Job Losses During the Onset of the COVID-19 Pandemic: Stay-at-home Orders, Industry Composition, and Administrative Capacity*

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Abstract

Six weeks after the national emergency declaration for the COVID-19 pandemic, unemployment insurance (UI) claims as a share of total state employment differed by as much as 24 percentage points across states. This paper examines three explanations for these dramatic differences: statewide stay-at-home orders, the industry composition of states' employment, and the historical utilization of states' UI systems. We find that i) the surge in UI claims began and often peaked before states enacted stay-at-home orders; ii) relative to other states, states that would eventually enact stay-at-home orders had high UI claim rates before the orders were enacted; iii) six weeks after the national emergency declaration, statewide stay-at-home orders accounted for less than 30% of the difference in cumulative initial UI claims between states-that-ever versus states-that-never enacted statewide stay-at-home orders; iv) industry-related exposure to the COVID-19 pandemic can account for twice as much of the state-level variation in UI claims relative to stay-at-home orders; and v) states with greater historical utilization of their UI systems by unemployed workers tended to both receive more UI claims and process these claims more quickly at the onset of the COVID-19 pandemic.

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1 Introduction

The COVID-19 pandemic has caused unemployment in the U.S. to rise to levels not seen since the Great Depression. In the second week following the March 13, 2020 declaration of a national emergency, workers filed initial unemployment insurance (UI) claims equal to 4.1% of covered employment in the United States — 28 times the average weekly initial UI claims in the previous year and nearly six times the peak during the Great Recession. By May 2nd, just seven weeks after the national emergency declaration, 30.5 million workers in the U.S. had filed initial claims for unemployment insurance. As state and local governments weigh the health benefits of stay-at-home orders enacted in order to slow the spread of COVID-19 cases against their potential economic costs, understanding the determinants of this historic rise in unemployment is critical.

We use states' daily and weekly UI claims data to show that states' stay-at-home orders occurred too late to be the drivers of the early surge in unemployment insurance claims.[1] Every state's initial UI claims soared during the three weeks following the national emergency declaration, no matter whether the state never enacted, had yet to enact, or was one of the first to enact a statewide stay-at-home order. These surges occurred so rapidly that many states' initial UI claim rates peaked before or in the same week that their stay-at-home orders were enacted. While states that did enact stay-at-home orders tended to have larger increases in their weekly initial UI claims and insured unemployment rates, these differences were already apparent prior to states' implementation of the stay-at-home orders. We estimate that four weeks under a statewide stay-at-home order causes the cumulative initial UI claims to rise by approximately 2.1 percentage points. To put this into perspective, six weeks after the national emergency declaration, the standard deviation of cumulative initial UI claims across states was 6.0 percentage points and there was a 7.3 percentage point difference in the average cumulative initial UI claims of states that ever-versus-never enacted stay-at-home orders.

Given that large differences in states' initial UI claims emerged before states enacted their stay-at-home orders and that these stay-at-home orders can only account for a small amount of the cross-sectional differences in states' UI claims, we study the role of other factors in explaining the substantial variation in initial UI claims across states. We show that two sources of pre-existing

[1] Our finding that UI claims surged prior to states' stay-at-home orders is consistent with the findings of other studies that states' stay-at-home orders occurred too late to drive the economic dislocation in consumer spending (Chetty et al. (2020)), store foot traffic (Goolsbee and Syverson (2020)) job postings (Kahn et al. (2020)), and hours worked (Chetty et al. (2020)).

variation — industrial composition and the utilization of states' unemployment insurance systems by unemployed workers – explain approximately one-fifth of the differential surges in states' UI claims during the first six weeks of the COVID-19 pandemic.

The COVID-19 pandemic has affected some industries more severely than others, and we find that industries in which jobs require more customer interaction and are harder to do from home saw larger surges in UI claims. For instance, the two hardest hit industries, Lodging & Food and Entertainment, saw total initial UI claims over the first seven weeks exceed 40% of industry employment.[2] In these two industries, few workers can work from home and most jobs require significant customer interaction.[3] Other industries such as Finance, Education, and Management saw milder disruption, with cumulative initial claims over same period totaling less than 10% of employment. These dramatic differences in job loss across industries have two important implications. First, this industry variation in initial UI claims helps explain why particular demographic groups have experienced worse job losses — as women, minorities, low-income workers, and younger workers are more likely to be employed in the hardest hit industries (see Alon et al. (2020) for a deeper investigation of this point). Second, the differences in the timing and magnitude of industries' surges in initial UI claims, in combination with variation in the industry composition of states' employment, can account for approximately 10% of the cross-sectional variation in states' initial UI claims in the first six weeks following the national emergency declaration.

We also examine the role of historical differences in the utilization of states' unemployment insurance systems by unemployed workers. We find that states with higher historical UI utilization rates tended to have larger surges in UI claims and these surges tended to occur more rapidly following the national emergency declaration. We present evidence that the front-loading of claims in states with higher historical UI utilization rates may reflect more severe processing delays in states with lower historical UI utilization rates. In particular, we find that the gap between the number of continuing UI claims and the accumulated initial UI claims is smaller in states with higher UI utilization rates — which implies a faster processing of the initial UI claims.

A key contribution of this paper is our evidence that the dynamics of states' initial UI claims following the national emergency declaration violate the assumptions required by many of the

[2]On average across the 24 states for which we have initial UI claims data at the level of 2-digit NAICS sectors.

[3]Our industry level finding is consistent with the studies that consider the association between unemployment during the onset of the COVID-19 pandemic and differences across occupations in the degree of customer interaction and the ability to work from home (see Brynjolfsson et al. (2020); Bartik et al. (2020); Mongey and Weinberg (2020); Mongey et al. (2020); Montenovo et al. (2020)).

standard econometric tools used to assess policy impacts. First, our finding that the surges in states' initial UI claims had already peaked (or were close to their peak) before many states announced or enacted their stay-at-home orders casts doubt on the key assumption required by event studies — that no other factors were affecting UI claims around the time of states' stay-at-home orders. Second, our finding that the relatively larger surges in UI claims in states with stay-at-home orders were already apparent before states enacted their stay-at-home orders indicates a violation of the parallel trends assumption required by difference-in-differences analyses.[4] These violations of the assumptions underlying these common methods may help explain explain recent studies' conflicting conclusions as to whether stay-at-home orders increased (Baek et al., 2020; Coibion et al., 2020; Gupta et al., 2020), decreased (Lin and Meissner, 2020), or had no effect (Kong and Prinz, 2020) on states' UI claims in the first few weeks following the national emergency declaration.[5]

This paper also contributes to the literature documenting the differing economic impact of the COVID-19 pandemic across industries, occupations, and demographic groups. In brief, there is evidence that the economic impacts of the pandemic have been more severe for workers with lower income (Cajner et al., 2020; Montenovo et al., 2020), less educated workers (Béland et al., 2020; Montenovo et al., 2020), women (Adams-Prassl et al., 2020) and non-white workers (Montenovo et al., 2020). Studies have also shown that an important mechanism driving the heterogeneous incidence of the COVID-19 shock across occupations are the degrees to which the work both can be done remotely and requires customer interaction (Mongey and Weinberg, 2020; Brynjolfsson et al., 2020; Alon et al., 2020; Béland et al., 2020; Montenovo et al., 2020). We find these job characteristics are similarly important for explaining differences in initial UI claims across industries. Furthermore, we find that differences in the industry composition of states' employment explained nearly twice

[4] Although Goodman-Bacon and Marcus (2020) focus on health-related outcomes, much of their discussion of the potential pitfalls of difference-in-differences designs when estimating the effects of state policies related to COVID-19 also relates to UI claims and other labor market outcomes. When Gupta et al. (2020) estimate their difference-in-differences analysis of the effect of the stay-at-home orders on states' unemployment rates using the monthly CPS, they acknowledge potential violations of the parallel trends assumption. They evaluate potential violations of the parallel trends assumption using Google searches for unemployment insurance, weekly initial UI claims, and state-level mobility data, and find mixed evidence on the presence of differences in pre-trends. We argue that there is strong evidence of differences in the pre-trends between states-that-ever versus states-that-never enacted stay-at-home policies.

[5] Lin and Meissner (2020) find that initial UI claims fell in the weeks after states implemented stay-at-home orders in event study regressions. Baek et al. (2020) use within-state variation in stay-at-home orders, finding that an additional week of a stay-at-home order increased county-level initial UI claims by 1.9% of employment in the first three weeks of the pandemic. Kong and Prinz (2020) uses high-frequency data on web searches for unemployment insurance, showing that non-essential service closures can account for increased search activity, but not school closures or stay-at-home orders. Rojas et al. (2020) find that school closures are associated with increased UI claims, though the effect is small relative to the common rise in UI claims across all states. Lastly, Gupta et al. (2020) use a difference-in-differences design with monthly CPS data to estimate that stay-at-home orders and non-essential service closures can account for 60% of the decline in employment between the January and April CPS.

as much of the cross-sectional variation in states' initial UI claims relative to states' stay-at-home orders. We also document an additional source of state-level heterogeneity in the administration and accessibility of unemployment insurance systems. Our findings suggest that greater historical utilization of a state's UI system is associated with faster receipt of initial UI filings and fewer delays in processing of claims, which generates significant cross-state differences in workers' access to the UI system.

2 Data

The primary outcome we study in this paper is unemployment insurance claims as reported by the BLS, state unemployment insurance agencies, and other sources without any seasonal adjustment. Although UI claims are an imperfect measure of unemployment and job loss,[6] they are the focus of this paper because they are reported at a weekly frequency (and, in some states, at a daily frequency). The high frequency of the UI claims data allows us to exploit variation in the timing of states' stay-at-home orders, all of which occurred within the three weeks between March 19th (California) and April 7th (South Carolina). Our primary source for both initial and continuing UI claims is the unemployment insurance weekly claims data from the Employment & Training Administration of the U.S. Department of Labor (2020).

We augment the state-level weekly UI claims data from the U.S. Department of Labor with UI claims data from state governments, the Upjohn Institute, and the California Policy Lab. Specifically, we obtain daily initial UI claims counts for eleven states, as well as Washington DC, that have reported this data during the COVID-19 pandemic.[7] We also obtain industry-level UI claims data for 24 states that break down the weekly initial UI claims by the industry of the claimants' dominant employer.[8]

[6]Three primary concerns regarding using UI claims as a proxy for unemployment. First, the eligibility criteria and generosity of the UI system differs across states, which results in differential UI claim rates across states. This weakens the cross-state comparability of both initial UI claims as a measure of job losses and continuing UI claims as a measure of unemployment. Second, since job tenure and compensation differ across industries, workers' eligibility for UI will also differ across industries. As a result, the incidence of UI receipt by unemployed workers will systematically differ across industries. And third, the unprecedented volume of new UI claims during the COVID-19 pandemic has stressed states' UI offices such that many unemployed workers have faced significant challenges in filing UI claims, but the severity of these challenges have differed across states. As a result, the reported claim activity may lag the actual job loss and unemployment, and the length of this lag may differ across states. See Rinz (2020) for a more detailed description of the UI claims data and how they relate to unemployment during the pandemic.

