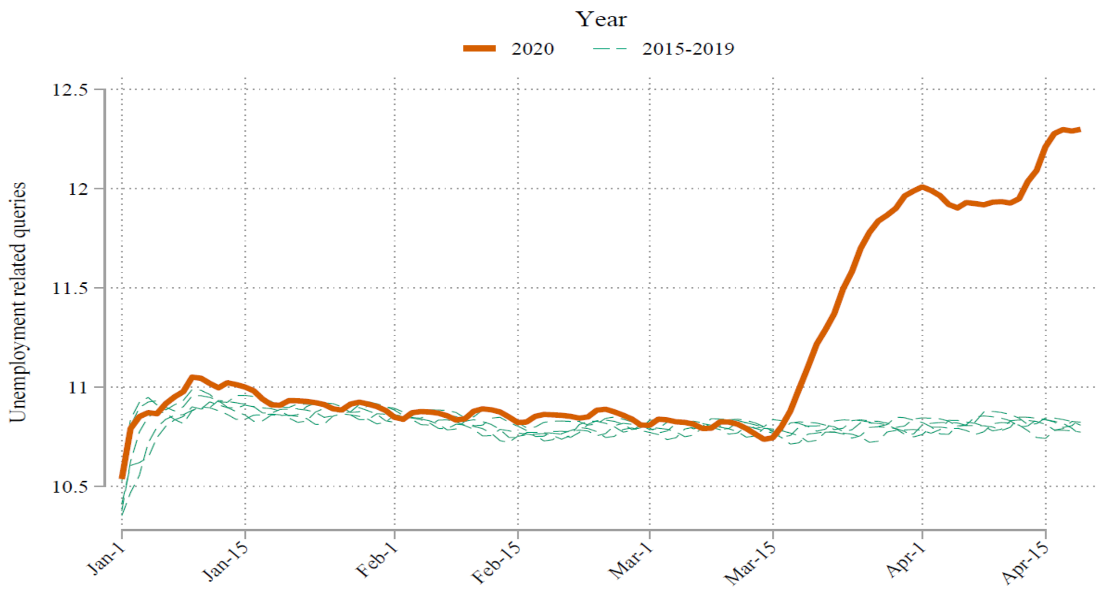
Figure 6: Deviation from Historical Trends on High Frequency Data: Google Trends Queries and Unemployment Insurance Claims

Panel A – Google Trends Queries



Notes: 7-day moving average of Google Trends log of queries per 10 million searches on unemployment related terms 2015-2020. In the analysis we specifically aggregate over the following individual terms: unemployment, stimulus, benefits, assistance, CARES Act, jobs, postings, Department of Labor, insurance claims, and claims. We present individual figures of trends and topics (if available) of these terms in Appendix A.1. Query accessed May 27, 2020 using Google Trends API, getTimelinesForHealth function of apiclient.discovery in Python.

