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STAY-AT-HOME ORDERS IN A FISCAL UNION

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ABSTRACT

State and local governments throughout the United States attempted to mitigate the spread of Covid-19 using stay-at-home orders to limit social interactions and mobility. We study the economic impact of these orders and their optimal implementation in a fiscal union. Using an event study framework, we find that stay-at-home orders caused a 4 percentage point decrease in consumer spending and hours worked. These estimates suggest a $10 billion decrease in spending and $15 billion in lost earnings. We then develop an economic SIR model with multiple locations to study the optimal implementation of stay-at-home orders. From a national welfare perspective, the model suggests that it is optimal for locations with higher infection rates to set stricter mitigation policies. This occurs as a common, national policy is too restrictive for the economies of mildly infected areas and causes greater declines in consumption and hours worked than are optimal.

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1 Introduction

State and local governments throughout the United States attempted to mitigate the outbreak of Covid-19 using stay-at-home orders to limit social interactions and mobility. Many praised these policies for slowing the spread of the virus while others protested them due to their perceived negative impact on the economy. The implementation of stay-at-home orders also created conflict between policy makers. Some called for a nationwide order while others believed discretion should be left to state and local governments.

This paper studies the economic and political controversy associated with stay-at-home orders and their implementation, both empirically and theoretically. Empirically, we use an event-study framework to estimate the marginal impact of stay-at-home orders on the economy. We build a daily, state-level panel of labor market and consumer spending variables using data from the time-clock company Homebase and non-profit organization Opportunity Insights. Our main sample spans several weeks before the issuance of the first stay-at-home order in March up to the first reopening in April. Both hours worked and consumer spending were already declining before the first statewide stay-at-home order was issued on March 19th. Thus, the economic decline was likely attributed to a combination of government-issued policies and behavioral responses by consumers and firms to the health risks posed by the pandemic. The event-study framework allows us to separate these economic effects by exploiting differences across states that issued a stay-at-home order and those that did not. Controls for the daily, case load of infections in each state and non-essential business closures help isolate the impact of stay-at-home orders. The control for non-essential closures also helps approximate the cumulative effect of mitigation policy.

Stay-at-home orders negatively impacted the labor market. Hours worked initially declines by 2 percentage points and continues downward to a trough of 6 percentage points (pp) two weeks after the order is enacted. High-income workers experience larger declines in employment compared to low-income workers. Early-treated states experience about twice the decline in hours worked (4pp) relative to late-treated states (2pp). Non-essential business closures did not exacerbate or mitigate the labor market response.

Stay-at-home orders also decreased consumer spending by 4 percentage points for several weeks. In contrast to employment, spending declines are larger in magnitude among low-income consumers compared to high-income consumers. Consumer anticipation effects vary across sectors. For example, we find evidence of stockpiling in grocery and food stores, but not in accommodation and food services. Early and late-treated states experience similar post-treatment declines in spending. However, stockpiling in the pre-treatment period is primarily driven by early-treated states. Unlike employment, non-essential business closures did exacerbate the consumer spending response.

The baseline estimates suggest that stay-at-home orders decreased consumer spending by \$10 billion and total earnings by \$15 billion. They also suggest that stay-at-home orders account for 10%-25% of the total employment decline observed during our sample. The non-essential business closure estimates suggest that the cumulative effect of mitigation policy is at least as large in magnitude as implied by the stay-at-home order estimates. The results are not driven by outliers. Lastly, in response to the virus, some counties implemented their own stay-at-home orders before their respective states. We also conduct our analysis at the county-level to account for these orders and find similar impacts compared to our baseline estimates.

In order to study the optimal implementation of stay-at-home orders, we extend the Susceptible-Infected-Recovered (SIR) model of Eichenbaum et al. (2020) to multiple locations. Each location initially experiences an idiosyncratic virus shock. Following this, the virus can spread through both consumption and employment *within* a given location. This interaction captures the natural societal responses to the virus as people reduce their economic activity in order to limit their risk of infection. Although locations are closed economies with respect to their economic transactions, individuals may also spread the virus *across* locations by travelling from place to place. Thus, the economic activity of one location is dependent upon other locations due to virus spillovers. The model incorporates travel cost across locations in order to specify the extent of virus spillovers. Lastly, in response to the transmission of the initial virus shocks, an aggregate social planner can implement mitigation policy—proxied by a consumption tax—to maximize the welfare of the country. Mitigation policy is able, but not required, to vary across locations. This allows for economic and welfare comparisons between a common national policy and location-specific policy.

