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Unemployment Effects of Stay-at-Home Orders: Evidence from High Frequency Claims Data

ChaeWon Baek*, Peter B. McCrory*, Todd Messer*, and Preston Mui*

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Abstract

Epidemiological models projected that, without effective mitigation strategies, upwards of 2 million Americans were at risk of death from the COVID-19 pandemic. Heeding the warning, in mid-March 2020, state and local officials in the United States began issuing Stay-at-Home (SAH) orders, instructing people to remain at home except to do essential tasks or to do work deemed essential. By April 4th, 2020, nearly 95% of the U.S. population was under such orders. Over the same three week period, initial claims for unemployment spiked to unprecedented levels. In this paper, we use the high-frequency, decentralized implementation of SAH orders, along with high-frequency unemployment insurance (UI) claims, to disentangle the relative effect of SAH orders from the general economic disruption wrought by the pandemic that affected all regions similarly. We find that, all else equal, each week of Stay-at-Home exposure increased a state's weekly initial UI claims by 1.9% of its employment level relative to other states. Ignoring cross-regional spillovers, a back-of-the-envelope calculation implies that, of the 17 million UI claims made between March 14 and April 4, only 4 million were attributable to the Stay-at-Home orders. This evidence suggests that the direct effect of SAH orders accounted for a substantial, but minority share, of the overall initial rise in unemployment claims. We present a stylized currency union model to provide conditions under which this estimate represents an upper or lower bound for aggregate employment losses attributable to SAH orders.

Keywords: COVID-19, Non-pharmaceutical Interventions, NPIs, Stay-at-Home orders, Unemployment Effect, UI Claims, Pandemic Curve, Recession Curve.
JEL Codes: C21, C23, E24, E65, H75, I18, J21

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1 Introduction

To limit the spread and severity of the COVID-19 pandemic, officials around the globe turned to non-pharmaceutical interventions (NPIs), such as shutting down schools, restricting economic activities to those deemed essential, and requiring people to remain at home whenever possible. In mid-March 2020, Ferguson et al. (2020) issued a report projecting that, in the absence of the effective implementation of NPI mitigation strategies, more than 2 million Americans were potentially at risk of death from the COVID-19 respiratory disease, with many more facing uncertain medical complications in the near- and long-run.

Soon after, state and local officials in the United States began announcing Stay-at-Home (SAH) orders, which restricted residents from leaving their homes except for essential activities. The earliest SAH order was implemented in the Bay Area, California on March 16th, 2020. Three days later, the governor of California issued a state-wide SAH order. By March 24th, more than 50% of the U.S. population was under a SAH order (see Figure 1). By April 4th, 95% of the U.S. population was under a state or local SAH order, likely substantially reducing the supply of and demand for locally produced goods and services.

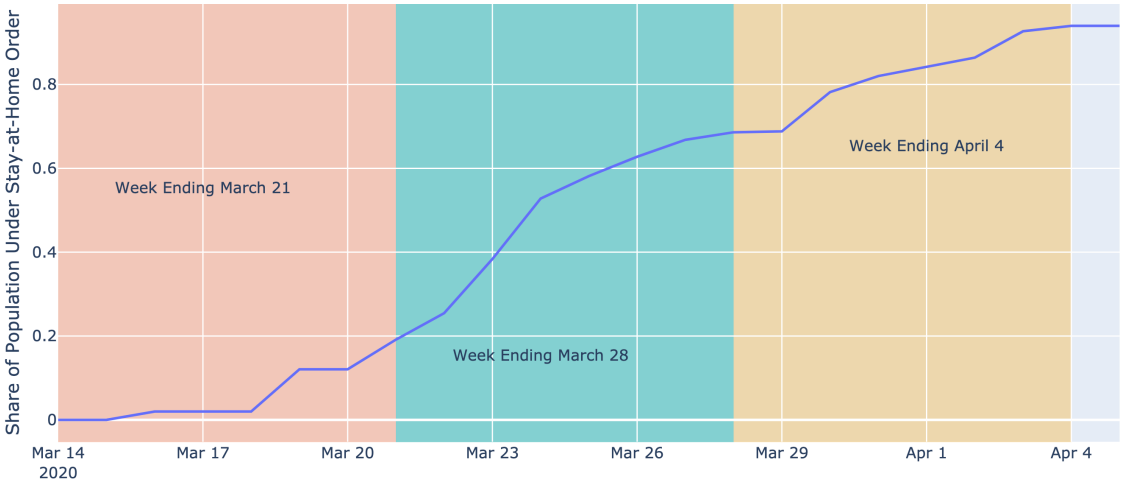
At the same time, there was mounting evidence of substantial disruption to labor markets in the United States. For the week ending March 21st, 2020, the Department of Labor (DOL) reported that more than 3.3 million individuals filed for unemployment benefits.[1] In the subsequent weeks ending March 28th and April 4th, initial claims for unemployment once again hit unprecedented highs of more than 6.9 million claims and 6.7 million claims, respectively. Taken together, total unemployment insurance (UI) claims over this three week period was almost 17 million.

How much of the initially observed increase in UI claims was attributable to the newly implemented SAH orders? This is not a straightforward question to answer since the increase in unemployment claims could plausibly be attributed to a multitude of factors other than SAH orders that occurred at the same time. For example, consumer and business sentiment both declined and economic uncertainty rose as the pandemic worsened. One stark example of this economic uncertainty was the swift drop in the value of the S&P 500 stock market index, which lost roughly 30% of its value between February 20 and March 16, the first day a SAH order was announced in the United States.

In this paper, we disentangle the local effects of SAH orders from the broader economic disruption brought on by the COVID-19 pandemic and other factors affecting all states equally. We do so by providing evidence of a direct causal link between the implementation of SAH orders and the

[1] For comparison, in this week one year prior, there were just over 200 thousand initial claims for unemployment insurance. This was also the first time since the DOL began issuing these reports that the flow into unemployment insurance exceeded the number of individuals with continuing claims.

Figure 1: Cumulative Share of Population under Stay-at-Home Order in the U.S.



Sources: Census Bureau, the *New York Times*; Authors' Calculations

observed increase in UI claims. To the best of our knowledge, this paper is the first systematic study of the causal link between SAH orders and UI claims in the United States. This is our main contribution.

We show that the decentralized implementation of SAH orders across the U.S. induced high-frequency regional variation as to when and to what degree local economies were subject to such orders. We leverage the cross-sectional variation in the length of time that states were exposed to such orders to estimate its effect on UI claims.[2,3]

We find that an additional week of exposure to SAH orders increased UI claims by approximately 1.9% of a state's employment level, relative to unexposed states. The effect is precisely estimated and robust to the inclusion of a battery of controls one might suspect are correlated with both local labor market disruption and SAH implementation, lending it a causal interpretation. The set of controls we consider include the severity of the local exposure to the coronavirus pandemic, state-level political economy factors, and each state's industry composition.

We use our cross-sectional estimate to calculate the implied aggregate effect of implemented SAH

[2]Our variable of interest pertains to the *government* implementation of SAH orders. Our design does not aim to capture the effects of, for example, social distancing behaviors that may have taken place in the absence of a government order.

[3]In this paper, we principally focus on UI claims for three reasons: (1) UI claims are among the highest frequency indicators of real economic activity—especially as it relates to the labor market; (2) These data are consistently reported at a subnational level; (3) The data are publicly and readily available.

orders on the number of new unemployment claims. This exercise yields an estimate of approximately 4 million UI claims attributable to SAH orders through April 4, comprising roughly 24% of total claims over the time period. We refer to this calculation as the relative-implied aggregate estimate of employment losses from SAH orders.

We then investigate whether the change in unemployment claims is due to demand or supply side factors using proxies for local economic activity. Using daily data from Google on local mobility trends by county, we estimate the on-impact effect of SAH orders on visits to retail and workplace locations, the former capturing demand shocks and the latter capturing supply shocks. We find sharp, on-impact, declines in mobility in both the retail and mobility indices, suggesting that SAH orders worked through both channels. The decline in both mobility indices persists for at least two and a half weeks, the horizon over which we estimate the event studies.

It is well known that cross-sectional research designs, such as the one employed in our paper, hold constant general equilibrium effects as well as other aggregate factors. Simply scaling up our cross-sectional estimate may therefore give a biased impression of the aggregate effect of SAH orders on UI claims in the United States.

To understand the nature of these general equilibrium forces, we present a simplified currency union model to provide conditions under which the relative-implied estimate represents an upper or lower bound on aggregate employment losses. When the SAH shock is viewed primarily as a technology shock—and in the empirically relevant case—our estimate represents an *upper bound* on the aggregate effect. However, when SAH orders are treated as a local demand shock, the interpretation is a bit more subtle and depends upon the persistence of the shock and degree of price flexibility. Across all combinations of price rigidity and persistence, we find that our back-of-the-envelope estimate, at most, understates aggregate employment losses by a factor of approximately two. With sticky prices and a zero-persistence shock, the relative-implied estimate associated with the SAH-induced local demand shock understates aggregate employment losses by 12%.

Our evidence from the mobility indices suggests that the SAH shock should be viewed as a combination of both local supply and demand shocks. The model results then imply a (non-binding) *upper bound* on UI claims from SAH orders through April 4, 2020 of approximately 8 million. Thus, relative to the total rise of around 16.5 million, at most around 50% of the total rise in UI claims over this period can be attributed to SAH orders.

Finally, we document the robustness of our empirical results by considering two alternative research designs. First, we consider a panel design that allows us to control separately for week fixed effects and state fixed effects. The inclusion of such fixed effects controls for time-varying aggregate effects and time-invariant state effects. Second, we estimate county-level specifications which allow us to control for unobserved state-level factors, such as each state's ability to respond to and process

unprecedented numbers of unemployment claims. We find similar results in both cases.

Related Literature

Our paper relates most obviously to the rapidly growing economic literature studying the COVID-19 pandemic, its economic implications, and the policies used to address the simultaneous public health and economic crises. The epidemiology literature has focused on the health effects of NPIs. In a notable study, Hsiang et al. (2020) estimate that, in six major countries, NPI interventions prevented or delayed over 62 million COVID-19 cases.[4] Our focus is, instead, on the macroeconomic effects of the coronavirus pandemic. Broadly speaking, the macroeconomic literature on COVID-19 has split into two distinct yet highly related strands. Here we provide a representative, albeit not exhaustive, review.

The first strand of research focuses on the relationship between macroeconomic activity, policy, and the unfolding pandemic. Gourinchas (2020) and Atkeson (2020) are early summaries of how the public health crisis and associated policy interventions interact with the economy. Both emphasize the trade-off between flattening the pandemic curve while steepening the recession curve. Similarly, Faria–e–Castro (2020) studies the effect of a pandemic-like event in a quantitative DSGE model in order to assess the economic damage associated with the pandemic along with the fiscal interventions employed in the U.S. to attempt to flatten the recession curve. Eichenbaum, Rebelo, and Trabandt (2020) derive an extension of the standard Susceptible-Infected-Recovered (SIR) epidemiological model to incorporate macroeconomic effects, formalizing the relationship between the flattening the pandemic curve and amplifying the recession curve. We view our paper as providing causally identified, empirical support for the claim that flattening the pandemic curve requires steepening the recession curve.

The second strand of research uses high-frequency data to understand the economic fallout wrought by the COVID-19 pandemic. Our paper aligns more closely with this strand of the literature. Baker et al. (2020) show that economic uncertainty measured by stock market volatility, newspaper-based economic uncertainty, and subjective uncertainty in business expectation surveys rose sharply as the pandemic worsened. Lewis, Mertens, and Stock (2020) derive a weekly national economic activity index and show that the COVID-19 outbreak had already had a substantial negative effect on the United States economy in the early weeks of the crisis. Hassan et al. (2020) use firm earnings calls to quantify the risks to firms as a result of the COVID-19 crisis. Coibion, Gorodnichenko, and Weber (2020b) examine how the pandemic affected the labor market in general. Using a repeated large-scale household survey, they show that by April 6th, 2020, 20 millions jobs were lost and the labor market participation rate had fallen sharply.

[4]The six countries are China, South Korea, Italy, Iran, France, and the United States.

Our paper also relates to empirical work studying the effect of lockdown policies more specifically. For example, Hartl, Wälde, and Weber (2020) study the effect of lockdowns in Germany on the spread of the COVID-19. In contrast to these papers, we use geographic variation to understand the effect of COVID-19 on economic activity. In that respect, our paper can be thought of a high frequency version of Correia et al. (2020), who find that over the long term, NPI policies implemented in response to the 1918 Influenza Pandemic ultimately resulted in faster growth during the recovery following the pandemic.

Other papers employing geographic variation in NPI implementation to understand their contribution to the economic fallout associated with COVID-19 pandemic include the following: Kong and Prinz (2020) use high-frequency Google search data as a proxy for UI claim activity to study the labor market effects of various NPIs; Coibion, Gorodnichenko, and Weber (2020a) study the effect of lockdowns on employment and macroeconomic expectations; Kahn, Lange, and Wiczer (2020) document broad declines job market openings in mid-March prior to implementation of SAH orders; Kudlyak and Wolcott (2020) provide evidence that the bulk of UI claims over this period were classified as temporary, suggesting that the long-run costs of lockdowns may be mitigated, so long as worker-firm matches persist until the recovery; and, Sauvagnat, Barrot, and Grassi (2020) document regional lockdowns depressed the market value of affected firms.

A closely related paper is Friedson et al. (2020), which uses the state-wide SAH order implementation in California along with high frequency data on confirmed COVID-19 cases and deaths to estimate the effect of this policy on flattening the pandemic curve. Unlike our approach, however, the authors in this paper use a synthetic control research design to identify the causal effects on this policy. The authors argue that the SAH order in California reduced the number of cases by 150K over three weeks; the authors perform a back-of-the-envelope calculation to calculate roughly 2-4 jobs lost over a three week period in California per case saved. In contrast to Friedson et al. (2020), we are able to directly estimate the causal effect of SAH orders on UI claims. Taking their benchmark number of cases saved over three weeks, we find that a SAH order implemented over three weeks in California would increase UI claims by 6.4 per case saved.