Panel B – Unemployment Insurance Claims per 1000 Workers

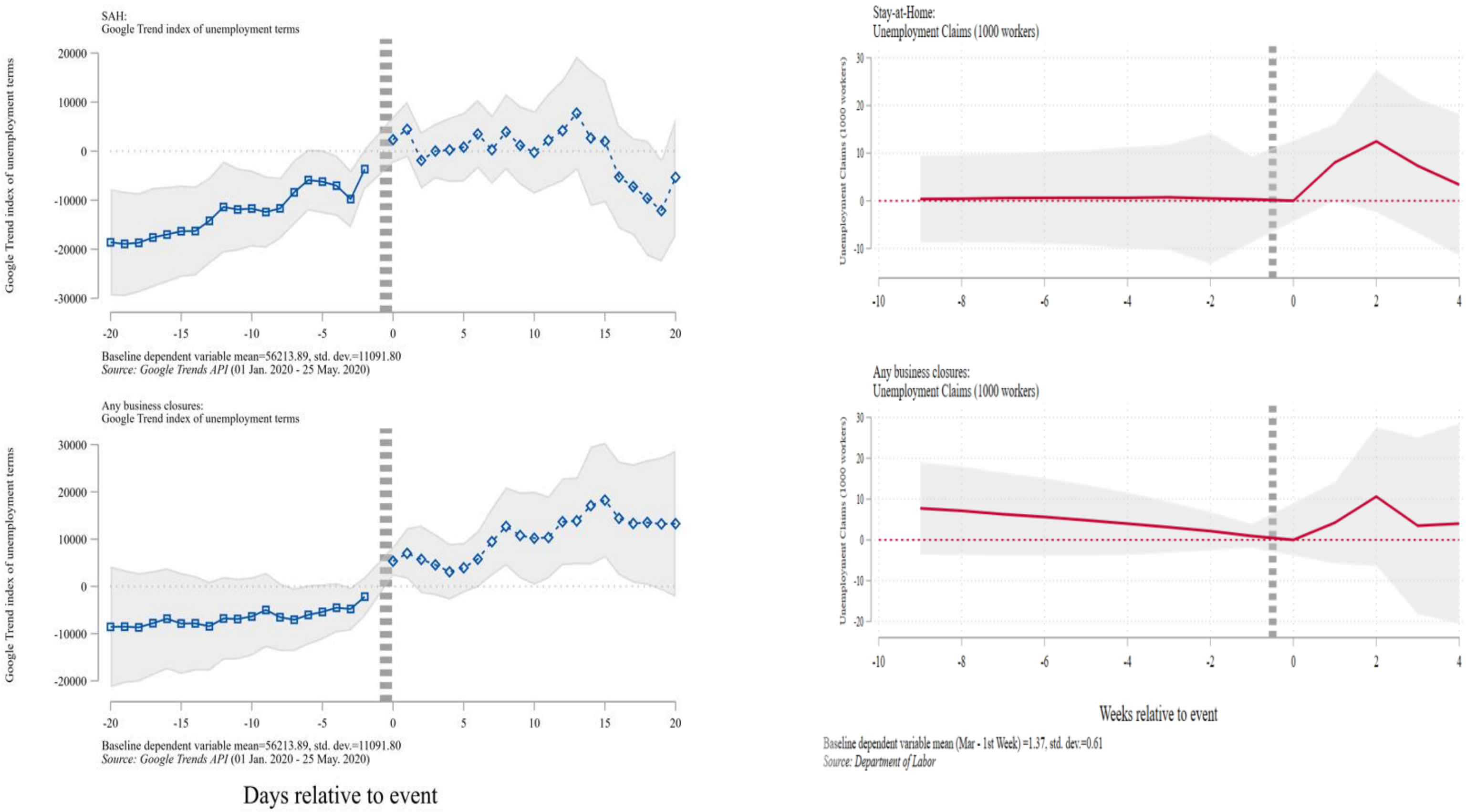


Notes: Weekly unemployment insurance claims per worker, 2015-2020. For any given year, the denominator is fixed on the covered employment during the first week of that year.

Figure 7: Effects of social distancing policies on unemployment-related internet search (left panel) and on Unemployment Insurance Claims per Worker (right panel).

(a) Google Trend Queries

(b) Unemployment Insurance Claim

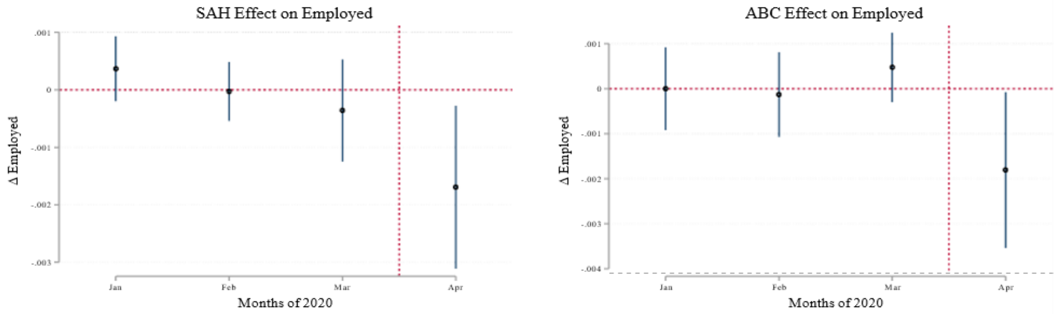


Note: The outcome is a measure of state-level daily searches for unemployment-related terms from Google Trends API (January 1 to May 25). The terms include unemployment, stimulus, benefits, assistance, CARES Act, jobs, postings, Department of Labor, insurance claims, and claims. The index reflects the daily share of all Google queries in a state that corresponds to unemployment-related terms (it multiplied by 10 million by Google).