The main contribution of our model is the cross-sectional distribution of consumption, hours worked, and infection rates that it generates. The single-location model of Eichenbaum et al. (2020) is equivalent to our multiple-location model if and only if the initial infection rates of locations are symmetric and travel costs across locations are zero. If either of these restrictions fails to hold, then aggregate dynamics will depend on the behavior of the cross-section.

We explore the behavior of the cross-sectional distribution and dynamics in the model through an application using US Census region data. The model suggests that travel cost across locations plays a significant role in the spread of the virus in the US. Allowing for complete pass-through of virus transmission overestimates the infection rates of every Census region. In contrast, allowing for no virus transmission across regions tends to underestimate the infection rates seen in the data. We propose a method to determine the rate of transmission across locations through the associated travel costs. This method provides a good fit of both the cross-sectional distribution and dynamics in the US Census data, and the procedure is generalized for use in other applications.

Lastly, we turn our analysis to optimal policy with multiple locations. From a national welfare perspective, it is optimal for locations with higher infection rates to set stricter mitigation policies. A common, national policy is too restrictive for mildly infected locations and causes greater consumption and employment declines than are optimal in these areas. Stricter policy in more severely affected locations causes infection rates, and subsequently mitigation policy, to converge over time. Thus, our finding that policy should be set at a local level is contingent on sufficient asymmetries in the infection rates of locations.

Other research has examined the empirical relationship between the economy, the spread of Covid-19, and the ensuing policy responses (Baker et al., 2020; Binder, 2020; Chetty et al., 2020; Rojas et al., 2020). Our empirical contribution is closely related to other difference-in-difference estimates that find negative effects of stay-at-home orders on the labor market (Allcott et al., 2020; Baek et al., 2020; Bartik et al., 2020; Beland et al., 2020; Gupta et al., 2020; Kong and Prinz, 2020; Lin and Meissner, 2020) and consumer spending (Allcott et al., 2020; Coibion et al., 2020; Goolsbee and Syverson, 2020). Importantly, our estimates for both the employment and spending effects are found using an event study specification using data at a daily frequency which builds on many of these papers. The high frequency data allows us to provide a clear characterization of both when and where stay-at-home orders were issued. The event study specification allows us to assess whether our results are driven by differences in observable pre-treatment trends across treated and control states, for which we do not find evidence. Of the papers mentioned above, our paper is most closely related to Allcott et al. (2020) who find similar results for both consumer spending and hours worked. The Opportunity Insights data allows us to analyze these effects by consumer and worker income quartiles as well as by industry, which are both new results.

Our paper is also related to the quantitative theoretical literature of infectious disease modelling (Kermack and Mckendrick, 1927; Angulo et al., 2013; Zakary et al., 2017; Bisin and Moro, 2020) and its application to economic activity and mitigation policy (Acemoglu et al., 2020; Atkeson, 2020; Jones et al., 2020). We extend the model of Eichenbaum et al. (2020) to multiple locations. This allows us to illustrate how the economy in one location can be affected by the spread of the virus in other locations. Antràs et al. (2020) and Fajgelbaum et al. (2020) also study the economic spillovers of virus transmission across locations. Antràs et al. (2020) focus primarily on endogenous trade adjustments while Fajgelbaum et al. (2020) assess cross-location mitigation policy such as travel restrictions. In contrast, our analysis centers on domestic mitigation policies such as stay-at-home orders. Overall, these models provide complementary, rather than contradictory, results which help to more fully characterize the economic effects of virus transmission across locations and the subsequent policy responses. Lastly, Baek et al. (2020) develop a model with stay-at-home orders characterized as an exogenous productivity or demand shock. By explicitly modelling the spread of the virus, we are able to endogenously model mitigation policy such as stay-at-home orders.[1] This allows us to evaluate their economic impact as well as their optimal implementation in response to the spread of the virus.