[7]The ten states reporting daily initial UI claims are: California, Florida, Minnesota, Montana, North Carolina, North Dakota, Nevada, Pennsylvania, Rhode Island, Texas, and Wisconsin.

[8]The 24 states reporting weekly industry-level initial UI claims data are: Alabama, California, Colorado, Connecticut, Florida, Georgia, Iowa, Idaho, Indiana, Kansas, Kentucky, Louisiana, Massachusetts, Maine, Michigan, North Dakota, Nebraska, New York, Oregon, Rhode Island, Texas, Utah, Virginia, and Washington. We exclude Montana,

Due to delays in the recording of claims and, in the case of the industry-level data, missing industry classifications for some employers, the state-level total of initial claims summed across days of the week or across industries typically differs from the weekly state-level initial UI claims reported by the U.S. Department of Labor. In such cases, we proportionally adjust the reported daily or industry claims such that the total initial claims aggregated across days of the week or industries matches the initial claims reported by the U.S. Department of Labor. We then normalize the daily state-level or weekly industry-level initial UI claims data. For the daily state-level initial UI claims, we normalize by the count of total insured employment in the state, yielding a measure of the share of insured employment filing UI claims on each day. For the weekly industry-level initial claims data, we normalize by the industry-level employment from the Quarterly Workforce Indicators (U.S. Census Bureau (2020)) as of the end of 2019:Q1 — resulting in a measure of the share of the state's total employment in the industry that filed initial UI claims in that week.[9]

We construct a proxy measure of the degree to which UI eligibility criteria differ across states by calculating the ratio of each state's insured unemployment rate on February 15, 2020 (calculated as the ratio of continuing claims to insured employment in the state) relative to the state's unemployment rate in February 2020 as reported by the Local Area Unemployment Statistics program of the Bureau of Labor Statistics (2020).[10]

For the timing of states' stay-at-home orders, we use the aggregated data from the *New York Times* (Mervosh et al. (2020)). In any instance where the state order took effect at 9pm or later, we date the order as taking effect the following day.[11]

Pennsylvania, and Wyoming because they report initial UI claims at higher level aggregations than 2-digit NAICS sectors. New Mexico and Nevada report continuing UI claims by industry at a weekly frequency, but we choose not to use these since the change in continuing claims is a very noisy measure of initial UI claims due to the time it takes to process claims.

[9] In the case of Missouri and Montana we use the industry employment from the end of 2018:Q1 since this is the most recent data available. For Alaska, which lacks QWI employment after 2016, we use industry employment as of March 2019 from the BLS Current Employment Survey.

[10] We also considered an alternative, but similar measure — the ratio of continuing UI claims to the number of unemployed workers in each state. The measure we chose to use multiplies this ratio of continuing UI claims to unemployed workers by the ratio of the state's labor force to its insured employment. We prefer this measure because adding this interaction helps adjust for discrepancies between the location in which a continuing claims is reported (determined by the state in which the individual works) versus the state in which the unemployed worker is reported (determined by the residence of the worker). Some states have very large numbers of out-of-state workers (DC, NY, ND are examples of this), causing large discrepancies in this measure. Adjusting for the ratio of the labor force to insured employment helps mitigate this mismeasurement.

[11] This shifted the timing of the stay-at-home orders for: Ohio, Indiana, Minnesota, New Hampshire, New Jersey, and Tennessee.

3 Determinants of UI claims during the COVID-19 crisis

In the first week following the national emergency declaration, states received 17 times more initial UI claims on average than in the week prior. Although initial UI claims rose dramatically in all states (Kahn et al. (2020) document this common rise in UI claims), there were significant differences in the magnitude of the rises. Rhode Island, Nevada, and Pennsylvania were particularly hard hit as they received initial UI claims for 6-7% of total insured employment in that first week alone. Georgia, South Dakota, and Mississippi, on the other hand, received initial UI claims totaling less than 0.5% of covered employment in that same week. (These states' initial UI claim rates were still 4-10 times larger than the previous week.)

For most states, the surges in initial UI claims have declined from the peaks realized in the first three weeks of the pandemic, but remain high relative to historical levels. As a result, cumulative initial UI claims six weeks after the national emergency declaration exceeded 19% of covered employment in the United States. However, there is tremendous variation in the magnitude of cumulative claims across states. In Rhode Island, Georgia, Kentucky, and Hawaii, cumulative initial UI claims over the same period exceeded 30% of covered employment, while in South Dakota and Utah cumulative initial UI claims totaled 8.1% and 9.4%, the lowest of all states.

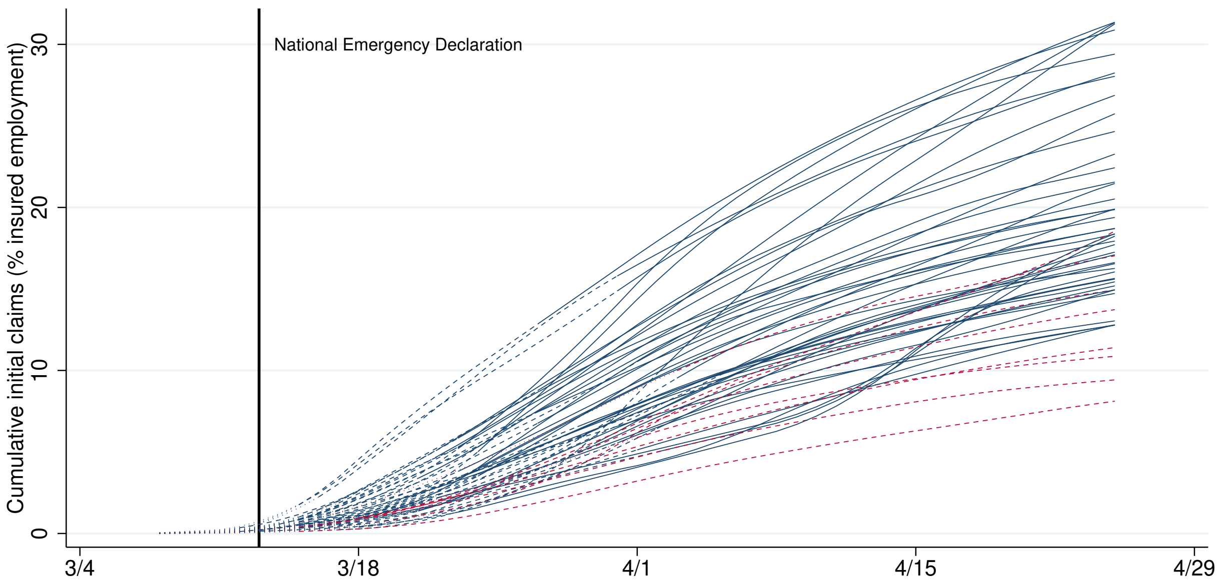
3.1 States' stay-at-home orders

Differences in whether and when states enacted statewide stay-at-home orders have been hypothesized as a driver of the large cross-state differences in the rate of initial UI claims since the onset of the pandemic. Over the three-week period from March 19th to April 7th, 42 states and the District of Columbia enacted statewide stay-at-home orders (see Appendix Section A for a map showing the timing of states' stay-at-home orders).

Figure 1 shows cumulative initial UI claims as a share of covered employment for each of the fifty states and the District of Columbia. The blue lines indicate states that enacted a stay-at-home order at any point, whereas the red lines correspond to states that never enacted such an order. The dashed lines indicate the period in which a state had closed schools but had not yet enacted a stay-at-home order. The solid lines indicate periods with stay-at-home orders in effect. We end the series on April 25, 2020 because some states began relaxing their stay-at-home orders in the last week of April.

An immediate take-away from Figure 1 is that states that never enacted stay-at-home orders

Figure 1: Large dispersion in state's cumulative initial UI claims



Notes: Cumulative initial unemployment insurance claims from 3/15 - 4/25/2020 as a share of insured employment (source: the Bureau of Labor Statistics and state governments). Blue indicates states that, at some point, had a statewide stay at home order. Red indicates states that never had a statewide stay at home order. Dotted lines (···) indicate periods without any school closure or stay-at-home order. Dashed lines (- -) indicate periods with school closures. Solid lines (—) indicate periods with stay-at-home orders in effect.

(red lines) tended to receive fewer initial UI claims over the first six weeks of the pandemic. A more subtle take-away, however, is that these differences were already apparent before states enacted their stay-at-home orders. This can be seen by examining the dashed red and blue lines, which indicate the periods when states had statewide school closures but not stay-at-home orders. The common rise in the dashed lines after the national emergency declaration shows that initial UI claims began their surge before states enacted their stay-at-home orders. Furthermore, the differences in the red and blue dashed lines show that, even before states began enacting their stay-at-home orders, the surges in initial UI claims were already larger in states that would eventually enact stay-at-home orders. Given that most stay-at-home orders took effect on the same day or the day immediately following their announcement, these pre-existing differences do not reflect announcement effects (but could reflect anticipation effects).

3.1.1 Stay-at-home orders enacted too late to drive the initial surge in UI claims

We find that stay-at-home orders had little role to play in the initial surge in UI claims in the first three weeks after the national emergency declaration since many of these stay-at-home orders were enacted after or towards the end of the initial surge. This is most evident in Figure 2a, which shows the evolution of the seven-day centered moving average of daily initial UI claims for the eleven states and the District of Columbia that report initial UI claims at a daily frequency. The solid blue lines indicate periods in which states had stay-at-home orders in place. The red lines indicate states that never enacted stay-at-home orders.

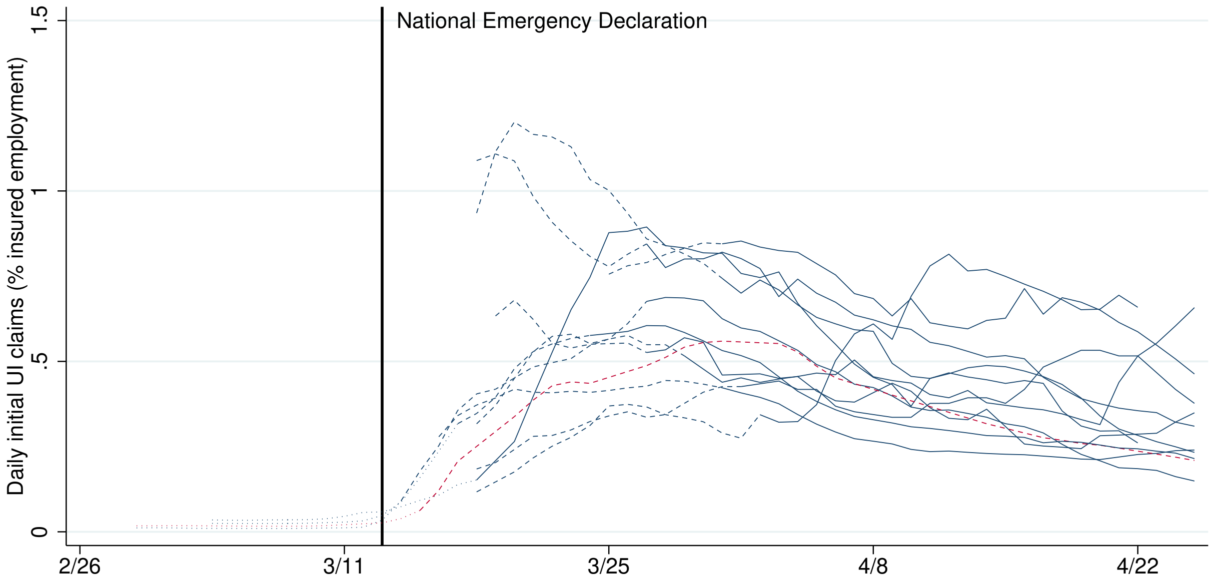
In five of the eleven states with daily UI claim data that ever enacted a stay-at-home order, initial UI claims peaked before the stay-at-home orders. In five of the other six states with stay-at-home orders, the initial UI claims had, on average, already risen to 68.6% of their peak by the time that the states enacted their stay-at-home orders. Only California, which on March 19th became the first state to enact a stay-at-home order, had a stay-at-home order in place just as its initial UI claims began to surge.