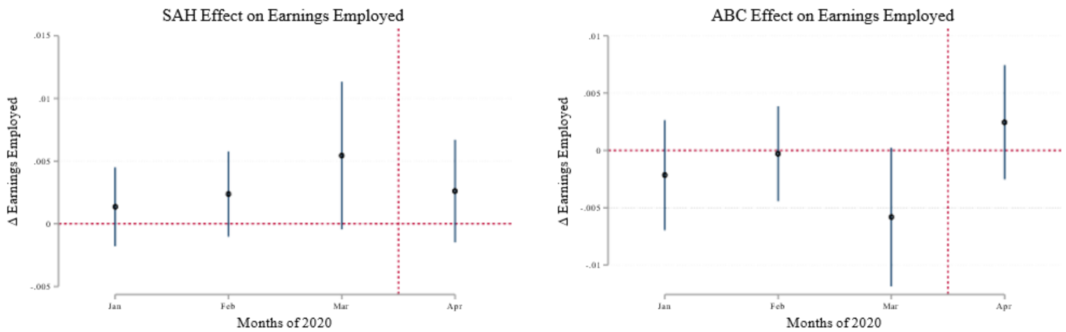
Note: Authors' calculation based on weekly reports on insurance claims from the Department of Labor. The results of the two panels come from the same regression analysis. The top panel represents the event time coefficients for the Any Business Closures measure. The bottom panel represents the coefficients for the Stay-At-Home orders.

Figure 8: Effects of Any Business Closure and Stay-At-Home orders on CPS Labor Outcomes: Employment, Earnings, and Hours Worked.