2 Data

2.1 State-Level Stay-at-Home Exposure

We construct a county-level dataset of SAH order implementation based on reporting by the *New York Times*. On March 24th, 2020, the *New York Times* began tracking all cities, counties, and states in the United States that had issued SAH orders and the dates that those orders became

effective.[5]

We calculate the number of weeks that each county c in the U.S. had been under a SAH order between day $t-k$ and day t (and counting the day that the policy became effective).[6] We denote this variable with $SAH_{c,s,t,t-k}$, where s indicates the state in which the county is located. Except when explicitly stated, we drop the $t-k$ subscript and set k to be large enough so that this variable records the total number of weeks of SAH implementation in county c through time t.

As an example, consider Alameda County, California. Alameda County was among the first counties to be under a SAH order when one was issued on March 16th, 2020. Here, $SAH_{Alameda,CA,Mar.28} = 13/7$, as Alameda County had been under Stay-at-Home policies for thirteen days. Los Angeles County, California, on the other hand, did not issue a SAH order before the State of California did so. We therefore set $SAH_{LosAngeles,CA,Mar.28} = 10/7$ since the state-wide order was issued in California on March 19th, 2020.

The previous two examples illustrate how, in some instances, county officials took action before the state in which they were located did. While we are able to use this county-level variation to study the impact of SAH orders on retail and workplace mobility, as measured by the Google mobility index, our main outcome of interest, new unemployment claims, is available to us only at the state-level.[7]

To aggregate county-level SAH orders to the state level, we construct a state-level measure of the duration of exposure to SAH orders by taking an employment-weighted average across counties in a given state. Formally, we calculate:

$$SAH_{s,t} \equiv \sum_{c \in s} \frac{Emp_{c,s}}{Emp_s} \times SAH_{c,s,t} \tag{1}$$

Employment for each county is the average level of employment in 2018 as reported by the BLS in the Quarterly Census of Employment and Wages (QCEW).[8] One can think of $SAH_{s,t}$ as the average number of weeks a worker in state s was subject to SAH orders by time t.

Figure 2 reports $SAH_{s,Apr.4}$ for each state in the U.S. and the District of Columbia. California had

[5]The most recent version of this page is available at `https://www.nytimes.com/interactive/2020/us/coronavirus-stay-at-home-order.html`. Over time, the *New York Times* stopped separately reporting sub-state orders when a state-wide SAH order was issued. We used the *Internet Archive* to verify the timing and location of SAH orders as reported in the *New York Times*.

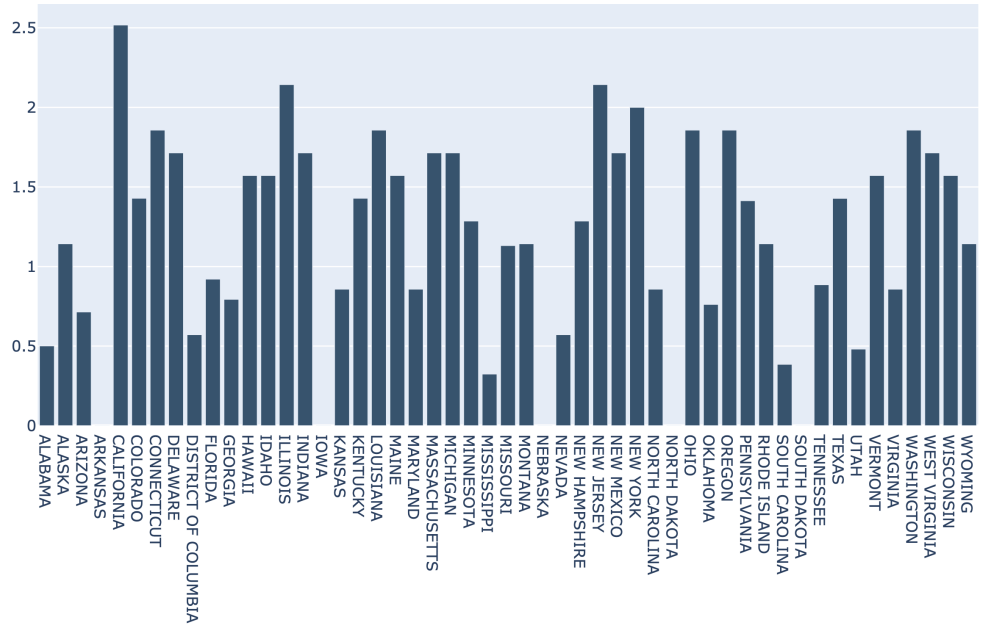
[6]When a city implements a SAH order, we assign that date to all counties in which that city is located—unless of course the county had already issued a SAH order.

[7]While we lack sufficient data to estimate county-level effects on UI claims, in Section 6 we consider county-level regressions in which we estimate the March to April change in log employment and the unemployment rate using data published by the Bureau of Labor Statistics. We find quantitatively similar results even after conditioning on state-level fixed effects.

[8]The annual averages by county in 2019 were, at the time of writing, not yet publicly available.

the highest exposure to SAH orders at 2.5, indicating that Californian workers were on average subject to SAH orders for two and a half weeks. Conversely, five states (Arkansas, Iowa, Nebraska, Northa Dakota, and South Dakota) had no counties under SAH orders by April 4. The average value across all states of $SAH_{s,Apr.4}$ is 1.2.

Figure 2: Employment-Weighted State Exposure to Stay-at-Home Policies Through Week Ending April 4



The Employment-Weighted exposure to SAH policies for a particular state is calculated by multiplying the number of weeks through April 4, 2020 that each county in the state was subject to SAH orders by the 2018 QCEW average employment share of that county in the state, and summing over each states' counties. Sources: Bureau of Labor Statistics, the *New York Times*; Authors' Calculations

2.2 Main Outcome Variable: State Initial Claims for Unemployment Insurance

Our main outcome of interest is initial unemployment insurance claims. Initial UI claims is among the highest-frequency real economic activity indicators available. As discussed in the introduction, initial claims for unemployment insurance for the week ending March 21st, 2020 were unprecedented,

with more than 3 million workers claiming benefits. By the end of that week, very few states or counties had issued SAH orders. Figure 1 shows that by March 21st, only around 20% of the U.S. population was under such directives. This suggests that a substantial portion of the initial economic disruption associated with the COVID-19 crisis may have occurred in the absence of SAH orders.

Let $UI_{s,t}$ indicate new unemployment insurance claims for state s at time t and UI_{s,t_0,t_1} denote cumulative unemployment claims for state s from time t_0 to t_1. In our baseline specification, we consider the effect of SAH orders on cumulative weekly unemployment insurance claims by state from March 14th, 2020 to April 4th, 2020:

$$UI_{s,Mar.21,Apr.4} = UI_{s,Mar.21} + UI_{s,Mar.28} + UI_{s,Apr.4} \tag{2}$$

We then normalize this variable by employment for each state, as reported in the 2018 QCEW, to construct our outcome variable of interest:

$$\frac{UI_{s,Mar.21,Apr.4}}{Emp_s} \tag{3}$$

Our choice of April 4th, 2020 as the end date for this regressions is driven by the observation that, by April 4th, 2020, approximately 95% of the U.S. population was under a SAH order. In Section 6, we consider 2-week and 4-week horizon specifications and find quantitatively similar results.

3 Empirical Specification

We now turn to our research design. Our main design is a state-level, cross-sectional regression:

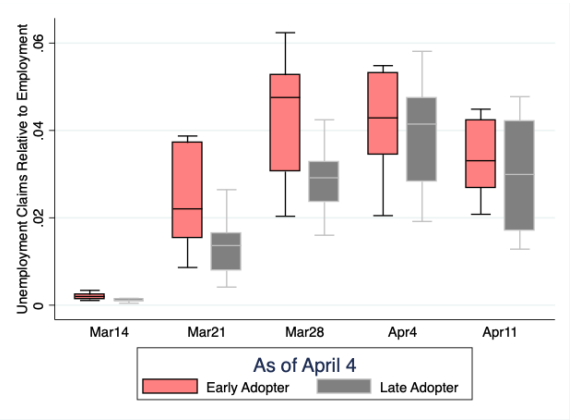
$$\frac{UI_{s,Mar.21,Apr.4}}{Emp_s} = \alpha + \beta_C \times SAH_{s,Apr.4} + X_s\Gamma + \epsilon_s \tag{4}$$

where α is a constant, β_C is the coefficient on state-level exposure to SAH orders, X_s is a vector of controls with associated vector of coefficients Γ, and ϵ_s represents the error term in this equation.

To illustrate the motivation for our empirical design, in Figure 3 we compare the evolution of UI claims to state employment of "early adopters," defined as those states being in the top quartile of SAH exposure through April 4, 2020, to that of "late adopters," defined as those states being in the bottom quartile.[9] This figure provides *prima facie* graphical evidence of the main result of

[9]The upper and lower edges of the boxes denote the interquartile range of each group, with the horizontal line denoting the median. As is standard, the "whiskers" denote the value representing 1.5 times the interquartile range

Figure 3: Box Plots by Week of Initial UI Claims Relative to Employment for Early and Late Adopters of SAH orders



For each state we calculate SAH exposure through April 4th by multiplying the number of weeks each county was subject to SAH through April 4 by the 2018 QCEW average employment share of that county in the state, and summing over each state's counties. Early adopters are those states in the top quantile of SAH exposure and late adopters are those states in the bottom quartile. Sources: Bureau of Labor Statistics, Department of Labor, and the *New York Times*; Authors' Calculations.

our paper: in the first few weeks, early adopters initially had a higher rise in unemployment claims relative to late adopters. By the week ending April 4th, 2020, the relative effect of adopting SAH orders early largely disappears, reflecting the fact that by this point approximately 95% of the U.S. population was under a SAH order, with most having been under the order for the full week ending April 4th.

This figure also suggests that SAH orders alone likely do not account for all of the rise in unemployment claims.[10] In the early weeks, late adopters also experienced historically unprecedented levels of UI claims even though early adopters had higher claims on average. For example, consider the week ending March 28. Here the difference between the median value of the two groups was approximately 1% of state employment; in that week, the median value of initial claims to employment for late adopters was roughly 3%, despite close to zero SAH exposure by this point. By April 4th, this difference almost completely disappears. Late adopters, who were under SAH orders for a much shorter period of time (or not at all, in some cases), converged to similar levels of unemployment claims relative to employment.

boundaries.

[10]We thank an anonymous referee for pointing out that this could have the alternative interpretation that local SAH order implementation had substantial negative spillover effects on the rest of the country. See Section 5 for a model-driven discussion of such potential spillover effects between states.

Confounding Factors

In order for our estimate $\hat{\beta}_C$ to have a causal interpretation, it must be the case that the timing of SAH orders implemented at the state and sub-state-level be orthogonal with unobserved factors affecting reported state-level UI claims.[11]

We provide further support for our causal interpretation by testing the magnitude and significance of the estimate $\hat{\beta}_C$ against the inclusion of three sets of important controls. The first set of controls considers the impact that the COVID-19 outbreak itself had on local labor markets. States that chose to implement SAH orders earlier may have done so simply because of the intensity, perceived or otherwise, of the local outbreak. In most macro-SIR models, a larger real outbreak would directly result in a larger drop in consumption due to a higher risk of contracting the virus associated with consumption activity (e.g. Eichenbaum, Rebelo, and Trabandt (2020)). To account for this concern, we control for the number of excess deaths, as reported by the Centers for Disease Control and Prevention (CDC), relative to population. We also include the share of the population over 60, as this demographic was more at risk of serious health complications arising from contracting COVID-19.

Additionally, one may be concerned that consumers' perceptions of the outbreak differed from its actual severity. During this time period, the reported number of new confirmed cases was an important statistic reported by the media. This statistic, which suffers from differential testing capability and definitions across states, differs from the measure of excess deaths as it focuses on how local labor markets may have interpreted the severity of the outbreak.[12] We therefore also include the total confirmed cases relative to population.[13] Note that the severity of the outbreak would lead to an upward bias in our estimate $\hat{\beta}_C$ if states were more likely to enact SAH orders when the local outbreak was worse or perceived to have been worse, which may itself have led to labor market disruptions.[14]

[11] An additional reason for preferring April 4th is that over longer horizons, there is greater risk of omitted variable bias (i.e. $\mathbb{C}ov[\epsilon_s SAH_{s,Apr.4}] \neq 0$). A salient example is the rollout of the Paycheck Protection Program (PPP) on April 3rd. (The PPP was a central component of the CARES Act, a two trillion fiscal relief package signed into law on March 27, 2020. The PPP authorized $350 billion dollars in potentially forgivable SBA guaranteed loans.) This program provided forgivable loans to small businesses affected by the economic fallout of the pandemic, so long as those loans were used to retain workers. On the margin, PPP incentivizes firms to not lay off their workers, which would tend to lower UI claims for the week after April 4th. Depending upon how this interacts with the differential timing of SAH implementation, the bias could go in either direction.

[12] Evidence from Fetzer et al. (2020) suggests that the arrival of confirmed COVID-19 cases leads to a sharp rise in measures of economic anxiety, which would have an effect on real economic activity through the change in household and firm beliefs about the future state of the economy.

[13] We rely upon confirmed COVID-19 cases as compiled at the county-by-day frequency by USAFacts. USAFacts is a non-profit organization that compiles these data from publicly available sources, typically from daily reports issued by state and local officials. See `https://usafacts.org/visualizations/coronavirus-covid-19-spread-map/` for more details.