Figure 2b shows a similar series for the 39 states that report initial UI claims at only a weekly frequency, 33 of which enacted a stay-at-home order at some point.[12] Similar to our finding for the states with daily UI data, many states' initial UI claim rates had already passed or were near their peaks prior to the states' implementation of stay-at-home orders. Eighteen of these states had initial UI claims peak before or in the same week that the state enacted its stay at home order. For those states whose UI claims peaked in the same week that they enacted their stay-at-home orders, initial UI claims had already risen to 68.0% of the peak in the week before the enactment of the stay-at-home order. Furthermore, even those states that never enacted a stay-at-home order exhibited surges in initial UI claims immediately following the national emergency declaration.

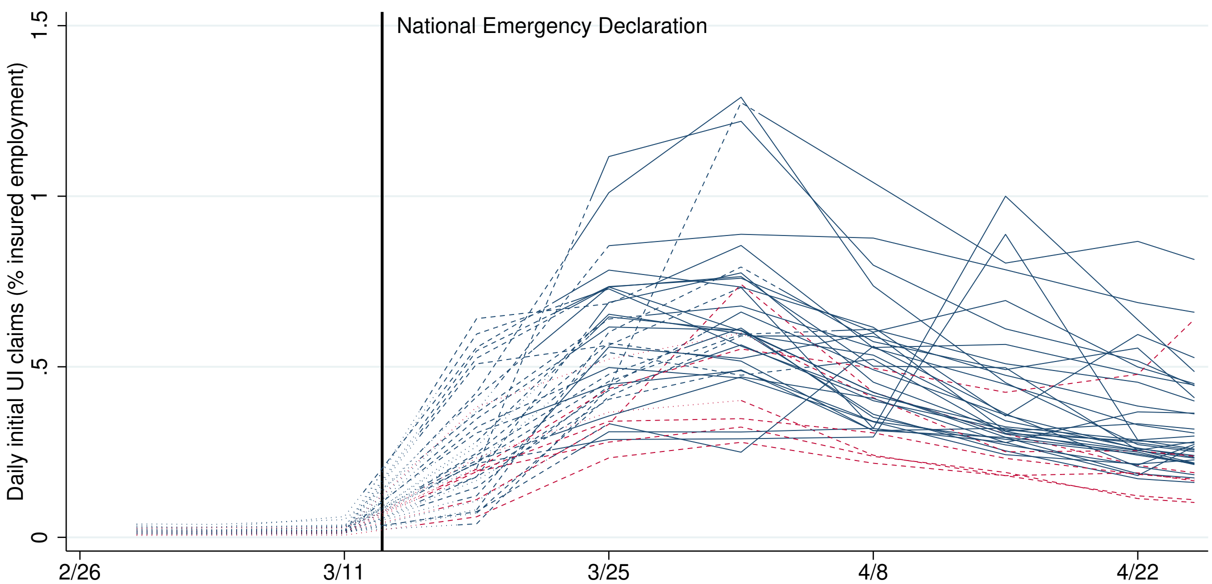
[12] To construct a series similar to the seven-day centered moving average shown in Figure 2, we use the total weekly claims to calculate the actual seven-day moving average of daily claims on the Wednesday of each week. We then use a linear interpolation on the actual Wednesday values to impute the seven-day moving average of daily initial UI claims on the other days of the week. We use the twelve states that report daily initial UI claims to test the quality of this interpolation. An OLS regression of these states' reported seven-day moving average of daily initial UI claims on a constant and the interpolated seven-day moving average has an R^2 of 0.891 (0.945 if Florida is excluded due to the issues with its weekly claim data from the U.S. Department of Labor) and yields a coefficient estimate of 0.987 for the interpolated moving average that is statistically indistinguishable from 1.0 with a P-value of 0.827.

Figure 2: Daily initial claims as share of insured employment by state

(a) Seven-day moving average of reported daily initial UI claims



(b) Interpolated seven-day moving average of daily initial UI claims

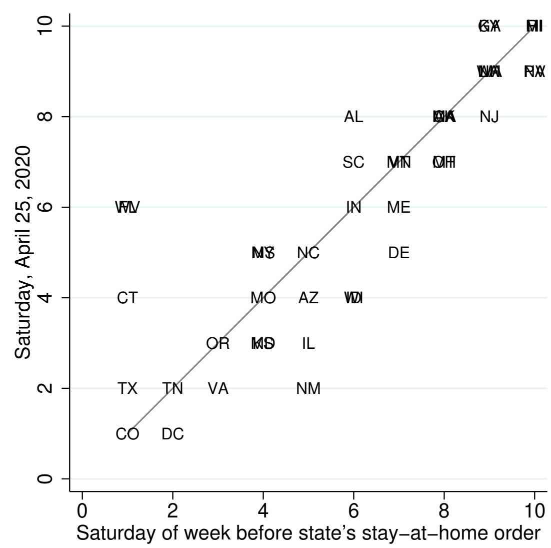


Notes: Seven-day centered moving average of the daily initial unemployment insurance claims as a percent of insured employment. Panel 2a shows reported daily initial UI claims for the twelve localities that report daily initial claims: CA, DC, FL, MN, MT, NC, ND, NV, PA, RI, TX, and WI. Panel 2b shows the actual seven-day centered moving average on Wednesdays and a linear interpolation for other days of the week for the 39 states that only report weekly initial UI claims. Blue indicates states that, at some point, had a statewide stay at home order. Red indicates states that never had a statewide stay at home order. Dotted lines (···) indicate periods without any school closure or stay-at-home order. Dashed lines (- -) indicate periods with school closures. Solid lines (—) indicate periods with stay-at-home orders in effect.

3.1.2 Differing pre-trends between ever and never stay-at-home states

We have shown that the surges in initial UI claims tended to occur before states enacted their stay-at-home orders. In addition, the large differences across states in the magnitude of their surges were already apparent before states enacted their stay-at-home orders. For instance, if we rank states each week by their cumulative initial UI claims as a share of employment, a state's relative ranking six weeks after the national emergency declaration (on April 25th) is highly correlated with the state's relative ranking in the week before the state enacted a stay-at-home order. This comparison for the states that enacted stay-at-home orders is shown in Figure 3.

Figure 3: Decile of cumulative initial UI claims pre/post stay-at-home order



Notes: X-axis: the state's decile rank of cumulative initial claims rate relative to other states in the week immediately before the state's stay-at-home order. Y-axis: the state's ranking relative to other states by the same measure in the week ending April 25, 2020. The gray line indicates the 45 degree axis along which a state's ranking is unchanged. States that never enacted a stay-at-home order are included when calculating the week-specific deciles of cumulative initial UI claims.

To further explore the extent to which differences in states' initial UI claim rates after enacting statewide stay-at-home orders reflect pre-existing differences in their responses to the COVID-19 pandemic, we estimate a modified event study model that accounts for the timing of states' enactment of the stay-at-home orders. We define three distinct cohorts of states, c, based on the first week in which the state s had a stay-at-home order in place for more than half of the week.[13]

[13] We group states by the timing of their stay-at-home orders and allow for time-varying effects of treatment in order to overcome the issues with difference-in-differences noted by Goodman-Bacon (2019). This definition groups together states based on whether they implemented their stay-at-home orders between 3/19-3/25, between

We difference the dependent variable, the weekly initial UI claim rate, with respect to its value for the week of February 29th, and estimate the average difference within each week between each cohort and the set of states that never enacted a stay-at-home order.[14] The difference between this and a typical event study design is simply a matter of timing and base values: we report coefficients with respect to calendar weeks rather than weeks before/after the stay-at-home order, and we use 2/29 as a common base period for all cohorts rather than the week of or prior to policy enaction. We prefer this specification because it makes clear the coincident timing of the surge in UI claims across all cohorts while still allowing for comparisons of UI claims relative to the baseline group of states before and after the implementation of stay-at-home orders.

The model we estimate is

$$\Delta^{2/29}\text{Initial UI}_{st} = \sum_{c=1}^{3} \beta_{ct}\text{Policy Cohort}_{cs} + X_{st}\theta_t + \alpha_t + \epsilon_{st} \tag{1}$$

The outcome of interest is the difference in state s's initial UI claims (as a share of covered employment) between the week ending February 29, 2020 and the week ending in period t. By including date fixed effects, α_t, we control for time-varying shocks that affect all states similarly. The cohort-by-week fixed effects β_{ct} capture the difference in average initial UI claims in week t between cohort c and the set of states that never enacted a stay-at-home order. We estimate specifications both with and without a set of control variables X_{st} that allow for time-varying effects of: i) differential vulnerability to the COVID-19 shock resulting from the state's industry composition (see Section 3.2 for more details on these industry measures), ii) the historical utilization of the state's UI system by unemployed workers (see Section 3.3 for more details on these UI system utilization measures), and iii) the log number of positive COVID-19 cases in the state in the previous week.