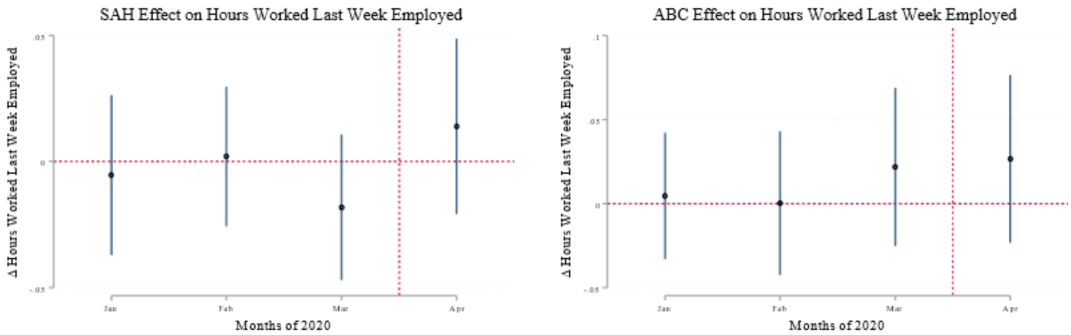
A. Employed



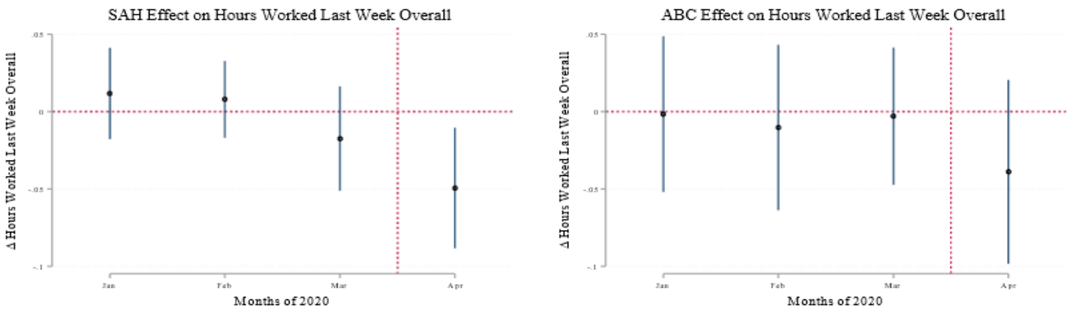
B. Earnings Employed



C. Hours Worked Employed

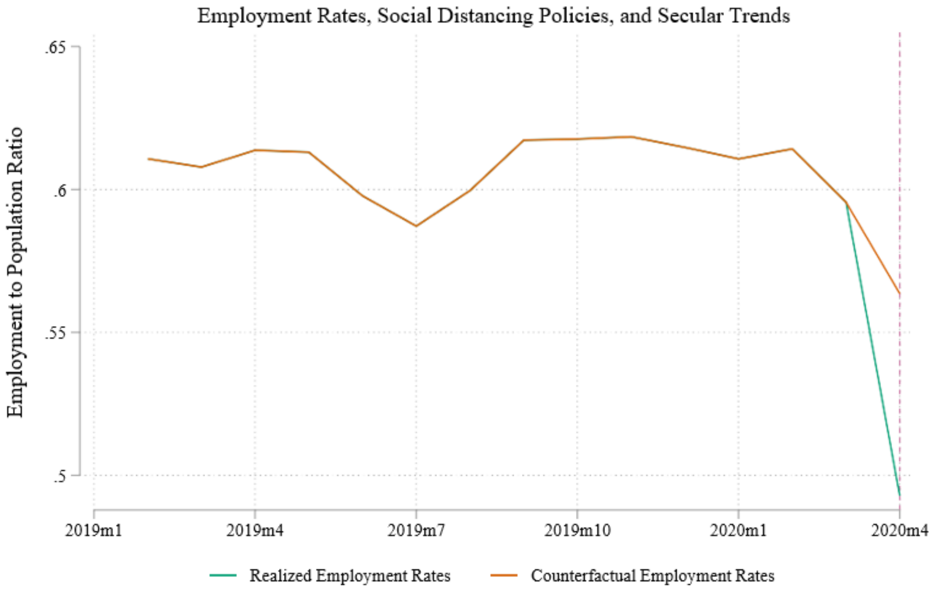


D. Hours Worked Overall



Notes: The coefficients plotted are obtained from running the CPS event study regression (model 3). The left panel of each row shows the coefficients for time indicators interacted with the number of days of State-at-Home orders in April. The panel on the right shows the analogous interaction for Any Business Closure. Observations from 2019 are used as a reference.

Figure 9: Differential in Employment rates due to Social Distancing Policiesy



Notes: Counterfactual employment corresponding to the baseline model presented in Table 1.

Table 1: Effects of social distancing policies on labor market outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Employed	Absent, - Empl.	Earn - Empl.	Earn - Overall	Hrs Last Wk	Hrs Last Wk - Overall
Mean	0.6000	0.0219	983.5	584.92	39.39	24.16
St. Dev.	0.4899	0.1464	691.80	719.57	12.47	21.52
Panel A: Baseline Analysis						
SAH x April	-0.0017**	0.0002	0.0025	-0.0031	0.0147	-0.0497**
	(0.0007)	(0.0003)	(0.0020)	(0.0056)	(0.0172)	(0.0197)
ABC x April	-0.0018**	0.0006	0.0026	-0.0050	0.0262	-0.0375
	(0.0009)	(0.0004)	(0.0025)	(0.0071)	(0.0245)	(0.0292)
Controls	X	X	X	X	X	X
R-squared	0.2624	0.0073	0.2305	0.3126	0.0732	0.2799
N	5,841,310	5,841,310	806,951	1,382,220	3,450,531	5,711,496
Panel B: Essential vs. Non-Essential						
SAH x April	-0.0019*	0.0004	0.0019	-0.0053	0.0584**	-0.0150
	(0.0011)	(0.0006)	(0.0055)	(0.0086)	(0.0254)	(0.0397)
ABC x April	-0.0042**	0.0014**	0.0037	-0.0212**	-0.0036	-0.1155**
	(0.0013)	(0.0005)	(0.0046)	(0.0099)	(0.0246)	(0.0437)
Essential x ABC x April	-0.0005	-0.0000	0.0011	-0.0038	-0.0593*	-0.0681*
	(0.0011)	(0.0005)	(0.0069)	(0.0110)	(0.0331)	(0.0405)
Essential x SAH x April	0.0031**	-0.0004	-0.0020	0.0187**	0.0389	0.1312**
	(0.0007)	(0.0003)	(0.0050)	(0.0074)	(0.0238)	(0.0241)
Essential Personnel	0.0196**	-0.0111**	0.1504**	0.2410**	1.7990**	2.0614**
	(0.0010)	(0.0006)	(0.0075)	(0.0148)	(0.0743)	(0.0876)
Controls	X	X	X	X	X	X
R-squared	0.0164	0.0103	0.2368	0.0885	0.0774	0.0717
N	3,755,517	3,755,517	806,951	876,962	3,450,531	3,625,703

Notes: Standard errors clustered at the state level in parentheses. The table presents the CPS analysis as described in Section 4 including interactions of policy exposure with Essential job classification. In the case of earnings and hours, the first in the pair of columns reports estimates conditional on employment, while the "overall" estimates treat people who are not employed as zeros. The set of control variables: Female, Having Child under 6 years old, Female x Having Child under 6 years old, Black, Hispanic, Age 21-25, Age 26-30, Age 31-40, Age 51-60, Age 61-70, Age 71+, Less than High School, Some College, Bachelor's Degree, Post Graduate Degree, Metropolitan Status. Significance levels: * $p < 0.1$, ** $p < 0.05$. The sample size for the Earnings variables is smaller because questions on earnings are asked only to the CPS outgoing rotation groups. The HIS Weekly Earnings (Overall) and the Tot. Hours Worked Last Week (Overall) have more observations than the HIS Weekly Earnings (Employed) and the Tot. Hours Worked Last Week (Employed) variables because the former replace zeros instead of missing values for all those individuals who are not employed. The weighted statistics for the employment outcomes are obtained from the observations in the basic monthly CPS from January 2015 to April 2020, and are weighted. For the earnings outcomes which refer only to the CPS outgoing rotations, a different set of weights is applied.