[14] Our controls for excess deaths and confirmed cases are taken as cumulative sums as of the end of the sample period, which is April 4th in the benchmark analysis. We experimented with using lagged values of these measures as pre-period controls, and they had no effect on the magnitude or significance of our coefficient of interest. These

The second set of controls we consider relates to the political economy of the state government. Some states may have had more generous social safety nets that led workers to separate from firms earlier than in states with less generous policies. Moreover, states with generous policies may also have been more likely to respond earlier to the pandemic, thereby generating bias. To account for this concern, we consider two political economy controls. First, we include the average UI replacement rate in 2019, as reported by the Department of Labor's Employment and Training Administration.[15] Second, we include the Republican vote share in the 2016 presidential election.[16] The first measure is designed to capture the generosity of the social safety net, while the latter is meant to capture political constraints on state and local officials to implement various public health NPIs.

Finally, our last set of controls is intended to address the concern that the timing of SAH implementation may be related to the sectoral composition within each state, and therefore the magnitude of job losses experienced by that state irrespective of SAH orders. To address this concern, we use a measure of predicted state-level UI claims as determined by industry composition within each state and the monthly change in jobs as reported in the national jobs report in March by the BLS. These numbers are based on a survey reference period that concluded on March 14th, 2020—fortuitously for us, two days before any SAH order was announced. Specifically we construct a Bartik-style control:

$$B_s = \sum_i \Delta \ln Emp_{i,March} \times \omega_{i,s} \tag{5}$$

where $\Delta \ln Emp_{i,March}$ is the monthly percentage change in employment in industry i (3-digit NAICs) for the month of March. $\omega_{i,s}$ is the share of employment in industry i in the state, as reported in the QCEW for 2018.

We also control for the extent of work-at-home capacity at the state-level. Dingel and Neiman (2020) construct an index denoting the share of jobs that can be done at home by cities, industries, and countries. We construct a state-level index by taking an state employment-weighted average of the Dingel and Neiman (2020) industry-level (2-digit NAICS) work-at-home index. It may be the case that states with a higher capacity to work from home may have been willing to implement SAH orders earlier if the labor market disruption of such policies was perceived to be lower when more workers are able to work from home. If this index is correlated with the number of initial UI claims received by the state in the absence of implementing SAH orders, then failing to include this

results are available upon request.

[15] See `https://oui.doleta.gov/unemploy/ui_replacement_rates.asp` for more details.

[16] As reported by the *New York Times* at `https://www.nytimes.com/elections/2016/results/president`.

control would introduce bias.[17]

Causal interpretations aside, the cross-sectional framework is nevertheless constrained in only answering the following question: By how much did UI claims increase in a state that implemented SAH orders *relative* to a state that did not? The constant term absorbs, for example, the general equilibrium effects of stay-at-home orders which would affect all states within the U.S.—not just those implementing SAH orders. To the extent that other states' labor markets were affected in any way by the local imposition of SAH orders, then $\hat{\beta}_C$ will fail to capture the *entire* effect of such policies. We postpone discussion of the mapping between the relative effect of SAH orders and their aggregate effect until after presenting our cross-sectional results.

Figure 4: Scatterplot of SAH Exposure to Cumulative Initial Weekly Claims for Weeks Ending March 21 thru April 4

The Employment-Weighted exposure to SAH policies for a particular state is calculated by multiplying the number of weeks through April 4, 2020 that each county in the state was subject to SAH orders by the 2018 QCEW average employment share of that county in the state, and summing over each states' counties. UI claims are cumulative new claims between weeks ending March 21, 2020 and April 4, 2020, divided by average 2018 QCEW average employment in the state. The size of each bubble is proportional to state population; The color gradient of each observation is determined by the number of confirmed COVID-19 cases per thousand people.

Sources: Bureau of Labor Statistics, Census Bureau, the *New York Times*, USAFacts.org, Department of Labor; Authors' Calculations

[17] In unreported regressions, we study whether the effect of SAH orders differentially depends upon the value of the work-at-home index; we find no evidence that this is the case.

4 Results

4.1 Effects of SAH Orders on State-Level UI Claims

In Table 1, we present results from estimating Equation (4). Column (1) shows the univariate specification, with no controls. The point estimate of approximately 1.9% (SE: 0.67%) implies that a one-week increase in exposure to SAH orders raises the number of claims as a share of state employment by 1.9% relative to states that did not implement SAH orders. Figure 4 displays this result graphically. The bubbles are shaded according to the intensity of the confirmed COVID-19 cases per thousand people and the size of the bubbles are proportional to state population.

In Column (2), we control for the number of confirmed COVID-19 cases per one thousand people, excess deaths by state, and the share of state population over the age of 60. As discussed, these are intended to control for factors related to the pandemic that might simultaneously affect both the timing of SAH implementation and the severity of state labor market disruptions. The change in the coefficient is immaterial—economically and statistically. In Column (3) we control for political economy factors: the state's UI replacement rate in 2019 and the 2016 Trump vote share. Our estimate $\hat{\beta}_C$ falls only slightly to 1.8%. In Column (4) we include controls for each state's sectoral composition (and in turn its sensitivity to both the pandemic-induced crisis and timing of SAH implementation). Our point estimate is again largely unchanged. Finally, in column (5), we select a parsimonious specification that captures dimensions of each set of controls. We control for confirmed cases, excess deaths, the UI replacement rate, and the WAH index (the only significant variable). In this specification, which is our preferred specification, the estimate of β_C is still 1.9%.[18,19]

Our results support the idea that policies that work to flatten the pandemic curve also imply a steepening of the recession curve (Gourinchas, 2020). To quantify this steepning of the recession curve, we use our point estimate of the relative effect on state-level UI claims of SAH orders to calculate a back-of-the-envelope estimate of the total implied number of UI claims between March 14 and April 4 attributable to SAH orders. We calculate the relative-implied estimate as

[18] In the appendix, we consider three additional robustness exercises at the state-level. We alternate the horizon over which the model is estimated (2 and 4 weeks), estimate the model by weighted least squares, and re-estimate the model dropping one state at a time. The results are quantitatively and qualitatively similar.

[19] In unreported regressions, we find that, when including all regressors, $\hat{\beta}_C$ is somewhat attenuated—albeit statistically indistinguishable from our baseline estimate; however, this attenuation is largely driven by the parametric assumption of linearity on the share of votes for Trump in 2016, which places substantial leverage on Wyoming and West Virginia. Dropping these states from the full specification with all control variables yields a point estimate of 1.8% (SE: 0.75%). These regressions are available upon request.

follows:[20]

$$\text{Relative-Implied-Aggregate-Claims} = \sum_s \hat{\beta}_C \times SAH_{s,Apr.4} \times Emp_s \tag{6}$$

where s indexes a particular state. This is a back-of-the-envelope calculation as it simply scales up the cross-sectional coefficient $\hat{\beta}_C$ according to each state's SAH exposure through April 4, 2020 and each state's level of employment.

This back-of-the-envelope calculation yields an estimate of 4 million UI claims attributable to SAH orders through April 4. Ignoring cross-regional spillovers, this relative-implied estimate suggests that approximately 24% of total claims through April 4, 2020 were attributable to such orders.

This calculation does not incorporate general equilibrium effects or spillovers that may have arisen as a result of local SAH implementation. As we discuss in Section 5, when the SAH order is interpreted as a local productivity shock, this represents an upper bound on aggregate employment losses; when, however, the SAH implementation is treated as a local demand shock, the analysis is a bit subtler. Yet, even in this case, we find that at most the relative-implied aggregate multiplier understates true employment aggregate employment losses by a factor of 2. Through the lens of the model, this provides an upper bound on total employment losses attributable SAH orders: 8 million UI claims through April 4, or approximately half of the overall spike in claims during the initial weeks of the economic crisis induced by the COVID-19 pandemic.

An alternative back-of-the-envelope calculation to assess the magnitude of our estimate is to instead focus the relative contribution of SAH orders in terms of typical cross-sectional variation in UI claims in our sample. Our estimates imply that a state which implemented SAH orders one week earlier saw an increase in UI claims by 1.9% of its 2018 employment level relative to a state one week later, which is slightly less than 50% of the cross-sectional standard deviation of employment-normalized claims between weeks ending March 21 and April 4.[21]

4.2 High Frequency Effects on Proxies for Local Economic Activity

In this subsection, we provide additional evidence that the SAH orders had immediate and highly localized effects on daily indicators of local demand and local supply. This exercise is important because of concerns that the state-level effects we estimate above simply reflect differential labor market disruptions that would have occurred in the absence of SAH orders in precisely those places most likely to implement SAH orders earliest. Additionally, the results thus far do not distinguish between whether the effects of SAH orders are in terms of supply or demand.

[20] We use the terminology "relative-implied" because in the cross-section we are only able to identify effects of SAH orders relative to states not implementing SAH orders. We discuss this issue at greater length in Section 5.

[21] We thank an anonymous referee for this particular recommendation.

Table 1: Effect of Stay-at-Home Orders on Cumulative Initial Weekly Claims Relative to State Employment for Weeks Ending March 21 thru April 4, 2020

	(1) Bivariate	(2) Covid	(3) Pol. Econ.	(4) Sectoral	(5) All
SAH Exposure thru Apr. 4	0.0194***	0.0192**	0.0178**	0.0209***	0.0187**
	(0.00664)	(0.00742)	(0.00818)	(0.00637)	(0.00714)
COVID-19 Cases per 1K		-0.00213			0.00194
		(0.00621)			(0.00676)
Excess Deaths per 1K		0.0446			0.0480
		(0.109)			(0.113)
Share Age 60+		0.237			
		(0.281)			
Avg. UI Replacement Rate			0.0719		0.0726
			(0.0794)		(0.0787)
2016 Trump Vote Share			-0.0225		
			(0.0508)		
Work at Home Index				-0.331+	-0.388+
				(0.192)	(0.229)
Bartik-Predicted Job Loss				-2.401	
				(7.528)	
Constant	0.0815***	0.0357	0.0621	0.181**	0.182**
	(0.00848)	(0.0543)	(0.0481)	(0.0742)	(0.0821)
Adj. R-Square	0.0829	0.0434	0.0618	0.0966	0.0763
No. Obs.	51	51	51	51	51

This table reports results from estimating equation (4): $\frac{UI_{s,Mar.21,Apr.4}}{Emp_s} = \alpha + \beta_C \times SAH_{s,Apr.4} + X_s\Gamma + \epsilon_s$, where each column considers a different set of controls X_s. Column (5)—a parsimonious model controlling for pandemic severity, political economy factors, and state sectoral composition—is our benchmark specification. The dependent variable in all columns is our measure of cumulative new unemployment claims as a fraction of state employment, as calculated in Equation (3). The interpretation of the SAH Exposure coefficient ($\hat{\beta}_C$; top row) is the effect on normalized new UI claims of one additional week of state exposure to SAH. The Employment-Weighted exposure to SAH for a particular state is calculated by multiplying the number of weeks through April 4, 2020 that each county in the state was subject to SAH with the 2018 QCEW average employment share of that county in the state, and summing over each states' counties.

Robust Standard Errors in Parentheses

+ $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

We estimate the local effect of SAH using high frequency proxies for economic activity from Google's Community Mobility Report, which measures changes in visits to establishments in various categories, such as retail and work.[22] Early on in the COVID-19 pandemic, Google began publishing data documenting how often its users were visiting different types of establishments. The data are reported as values relative to the median visitation rates by week-day between January 3, 2020 and

[22] `https://www.google.com/covid19/mobility/`

February 6, 2020.[23],[24]

We use the retail and workplace mobility indices because these two indices are consistently recorded for the time sample we study. Failing to find an effect on these proxies for local economic activity would call into question the results we find in the aggregate, at the state-level. We interpret retail mobility as broadly representing "demand" responses to SAH orders and workplace mobility as representing "supply," at least on-impact. Over longer-horizons, workers laid off because of demand-side disruptions will, naturally, cease commuting to and from work.

Formally, we estimate event studies of the following form:

$$Mobility_{c,t} = \alpha_c + \phi_{CZ(c),t} + \sum_{k=\underline{K}}^{\overline{K}} \beta_k SAH_{c,t+k} + X_{c,t} + \underline{D}_{c,t} + \overline{D}_{c,t} + \varepsilon_{c,t} \tag{7}$$

where $Mobility_{c,t}$ represents either the retail or workplace mobility index published by Google for county c on day t, and $SAH_{c,t}$ is a dummy variable equal to 1 on the day a county imposes SAH orders. We set $\underline{K} = -17$ and $\overline{K} = 21$ so that the analysis examines three weeks prior and two and half weeks following the imposition of SAH orders.[25] The event study is estimated over the period February 15th through April 24th, 2020. We non-parametrically control for county size by discretizing county employment into fifteen equally sized bins and interacting each bin with time fixed effects. α_c refers to the inclusion of county fixed effects. To isolate the local effect of SAH orders on economic activity, we also include commuting zone-by-time fixed effects.[26] This implies that our event-study estimates are identified only off of differential timing of SAH implementation among counties contained within the same commuting zone.

Results for retail mobility are presented in Figure 5. The day SAH orders went into effect, there was an immediate decline of approximately 2% in retail mobility. This falls further to 7% the day after SAH order implementation, before slowly recovering to approximately 2% lower retail mobility two and half weeks following the SAH order imposition.[27] The large transitory dip may reflect sentiment among consumers to shut-in before revisiting grocery stores and pharmacies. Alternatively, the

[23]One possible limitation of this data is that the sample of accounts included in the surveys is derived from only those with Google Accounts who opt into location services. We believe sample selection bias is unlikely to be a major concern given Google's broad reach (there are over 1.5 billion Gmail accounts, for example).