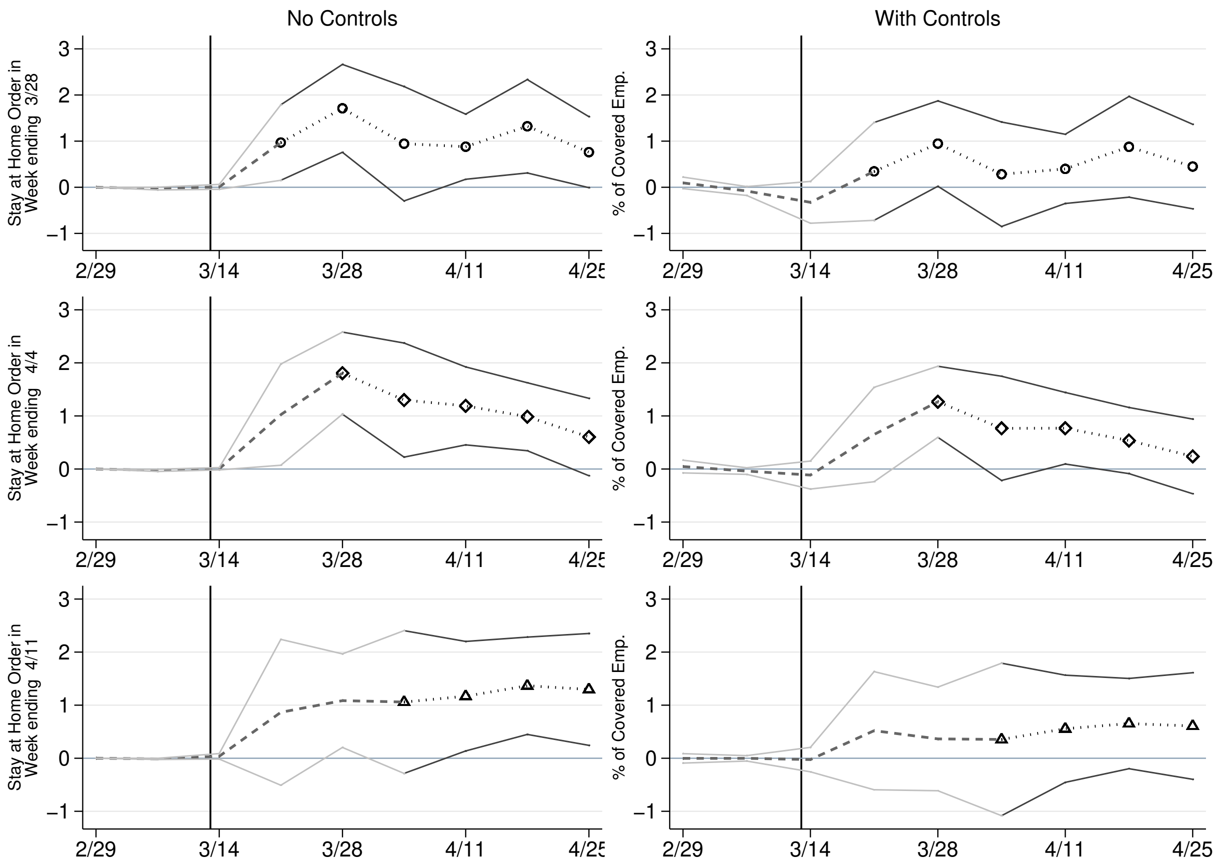
Figure 4 shows the results for each of the three cohorts in the two model specifications — one

3/26-4/1, or between 4/2-4/8. The states in each policy cohort are as follows. 3/19-3/25 (week ending 3/28): California, Connecticut, Delaware, Illinois, Louisiana, Massachusetts, Michigan, New Jersey, New, Mexico, New York, Ohio, Oregon, Washington, West Virginia. 3/26-4/1 (week ending 4/4): Alaska, Arizona, Colorado, Hawaii, Idaho, Indiana, Kansas, Kentucky, Maryland, Minnesota, Montana, New Hampshire, North Carolina, Rhode Island, Vermont, Virginia, Wisconsin. 4/2-4/8 (week ending 4/11): Alabama, Washington D.C., Florida, Georgia, Maine, Mississippi, Missouri, Nevada, Pennsylvania, South Carolina, Tennessee, Texas.

[14] We prefer an estimation model using a long-difference (with respect to a pre-pandemic date) rather than a state fixed effect. Both models control for time-invariant differences in states' initial UI claim rates. If, however, states' UI claims also exhibited heterogeneous responses to the COVID-19 pandemic shock, then the state fixed effects also absorb some portion of this heterogeneous response — with the amount absorbed decreasing with the length of the pre-pandemic period included in the sample. The long-difference model, on the other hand, avoids absorbing any of this effect, and thus allows us to better explore the sensitivity of the stay-at-home policy effect estimates to the inclusion of controls related to states' heterogeneous exposure to the COVID-19 shock.

including only week fixed effects (left-hand column) and the other also including the additional control variables (right-hand column). In both specifications, following the national emergency declaration, states in each stay-at-home cohort consistently had higher initial UI claims relative to the states without stay-at-home orders. However, these differences were already apparent in the weeks before the stay-at-home orders were enacted.

Figure 4: Difference in Initial UI Claims Rate: Stay-at-Home Order Cohorts and Never States



Notes: Outcome variable is the change in states' initial UI claims (as a share of covered employment) between the week ending February 29, 2020 and the week ending in period shown on the X-axis. The three figures in the left column correspond to the dynamic estimate of the effect of having a stay-at-home order in effect (relative to those states that never enacted a stay-at-home order) for the three policy cohorts of 3/19-3/25 (week ending 3/28), 3/26-4/1 (week ending 4/4), and 4/2-4/8 (week ending 4/11). The three figures on the right are the same dynamic effects but with the specification including control variables for the time-varying effects of: i) differential COVID-19 exposure resulting from the state's industry composition, ii) the historical utilization of the state's UI system by unemployed workers, and iii) the log number of positive COVID-19 cases in the state in the previous week. The dashed lines indicate periods before the implementation of a stay-at-home order, whereas the dotted lines indicate periods with a stay-at-home order in effect for the cohort. 95% confidence intervals are displayed where the standard errors are clustered at the state level.

We test for these differing pre-trends in two ways. First, for each policy cohort and in both model specifications, we fail to reject the null hypothesis at the 5% level that the coefficient estimate in the period immediately before the stay-at-home order is equal to the maximum of the weekly coefficient estimates after the stay-at-home policy. Second, we find that statistically significant differences (at the 5% level) emerge between the stay-at-home policy cohorts and states without stay-at-home orders after the national emergency declaration but before the stay-at-home orders for all of the policy cohorts, even without any controls. Including control variables for pre-existing differences between states reduces the estimated differences between policy cohorts and states without stay-at-home orders by about 1/3, resulting in most of the coefficient estimates — both before and after the stay-at-home orders — no longer being statistically significant at the 5% level. The lone exception is the middle cohort of states, which had significantly higher UI claims than the control group in the week prior to their implementation of stay-at-home orders.

3.1.3 Violations of assumptions underlying event study and difference-in-differences analyses

In order to explore the implications of these findings for standard econometric methods used to estimate policy effects, we consider the following dynamic potential outcome framework. We are interested in the effect of a statewide stay-at-home policy having been in place for d periods ($D_{st}^d = 1$) on Y_{st}, a measure of state s' UI claims in period t — allowing for the effect of the stay-at-home policy, β^d, to vary with duration, d. As is standard with event study or difference-in-differences analyses, we assume that there are both time-invariant differences across states in their rates of UI claims (α_s) and that there are time-specific shocks common to all states (γ_t). Complicating matters, however, is the evidence we presented above that following the national emergency declaration: i) many states' weekly initial UI claim rates had already peaked prior to their stay-at-home orders; and ii) persistent differences in the magnitude of states' cumulative initial UI claims rates were already apparent before the stay-at-home orders. These findings indicate that states' rates of job loss were also differentially responding to a coincident shock (the COVID-19 pandemic). As a result, the potential outcomes framework must also allow for state-specific time-varying responses to the COVID-19 shock (Ω_{st}). Thus, the potential outcomes for state s in period t are:

$$Y_{st} = \alpha_s + \gamma_t + \Omega_{st} + \sum_d \beta^d D_{st}^d \tag{2}$$

The state-specific, time-varying responses to the COVID-19 shock, Ω_{st}, can be further decomposed into a time-varying component that is a function of pre-existing factors ($\theta_t(W_s)$) plus an orthogonal time-varying state-specific component (η_{st}).

$$\Omega_{st} = \theta_t(W_s) + \eta_{st} \tag{3}$$

Under this potential outcomes framework, the finding in Section 3.1.1 that many states' initial UI claim rates had already peaked before they implemented their stay-at-home orders suggests that $\gamma_t + \Omega_{st}$ is large relative to both the pre-national emergency periods and the periods with stay-at-home orders. If the effect of the COVID-19 pandemic on states' UI claims is similar across all states, which implies that Ω_{st} is negligible, then a difference-in-differences analysis (or an event-study analysis that either includes non-treated comparison groups or has staggered policy adoption) will deliver consistent estimates of the policy effect of stay-at-home orders.

Unfortunately, the findings from Section 3.1.2 provide evidence that the state-specific, time-varying factors affecting the response of states' initial UI claims to the COVID-19 pandemic, Ω_{st}, are large. Specifically, the stability of states' relative rankings before and after their stay-at-home orders (when ranked by their cumulative initial UI claim rates) indicates that the heterogeneous response of states' initial UI claims to the COVID-19 pandemic, Ω_{st}, is large relative to the effect of stay-at-home orders on UI claims, β^d. Furthermore, that the large differences between ever and never stay-at-home states was already apparent before the stay-at-home orders shows that Ω_{st} is correlated with states deciding whether and when to enact stay-at-home orders. This implies a violation of the parallel trends assumption underlying difference-in-differences analyses.

Importantly, we find evidence that these time-varying differences in states' responses to the COVID-19 shock are strongly related to time-invariant state characteristics. The large role of $\theta_t(W_s)$, the time-varying response of states' UI claims to the COVID-19 shock based on the states' time-invariant characteristics, is indicated by both the stability of states' relative ranking according to their cumulative initial UI claims, and the large decrease in the estimated effect of the stay-at-home policies on UI claims when the model includes controls for time-varying responses based on differences in states' industry-exposure and UI system utilization. In Section 3.1.4, we describe and implement a procedure that relies on the importance and stability of the $\theta_t(W_s)$ functions in order to estimate a dynamic treatment effect of stay-at-home orders on UI claims for a subset of the treated states.

3.1.4 Estimating the effect of stay-at-home orders on UI claims using a matched dynamic difference-in-differences estimator

This section proposes and implements a procedure for estimating the average dynamic treatment effect of stay-at-home orders on UI claims for a subset of the states that adopted these stay-at-home orders. The procedure first pairs of states based on the similarity of their initial UI claims during the period between the national emergency declaration and the earliest stay-at-home order of each pair. The procedure then estimates the effect of stay-at-home orders on states' UI claims by evaluating the differing evolution of UI claims within each pair of states during the period in which only one state in the pair has a statewide stay-at-home order in effect.