Appendix

Appendix A.

Table A1.1: Variation in days since policy adoption as of April 12, 2020, by early pandemic severity.

		Days Since Adoption - Stay at Home				Days Since Adoption - Any Business Closure			
	State Ranking	**Mean**	**SD**	**Min**	**Max**	**Mean**	**SD**	**Min**	**Max**
Case Rates	0-25 percentile (Very High Early Severity; ≥0.41 cases per 100k)	12.15	17.65	0	49	18.31	21.68	0	58
	26-50 percentile (High Early Severity; 0.21-0.40 cases per 100k)	12.42	17.80	0	50	19.85	22.87	0	57
	51-75 percentile (Low Early Severity; 0.14-0.20 cases per 100k)	15.35	19.18	0	52	20.04	22.97	0	57
	76-100 percentile (Very Low Early Severity; 0-0.13 cases per 100k)	12.38	18.76	0	54	18.38	22.92	0	58
Death Rates	0-10 percentile (High Early Severity; ≥0.01 deaths per 100k)	12.98	18.16	0	52	19.56	22.53	0	58
	11-100 percentile (Low Early Severity; <0.01 deaths per 100k)	13.69	19.10	0	54	17.00	22.31	0	58

Notes: States were ranked by their cumulative number of COVID-19 cases and deaths per 100,000 state population on March 15, 2020, as reported in the *New York Times* data[16] as measures of the early pandemic severity. The table summarizes the number of days each policy (ABC and SAH) had been in effect by April 12, 2020 (the CPS focal date for the April CPS) by the quartile of the early pandemic severity measures. Since nearly two-thirds of the states had not yet had their first confirmed COVID-19 death by mid-March, we only consider the first quartile or below in the case of death rates.