The rest of the paper proceeds as follows. Section 2 presents a timeline of the spread of Covid-19 throughout the U.S., the subsequent economic and policy responses, and formalizes the event study specification. Section 3 presents the baseline event study results and robustness checks. Section 4 describes the economic SIR model. Section 5 compares the multiple-location model with the single-location model, applies the multiple-location model to Census region data, and presents the results for optimal mitigation policy. Section 6 concludes.

2 Empirical Framework: Isolating the economic impact of stay-at-home orders

We begin by creating a daily panel of several labor market and consumer spending variables. Our panel spans from March up to April 24, the first day a stay-at-home order was lifted. This allows our analysis to focus specifically on the initial virus outbreak and asymmetric policy responses across states. The second subsection provides a timeline of the outbreak of the virus, the enactment of stay-at-home orders, and the economic response during this period. The third subsection formalizes our event-study specification.

2.1 Variable Definitions

Baseline Labor Variables We obtain our primary labor market measures from the time-clock company Homebase.[2] Homebase helps over 60,000 businesses track the hours worked

[1] This is one of the novel contributions of Eichenbaum et al. (2020) which remains in our extension.

[2] An interactive database from Homebase can be found here: `https://joinhomebase.com/data/`

of 1 million hourly employees. Their data provides three labor-market measures: businesses open, employees working, and hours worked. A business is defined as open if at least one employee clocked in for a given day. Employees working is counted as the number of unique employees who clocked in at least once. Hours worked is then determined from the time cards of all employees. Total hours worked is the primary outcome variable as it corresponds to the model developed in Section 4. All variables are measured at the state level. Each variable is normalized relative to the median for that day of week using data from January 4, 2020 through January 31, 2020.

Supplemental Labor Variables We supplement our Homebase data with an employment measure constructed by the non-profit organization Opportunity Insights (OI) in Chetty et al. (2020). Employment is normalized relative to the average level from Jan. 4-31. This employment data provides several advantages and disadvantages compared to the Homebase data. The first advantage is that the OI data is available at both the state and county level. This allows us to account for stay-at-home orders issued by individual counties. Second, employment is measured by low (<$27,000 per year), medium ($27,000-$60,000), and high income (>$60,000) earners at both the state and county level.[3] Lastly, the state-level data is disaggregated into four NAICS sectors: Professional and Business Services, Education and Health Services, Retail and Transportation, Leisure and Hospitality.

However, the OI data has several disadvantages. The OI data is constructed using worker-level data from *Earnin* and firm-level payroll data from *Paychex* and *Intuit*.[4] This creates the conflict that observations are recorded when workers are paid rather than the specific date they are employed as in the time-clock data from Homebase. To circumvent this issue, Chetty et al. (2020) make the assumption that individuals are employed for all days in their pay period. If a stay-at-home order occurred in the middle of an individual's pay period, employment effects would not be seen until their next pay period. Thus, the implications of

[3] These are approximate measures. Quartiles are based upon the hourly wage distribution in 2019. Income levels are more accurately $<$ \$13.00/Hour, \$13.00 $-$ \$29.17, and $>$ \$29.17/Hour.

[4] Please see Chetty et al. (2020) for a more detailed description of their employment series.

this assumption are too strong for an event study analysis which hinges upon variation at the time of treatment. To account for the difference between the end of the pay period and the date of employment, we lag the data by an additional week.[5] Lastly, their employment series is constructed as a seven-day look-back moving average. This attenuates the overall effect of a one-day change in employment which is of primary interest in the context of stay-at-home orders. For these reasons, we use the Homebase data for our baseline estimates on the labor market and the OI data for comparisons and robustness checks.[6]