For each base state in the treatment group, s^b, we construct a set of potential comparison states that includes all states that either never implemented a stay-at-home order or implemented a stay-at-home order at least six days later than the base state. From this set of potential comparison states, the procedure selects the comparison state, s^c, whose dynamics of weekly initial weekly UI claims most closely match those of the base state in the period after the national emergency declaration but before the base state enacts its stay-at-home order. After discarding base states for which no close match can be found,[15] we estimate a dynamic difference-in-differences model of the gap in UI claims between the base and comparison states in all periods in which the comparison state has yet to implement a stay-at-home order. Specifically, we estimate:

$$\Delta^{2/29}UI_{s^b,t} - \Delta^{2/29}UI_{s^c,t} = \sum_d \beta^d D^d_{s^b,t} + \left(X_{s^b,t} - X_{s^c,t}\right)\theta_t + \epsilon_{s^c,s^b,t} \quad (4)$$

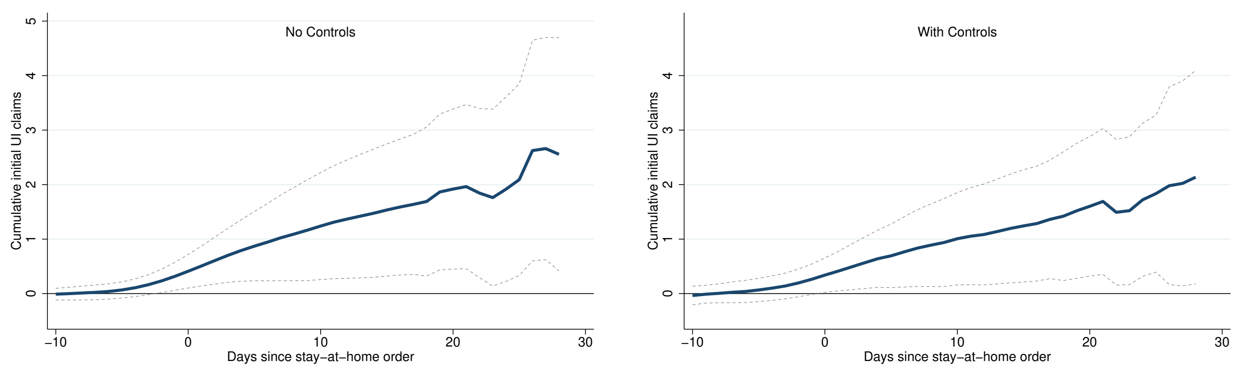
The outcome of interest is the difference in the cumulative initial UI claims since the national emergency. To construct this cumulative measure at a daily frequency, we use: i) the 7-day centered moving average of the reported daily initial UI claims for the 12 states that report them, and ii) the interpolated 7-day moving average for states that only have initial UI claims data at a weekly frequency. Differencing states' UI claim rates relative to the week-ending February 29th eliminates time-invariant state-specific characteristics (α_s) affecting the level of states UI claims. Similarly, evaluating the gap in UI claims between the base state and the comparison state in each period eliminates time-varying shocks common to all states (γ_t). Furthermore, because initial UI claims

[15] We discard any base state for which the average gap in weekly UI claims between the base state and its closest potential comparison state is more than 0.5 percentage points. Based on this criteria, we identify matched pairs for 31 of the 43 states that enacted stay-at-home orders. The twelve states for which we failed to identify a close match were: AL, DC, GA, KY, MA, ME, MN, MT, NH, NV, PA, and RI.

evolved similarly during the period between the national emergency declaration and the start of the base state's stay-at-home order, the evolution of $\Omega_{s^b,t}$ and $\Omega_{s^c,t}$ during this period must be similar.

We estimate Equation 4 both with and without additional control variables. The control variables include day-specific effects of the differences in the matched pairs' historical utilization of the states' UI systems by unemployed workers and in the exposure to the COVID-19 shock based on the industry composition of employment in the states. The estimated dynamic effects of stay-at-home orders on states' cumulative initial UI claims for the two specifications are shown in Figure 5. These estimates indicate that among the 31 states in the treatment group, four weeks under a stay-at-home order is associated with an additional 2.1 percentage points in the state's cumulative initial UI claims as a share of covered employment. This effect is 28.8% of the 7.3 percentage point difference in the average cumulative initial UI claims between states that adopted and never adopted stay-at-home orders.

Figure 5: Stay-at-home orders' effect on cumulative initial UI claims



Notes: Plots the estimated difference in cumulative initial UI claims as a share of covered employment between a state with a stay-at-home order in effect for x days relative to matched state without a stay-at-home order in effect. Estimated using 31 states for which matched pairs with sufficiently similar pre-policy initial UI trends. Control variables in the right-hand panel allow for time-varying effects of the difference in the state pairs' historical utilization of each state's UI system by unemployed workers and in the cumulative industry-exposure proxy given the industry composition of employment in each state. Dashed lines indicate the 95% confidence intervals with standard errors clustered at the state level. The R^2 is 0.21 in the model without control variables and 0.47 in the model with control variables.

Interpreting the estimated treatment effect from this procedure as the causal effect of the stay-at-home orders on UI claims requires that two assumptions hold. First, that the idiosyncratic component of Ω_{st}, η_{st}, is negligible relative to the dynamic treatment effects β^d. And second, that the variables included in W_s have a similar effect on θ_t (relative to each other) in each period t (i.e. $\theta_t = \nu_t g(W_s)$, where $g()$ is a time-invariant weighting of the components of W_s). The intuition behind these two assumptions is that in order for this procedure to absorb the states' heterogeneous and time-varying responses to the COVID-19 pandemic, it must be the case that the factors that cause a pair of states' initial UI claims to evolve similarly in the first few weeks of the pandemic must remain the relevant factors determining the time-varying heterogeneous response of states in the later weeks when the stay-at-home orders are in effect.

3.2 States' industry composition & industry-specific shocks

Using industry-level initial UI claims data from 24 states, we find significant differences in the severity of job losses across 2-digit NAICS sectors. Roughly 20 to 25% of the cross-sectional variation in industries' initial UI claim rates since the national emergency declaration can be explained by a combination of the share of employees who can work from home and the degree to which jobs require customer interaction. The variation in initial UI claims across industries has important distributional consequences, as the hardest hit industries disproportionately employ women, minorities, less educated, and lower-income workers. Additionally, these differences in the magnitude of the surge in UI claims across industries, in conjunction with differences in the industry composition of employment across states, can explain roughly 10% of the variation in the magnitude and timing of the surges in initial UI claims across states during the first six weeks following the national emergency declaration.

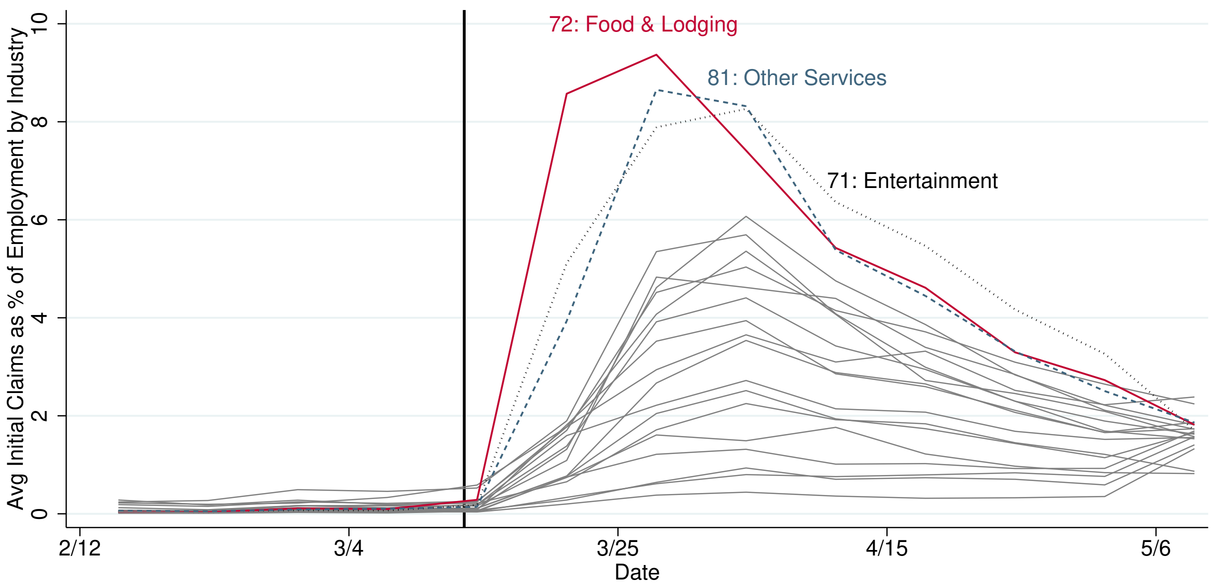
Disparities in job losses across industries are striking. The Lodging & Food, Entertainment, and Other Services sectors were the three hardest hit industries — with cumulative initial UI claims averaging 40%, 38%, and 35% of industry employment in the six weeks since the national emergency declaration. Over those same six weeks, initial UI claims have exceeded 18% of industry employment for eleven of the twenty 2-digit NAICS sectors (these eleven industries accounted for 74.9% of U.S. private sector employment at the end of 2019:Q1). There are, however, five 2-digit NAICS sectors (accounting for 9.7% of U.S. private sector employment in 2019:Q1) in which initial UI claims were less than 10% of industry employment.[16]

The job losses also occurred more rapidly in the hardest hit industry sectors. Figure 6 shows the evolution of weekly initial UI claims as a share of industry employment for each of the 20 NAICS 2-digit industry sectors. Initial UI claims in the Food & Lodging sector responded particularly quickly, jumping to over 9% of employment in the first week after the national emergency declaration. One

[16]These 2-digit NAICS industry sectors (and their cumulative initial UI claims as a share of industry employment) were: Utilities (2.0%), Finance (4.1%), Public Administration (4.5%), Educational Services (6.6%), and Management of Companies (8.0%).

week later, in the week ending March 28th, the Entertainment and Other Services sectors also experienced initial UI claims of over 8%, and most other sectors peaked in the week ending April 4th.

Figure 6: Weekly Initial UI Claims as a Share of Industry Employment



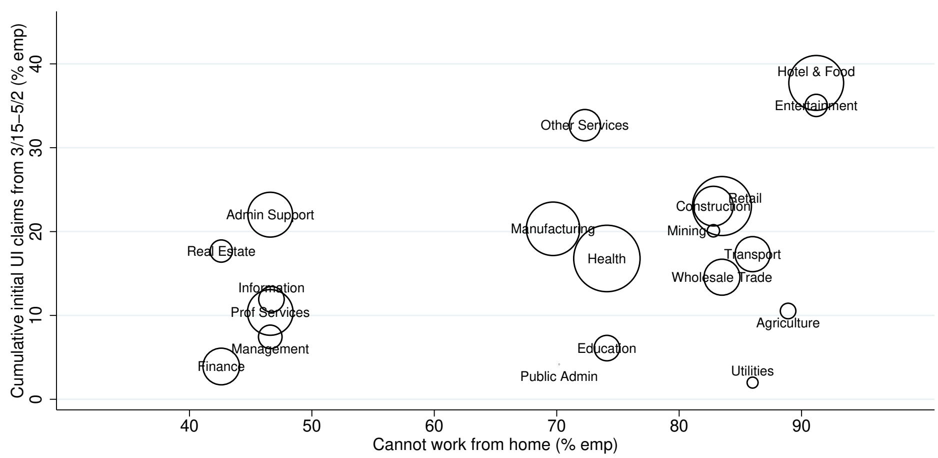
Notes: Average weekly initial UI claims at the 2-digit NAICS level (as a share of industry employment in the state at the end of 2019:Q1) for the 24 states that report initial UI claims by industry.