[24]Note that for privacy reasons, data is missing for some days for some counties. When possible, we carry forward the last non-missing value. Excluding counties with missing values yields the same result; this figure is available from the authors upon request.

[25]Because our sample is necessarily unbalanced in event-time, we also include "long-run" dummy variables, $\underline{D}_{c,t}$ and $\overline{D}_{c,t}$. $\underline{D}_{c,t}$ is equal to 1 if a county imposed SAH orders at least $\overline{K}$ days prior. $\overline{D}_{c,t}$ is equal to 1 if a county will impose SAH at least $\underline{K}$ periods in the future.

[26]We use the United States Department of Agriculture (USDA) 2000 county to commuting zone crosswalk. This is available at `https://www.ers.usda.gov/data-products/commuting-zones-and-labor-market-areas/`.

[27]Restricting the sample to exclude never-takers yields the same result. This design identifies the mobility effects off of counties that ultimately implemented SAH orders but at different times.

transitory nature of the shock may reflect negative, within-labor market spillovers of SAH orders, given our inclusion of commuting zone-by-time fixed effects. Regardless, the lack of a pretrend is noticeable and provides additional support for a causal interpretation.

Results for retail mobility capture the demand effect of SAH orders. However, SAH orders may have also had an effect on the supply of locally produced goods and services. For example, SAH orders may affect firms' ability to produce by preventing households from accessing their places of work. To test the importance of the supply channel, we re-estimate our event study using workplace mobility as the outcome variable.[28]

Figure 6 shows the result. As with the retail mobility event study, the workplace mobility index exhibits no differential pre-trend prior to the county-level imposition of SAH orders. In the first two days following the imposition of SAH orders, workplace mobility declined sharply relative to non-treated counties within its commuting zone. This relative decline in workplace mobility persists for nearly two and half weeks following. This suggests a sharp reduction in firms' ability to produce using labor.

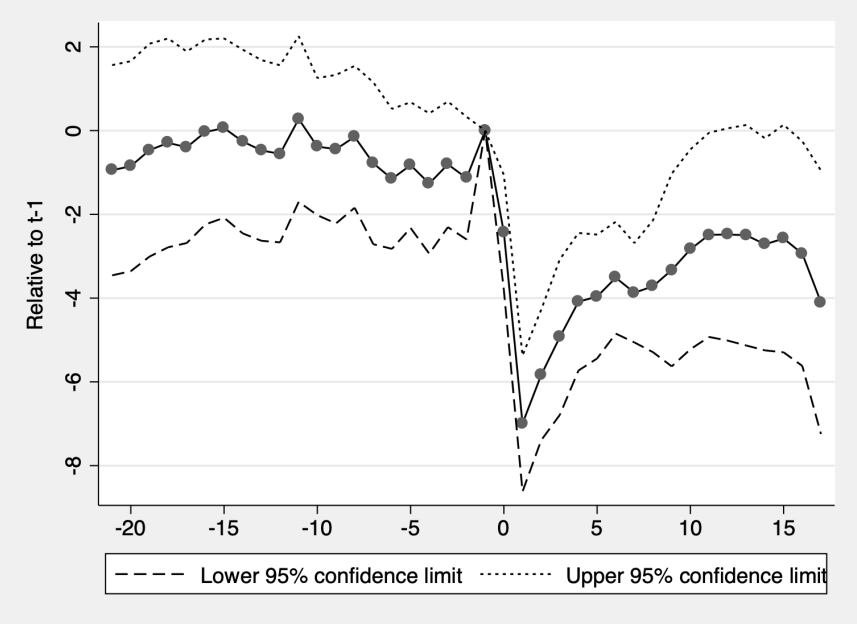
We draw two conclusions from these high-frequency event studies. First, the lack of pretrends in the event studies suggest that the timing of SAH orders can be seen as plausibly randomly assigned with respect to local labor market conditions. This provides corroborating evidence for our cross-sectional identification strategy. In particular, it suggests that there were real effects of the SAH orders on local economies. Second, SAH orders had *both* local supply and local demand effects. Both retail mobility and workplace mobility fell substantially on impact and remained persistently low for at least two weeks following implementation of SAH orders.

5 Aggregate Versus Relative Effects

Our empirical strategy relies on cross-sectional variation in the timing and location of SAH orders to identify the relative effect such policies had on labor markets during the initial weeks of the COVID-19 outbreak in the United States. In this section, we discuss in greater detail the sorts of spillovers that are likely to be relevant and the conditions under which the relative-implied aggregate estimate (see equation (6)) represents a lower or upper bound on the aggregate effects of SAH orders on UI claims. This is important for how one should interpret our back-of-the-envelope calculation that in the early period of the crisis, approximately only 24% of UI claims through April 4, 2020 were related to SAH orders.

[28] An obvious concern with simply replacing the outcome variable is that changes in workplace mobility, unlike retail mobility, is highly dependent on the ability of individuals to work from home. The timing of SAH orders may be partially driven by the ability of workers in some regions to transition to working at home. In unreported regressions, we also non-parametrically control for this possibility by partitioning the WAH variable into 15 equally sized bins and interacting each bin with time fixed effects. The event study is essentially unchanged.

Figure 5: County Retail Mobility Event Study



This figure plots estimated coefficients from the county-level, event-study specification in equation (7), where coefficients have been normalized relative to one day prior to county-level SAH orders went into effect. The model includes as controls county fixed effects, commuting zone-by-time fixed effects, and indicators for county employment bins interacted with time to non-parametrically control for county size. The outcome variable is the retail mobility index published in Google's Community Mobility Report. This index is constructed using visits and duration of visits to retail establishments. The time unit is days.

Standard Errors: Two-Way Clustered by County and Day

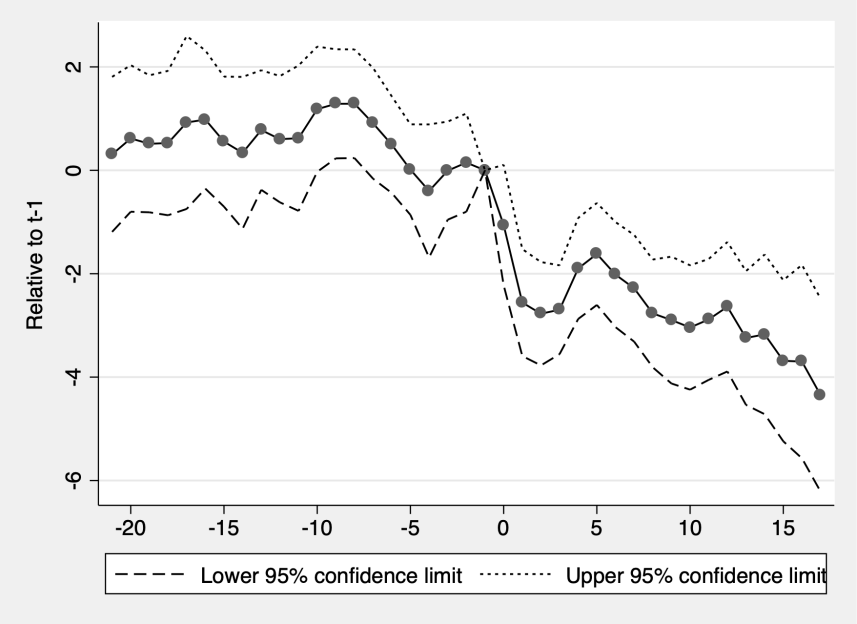
Sources: Google, the *New York Times*; Census Bureau; United States Department of Agriculture; Authors' Calculations

To the extent that there are cross-regional (either positive or negative) spillovers of SAH orders, our estimate will not capture the *aggregate* effect of SAH orders. This limitation is related to the stable unit value (SUTVA) assumption in the causal inference literature, which requires that potential outcomes be independent of the treatment status of other observational units. Because of considerable trade between U.S. states, SUTVA is likely to be violated in our setting.[29]

To guide our discussion, we use a benchmark currency-union model to study the effects of SAH orders on the local economy, the rest of the currency union, and the entire economy as a whole. We present results for an economy characterized either by sticky prices or flexible prices, with SAH orders modeled as either a pure local demand shock or a pure local productivity/supply shock; the

[29]SUTVA violations are likely to be more salient in the cross-section when the model is estimated over longer horizons. This is, in part, why we choose as our baseline the 3-week horizon specification.

Figure 6: County Workplace Mobility Event Study



This figure plots estimated coefficients from the county-level, event-study specification in equation (7), where coefficients have been normalized relative to one day prior to county-level SAH orders went into effect. The model includes as controls county fixed effects, commuting zone-by-time fixed effects, and indicators for county employment bins interacted with time to non-parametrically control for county size. The outcome variable is the workplace mobility index published in Google's Community Mobility Report. This index is constructed using visits and duration of visits to places of employment. The time unit is days.

Standard Errors: Two-Way Clustered by County and Day

Sources: Google, the *New York Times*; Census Bureau; United States Department of Agriculture; Authors' Calculations

evidence from Subsection 4.2 suggests that both channels were operative.[30] We then briefly summarize other important cross-regional spillovers not well-captured by the currency model we study. The most salient of these spillovers relate to the *informational* effect of early SAH implementation in some parts of the country.

5.1 Currency Union Model: Supply and Demand Shock Implications of SAH Orders

In this section, we consider the implications of local demand or supply shocks in a benchmark currency union model under either sticky or flexible prices. The model we consider is a simpler version

[30] Additionally, as is discussed in Brinca, Duarte, and Faria-e Castro (2020), it is appropriate to view the COVID-19 pandemic (and associated policy responses) as some combination of demand and supply shocks. We consider pure demand and supply shocks to illustrate the economic implications of each in isolation.

of the baseline, separable utility, complete markets model presented in Nakamura and Steinsson (2014), modified to incorporate productivity shocks and discount rate shocks (to model negative local supply and demand shocks, respectively).[31] We follow Nakamura and Steinsson (2014) in calibrating the model to the U.S. setting. The full model specification is relegated to the Appendix; here we present only those aspects of the model modified to study the effects of SAH orders.

5.1.1 Modeling SAH Orders

Our first model experiment is to treat the implementation of SAH orders as a pure local demand shock. To incorporate this into the model, we introduce a consumption preference shock, δ_t. This preference shock causes home region households to prefer, all else equal, delaying consumption into the future. This may be a reasonable way to model the SAH shock for a variety of reasons. First, to the extent that the drop in retail mobility, as shown in Figure 5, represents a decline in goods consumption, households may simply be delaying such purchases until temporarily closed stores reopen. Second, the inability to purchase locally furnished goods and services may lead households to temporarily save more than they might otherwise choose to do, which would be observationally equivalent to a discount rate shock only to consumption.

Households in the home region maximize the present discounted value of expected utility over current and future consumption C_t and labor supply N_t.

$$\mathbb{E}_0 \sum_{t=0}^{\infty} \beta^t \left[\delta_t \frac{(C_t)^{1-\sigma}}{1-\sigma} - \chi \frac{(N_t)^{1+\psi}}{1+\psi} \right],$$

where β is the rate of time discounting, σ is the inverse intertemporal elasticity of substitution, ψ is the inverse Frisch elasticity of labor supply, and χ is the weight on labor supply. The discount rate shock process follows

$$\log \delta_t = \rho^{\delta} \log \delta_{t-1} + \epsilon_t^{\delta}. \tag{8}$$

We close the household side of the model by assuming preferences for varieties are constant elasticity of substitution (CES), which gives rise to the standard CES demand curve via cost minimization.

Alternatively, the SAH orders may be modeled as a local productivity shock. Even if demand for

[31] Implications from a model with different preference structures (e.g. Greenwood, Hercowitz, and Huffman (1988) preference) and with incomplete market are qualitatively the same. Unlike the original focus of Nakamura and Steinsson (2014), the model we consider does not incorporate government spending shocks, as that is not our focus in this paper.

locally produced goods is unchanged, firms may be constrained in supplying the goods and services demanded by local households or by the rest of the currency union. We model this interpretation as a region-level productivity shock for intermediate-goods-producing firms. A firm i in the home region faces the following production function

$$y_{h,t}(i) = A_t N_{h,t}(i)^{\alpha},$$

where $y_{h,t}(i)$ is the output of a firm i, $N_{h,t}(i)$ is the amount of labor input hired by the firm, and A_t is region-wide technology in the home region. α is the returns to scale parameter on labor. The aggregate supply shock A_t evolves according to the following process:

$$\log A_t = \rho^A \log A_{t-1} + \epsilon_t^A. \tag{9}$$

Firms maximize profits subject to demand by households. Nominal rigidities are specified à la Calvo (1983) with associated price-reset parameter θ.

Finally, we close the model by assuming bond markets are complete, labor markets are perfectly competitive, and, when prices are sticky, the monetary authority follows a union-wide Taylor rule. A full derivation is available in the Appendix.