[16] https://github.com/nytimes/covid-19-data

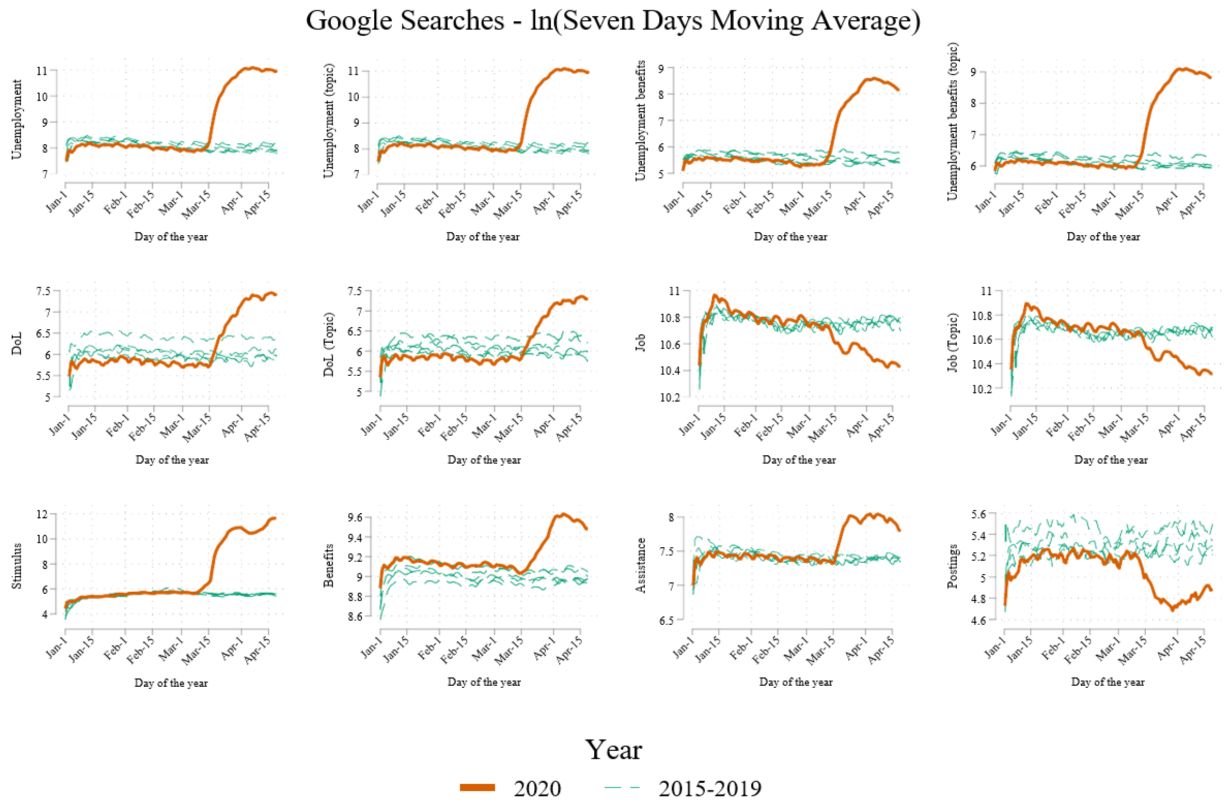
Appendix B: Google Searches

We pull data from queries related to unemployment and unemployment benefits as suggested in the Google Trend webpage, and we present it as such in Figure A1.1. Each sub-figure represents a series of the total number of searches in a state per each 10 million searches. We show results for searches for the following terms: "Unemployment", "DoL", "Stimulus", "Unemployment benefits", "Job", "Benefits", "Assistance", and "Postings".

The separate graphs in Fig B.1 display trends for different individual terms. Interestingly, searches for the term and topic "Job" actually decrease during the beginning of the outbreak. This might indicate a labor-supply-related change unique to this recession: individuals looking for a job might slow their job search, possibly due to fear of virus exposure or recognition of business closures.

From these queries we build a measure for the total unemployment related queries, the variable we use for the analysis presented in Figure 6 panel (a) and in the event study graph plotting Google search data, Figure 7 panel (a). To construct this specific measure, we aggregate all these individual unemployment-related terms to a state-level search index as the outcome. The terms: unemployment, stimulus, benefits, assistance, CARES Act, jobs, postings, Department of Labor, insurance claims, and claims.

Figure B.1: Deviation from Historical Trends: Google Trends Queries per 10 Million searches.