3.2.1 Job characteristics and cross-industry differences in initial UI claims

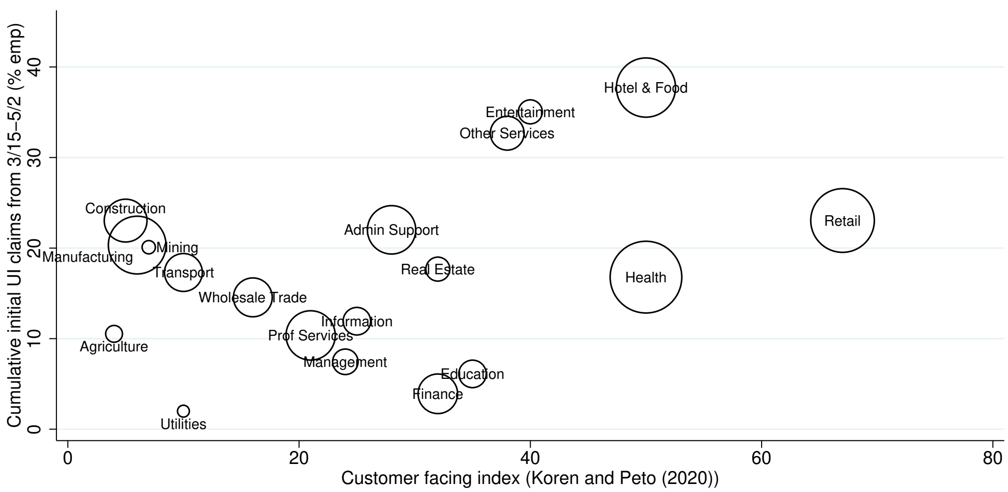
As has been shown by studies examining unemployment in the March CPS (Mongey and Weinberg (2020), Montenovo et al. (2020)), we find that the industries with the most rapid and pronounced job losses are also those in which the fewest jobs can be done from home and where most jobs require customer interaction. Figure 7 shows the cumulative initial UI claims for each 2-digit NAICS sector in the six weeks following the national emergency declaration. Panel 7a plots these cumulative initial UI claims relative to the share of workers in the industry who report being unable to work from home in the 2017-2018 American Time Use Survey (Bureau of Labor Statistics (2019)). Panel 7b shows the cumulative initial UI claims in each industry relative to the index, developed by Koren and Pető (2020), of the degree to which jobs in the industry require customer interaction. These figures show that less than 10% of workers report being able to work from home in the Lodging & Food and Entertainment sectors. Thus, the two hardest hit sectors are also those whose workers are the least able to work from home across all 2-digit NAICS industry sectors (Bureau of Labor Statistics (2019)). Similarly, Koren and Pető (2020) find that the Lodging & Food and the Entertainment industries are two of the top four industries whose jobs require customer interaction.[17]

Figure 7: Cumulative initial UI claims & job characteristics

(a) Share of workers who cannot work from home



(b) Index of degree of customer interaction required



Notes: Y-axis shows the average cumulative initial unemployment insurance claims between 3/15-4/25/2020 for each industry as a share of the industry's end-of-quarter employment (as of 2019:Q1) in the 24 states that report 2-digit NAICS-level breakdowns of weekly unemployment insurance initial claims. The X-axis of Panel 7a reports the percent of workers in an industry who report they could not work from home (source: Bureau of Labor Statistics Annual Time Use Survey). The X-axis of Panel 7b reports the Koren and Pető (2020) index of customer facing interactions required by occupations in a given industry (weighted by the occupations share of employment in the industry). The size of the circle corresponds to the national share of 2019:Q1 end-of-quarter employment in the given industry (from the Quarterly Workforce Indicators).

[17]Retail and Health are the other two industries requiring the most customer interaction. These two industries have also been hit hard by the COVID-19 pandemic — with the total initial UI claims in the first six week averaging 23.0% and 16.8% of states' 2019:Q1 end-of-quarter employment in the respective industries.

3.2.2 Initial UI claims and the industry composition of states' employment

This section examines the extent to which differences in the magnitude of states' surges in initial UI claims can be explained by differences in the industry composition of the states' employment. In order to answer this question, we construct a proxy for a state's industry-related exposure to the COVID-19 pandemic. Specifically, for each week t, we construct the following shift-share proxy for each state s's exposure to the COVID shock resulting from the industry composition of the state's employment.

$$\text{Industry Exposure}_{st} = \sum_j \bar{\omega}_{jt}^{-s} E_{s,j,2019:Q1} \tag{5}$$

where j denotes 2-digit NAICS industry sectors and the summation is across industry-level interactions of i) the week- and industry-specific average of initial UI claims rates in other states (ω_{jt}^{-s}), and ii) the state's industry-specific end-of-quarter employment shares a year prior ($E_{s,j,2019:Q1}$).

We then use a similar model specification as in Equation 1, but with the independent variable of interest being this share-shift industry exposure proxy.[18] Specifically, we estimate:

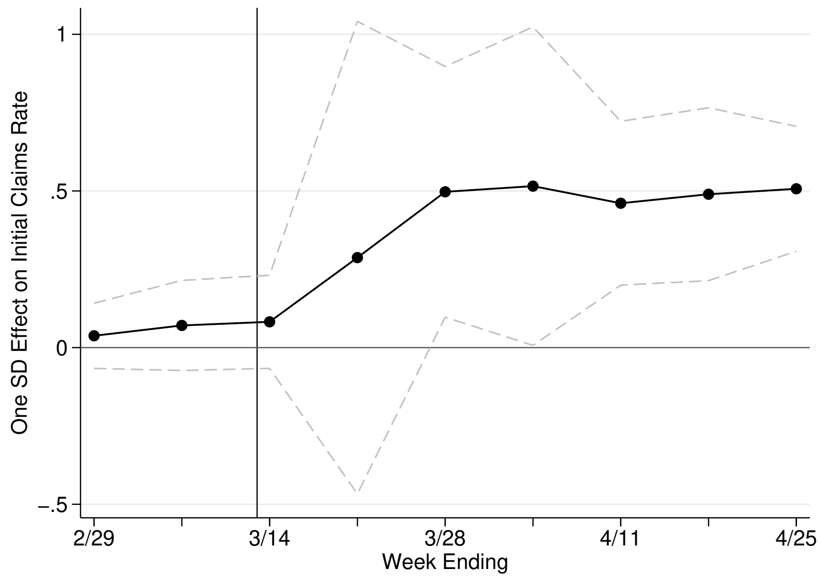
$$\Delta^{2/29}\text{Initial UI}_{st} = \beta_t \text{Industry Exposure}_{st} + X_{st}\theta_t + \alpha_t + \epsilon_{st} \tag{6}$$

where the stay-at-home order by policy cohort variables are now included in the set of control variables X_{st}.

Figure 8 shows that starting two weeks after the national emergency declaration, a one standard deviation higher value of this industry exposure proxy is associated with 0.5 percentage point increase in weekly initial UI claims. Including this proxy measure for states' industry-related exposure in our preferred model accounts for 10% of the otherwise unexplained cross-sectional variation in states' weekly initial UI claims in the first six weeks following the national emergency declaration (which is twice that of the stay-at-home orders). We believe that this is likely an underestimate of the importance of industry composition in explaining variation in states' initial UI claim rates because of attenuation bias generated by measurement error in our proxy variable for industry exposure. This measurement error results from the coarseness of our measure of the industry-specific shocks, which we can only capture at the 2-digit NAICS level since very few states report industry-specific UI claims at a more granular level.

[18] To facilitate the interpretation of the coefficient estimates, we standardize this variable within each week.

Figure 8: Estimated Effect of Predicted Industry Unemployment Claims on Weekly Initial Claims Rate



Notes: Estimated coefficients from the modified event-study model detailed in Equation (1). The coefficients plotted here are the effect of a one standard deviation increase in the industry exposure shift-share measure on the weekly initial claims rate, differenced from 2/29. Model includes date by policy cohort fixed effects and controls for states' UI utilization and lagged Covid cases. Standard errors are clustered at the state level.

3.3 Historical utilization of states' UI systems

Differences in the magnitude of the surge in UI claims across states are also associated with differences in the historical utilization of states' UI systems by unemployed workers. We consider three ways that the historical utilization of the UI system generated cross-sectional variation in states' UI claim rates following the national emergency declaration: differing eligibility criteria for unemployment insurance, the ease of applying for unemployment insurance during the historic surge of claims, and the timeliness of processing claims once they were filed.

First, eligibility criteria for both initial claimants and continuing claimants differs significantly across states. This results in dramatic differences across states in the share of unemployed workers who receive UI benefits (Blank and Card (1991)). For instance, in the week ending February 15, 2020, the ratio of Rhode Island's insured unemployment rate relative to the official BLS unemployment rate (which we will refer to as the "UI utilization ratio") was 0.95 — indicating that almost all unemployed workers in Rhode Island were receiving UI benefits in February. At the other end, the UI utilization ratio for Florida was 0.12 — nearly eight times smaller, and less than all other states.

We find that these pre-existing differences in UI utilization across states are associated with the magnitude of a state's surge in initial UI filings. To show this, we estimate a model very similar to that in Equation 1 but where the explanatory variable of interest is the UI utilization ratio. Specifically, we regress the change in the insured unemployment rate (continuing UI claims as a share of insured employment) on states' UI utilization ratios, allowing for effects to vary by week, and a set of control variables:

$$\Delta^{2/29}\text{Insured Unemployment}_{st} = \beta_t \text{UI Utilization Ratio}_{st} + X_{st}\theta_t + \alpha_t + \epsilon_{st} \tag{7}$$

where the set of control variables include the industry exposure proxy described in Section 3.2.2 interacted with time, the lagged log number of COVID-19 cases in the state, and stay-at-home policy cohort by time interactions as described in Section 3.1.2. The time-varying coefficient estimates for the UI Utilization Ratio are shown in column (3) of Table 1. Six weeks after the national emergency declaration, a one standard deviation higher historical UI utilization ratio for a state is associated with an 1.7 percentage point higher insured unemployment rate. In this specification, variation in states' historical UI utilization ratio accounts for 15% of the otherwise unexplained cross-sectional variation in states' insured unemployment rate in the early weeks of the pandemic.

Second, many state UI systems have been overwhelmed by the unprecedented surge in UI claims. As a result, laid off and furloughed workers have had to overcome jammed phone lines and website crashes at state UI agencies in order to file a claim for unemployment insurance. We find evidence indicating a front-loading of UI claims in states with higher historic UI utilization rates, which suggests that these hurdles for filing initial claims may have been less burdensome in these states. Higher historical state UI utilization ratios are associated with larger surges in initial UI claim rates in the two weeks immediately after the national emergency declaration. Column (1) of Table 1 shows that a one standard deviation higher historical UI utilization ratio is associated with an 1.3 percentage point higher level of cumulative initial UI claims just two weeks after the national emergency declaration. Over time, however, the association of the historical UI utilization ratio with initial UI claims turns negative such that higher historical UI utilization ratios were associated with fewer new weekly initial UI claims six weeks after the the national emergency declaration. The time-varying nature of this relationship suggests that workers in states with higher UI utilization in normal times were able to file claims in a more timely manner, leading to the front-loading of

Table 1: Historical utilization of UI system & UI claims at the onset of the COVID-19 pandemic

Dependent Variable:	Cumulative Initial Claims		Insured Unemployment Rate	
	(1)	(2)	(3)	(4)
UI Utilization Ratio				
Week Ending March 14	0.109***	0.153*	-0.0291	0.0586
	(0.0230)	(0.0590)	(0.0343)	(0.0556)
Week Ending March 21	0.757*	0.579	0.143	-0.308
	(0.331)	(0.468)	(0.203)	(0.171)
Week Ending March 28	1.196*	1.316	0.776*	0.316
	(0.483)	(0.740)	(0.319)	(0.495)
Week Ending April 4	1.345*	1.620	0.673	-0.266
	(0.617)	(0.867)	(0.403)	(0.609)
Week Ending April 11	1.333	1.647	1.677**	1.335
	(0.734)	(0.998)	(0.489)	(0.829)
Week Ending April 18	1.410	1.507	1.343*	-0.300
	(0.782)	(1.036)	(0.587)	(0.941)
Week Ending April 25	1.267	1.065	1.697**	3.029**
	(0.859)	(1.091)	(0.622)	(1.103)
Sample ends	4/25	4/25	4/25	4/25
Weighted	No	Covered Empl	No	Covered Empl
N	510	510	510	510
R^2	0.886	0.879	0.858	0.848

* $p < 0.05$, ** $p < .01$, *** $p < .001$
Notes: Weekly, state level data. The UI utilization ratio is standardized. All models include policy cohort by date fixed effects and controls for lagged COVID-19 cases, predicted UI claims based on industry, and historical UI utilization. States have at least 4 days of a policy order in place to be counted as having that policy in a given week. Standard errors clustered at the state level.