5.1.2 Model Results: Modeling SAH Order Shocks under Flexible and Sticky Prices

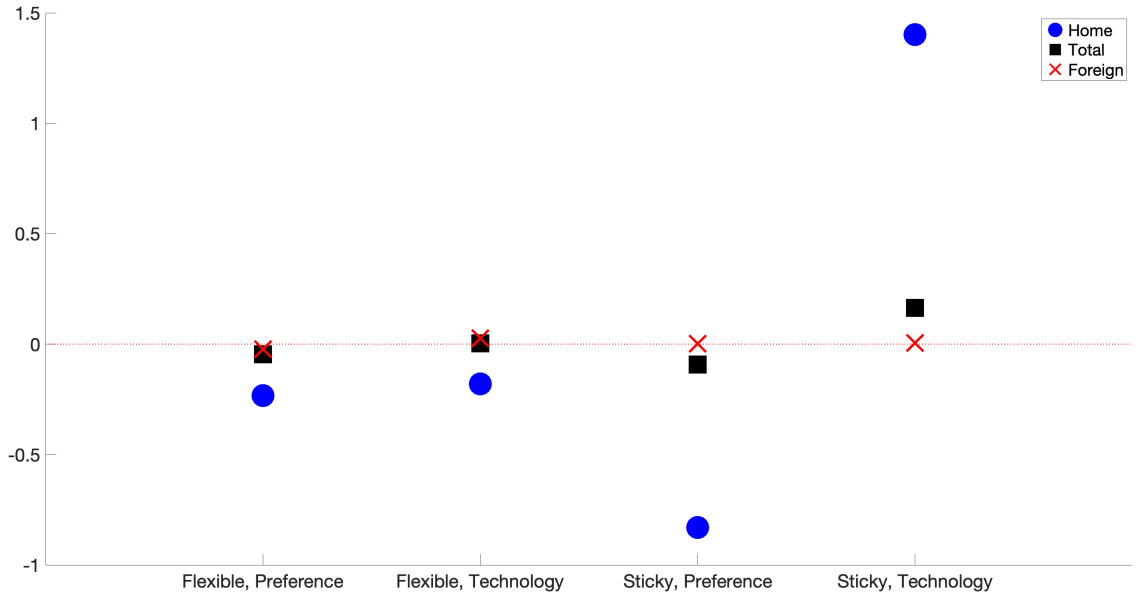
We model the implementation of SAH orders as a one-time negative shock with either $\epsilon_t^{\delta} = -1$ (for local demand shocks) or $\epsilon_t^A = -1$ (for local supply shocks). We choose zero decay parameters on the shock series to illustrate the dynamics of the model in settings in which the shock induced by the SAH order is temporary. Specifically, we set $\rho^A = \rho^{\delta} = 0$. For the purposes of mapping the relative-implied employment losses to aggregate employment losses, this is without loss for the results for the technology shock but not without loss with respect to the demand shock with sticky prices. Below, we discuss what happens when the demand shock exhibits some persistence.

We calibrate the remaining variables according to Nakamura and Steinsson (2014) (see their Section III.D.). When working with the sticky price model, we set the Calvo parameter $\theta = 0.75$. In the flexible price model, we set $\theta = 0$.

We consider each of the two types of shocks in isolation under either sticky prices or fully flexible prices. In each of the four scenarios, we calculate the on-impact responses of home region employment, foreign region employment, and aggregate employment to the local shock. Because the model is calibrated to a quarterly frequency and because our empirical design estimates the relative effect over a short horizon (3-weeks), the relevant horizon for mapping the model to the cross-section is the *on-impact* relative effect between employment in the shocked home region and the non-shocked

foreign region.

Figure 7: On-Impact Response of Home Employment, Foreign Employment, and Union-Wide Employment to a Local SAH-induced: (i) Technology Shock with Flexible Prices, (ii) Technology Shock with Sticky Prices, (iii) Preference Shock with Flexible Prices, and (iv) Preference Shock with Sticky Prices



This figure shows the on-impact responses of aggregate employment and employment in each region to local demand (preference) and supply (technology) shocks with flexible or sticky prices. Each column represents different scenarios. In both all cases, the shocks persist for a single quarter only ($\rho^{\delta} = \rho^{A} = 0$; see equations (8) and (9)). The blue circles show the responses of employment in a home region, the red crosses are the responses of employment in a foreign region, and the black squares are the responses of aggregate employment. In the first three scenarios, the on-impact effect of home region employment declines *relative* to employment in the foreign region; this is consistent with our cross-sectional estimates of a positive coefficient on SAH exposure. The final column in which prices are sticky an the SAH orders are modeled as a technology shock produces a counterfactual prediction that employment is higher in the home region relative to the foreign region.

The results from these exercises are reported in Figure 7 and Table 2. Figure 7 shows the *on-impact* responses of employment in a home region (blue circles) and a foreign region (red crosses), and aggregate employment (black squares) under the four different scenarios. Table 2 then compares the relative-implied aggregate employment calculated from the differences between the responses of home and foreign employment and the responses of aggregate employment under different scenarios.[32]

[32]Formally, the relative-implied estimate in the model is calculated as $n(\ell_t - \ell_t^*)$, where ℓ_t and ℓ_t^* represent log deviations from steady state of home and foreign region per-capita employment respectively. n is the size of the home-region. This is exactly the model-analog of the relative-implied estimate reported in equation (6).

Table 2: On-Impact Response of Union-Wide Employment and Relative-Implied Aggregate Employment to a Local SAH-induced: (i) Preference Shock with Flexible Prices, (ii) Preference Shock with Sticky Prices, (iii) Technology Shock with Flexible Prices, and (iv) Technology Shock with Sticky Prices

	Flexible				Sticky		
	Total	Implied	Factor		Total	Implied	Factor
Preference Shock	-0.047	-0.021	2.21	$\rho^{\delta} = 0.9$	-0.032	-0.075	0.43
				$\rho^{\delta} = 0.0$	-0.093	-0.083	1.12
Technology Shock	0.003	-0.021	-0.16		0.1642	0.1398	1.18

This table shows the on-impact responses of aggregate employment and the relative-implied employment to a local demand (preference) and supply (technology) shocks with flexible or sticky prices. The columns labeled "Total" correspond to the model-implied on-impact aggregate employment change (i.e. a population-weighted average of the employment change in the home and foreign regions). The columns labeled "Implied" correspond to the relative-implied aggregate change in the model. This is calculated as the difference between the on-impact employment effect in the home region and the on-impact employment effect in the foreign region, together multiplied by the size of the home region. This is the model analog of the relative-implied aggregate estimate in equation (6). A negative value for the implied column implies that the model is consistent with our cross-sectional estimate. The columns labeled "Factor" takes the ratio of the on-impact aggregate employment effect to the relative-implied effect. A negative value in this column (Flexible prices and Technology shock) implies that the relative-implied employment effect is of the opposite sign to the aggregate employment effect.

In the model, only three of the four stylized scenarios we consider produce relative effects of SAH orders that are consistent with the positive coefficient we estimate in the data. When the SAH orders are modeled as local productivity shocks, only the flexible price equilibrium produces an immediate, relative decline in employment in the home region subject to the shock. When the SAH orders are instead modeled as local demand shocks, both the sticky price and flexible price economies produce a steeper decline in the shocked home region's employment relative to the rest of the economy, as suggested by the cross-sectional evidence presented above.

When SAH orders are modeled as negative productivity shocks with fully flexible prices, the immediate, relative effect of SAH orders is an *upper bound* on the aggregate employment effect over the same horizon. This is because the decline in local employment arising from the SAH order is offset by an increase in employment in the rest of the economy. The mechanism is that in the flexible price case, the negative productivity shock in the home region translates into an improvement in the foreign region's terms of trade. This, in turn, increases labor demand in the foreign region, which increases employment in the foreign region.

In contrast, when prices are fully flexible in response to an SAH-induced home-region demand shock, the relative-implied estimate represents a *lower* bound on aggregate employment losses. This is because employment in both the home and foreign regions fall in response to the shock. With prices being fully flexible, the negative preference shock in the home region leads to a decline

in prices for home goods relative to foreign goods, making foreign consumption more expensive. This, in turn, decreases demand for foreign goods, resulting in a decline in foreign employment, which is necessary for market clearing. When prices are fully flexible and the effect of SAH orders is a pure local demand shock, aggregating the relative employment losses understates the aggregate employment losses by a factor of about two (see Table 2, Row 1, Column 3).

The case with sticky prices and SAH orders modeled as a pure local demand shock lies in between the previous two scenarios. When the local demand shock is sufficiently persistent, the immediate, relative effect of SAH orders could potentially *overstate* the aggregate employment effect. This is because employment in the foreign region increases on impact. Meanwhile, when the demand shock has essentially no persistence, so that it only affects demand in the home region for a single quarter, employment in the foreign region also falls on impact, implying that the (aggregated) relative employment effect again understates aggregate employment losses, in the quarter of the shock (See Figure 7). Regardless, the degree to which this on-impact effect understates aggregate employment losses is bounded above by the response under flexible prices to a local demand shock.

The evidence presented in Subsection 4.2 suggests that SAH orders represented a shock to both the supply of and demand for locally produced goods. This on its own implies that the flexible price, preference shock scenario provides a non-binding upper bound on aggregate employment losses. Specifically, in this scenario the relative-implied aggregate estimate would understate employment losses by roughly a factor of two. The distance from this upper bound increases, moreover, with price rigidity and the persistence of the SAH shock. In the baseline calibration, when prices are sticky and the demand shock has no persistence, the relative-implied job losses understates aggregate employment losses by 12%.

5.2 Other Cross-Regional Spillovers

The benchmark currency-union model presented in the previous section illustrates how locally implemented SAH orders would affect the local economy, other regions in the currency union and the entire economy as a whole. The spillover forces in the model work through the trade in goods between regions and associated price and expenditure switching effects. However, there may be other important cross-regional spillovers that are not well-captured by the model, but may nevertheless be important for interpreting our empirical results in light of the aggregate effects of SAH orders.

An important example is an *informational effect* of early SAH implementation in some parts of the economy. For example, the early imposition of SAH orders in some regions may signal to the rest of the country that a SAH order is likely to be imposed some time in the near future. This informational channel can be incorporated into the model by assuming that the foreign region learns, on-impact, that a SAH order will be imposed in the foreign region in the subsequent period. We

experimented with this specific informational channel of local SAH order implementation and found that the upper and lower bounds provided in the previous subsection continued to hold.[33]

A more subtle informational effect of SAH implementation relates to the credible signal it sends about the severity of the COVID-19 pandemic and the potential economic disruptions it is likely to induce, even in the absence of any additional SAH orders. In this interpretation, the SAH orders have spillover effects on the rest of the economy through the changes they induce to beliefs held by households and firms about the future path of the economy. As opposed to other signals conveyed by public officials about the severity of the pandemic, SAH implementation is a credible signal because it imposes non-trivial costs on the economy. This could, in turn, lead to a reduction in demand as a result of increased economic anxiety and fear of exposure to the COVID-19.

If this second informational effect of local SAH implementation ultimately led to job losses throughout the rest of the country, then our relative-implied estimate would understate the aggregate job losses attributable to SAH orders. Neither the model nor the empirical design takes this particular spillover mechanism into account. We view understanding the role of SAH orders as credibly communicating the severity of the pandemic as an important and interesting avenue for future research.[34]

Another important example is spillovers through firm networks—internal and external.[35] For example, complex supply chains may cause economic activity to decline in parts of the country where SAH orders are not yet enacted if the sourcing of intermediate inputs is affected. Alternatively, national chains may close establishments located in regions without SAH orders due to losses in other major markets with SAH orders. Arguably, these sorts of spillovers would lead our relative-implied estimate of job losses to understate true aggregate employment losses. However, we believe these channels are minor, as the adjustments would need to occur over a very short period time. The horizon of our empirical specifications is three weeks, during which time existing inventories were likely to be sufficient for production.[36]

[33] These results are available upon request.

[34] Coibion, Gorodnichenko, and Weber (2020a) provide evidence that local SAH orders led households in the affected regions to hold more pessimistic views of the future path of the economy. This is a separate, though related, channel than the *aggregate* change in beliefs that may have occurred following the early imposition of SAH orders.

[35] We thank an anonymous referee for pointing this out.

[36] It is a well known observation that inventories generally adjust more slowly to changes in sales, consistent with the claim that this particular source of bias is most relevant at lower frequencies and longer horizons. (See Ramey and West, 1999; Bils and Kahn, 2000).

6 Alternative Specifications

6.1 Panel Specification

One concern with the cross-sectional specifications is that there may be some unobserved aggregate factor that induced large increases in UI claims at the same time that states and local municipalities implemented SAH orders. Alternatively, there may be time-invariant state-specific factors that drove both increases in unemployment claims and SAH orders. To address these concerns, we employ a panel specification, which allows us to control for week and state fixed effects.

We modify the specification so that the outcome variable is the flow value of initial claims on date t and the SAH order treatment is the share of the *current week* that a state was subject to SAH orders, where we take a weighted average of county-level exposure as before.[37]

$$\frac{UI_{s,t}}{Emp_s} = \alpha_s + \phi_t + \beta_P \times SAH_{s,t,t-7} + \mathbf{X}_{s,t}\Gamma + \epsilon_{s,t} \tag{10}$$

We consider a variety of state-time controls. We include two lags of $SAH_{s,t,t-7}$ to account for dynamics in the effect of SAH orders on unemployment claims. Additionally, we include the share of the population that works from home, the number of confirmed cases per one thousand people, and the Bartik-style employment control from before. Each of these three controls is interacted with a dummy equal to one for weeks ending March 21st, 2020 and onward.[38] We estimate the following fixed effects panel regression on weekly observations for the week ending January 4 through the week ending April 11.[39]

Table 3 provides our estimate of $\hat{\beta}_P$ for the contemporaneous effect and two lags. Column (1) presents the results with no lags. The point estimate of 0.90% (SE: 0.35%) suggests that a full week of SAH order exposure increased unemployment claims by .90% of total state-level employment. In column (2), we include two lags of SAH orders. The point estimate on the contemporaneous effect is little changed, though it rises slightly. Importantly, neither of the coefficients on the first nor the second lag is significant. This result suggests that, in our sample, that SAH orders have constant, contemporaneous effects on UI claims. At longer horizons, we would suspect non-linearities to eventually kick in, with the effect of SAH orders declining. Finally, our point estimates are little changed when including additional controls in Column (3).

[37] Because in our sample no state or local municipality reopened, once $SAH_{s,t,t-7} = 1$ it remains equal to one for all remaining weeks.

[38] Note that because our measures of work-from-home and employment loss are constant across time, we are controlling for the relative effect of each from before the week ending March 21st.

[39] We drop the first two weeks in all specifications to ensure the sample size is constant throughout.