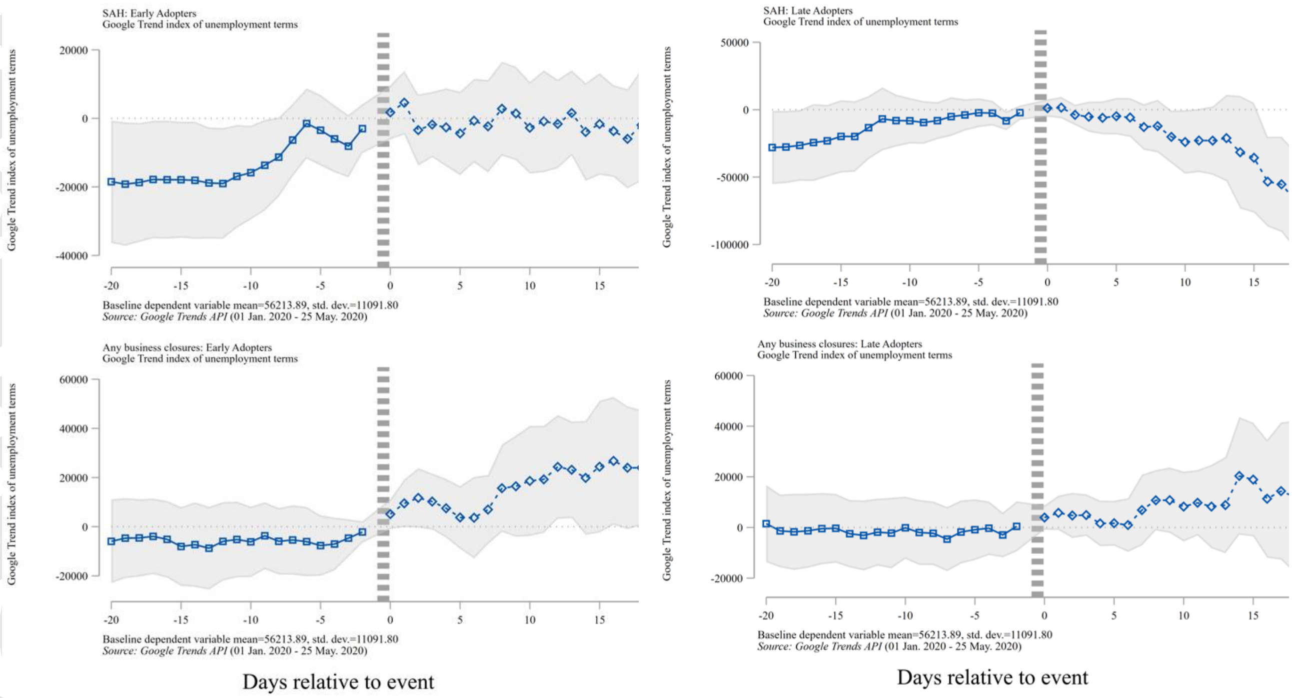


Notes: 7-day moving average of Google Trends log of queries per 10 million searches on unemployment terms and topics. The related topics, if available, were selected as suggested by the Google Trend webpage. For the analysis in this article (panel (a) in Figures 6 and 7), we specifically aggregate over the following individual terms: unemployment, stimulus, benefits, assistance, CARES Act, jobs, postings, Department of Labor, insurance claims, and claims. Query accessed May 27, 2020 using Google Trends API, getTimelinesForHealth function of api client.discovery in Python.

Appendix C:

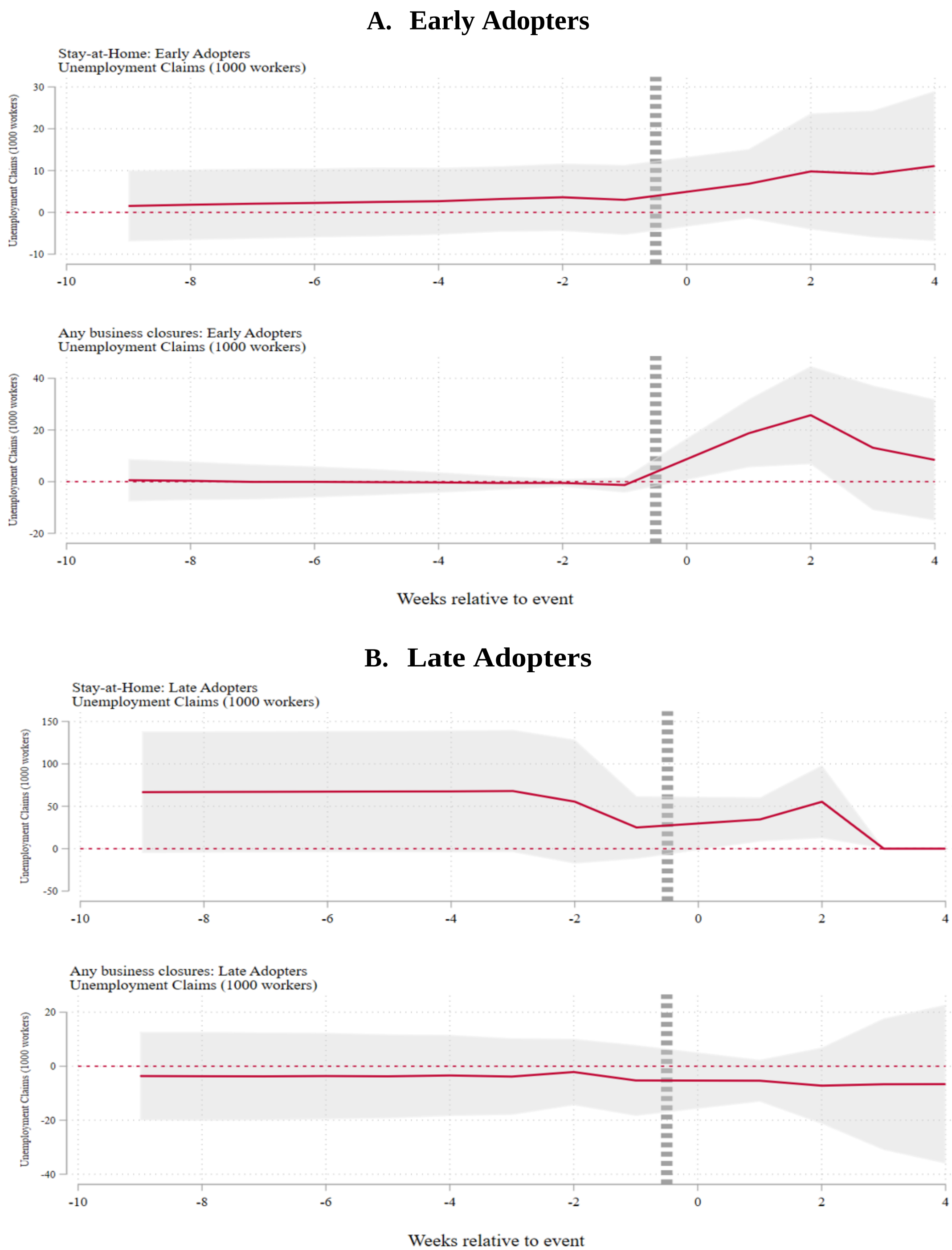
Figure C.1: Effects of restaurant/business closures and stay-at-home orders on unemployment-related internet search (left panel). State-level heterogeneity analysis by early vs. late policy adoption.