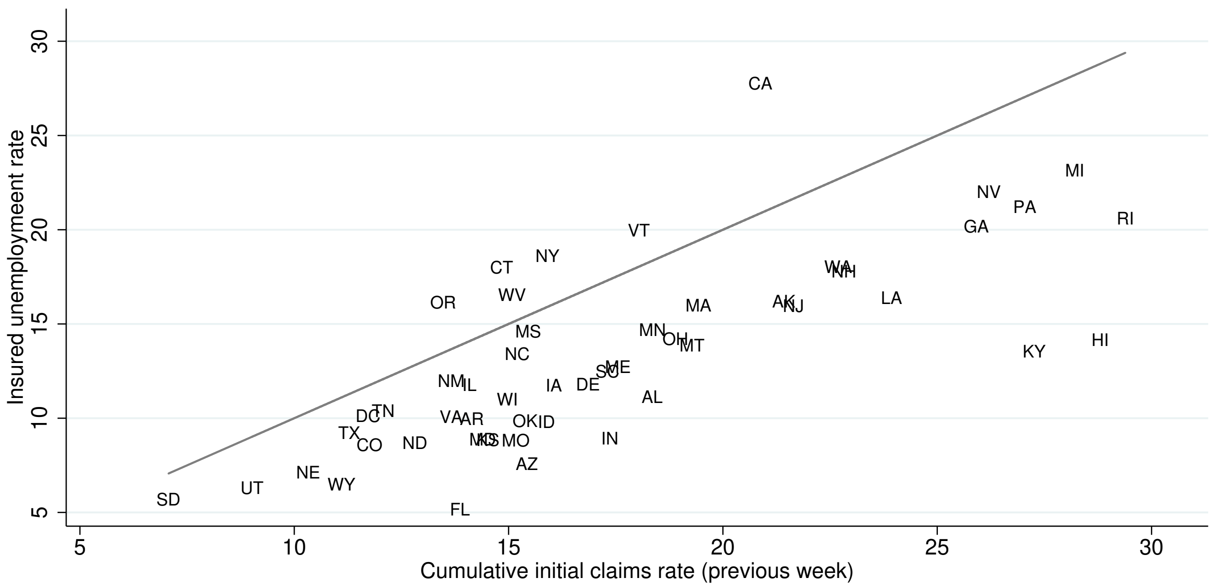
initial UI claims that we observe.

Third, the challenges faced by state UI agencies as a result of the surge in claims were not limited to the receipt and recording of new claims, but also involved the timeliness of agencies' evaluation of claimant eligibility. Since claimants may only begin filing continuing UI claims starting the week after their initial claims have been processed, one measure of a state UI agency's pace of claims processing is the gap between the initial UI claims accumulated since the start of the national emergency relative to the current week's continuing UI claims.[19] In Figure 9, this gap is

[19] Both of these are measured as a share of covered employment. The accumulation of initial UI claims is through the week prior to the measured continuing UI claims. This measure is very specific to the current period for two

represented by the distance of each state to the 45-degree line. Most states fall below the 45-degree line, implying that some portion of the accumulated initial UI claims have not been converted into continuing UI claims. Although some of this gap can be explained by denied applications,[20] we find that some of this gap is associated with the historical utilization of the states' UI systems. Specifically, we find that a one standard deviation higher historical UI utilization ratio is associated with an 8.2% smaller gap between the accumulated UI claims and the insured unemployment rate six weeks after the national emergency declaration — implying that states with historically higher UI utilization ratios managed to process the surge in initial claims more rapidly.

Figure 9: Processing gaps: Continuing UI claims vs. cumulative initial claims



Notes: X-axis: cumulative initial UI claims from 3/15-4/18/2020 as a share of each state's insured employment. Y-axis: continuing unemployment insurance claims as a share of each state's insured employment on April 25, 2020.

reasons. First, the outflows from unemployment over this seven week period are likely to be quite small relative to the number of initial claims filed. In normal times, however, many individuals may only receive UI benefits for a brief spell, thus being counted in the initial claims, but never filing a continuing claim. Second, even in "normal" recessions, the flows into unemployment are typically spread over many months and do not have an obvious date from which to begin accumulating initial claims. As a result, in normal times, the relationship between the accumulated (or lagged) initial claims as a share of employment and the insured unemployment rate is much weaker.

[20]Some states actively encouraged individuals who would only be eligible under the Pandemic Unemployment Assistance (PUA) program to apply for standard UI benefits so as to expedite the processing of their PUA applications.

4 Conclusion

The sobering conclusion of this paper is that the dramatic job losses experienced since the start of the COVID-19 pandemic are unlikely to be erased by simply removing states' stay-at-home orders. Much of the dramatic surge in job loss since the national emergency declaration has been driven by factors other than states' stay-at-home orders. While states that did not enact stay-at-home orders tended to experience smaller surges in UI claims, we demonstrate that these states' smaller surges in UI claims largely reflect differences in their exposure to the COVID-19 pandemic shock. We estimate that the stay-at-home orders can account for less than 30% of the gap between the states that ever-versus-never enacted stay-at-home orders.

This paper also cautions against using standard difference-in-differences or event study analyses to estimate the effect of stay-at-home orders on job loss or unemployment at the onset of the COVID-19 pandemic. Using high-frequency UI claims data, we show that the dynamics of UI claims following the national emergency declaration violate the assumptions required for these econometric methods to deliver causal estimates of the effect of stay-at-home orders on UI claims.

We document significant differences in UI claims across industries. These differences in UI claim rates across industries, in combination with differences in the industry composition of states' employment, prove to be more significant explanators of the cross-sectional variation in states' cumulative initial UI claims (relative to statewide stay-at-home orders). The differential exposure across industries to the COVID-19 shock also has important implications for which types of workers have borne the brunt of the initial downturn. The industries hardest hit by the COVID-19 shock also tend to disproportionately employ women, minorities, low income workers, and less-educated workers.

Lastly, we document that states with greater historical utilization of their UI systems by unemployed workers tended to receive more initial UI claims and process them faster at the onset of the COVID-19 pandemic. We show evidence that the slower processing of initial UI claims in states with lower historical utilization of their UI systems also translated into slower approval of workers' UI claims — implying that workers in these states had to wait longer in order to receive their UI benefits. Not only do these delays exacerbate the economic stresses on these workers and their families, but they also reduce the effectiveness of the UI system as an automatic stabilizer.

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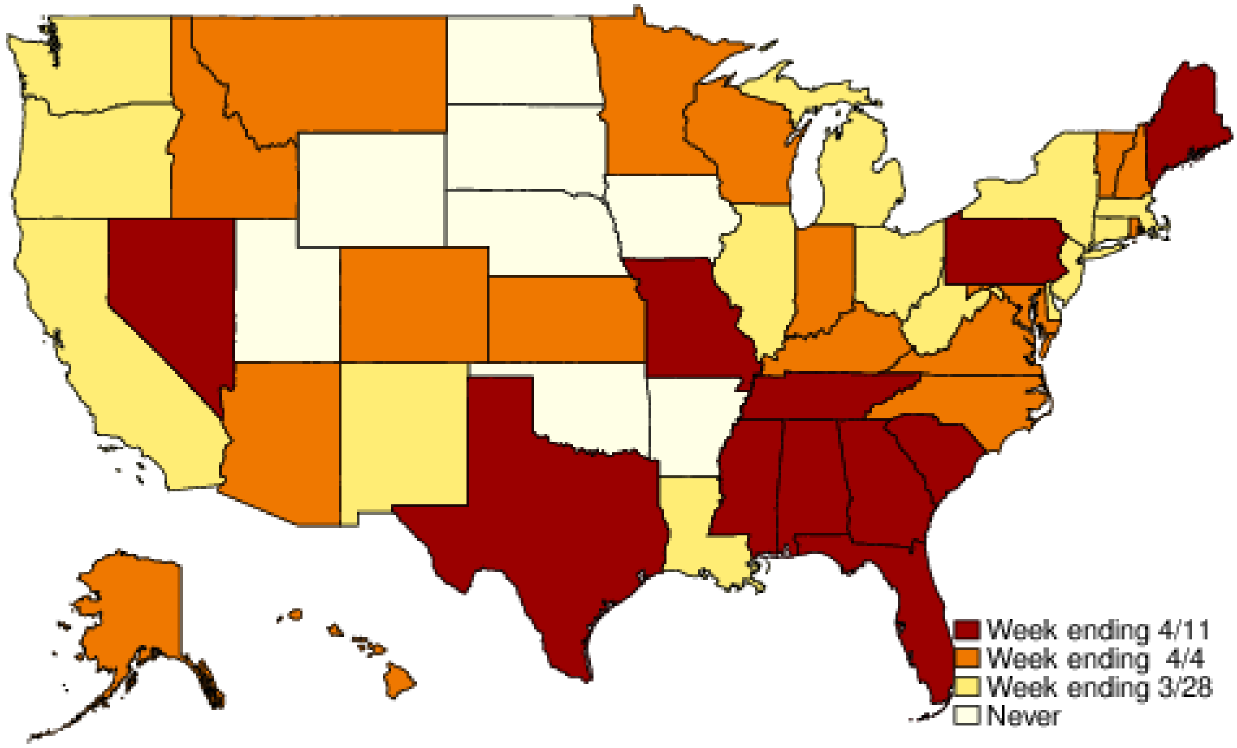
A State Stay-at-Home Orders