Table 3: Panel Specification: Effect of Stay-at-Home Orders on Initial Weekly Claims Relative to State Employment

	(1)	(2)	(3)	(4)
SAH Exposure Current Week	0.00919**	0.0101***	0.00997***	0.0125***
	(0.00350)	(0.00321)	(0.00329)	(0.00353)
SAH Exposure First Lag		-0.00293	-0.00367	-0.00299
		(0.00359)	(0.00358)	(0.00372)
SAH Exposure Second Lag		0.00245	-0.00115	0.000809
		(0.00230)	(0.00302)	(0.00332)
State FE	Y	Y	Y	N
Week FE	Y	Y	Y	Y
Post-March 21 X Work at Home Index	N	N	Y	Y
Post-March 21 X Excess Deaths per 1K	N	N	Y	Y
Post-March 21 X COVID-19 Cases per 1K	N	N	Y	Y
Post-March 21 X Avg. UI Replacement Rate	N	N	Y	Y
Adj. R-Square	0.826	0.822	0.831	0.801
No. Obs.	765	663	663	663

This table reports results from estimating equation (10): $\frac{UI_{s,t}}{Emp_s} = \alpha_s + \phi_t + \beta_P \times SAH_{s,t,t-7} + \mathbf{X}_{s,t}\Gamma + \epsilon_{s,t}$, where each column considers a different set of controls X_s. The dependent variable in all columns is weekly initial unemployment claims as a fraction of state employment. The interpretation of the SAH Exposure coefficient ($\hat{\beta}_P$; top row) is the effect on normalized new UI claims of a full week of state exposure to SAH. The Employment-Weighted exposure to SAH for a particular state is calculated by multiplying the share of the current week each county in the state is subject to SAH by the 2018 QCEW average employment share of that county in the state, and summing over each states' counties. UI claims are cumulative new claims during the period, divided by average 2018 QCEW average employment in the state.

Standard Errors Clustered by State in Parentheses

+ $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Our estimates $\hat{\beta}_P$ in the first three columns tend to be somewhat lower than what we find in our benchmark, cross-sectional design. In particular, the panel design implies that each week of SAH exposure increased UI claims by 1% of state employment; in contrast, our estimates of $\hat{\beta}_C$ imply that each week of SAH exposure increased UI claims by approximately 1.9% of state employment. While, at first glance, β_C and β_P aim to estimate the same moment, the inclusion of state and time fixed effects imply that they are not directly comparable.[40] In column (4), we consider the panel specification in which we drop state fixed effects, to make the panel and cross-sectional regressions comparable: the point estimate rises to 1.2% and is statistically indistinguishable from what we find in the cross-section.

[40]See Kropko and Kubinec (2020) for a discussion of the proper interpretation of two-way fixed effect estimators in relation to one-way fixed effect estimators.

6.2 County-Level Employment and Unemployment Effects

Another major concern with the estimates of Equation (4) is that states may have experienced substantial difficulty in scaling up their systems to process the historically unprecedented numbers of unemployment claims. For example, it is well known that some states' unemployment insurance systems rely on archaic computer programming languages.[41] Thus, it is reasonable to be worried that states with more cumbersome systems may systematically report lower UI claims numbers relative to those states with more efficient systems.

A priori, the induced omitted variable bias could go in either direction. On the one hand, states with stronger UI systems may have also been more inclined to respond aggressively to the COVID-19 pandemic with SAH orders, generating an upward bias in our estimates. On the other hand, the severity of labor market disruptions from the COVID-19 pandemic may have both made it more difficult for states to process new claims *and* made them more likely to impose SAH orders earlier—thus, generating a downward bias. While we have already controlled for measures of COVID-19 in our estimates of Equation (4), in this subsection we present an alternative design at the county-level using employment and unemployment as outcomes, albeit at a lower frequency. Using total employment, rather than unemployment insurance claims, allows us to sidestep the issue of whether states could meet demand for UI claims. This design also allows for the inclusion of state fixed effects to identify the relative effect of SAH orders using within-state variation in the timing of SAH implementation.

We analyze the effects of SAH orders at the county-level relying upon local area unemployment and employment statistics constructed by the Bureau of Labor Statistics (BLS). The downside is that this data is constructed at the monthly frequency, rather than the weekly frequency in our main specification. The BLS primarily relies upon the Current Population Survey (CPS) as the primary input into constructing estimates of county-level employment and unemployment.[42] Fortunately, the survey reference periods for the CPS aligns quite nicely with measuring household employment and unemployment just prior to the broad implementation of SAH orders and one month hence. The reference week for the CPS for March 2020 was March 8th through March 14th and the reference week for April was April 12th through April 18th.

We estimate analogs of our state-level regression at the county-level, using as our outcome variable either the log change in employment or the change in the unemployment rate between March 2020 and April 2020. County-level treatment is the weekly SAH exposure through April 15, 2020.

[41] See, for example, "'COBOL Cowboys' Aim To Rescue Sluggish State Unemployment Systems" by NPR (`https://www.npr.org/2020/04/22/841682627/cobol-cowboys-aim-to-rescue-sluggish-state-unemployment-systems`).

[42] For additional details on the methodology employed by the Bureau of Labor Statistics, see .

Formally, we estimate the following regression by ordinary least squares:

$$\Delta y_{c,s,April} = \alpha_s + \beta^{y}_{C,county} \times SAH_{c,s,Apr.15} + X_{c,s}\Gamma + \epsilon_{c,s} \tag{11}$$

where $y_{c,s,April}$ indicates the monthly change between March and April in either log employment or the unemployment rate. α_s are state-level fixed effects which control for all state-level policies implemented between mid-March and mid-April that may have been systematically related to observed UI claims during that period. We also report results when constraining $\alpha_s = \alpha$ to provide a natural benchmark against our state-level regression. We also control for the number of confirmed COVID-19 cases per thousand people and the WAH index, which are our only controls available at the county-level.[43]

Because the first outcome variable we consider at the county-level is the log change in county employment, we expect that the estimated relative effect of SAH orders on local employment, $\hat{\beta}^{emp}_{C,county}$, will be comparable to our estimate of the same parameter at the state-level.[44] If the timing of the decentralized implementation of SAH orders was orthogonal to state-level economic conditions and if there were negligible spillovers from treated counties to untreated counties within the same state, then we would expect to see a relatively stable coefficient regardless of whether we include state fixed effects, α_s, or not.

Table 4 provides the results for the effects of SAH orders on employment. The first column shows the results restricting $\alpha_s = \alpha$ (e.g., no state fixed effects). The point estimate suggests that the relative effect of SAH exposure on employment at the county-level is to reduce employment by of -1.8% (SE: .57%). That we use a different outcome variable and different level of disaggregation yet obtain a coefficient of similar magnitude is encouraging.

Columns (2) and (3) focus on the 12 states for which there is variation across counties in the timing of SAH orders. The magnitude of the estimate falls by about one third, regardless of whether we include controls—although this difference is not statistically significant. If, as we argue above, the timing of SAH implementation was orthogonal to policies and economic conditions at the state-level[45], then the decline in the point estimate is suggestive evidence of negative spillovers between treated and untreated counties. While this may be the appropriate interpretation, it appears that the bulk of employment losses were nevertheless concentrated within the labor markets in which SAH orders were implemented.

[43] We control for the number of confirmed COVID-19 cases through April 15th to align with the timing of the surveys used by the BLS to construct county-level employment and unemployment statistics.

[44] Note that because we use the 2018 QCEW to normalize UI claims at the state-level, we should expect the county-level estimates to be slightly lower in magnitude since the state-level regressions calculates the percent change off of a smaller base value.

[45] And the average treatment effect among counties in the twelve states appearing in columns (2)-(4) is the same as for counties.

Table 4: County-Level Specification: Effect of Stay-at-Home Orders on Local Employment Growth

	(1) $\Delta \ln Emp$	(2) $\Delta \ln Emp$	(3) $\Delta \ln Emp$	(4) $\Delta \ln Emp$
SAH Exposure thru Apr. 15	-0.0176***	-0.0124**	-0.0130**	-0.00906**
	(0.00568)	(0.00464)	(0.00454)	(0.00400)
Covid-19 Cases per 1K Emp			-0.0000223	-0.0000981
			(0.0000301)	(0.000107)
Work at Home Index			0.0548	0.0548
			(0.0456)	(0.0542)
Constant	-0.0824***	-0.113***	-0.129***	-0.135***
	(0.0147)	(0.00900)	(0.0157)	(0.0139)
Dep Mean	-0.12	-0.14	-0.14	-0.14
States	51.00	12.00	12.00	12.00
State FE	No	Yes	Yes	Yes
CZ FE	No	No	No	Yes
Adj. R-Square	0.10	0.62	0.63	0.74
No. Obs.	3141.00	1116.00	1116.00	453.00

This table reports results from estimating equation (11): $\Delta \ln Emp_{c,s,April} = \alpha_s + \beta^{Emp}_{C,county} \times SAH_{c,s,Apr.15} + X_{c,s}\Gamma + \epsilon_{c,s}$, where each column considers a different set of controls X_s. The dependent variable in all columns is $\Delta \ln Emp$, which refers to the log change in county employment between March, 2020 and April, 2020 as estimated by the BLS. SAH exposure for a particular county is calculated as the number of weeks that the county was subject to SAH orders through April 15, 2020. Columns (2) thru (4) include state fixed effects; Column (3) includes fixed effects for USDA defined commuting zones (CZ).

Standard Errors Clustered by State in Parentheses

+ $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Finally, in the last column, we include commuting zone fixed effects and find that the coefficient is roughly a third of the effect estimated in column (3). Following a similar logic as in the previous paragraph, this would suggest that not only were the bulk of employment losses concentrated within the labor market, they were moreover concentrated within the specific counties in which the SAH orders were implemented.

Table 5 provides the results for the effects of SAH orders on the change in the county-level unemployment rate. As with the employment specification, the first column does not include state fixed effects. In columns (2) and (3) we include state fixed effects; in the final column, we condition further on commuting zone fixed effects. Consider the result reported in column (3), the state fixed effects specification with controls for local COVID-19 pandemic and capacity for the local labor force to work from home: the point estimate is 1.5 (SE: 0.331), implying that each week of SAH exposure at the county-level increased the local unemployment rate by 1.5.

In sum, we view the panel and county-level results as corroborating evidence of the main result in this paper: that the cross-sectional effect of SAH orders had real costs to the labor markets in

Table 5: County-Level Specification: Effect of Stay-at-Home Orders on Local Unemployment Rate

	(1) ΔUR	(2) ΔUR	(3) ΔUR	(4) ΔUR
SAH Exposure thru Apr. 15	1.574***	1.382***	1.572***	0.945***
	(0.400)	(0.331)	(0.331)	(0.218)
Covid-19 Cases per 1K Emp			-0.000409	0.00944
			(0.00416)	(0.00739)
Work at Home Index			-12.28**	-5.443
			(5.335)	(5.106)
Constant	4.114***	4.425***	7.920***	6.705***
	(0.888)	(0.642)	(2.004)	(1.867)
Dep Mean	7.69	7.11	7.11	7.32
States	51.00	12.00	12.00	12.00
State FE	No	Yes	Yes	Yes
CZ FE	No	No	No	Yes
Adj. R-Square	0.13	0.39	0.40	0.59
No. Obs.	3141.00	1116.00	1116.00	453.00

This table reports results from estimating equation (11): $\Delta UR_{c,s,April} = \alpha_s + \beta^{UR}_{C,county} \times SAH_{c,s,Apr.15} + X_{c,s}\Gamma + \epsilon_{c,s}$, where each column considers a different set of controls $X_{c,s}$. The dependent variable in all columns is ΔUR, which refers to the change in the county unemployment rate between March, 2020 and April, 2020 as estimated by the BLS. SAH exposure for a particular county is calculated as the number of weeks that the county was subject to SAH orders through April 15, 2020. Columns (2) thru (4) include state fixed effects; Column (3) includes fixed effects for commuting zones (CZ) classified by the USDA in 2000.

Standard Errors Clustered by State in Parentheses

+ $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

the early weeks of the crisis, but that such costs were likely dwarfed by other factors. While not inconsistent with our state-level analysis, broadly the panel and county-level designs yield somewhat lower point estimates than in our benchmark specification. In this respect, relative to a null that all observed UI claims were attributable to SAH orders, the state-level specification yields the most conservative estimate of the relative effect of such orders on local labor markets. Through the lens of our theoretical model, these cross-sectional estimates imply, at most, a non-binding upper bound of half of total UI claims through April 4, 2020 being attributable to SAH orders.

7 Conclusion

While non-pharmaceutical interventions (NPIs) are necessary to slow the spread of viruses such as COVID-19, they likely steepen the recession curve. But to what extent? We provide estimates of how much one prominent NPI disrupted local labor markets in the short run in the U.S. in the early weeks of the coronavirus pandemic.

In particular, we investigate the effect of Stay-at-Home (SAH) orders on new unemployment claims

in order to quantify the causal effect of NPIs (i.e., flattening the pandemic curve) on economic activity (i.e., steepening the recession curve). The decentralized implementation of SAH orders in the U.S. induced both geographic and temporal variation in when regions were subject to restrictions on economic and social mobility. Between March 14th and April 4th, the share of workers under such orders rose from 0% to almost 95%. This rise was gradual but steady, with new areas implementing SAH orders on a daily basis. We couple this variation in SAH implementation with high-frequency unemployment claims data to quantify the resulting economic disruption.

We find that a one-week increase in stay-at-home orders raised unemployment claims by 1.9% of state-level employment. This estimate is robust to a battery of controls, including the severity of the local COVID-19 pandemic, the local political economy response, and the industry mix of the local economy. Using Google mobility data, we find evidence of both supply and demand driven effects. A back-of-the-envelope calculation using our estimate implies that SAH orders resulted in a rise of 4 million unemployment insurance claims, about a quarter of the total unemployment insurance claims during this period. A stylized currency union model suggests that in some empirically relevant cases, this estimate can be seen as an upper bound. When it instead represents a lower bound, it at most understates job losses by a factor of two.