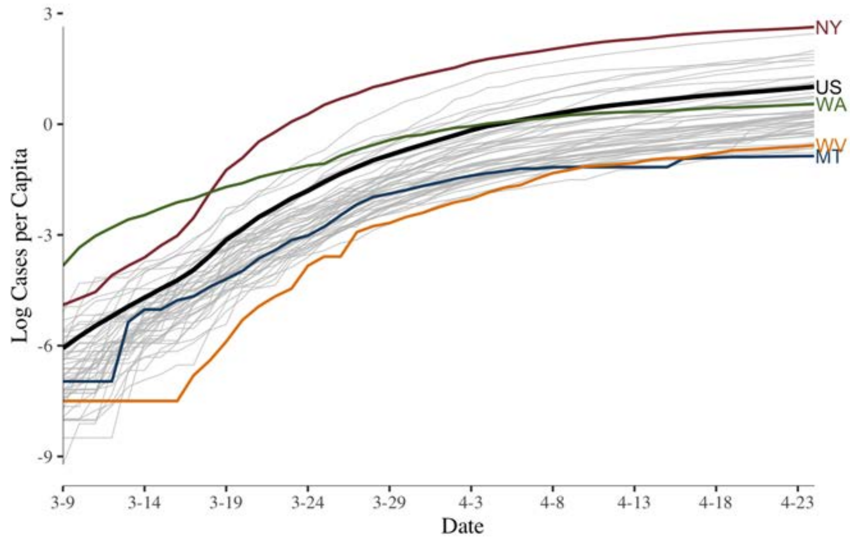
Expenditure Variables We use credit and debit card spending data from Affinity Solutions to estimate changes in consumer expenditures. This expenditure data is also constructed by Chetty et al. (2020) and is available at more granular levels than the Homebase data. At the state level, expenditure data is provided by income quartile. Consumers are considered high-income if they live in a ZIP code with median income in the top quartile of ZIP codes. Similarly, low-income consumers live in the bottom quartile of ZIP codes. Middle-income consumers are then defined as living in the second and third quartiles. Expenditure data is also separated into several merchant groups defined by Affinity Solutions: Apparel and General Merchandise, Entertainment and Recreation, Grocery, Health Care, Restaurants and Hotels, Transportation. Total expenditure data is also available at the county level, but is not available by income quartile or merchant group. A seven-day look-back moving average is taken for all consumption variables, and then measured relative to the median for that day of week using data from January 4, 2020 through January 31, 2020.[7]

[5] Chetty et al. (2020) use Earnin data only for workers that are paid weekly or bi-weekly. They make the assumption that there is a one-week lag from the end of the pay period to the date an individual is paid. By lagging their employment series an additional week, we essentially shift the data to the beginning of the two-week pay cycle and assume there is no lag between the end of the pay period and payment date. Chetty et al. (2020) also convert weekly employment data from Paychex to daily by assuming that employment is constant within each week. Lagging an additional week also helps control for the effects of this assumption.

[6] Appendix Figure A.1 plots these two measures for comparison.

[7] See Chetty et al. (2020) for a full description of the construction of the expenditure series.

Figure 1: Coronavirus outbreak by US state



Note: This figure plots the natural log of cases per 1,000 residents for each state. To account for periods of zero infection in the beginning of our sample, we add one to the number of cases for all observations before calculating the infection rate. The black line represents the national total. Data is obtained from USAFacts.

Infection variables Our daily panel spanning this period uses cases reported by the non-profit organization *USA Facts*.[8] Cases are reported at the county level which we then aggregate to the state level. Primary source information for infections and tests comes from the CDC and state and local health agencies. Population data are obtained from the US Census to calculate case load per capita.

2.2 Context and timeline

The United States recorded its first case of the novel coronavirus disease of 2019 (Covid-19) on January 20th, 2020 when a man from the state of Washington became ill after a visit to Wuhan, China. The timing and spread of the virus differed across states after this initial infection. To see this, Figure 1 plots the natural log of cases per capita for each state from March 9th through April 24th. West Virginia was the last state to document a confirmed case on March 17th, almost two months after the first case in Washington. At this point,

[8]Specifically, cases and deaths are obtained from the website: `https://usafacts.org/visualizations/coronavirus-covid-19-spread-map/`.