A. Early Adopters **B. Late Adopters**



Notes: The outcome is a measure of state-level daily searches for unemployment-related terms from Google Trends API (January 1 to May 25). The terms include unemployment, stimulus, benefits, assistance, CARES Act, jobs, postings, Department of Labor, insurance claims, and claims. The index reflects the daily share of all Google queries in a state that corresponds to unemployment-related terms (multiplied by 10 million by Google). Each panel is a separate regression. Early/late Stay-at-home order adopters are defined as those that implemented these orders more/less than the 17.5 days (national median) as of April 12, 2020, the focal April CPS date. Early/late ABC adopters are defined as those that implemented these orders more/less than the 26 days (national median) as of April 12, 2020. Baseline means as of February 15, 2020.

Figure C.2: Effects of restaurant/business closures and stay-at-home orders on Unemployment Insurance Claims per Worker (right panel). State-level heterogeneity analysis by early vs. late policy adoption.



Notes: Authors' calculation based on weekly reports of insurance claims from the Department of Labor. Each panel is a separate regression. The top panel represents the event time coefficients for each policy for early adopters (above the national median days since ABC (26 days) and SAH (17.5 days) policy adoption as of April 12, 2020). The bottom panel represents the coefficients for each policy for late adopters (below the national median days since ABC (26 days) and SAH (17.5 days) policy adoption as of April 12, 2020).

Table C.1. Effects of social distancing policies on labor market outcomes for the subset of states that consistently had a Democrat or Republican governor throughout the study period.

	(1) Employed	(2) Absent, Empl.	(3) Earn., Empl.	(4) Earn., Overall	(5) Hrs Last Wk	(6) Hrs Last Wk, Overall
SAH x April	-0.00146	0.0000398	0.00192	-0.00240	-0.00296	-0.0509**
	(0.001)	(0.000)	(0.002)	(0.007)	(0.014)	(0.023)
ABC x April	-0.00131	0.000611	0.00278	-0.00503	0.0386	-0.0145
	(0.001)	(0.000)	(0.003)	(0.008)	(0.024)	(0.031)
N	4508068	4508068	622624	1066358	2665015	4409316

Notes: The table presents the CPS analysis as described in the Methods section, for the substate of states that consistently had either a Republican or a Democrat governor for the full CPS study period of January-April, 2015-2020. Fourteen states - AK, HI, IL, KS, KY, LA, ME, MI, NH, NJ, NM, NV, VT, WI – where the political affiliation of the governor switched during the study period were excluded from these analyses. Standard errors clustered at the state level in parentheses. In the case of earnings and hours, the first in the pair of columns reports estimates conditional on employment, while the overall estimates treat people who are not employed as zeros. The set of control variables: Female, Having Child under 6 years old, Female x Having Child under 6 years old, Black, Hispanic, Age 21-25, Age 26-30, Age 31-40, Age 51-60, Age 61-70, Age 71+, Less than High School, Some College, Bachelor's Degree, Post Graduate Degree, Metropolitan Status. The sample size for the Earnings variables is smaller because questions on earnings are asked only to the CPS outgoing rotation groups. The HIS Weekly Earnings (Overall) and the Tot. Hours Worked Last Week (Overall) have more observations than the HIS Weekly Earnings (Employed) and the Tot. Hours Worked Last Week (Employed) variables because the former replace zeros instead of missing values for all those individuals who are not employed. The weighted statistics for the employment outcomes are obtained from the observations in the basic monthly CPS from January 2015 to April 2020 and are weighted. For the earnings outcomes which refer only to the CPS outgoing rotations, a different set of weights is applied
* p < 0.10, ** p < 0.05, *** p < 0.01