Our estimates imply that a sizeable share of the increase in unemployment in the early weeks of the COVID-19 crisis was due to other channels, such as decreased consumer sentiment, stock market disruptions, and social distancing that would have occurred in the absence of government orders. We see our paper as providing tentative evidence that removing government SAH orders may relieve only a fraction of the economic disruption arising from the COVID-19 pandemic while at the same time exacerbating the public health crisis, thus implying that the economic disruption may persist until the pandemic itself is resolved.

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A Additional Empirical Results

A.1 Alternative Cross-Sectional Specifications

The first type of robustness check we do is varying the horizon over which the cross-sectional regression is estimated, considering two natural alternative specifications: a two week horizon and a four week horizon. For the two week horizon specification, we consider cumulative initial claims between March 14 and March 28 regressed on SAH exposure over the same window; for the four week specification, the end date is April 11. We include the same set of controls as in our benchmark specification (Table 1, Column (5)).

Columns (1) and (2) of Table 6 report the results from varying the horizon over which the model is estimated. Relative to our baseline result of 1.9%, estimating the model over just two weeks lowers the point estimate slightly to 1.83% (SE: 0.91%). Conversely, when the model is estimated over a four week horizon, the point estimate is 1.7% (SE: 0.59%).

In Column (3) of Table 6 we estimate the effect of SAH exposure on UI claims, over the same three week horizon as in the benchmark case, weighting observations by state-level employment from the QCEW in 2018 (an approached advocated for by some papers in the local multiplier literature).[46] Again, we consider the same set of controls as in our benchmark specification. The point estimate from the WLS regression is elevated slightly: 2.10% (SE: 0.54%). Regardless, weighting delivers quantitatively similar estimates.

[46] For arguments in either direction, see Ramey (2019) and Chodorow-Reich (Forthcoming), respectively. See also Solon, Haider, and Wooldridge (2015).

Table 6: Effect of Stay-at-Home Orders on Cumulative Initial Weekly Claims Relative to State Employment: (i) 2-Week Horizon, (ii) 4-Week Horizon, (iii) Weighted Least Squares

	(1) Thru Mar. 28	(2) Thru Apr. 11	(3) WLS
SAH Exposure (varied horizons)	0.0183**	0.0166***	0.0209***
	(0.00908)	(0.00592)	(0.00541)
COVID-19 Cases per 1K	0.00197	0.000854	-0.00472
	(0.0109)	(0.00463)	(0.00306)
Excess Deaths per 1K	-0.0819	0.0691	0.214**
	(0.0959)	(0.0787)	(0.106)
Work at Home Index	-0.152	-0.587**	-0.486+
	(0.184)	(0.261)	(0.258)
Constant	0.111+	0.303***	0.242**
	(0.0649)	(0.0920)	(0.0921)
Adj. R-Square	0.0125	0.129	0.172
No. Obs.	51	51	51

This table reports results from estimating equation (4): $\frac{UI_{s,Mar.21,T}}{Emp_s} = \alpha + \beta_C \times SAH_{s,T} + X_s\Gamma + \epsilon_s$, where columns (1) and (2) estimate the model over horizon T = March 28, 2020 and T = April 11, 2020; column (3) estimates the model with T =April 4, 2020 by weighted least squares, weighting by state employment. In line with our benchmark specification (Table 1, Column (5)), in each column we specify a parsimonious model controlling for pandemic severity, political economy factors, and state sectoral composition. The dependent variable in all columns is our measure of cumulative new unemployment claims as a fraction of state employment, as calculated in Equation (3). The interpretation of the SAH Exposure coefficient ($\hat{\beta}_C$; top row) is the effect on normalized new UI claims of one additional week of state exposure to SAH. The Employment-Weighted exposure to SAH for a particular state is calculated by multiplying the number of weeks through T that each county in the state was subject to SAH with the 2018 QCEW average employment share of that county in the state, and summing over each states' counties.

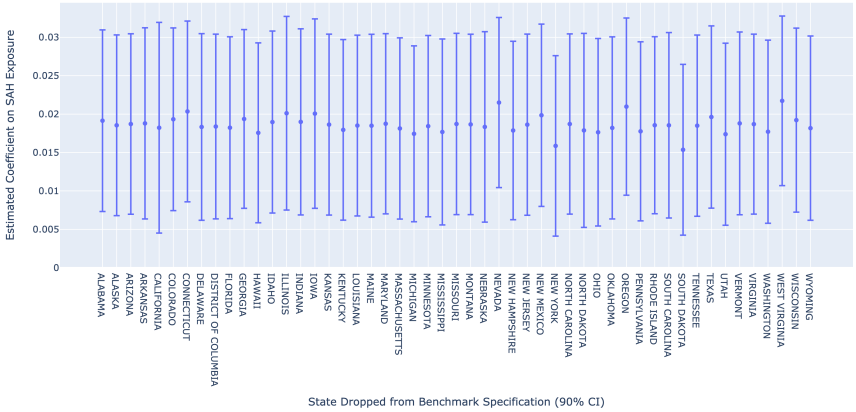
Robust Standard Errors in Parentheses

+ $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.2 Influence of Specific States

One may also be concerned that individual states' responses, either in terms of rising unemployment claims or SAH orders, is driving our results. To understand whether this is the case, we replicate our benchmark specification (column (5) in Table 1) from above, dropping one state at a time. The resulting coefficient estimates for β_C are available in Figure 8, along with 90 percent confidence intervals constructed from robust standard errors.

Figure 8: Benchmark Specification Estimated Dropping One State at a Time



This figure reports results from estimating equation (4): $\frac{UI_{s,Mar.21,Apr.4}}{Emp_s} = \alpha + \beta_C \times SAH_{s,Apr.4} + X_s\Gamma + \epsilon_s$, dropping one state at a time from the estimation. The set of controls, X_s, are those that appear in the benchmark specification (Table 1, Column (5))—a parsimonious model that controls for pandemic severity, political economy factors, and state sectoral composition. The dependent variable is our measure of cumulative new unemployment claims as a fraction of state employment, as calculated in Equation (3). The interpretation of the SAH Exposure coefficient ($\hat{\beta}_C$; top row) is the effect on normalized new UI claims of one additional week of state exposure to SAH. The Employment-Weighted exposure to SAH for a particular state is calculated by multiplying the number of weeks through April 4, 2020 that each county in the state was subject to SAH with the 2018 QCEW average employment share of that county in the state, and summing over each states' counties.

B Local SAH Orders in a Currency Union Model

We develop a framework to help us interpret the "relative effect"—which we estimate in the data—as compared to the "aggregate effect" of stay-at-home orders. To that end, we use a simple version of Nakamura and Steinsson (2014) of a two-country monetary union model, albeit abstracting from government spending as that is not the focus of our paper.

Households

Consider a currency union comprised of two regions: a home region of size n, and a foreign region of size $1-n$. In each region, there are infinitely many households with *identical* preferences and initial wealth.

A household j in home region has the following preferences:

$$\mathbb{E}_0 \sum_{t=0}^{\infty} \beta^t \left[\delta_t \frac{\left(C_t^j\right)^{1-\sigma}}{1-\sigma} - \chi \frac{\left(N_t^j\right)^{1+\psi}}{1+\psi} \right]$$

where

$$C_t^j = \left[\phi_H^{\frac{1}{\eta}} \left(C_{H,t}^j\right)^{\frac{\eta-1}{\eta}} + \phi_F^{\frac{1}{\eta}} \left(C_{F,t}^j\right)^{\frac{\eta-1}{\eta}} \right]^{\frac{\eta}{\eta-1}}, \text{ with } \phi_H + \phi_F = 1,$$

$$C_{H,t}^j = \left(\int_0^n \left(\frac{1}{n}\right)^{\frac{1}{\epsilon}} c_{h,t}^j(i)^{\frac{\epsilon-1}{\epsilon}} \mathrm{d}i \right)^{\frac{\epsilon}{\epsilon-1}}, \quad C_{F,t} = \left(\int_n^1 \left(\frac{1}{1-n}\right)^{\frac{1}{\epsilon}} c_{f,t}^j(i)^{\frac{\epsilon-1}{\epsilon}} \mathrm{d}i \right)^{\frac{\epsilon}{\epsilon-1}}.$$

Total consumption of a household j in a home region is a CES aggregator of a *bundle* of home goods, $C_{H,t}^j$ and a *bundle* of foreign goods, $C_{F,t}^j$. Here, ϕ_F denotes the steady state share of the foreign goods imported from by a household in the home region. When $\phi_H = 1 - \phi_F > n$, there is home bias.[47] η is the elasticity of substitution between home goods and imported goods from a foreign region, and ϵ denotes the elasticity of substitution across differentiated goods. β is discount factor and δ_t denotes consumption-preference shock in a home region, which evolves according to the following law of motion:

$$\log \delta_t = \rho^\delta \log \delta_{t-1} + \epsilon_t^\delta.$$

[47] In the baseline calibration following Nakamura and Steinsson (2014), we calibrate $\phi_H = 0.69$ and $n = 0.1$, so that there is significant home bias.

Then optimal allocations of expenditures (per household) are given by

$$C_{H,t}^j = \phi_H \left(\frac{P_{H,t}}{P_t}\right)^{-\eta} C_t^j, \quad C_{F,t} = \phi_F \left(\frac{P_{F,t}}{P_t}\right)^{-\eta} C_t^j,$$
$$c_{h,t}^j(i) = \left(\frac{p_{h,t}(i)}{P_{H,t}}\right)^{-\epsilon} C_{H,t}^j, \quad c_{f,t}^j(i) = \left(\frac{p_{f,t}(i)}{P_{F,t}}\right)^{-\epsilon} C_{F,t}^j,$$

with price indices defined as follows:

$$P_t = \left[\phi_H P_{H,t}^{1-\eta} + \phi_F P_{F,t}^{1-\eta}\right]^{\frac{1}{1-\eta}},$$
$$P_{H,t} = \left[\frac{1}{n}\int_0^n p_{h,t}(i)^{1-\epsilon} \mathrm{d}i\right]^{\frac{1}{1-\epsilon}},$$
$$P_{F,t} = \left[\frac{1}{1-n}\int_n^1 p_{f,t}(i)^{1-\epsilon} \mathrm{d}i\right]^{\frac{1}{1-\epsilon}}.$$

Here, P_t denotes consumper price index of a home region, and $P_{H,t}$ ($P_{F,t}$) is producer price index of home (foreign) goods.

In our baseline specification, we assume identical households in a given region with the same initial wealth and *complete* financial markets, which makes aggregation straightforward. Thus, we have

$$c_{h,t}(i) \equiv \int_0^n c_{h,t}^j(i)\mathrm{d}j = \left(\frac{p_{h,t}(i)}{P_{H,t}}\right)^{-\epsilon} C_{H,t}, \quad c_{f,t}(i) \equiv \int_0^n c_{f,t}^j(i)\mathrm{d}j = \left(\frac{p_{f,t}(i)}{P_{F,t}}\right)^{-\epsilon} C_{F,t}$$
$$C_{H,t} = \int_0^n C_{H,t}^j \mathrm{d}j = \phi_H \left(\frac{P_{H,t}}{P_t}\right)^{-\eta} C_t, \quad C_{F,t} = \int_n^1 C_{F,t}^j \mathrm{d}j = \phi_F \left(\frac{P_{F,t}}{P_t}\right)^{-\eta} C_t,$$
$$C_t = \int_0^n C_t^j \mathrm{d}j = nC_t^j,$$

where variables without j superscript are aggregate variables in a home region.

With the optimal allocations, we can write household j's budget constraint (in real terms with the home region's CPI as a numeraire) as follows:

$$C_t^j + \mathbb{E}_t\left[M_{t,t+1}B_{t+1}^j\right] \leq B_t^j + \frac{W_t}{P_t}N_t^j + \int_0^1 \frac{\Xi_{h,t}^j(i)}{P_t}\mathrm{d}i - \frac{T_t^j}{P_t}.$$

Note that W_t is home region's nominal wage, and N_t^j is a household j's labor supply. Here, we assume perfect immobility across the regions, meaning wages will be determined at the regional level. B_{t+1}^j is a household j's state-contingent asset holdings and note again that we assume complete financial markets. Here P_t denotes price index that gives the minimum price of one unit of consumption

good, C_t. *i.e.* P_t is the Consumer Price Index (CPI) in the home region.

Optimality conditions for $j \in (0, n]$ are

$$\chi \left(N_t^j\right)^{\psi} = \delta_t \left(C_t^j\right)^{-\sigma} \frac{W_t}{P_t},$$
$$\delta_t \left(C_t^j\right)^{-\sigma} = \beta \mathbb{E}_t \left[\delta_{t+1} \left(C_{t+1}^j\right)^{-\sigma} \frac{1+i_t}{1+\pi_{t+1}}\right],$$

where i_t is one-period nominal spot interest rate which satisfies $\mathbb{E}_t[M_{t,t+1}] = 1/(1+i_t)$.