Table 1: Timeline of Stay-at-Home Orders

March 15	March 16	March 17	March 18	March 19	March 20	March 21
				California		
March 22	**March 23**	**March 24**	**March 25**	**March 26**	**March 27**	**March 28**
Illinois	New York	Connecticut	Hawaii	Colorado	Kentucky	Minnesota
New Jersey	Oregon	Delaware	Idaho	Vermont		Montana
	Washington	Louisiana	Indiana			New Hampshire
		Massachusetts	West Virginia			Rhode Island
		Michigan	Wisconsin			
		New Mexico				
		Ohio				
March 29	**March 30**	**March 31**	**April 1**	**April 2**	**April 3**	**April 4**
Alaska	Kansas	North Carolina	Arizona	Maine	Florida	Mississippi
	Virginia	Maryland	District of Columbia	Pennsylvania	Georgia	
			Nevada	Texas		
			Tennessee			
April 5	**April 6**	**April 7**	**April 8**	**April 9**	**April 10**	**April 11**
Alabama	Missouri		South Carolina			

Note: This table presents the timeline of the enforcement of stay-at-home orders. Some policies began at a specific time of the day such as 6:00am or 6:00pm. Orders that occur after noon are recorded on the following day. Dates of stay-at-home orders are obtained from the *New York Times*.

Montana had a case rate similar to the national total. However, low case growth led them to have the least cases per capita on April 24th. In contrast, persistently high case growth in New York led them to maintain the highest case rate per capita from mid-March through the end of April.

As infection rates grew, many local and state governments issued various social distancing policies to help limit personal contact and combat the spread of the coronavirus. Our analysis centers on statewide stay-at-home orders. These orders generally closed all non-essential businesses, and encouraged people to stay at home except for essential activities such as shopping for food or seeking medical care. Over 90% of the US population was placed under a stay-at-home order in the span of three weeks across forty-two states and the District of

Columbia. Table 1 documents the timeline of these stay-at-home orders.[9] California was the first state to announce an order on March 19th. Twenty-five additional states issued orders the following week. South Carolina was the last state to issue an order on April 8. Some orders were issued to start at a specific time of the day (e.g. 6:00am or 6:00pm). As our panel is at a daily frequency and cannot capture this, we consider states with orders enacted after Noon to start treatment the following day.[10]

Despite widespread recognition of their need from a public health perspective, stay-at-home orders were met with opposition. Protests over the economic impact of these orders occurred in many states (NBC News, 2020; Fox News, 2020). One protester in New Jersey said, "Businesses are suffering, unemployment checks are not being sent, landlords are not getting rent. We feel like these directives are causing more suffering than is necessary." (NJ.com, 2020) A poll conducted in May estimated that 35% of people "strongly or somewhat agreed" that restrictions and closures had been too severe (USA Today, 2020).

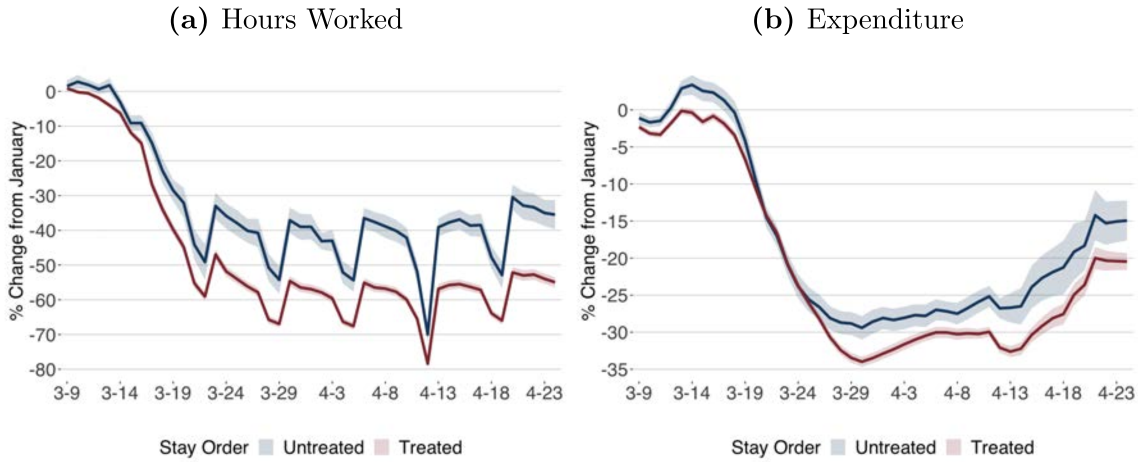
Figure 2 plots the the response of hours worked and consumer expenditures during the outbreak of Covid-19 from March 9th to April 24th. The red line represents the average over states which issued a stay-at-home order, and the blue line depicts those that did not. Standard errors are shaded in their respective colors. Both panels show that employment and expenditure were declining before the first statewide stay-at-home order was issued on March 19th. We also see that these declines differed across treatment groups throughout most of the sample.