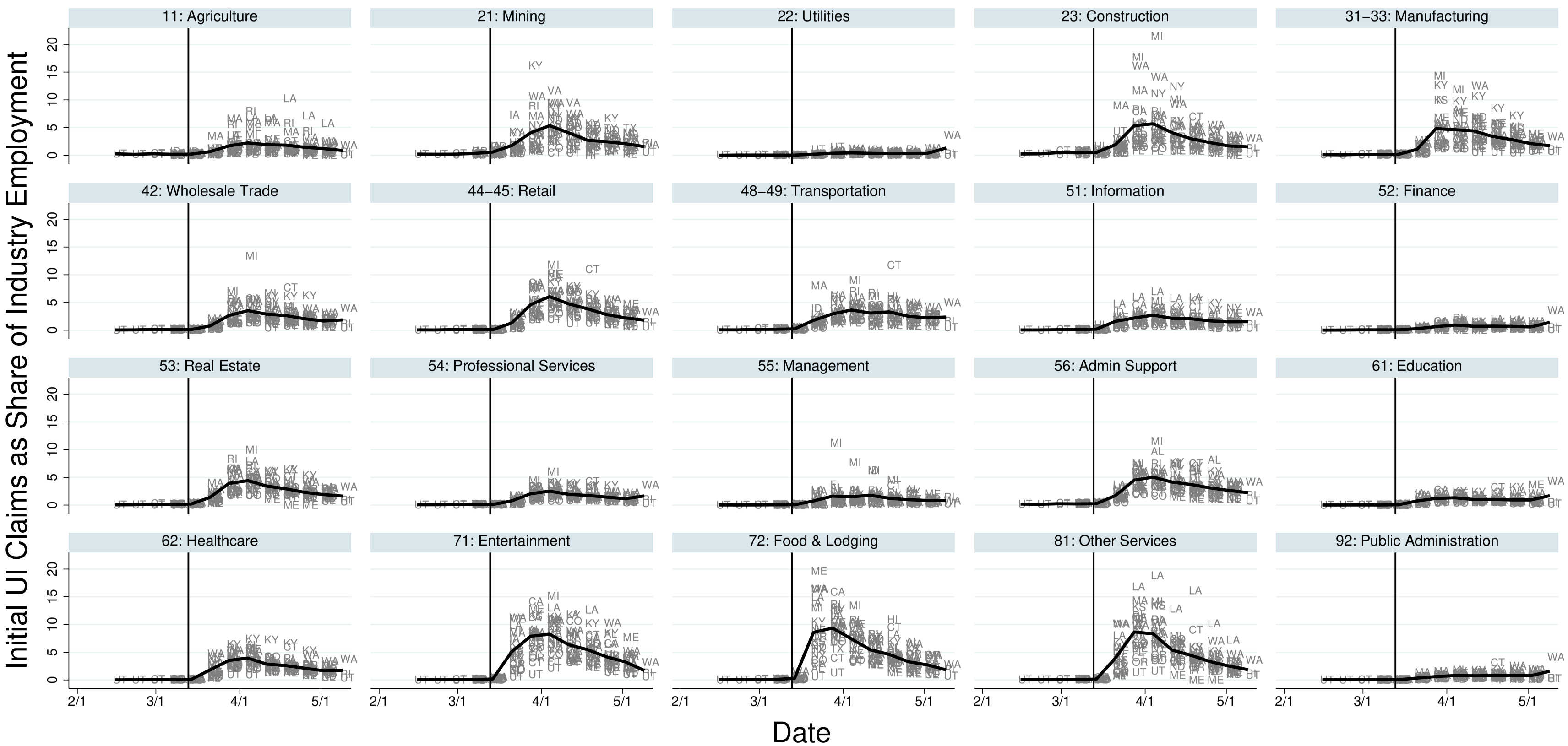
Figure 10: Timing of stay-at-home orders by state



Notes: Color indicates the first week in which state had a statewide stay-at-home order in place for more than half the week.

B Industry Changes in Claims

Figure 11: Weekly Initial UI Claims as a Share of Industry Employment



Notes: Red line = NAICs 72, blue dashed line = 82, black dash-dot line = 71

C Mobility plummets after the national emergency declaration

We use cell phone mobility data from Google LLC (2020) to measure the daily change in individuals' mobility during the COVID-19 pandemic. Google provides six distinct measures of state-level mobility at a daily frequency based on the type of place being visited: retail and recreation, grocery & pharmacy, parks, transit stations, residential, and workplaces. Although all of these measures exhibit interesting dynamics over the crisis, our analysis focuses on the retail and recreation mobility measure since we believe this best captures changes in mobility related to consumer activity. The retail and recreation measures compares mobility trends for places such as restaurants, cafes, shopping centers, theme parks, libraries, and movie theaters relative to those same locations over the period from January 3 to February 6, 2020.

Mobility in the United States declined dramatically immediately following the national emergency declaration — with Google's measure of mobility related to retail and recreation activity falling by an average 46 percentage points across states in the two weeks following the declaration.[21] Figure 12 shows Google's daily mobility measure for states' retail and recreation activity, as well as the other measures. The red lines indicate states that never enacted a stay-at-home order and the blue lines indicate states that, at some point, had in place a statewide stay-at-home order. The dashed lines indicate periods in which schools were closed statewide. The solid lines indicate periods with stay-at-home orders in place. There are three key take-aways from this figure.

First, the precipitous decline in mobility that occurred immediately after the national emergency declaration was common to all states. We find that the retail and recreation mobility time series for every state exhibits a structural break in the week immediately following the national emergency, with the break occurring between March 17-March 19, 2020 for nearly every state — with two exceptions being the District of Columbia (March 15th) and Hawaii (March 20th).[22]

Second, depending on the state, much or all of the mobility decline had already occurred before the states began issuing stay-at-home orders. In Figure 12, the transitions from dashed lines (indicating school closures) to solid lines (indicating stay-at-home orders being in effect) show how much of the decline in each state's retail and recreation related activity had already occurred before the state's stay-at-home order. On average, mobility in states had already fallen to 87.7% of the

[21] Similarly dramatic mobility changes occurred in transit, work, and residential activities (see Appendix C for similar figures for each of these measures).

[22] We estimate a supremum Wald test on the mean of the centered seven-day moving average of the mobility measure for each state over the trimmed sample from February 27 through April 21, 2020. In every case the p-value for the identified structural break is below 0.001.

Figure 12: Google Mobility Measures

Figure 13: Retail and Recreation

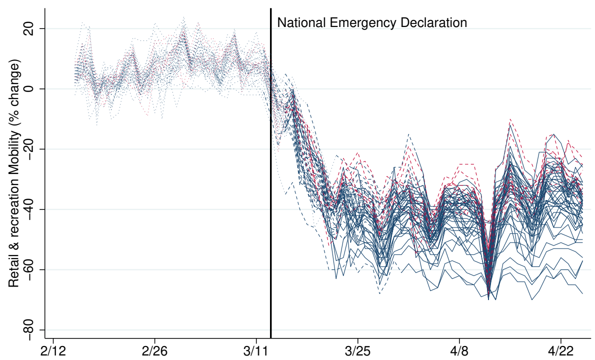


Figure 14: Residential

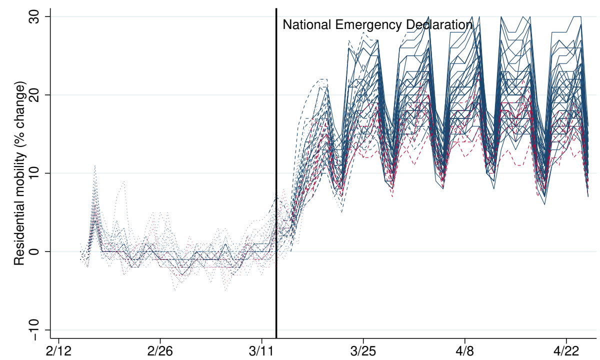


Figure 15: Transit

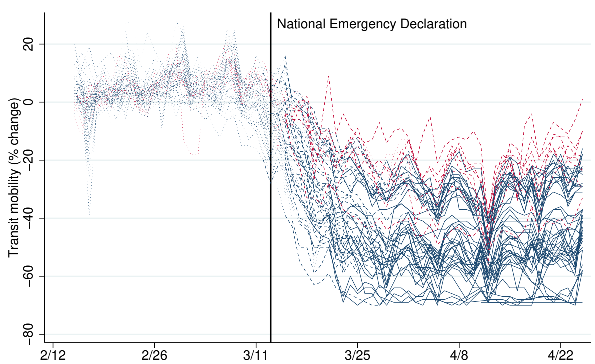
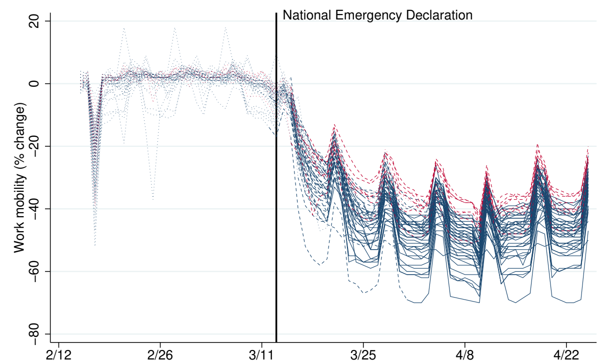


Figure 16: Workplace



Notes: Change in Google's mobility measure relative to the period from January 3 to February 6, 2020. Blue indicates states that, at some point, had a statewide stay at home order. Red indicates states that never had a statewide stay at home order. Dotted lines (· · ·) indicate periods without any school closure or stay-at-home order. Dashed lines (- -) indicate periods with school closures. Solid lines (—) indicate periods with stay-at-home orders in effect.

states' lowest level by the time that the state enacted a stay-at-home order (measured using the centered seven-day moving average of mobility). Even in California, which became the first state to enact a stay-at-home order on March 19th, retail and recreation mobility had already fallen to 51.5% of California's lowest mobility level by the time of the state's stay-at-home order. Furthermore, all of the states' estimated structural breaks in retail and recreation mobility occurred before the states enacted stay-at-home orders.

Third, states differed significantly in the depth of their mobility declines, with a 21.6 percentage point gap between the largest and smallest two-week decline in retail and recreation activity.[23]

[23]Michigan had the largest two-week decline in retail and recreation related activity, falling 54.3 percentage points;

While states that implemented stay-at-home orders tended to have larger mobility declines, the divergence in levels was already well established before states enacted stay-at-home orders.

One explanation for the larger mobility declines in states with stay-at-home orders is that individuals' mobility decisions responded to the severity of the COVID-19 pandemic in their state at the time of the national emergency declaration. Table 2 shows that the state-level variation in the log number of COVID-19 cases as of March 14th can account for 16-36% of the variation across states in the magnitude of their mobility declines over the subsequent two weeks. For each of the state-level retail & recreation, work, transit, and residential mobility measures from Google, we calculate the change between the average mobility in the two-week period immediately before the national emergency declaration versus the two-week period starting 14 days after the declaration. This captures the dramatic decline, and then plateauing, of states' residents' mobility. We then regress these measures of states' mobility declines on the standardized log number of confirmed COVID-19 cases in the state as of March 14th. As shown in Table 2, we find that a one standard deviation increase in the log number of positive COVID-19 cases in a state is associated with a 4.0 percentage point larger decline in retail and recreation mobility.

Table 2: Larger mobility changes in states with more COVID-19 cases on March 14th

	(1) Retail	(2) Work	(3) Transit	(4) Residential
Std log COVID-19 (3/14)	-2.2**	-3.9***	-5.9***	1.6***
	(0.8)	(0.6)	(1.4)	(0.3)
Std change log COVID-19 (3/14 to 3/21)	-1.7*	-1.8*	-3.2*	0.8**
	(0.6)	(0.8)	(1.5)	(0.3)
Observations	50	50	50	50
R^2	0.186	0.328	0.200	0.315

Notes: Outcome variable is each state's change in average mobility in the two-week period from 3/28-4/11 to the average mobility in the two-week period from 2/29-3/13. Explanatory variable is the standardized log of positive COVID-19 cases in the state as of March 14th, the day immediately following the national emergency declaration. Robust standard errors. *, **, and *** denote statistical significance at the 5%, 1% and 0.1% levels respectively.

whereas Nebraska had the smallest decline of 32.7 percentage points.