Households in the foreign region are symmetric relative to those in the home region, and we use $*$ to denote foreign variables. So we have

$$C_t^{*j} = \left[(\phi_H^*)^{\frac{1}{\eta}} \left(C_{H,t}^{*j}\right)^{\frac{\eta-1}{\eta}} + (\phi_F^*)^{\frac{1}{\eta}} \left(C_{F,t}^{*j}\right)^{\frac{\eta-1}{\eta}} \right]^{\frac{\eta}{\eta-1}}, \quad \text{with} \quad \phi_H^* + \phi_F^* = 1.$$

For *aggregate* optimal allocations in the foreign region, we have

$$c_{h,t}^*(i) \equiv \int_n^1 c_{h,t}^{*j}(i) \mathrm{d}j = \left(\frac{p_{h,t}^*(i)}{P_{H,t}^*}\right)^{-\epsilon} C_{H,t}^*, \quad c_{f,t}^*(i) \equiv \int_n^1 c_{f,t}^{*j}(i) \mathrm{d}j = \left(\frac{p_{f,t}^*(i)}{P_{F,t}^*}\right)^{-\epsilon} C_{F,t}^*$$
$$C_{H,t}^* = \int_n^1 C_{H,t}^{*j} \mathrm{d}j = \phi_H^* \left(\frac{P_{H,t}^*}{P_t^*}\right)^{-\eta} C_t^*, \quad C_{F,t}^* = \int_n^1 C_{F,t}^{*j} \mathrm{d}j = \phi_F^* \left(\frac{P_{F,t}^*}{P_t^*}\right)^{-\eta} C_t^*,$$
$$C_t^* = \int_n^1 C_t^{*j} \mathrm{d}j = (1-n) C_t^{*j}.$$

Optimality conditions for foreign households for $j \in [n, 1)$ are

$$\chi \left(N_t^{s,j*}\right)^{\psi} = \delta_t^* \left(C_t^{j*}\right)^{-\sigma} \frac{W_t^*}{P_t^*},$$
$$\delta_t^* \left(C_t^{j*}\right)^{-\sigma} = \beta \mathbb{E}_t \left[\delta_{t+1}^* \left(C_{t+1}^{j*}\right)^{-\sigma} \frac{1+i_t}{1+\pi_{t+1}^*}\right].$$

Terms of Trade, and Real Exchange Rate

Before moving on to firms in each region, let us define terms showing the relationships between various price measures. First, we define terms of trade, S_t as

$$S_t \equiv \frac{P_{F,t}}{P_{H,t}}.$$

From this, we can write the relationship between CPI and Producer Price Index (PPI) in a home region as:

$$g(S_t) \equiv \frac{P_t}{P_{H,t}} = \left[\phi_H + \phi_F S_t^{1-\eta}\right]^{\frac{1}{1-\eta}}, \quad \frac{P_t}{P_{F,t}} = \frac{P_t}{P_{H,t}} \frac{P_{H,t}}{P_{F,t}} = \frac{g(S_t)}{S_t}.$$

For the case of the foreign region, we have

$$g^*(S_t) \equiv \frac{P_t^*}{P_{H,t}^*} = \left[\phi_H^* + \phi_F^* S_t^{1-\eta}\right]^{\frac{1}{1-\eta}}, \quad \frac{P_t^*}{P_{F,t}^*} = \frac{P_t^*}{P_{H,t}^*} \frac{P_{H,t}^*}{P_{F,t}^*} = \frac{g^*(S_t)}{S_t}.$$

Finally, we write the real exchange rate in terms of $g(S_t)$ and $g^*(S_t)$ as follows:

$$Q_t = \frac{P_t^*}{P_t} = \frac{g^*(S_t)}{g(S_t)}.$$

Firms

We assume that there is a continuum of intermediate-goods-producing firms in each region, producing differentiated intermediate goods by using labor as input. We assume a competitive labor market.

Production technologies of each intermediate-goods-producing firms are given by

$$y_{h,t}(i) = A_t N_{h,t}(i)^\alpha, \quad \alpha < 1,$$
$$y_{f,t}(i) = A_t^* N_{f,t}^*(i)^\alpha, \quad \alpha < 1,$$

where $y_{h,t}(i)$ ($y_{f,t}(i)$) is the production output of a firm i in the home (foreign) region, $N_{h,t}(i)$ ($N_{f,t}^*(i)$) is the amount of labor input hired by a firm i in the home (foreign) region, and A_t (A_t^*) is region-wide technology in the home (foreign) region. Both technology processes evolve according to the following laws of motion:

$$\log A_t = \rho^A \log A_{t-1} + \epsilon_t^A,$$
$$\log A_t^* = \rho^{A*} \log A_{t-1}^* + \epsilon_t^{A*}$$

This implies that region-wide labor demand can be written as

$$\begin{aligned}
N_t &= \int_0^n N_{h,t}(i)\mathrm{d}i = \int_0^n \left(\frac{y_{h,t}(i)}{A_t}\right)^{\frac{1}{\alpha}} \mathrm{d}i = \left(\frac{1}{A_t}\right)^{\frac{1}{\alpha}} \int_0^n y_{h,t}(i)^{\frac{1}{\alpha}} \mathrm{d}i \\
&= \left(\frac{Y_{H,t}}{A_t}\right)^{\frac{1}{\alpha}} \int_0^n \frac{1}{n} \left(\frac{p_{h,t}(i)}{P_{H,t}}\right)^{-\frac{\epsilon}{\alpha}} \mathrm{d}i = \left(\frac{Y_{H,t}}{A_t}\right)^{\frac{1}{\alpha}} \Delta_t^{\frac{1}{\alpha}}, \\
N_t^* &= \int_0^n N_{f,t}^*(i)\mathrm{d}i = \int_n^1 \left(\frac{y_{f,t}(i)}{A_t^*}\right)^{\frac{1}{\alpha}} \mathrm{d}i = \left(\frac{1}{A_t^*}\right)^{\frac{1}{\alpha}} \int_n^1 y_{f,t}(i)^{\frac{1}{\alpha}} \mathrm{d}i \\
&= \left(\frac{Y_{F,t}}{A_t^*}\right)^{\frac{1}{\alpha}} \int_n^1 \frac{1}{1-n} \left(\frac{p_{f,t}(i)}{P_{i,t}}\right)^{-\frac{\epsilon}{\alpha}} \mathrm{d}i = \left(\frac{Y_{F,t}}{A_t^*}\right)^{\frac{1}{\alpha}} (\Delta_t^*)^{\frac{1}{\alpha}},
\end{aligned}$$

by defining $\Delta_t \equiv \frac{1}{n}\int_0^n \left(\frac{p_{h,t}(i)}{P_t}\right)^{-\epsilon} \mathrm{d}i$, and $\Delta_t^* \equiv \frac{1}{1-n}\int_n^1 \left(\frac{p_{f,t}(i)}{P_t^*}\right)^{-\epsilon} \mathrm{d}i$ as price dispersion terms in each region.

Firms are subject to Calvo-type pricing frictions, so they solve the following problem:

$$\max_{p_{h,t}^{\#}(i)} \mathbb{E}_t \left[\sum_{k=0}^{\infty} Q_{t,t+k}\theta^k \left(p_{h,t}^{\#}(i) - MC_{h,t+k|t}(i)\right) y_{h,t+k|t}(i)\right]$$

subject to $y_{h,t+k|t}(i) = \left(\frac{p_{h,t}^{\#}(i)}{P_{H,t}}\right)^{-\epsilon} \left(C_{H,t} + C_{H,t}^*\right)$, and with $Q_{t,t+k} = \beta^k \frac{\delta_{t+k}u'(C_{t+k})}{\delta_t u'(C_t)}$. Note that here, $C_{H,t}^*$ denotes a composite index of foreign consumption of home goods, and $MC_{h,t+k|t}(i)$ is nominal marginal cost.

Then optimality conditions for pricing are given by

$$p_{h,t}^{\#}(i) = \frac{\epsilon}{\epsilon - 1} \frac{\mathbb{E}_t \sum_{k=0}^{\infty} (\beta\theta)^k \delta_{t+k} u'(C_{t+k}) mc_{h,t+k|t}(i) P_{H,t+k}^{\epsilon} \left(C_{H,t} + C_{H,t}^*\right)}{\mathbb{E}_t \sum_{k=0}^{\infty} (\beta\theta)^k \delta_{t+k} u'(C_{t+k}) P_{H,t+k}^{\epsilon-1} \left(C_{H,t} + C_{H,t}^*\right)},$$

with $mc_{h,t+k|t}(i)$ is real marginal cost of a firm i in terms of PPI, $P_{H,t}$.

Aggregate real marginal cost with $\alpha < 1$ can be written as follows:

$$\begin{aligned} mc_{h,t}(i) &= \frac{W_t/P_{H,t}}{\alpha A_t N_{h,t}(i)^{\alpha-1}} = \frac{w_t}{\alpha A_t} N_{h,t}(i)^{1-\alpha} \\ &= \frac{w_t}{\alpha A_t}\left(\frac{y_{h,t}(i)}{A_t}\right)^{\frac{1-\alpha}{\alpha}} = \frac{w_t}{\alpha A_t}\left(\frac{Y_{H,t}}{A_t}\right)^{\frac{1-\alpha}{\alpha}}\left(\frac{y_{h,t}(i)}{Y_{H,t}}\right)^{\frac{1-\alpha}{\alpha}} \\ &= mc_{H,t}\left(\frac{p_{h,t}(i)}{P_{H,t}}\right)^{-\frac{\epsilon(1-\alpha)}{\alpha}}, \\ mc_{H,t} &\equiv \frac{w_t}{\alpha A_t}\left(\frac{Y_{H,t}}{A_t}\right)^{\frac{1-\alpha}{\alpha}}. \end{aligned}$$

with $w_t \equiv W_t/P_{H,t}$.
Combining this with the previous optimal pricing equation then generates

$$p_{h,t}^{\#}(i)^{1+\frac{\epsilon(1-\alpha)}{\alpha}} = \frac{\epsilon}{\epsilon-1}\frac{\mathbb{E}_t \sum_{k=0}^{\infty}(\beta\theta)^k u'(C_{t+k}) mc_{H,t+k} P_{H,t+k}^{\epsilon/\alpha} Y_{H,t+k}}{\mathbb{E}_t \sum_{k=0}^{\infty}(\beta\theta)^k u'(C_{t+k}) P_{H,t+k}^{\epsilon-1} Y_{H,t+k}}.$$

We have similar conditions for intermediate-goods-producing firms in the foreign region.

International Risk Sharing Condition and Market Clearing Conditions

Combining each region's Euler equation gives

$$\delta_t\left(\frac{1}{n}C_t\right)^{-\sigma} = \kappa\delta_t^*\left(\frac{1}{1-n}C_t^*\right)^{-\sigma}\frac{1}{\mathcal{Q}_t},$$

with complete markets and symmety of initial conditions, $\kappa = 1$, generating

$$\delta_t^{-\frac{1}{\sigma}} C_t = \frac{n}{1-n}\delta_t^{*-\frac{1}{\sigma}} C_t^* \mathcal{Q}_t^{\frac{1}{\sigma}},$$

with $\mathcal{Q}_t \equiv P_t^*/P_t$ for the real exchange rate.

Goods market clearing conditions in each region are:

$$\begin{aligned} Y_{H,t} = C_{H,t} + C_{H,t}^* &= \phi_H\left(\frac{P_{H,t}}{P_t}\right)^{-\eta} C_t + \phi_H^*\left(\frac{P_{H,t}^*}{P_t^*}\right)^{-\eta} C_t^*, \\ Y_{F,t} = C_{F,t} + C_{F,t}^* &= \phi_F\left(\frac{P_{F,t}}{P_t}\right)^{-\eta} C_t + \phi_F^*\left(\frac{P_{F,t}^*}{P_t^*}\right)^{-\eta} C_t^*. \end{aligned}$$

Finally, we close the model by imposing the following monetary policy rule:

$$i_t = \rho_i i_{t-1} + (1-\rho_i)(\phi_\pi \pi_t^{agg} + \phi_y \hat{y}_t^{agg}),$$

where π_t^{agg} is a union-wide inflation rate and $\hat{y}_t^{agg}$ is union-wide output gap.

Modelling Stay-at-Home Orders

We model the imposition of SAH orders in two ways: (i) as a local supply shock, and (ii) as a local demand shock. When we model the SAH as a local productivity shock, we introduce the negative productivity shock for intermediate-goods-producing firms by setting negative values for ϵ_t^A. Alternatively, we also model the imposition of SAH orders via a negative preference shock, since SAH orders may directly reduce consumption by limiting retail mobility, as discussed in Subsection 4.2. In this case, we introduce negative shocks to ϵ_t^δ.

C Data Appendix

Table 7 reports all sources used in this paper.

Table 7: Data Sources

Variable	Source
Initial Unemployment Claims (Accessed 6/17/2020)	FRED (Mnemonic *ICLAIMS, where * indicates state abbreviation)
County Employment Data	BLS `https://www.bls.gov/lau` (Accessed 6/4/2020)
Stay-at-Home Orders (Accessed with *Internet Archive*)	*New York Times* `https://www.nytimes.com/interactive/2020/us/coronavirus-stay-at-home-order.html.`
Covid Confirmed Cases (Accessed 6/5/2020)	UsaFacts `https://usafacts.org/visualizations/coronavirus-covid-19-spread-map/`
State Excess Deaths (Accessed 6/4/2020)	CDC `https://www.cdc.gov/nchs/nvss/vsrr/covid19/excess_deaths.htm`
Share Age 60 (Accessed 6/16/2020)	Census Bureau `https://www.census.gov/data/tables/time-series/demo/popest/2010s-state-detail.html`
Average UI Replacement Rate (Accessed 6/16/2020)	Department of Labor's Employment and Training Administration `https://oui.doleta.gov/unemploy/ui_replacement_rates.asp`
2016 Trump Vote Share (Accessed 6/17/2020)	*New York Times* `https://www.nytimes.com/elections/2016/results/president.`
Work at Home Index	Dingel and Neiman (2020)
March Employment Losses for Bartik (Accessed 4/10/2020)	BLS `https://download.bls.gov/pub/time.series/ce/ce.industry`
Google Mobility Reports (Accessed 5/21/2020)	`https://www.google.com/covid19/mobility/`