Panel (a) shows that total hours worked in states which issued a stay-at-home order declined by 34% on March 18th. On this date, states that did not issue an order only had a decline of 23%. As a stay-at-home order had not yet been issued, they cannot account for this economic decline or the 11 percentage point (pp) difference across groups. However, these differences did grow over time. On April 23rd, the day before the first lifting of a stay-

[9] Appendix Figure A.2 presents a timeline of the percentage of the US population under a stay-at-home order.

[10] Primary source information for state announcements was compiled by the *New York Times*: `https://www.nytimes.com/interactive/2020/us/coronavirus-stay-at-home-order.html`

Figure 2: Economic Response to Covid-19



Note: This figure plots the time series of spending and hours worked for states which issued a stay-at-home order (red) and states that did not (blue) from March 9th to April 24th. Estimates are averages over states in their respective treatment groups. Shaded regions represent the standard error for that date. Hours and expenditure data are provided by Homebase and Affinity Solutions, respectively.

at-home order, declines in hours worked were 54% and 35% for the treated and untreated groups, respectively. Thus, the differences in hours worked across groups had grown by 8 percentage points from March 18th (11pp) to April 23rd (19pp). Panel (b) shows a similar pattern in expenditures. The percentage point difference in expenditure across groups grew by 2 percentage points from on March 18th (3pp) to on April 23rd (5pp).[11]

2.3 Event Study Specification

The simple analysis above helps provide the intuition behind a difference-in-differences estimation. Although it provides suggestive evidence that stay-at-home orders may have contributed to the economic decline of states, it does not account for differences in characteristics of states as in Figure 1 or the variation in treatment timing in Table 1. To account for these complications, the following event study specification is estimated:

$$y_{it} = \alpha_i + \gamma_t + \Gamma X_{it} + \sum_{-10 \leq k \leq 15}^{k \neq -1} \beta_k I(StayHome_{it} = k) + \epsilon_{it} \tag{1}$$

[11]Appendix Table A.1 performs a similar analysis with the remaining labor, expenditure, and infection variables.

This empirical strategy strategy uses variation in both *when* and *where* stay-at-home orders were issued. Our primary outcome variable, y_{it}, is either hours worked or consumption expenditures in state i at time t. State fixed effects, α_i, control for both observable and unobservable differences across states that do not change over time. Time fixed effects, γ_t, control for variables that affect all states in a given period such as the weekly seasonality of hours worked seen in Panel (a) of Figure 2 or national news about the coronavirus. Our sample begins on March 9th, ten days before the first statewide stay-at-home order in California, and ends on April 23rd, the day before the first statewide reopening in Alaska. $StayHome_{it}$ represents the current date relative to the date a stay-at-home order was issued in a given state. This variable is always equal to -1 for non-treated states which serve as the control group.

The coefficients of interest, the β_k's, represent the average difference between the control and treated group at period k relative to the difference at $k = -1$, the day before the order is enacted for treated states. We present estimates for event times $k \in [-10, 15]$. Event times 0 to 15 estimate the causal effect of a stay-at-home order on hours worked and consumption expenditures. These estimates represent the average treatment effect for the treated (ATT). Event times -10 to -2 serve as a falsification test that our results are not driven by pre-existing differences in trends between the treatment and control groups before a stay-at-home order is issued. All of our presented estimates use a balanced set of states. This means our coefficients do not reflect changes in the composition of the treatment group.[12]

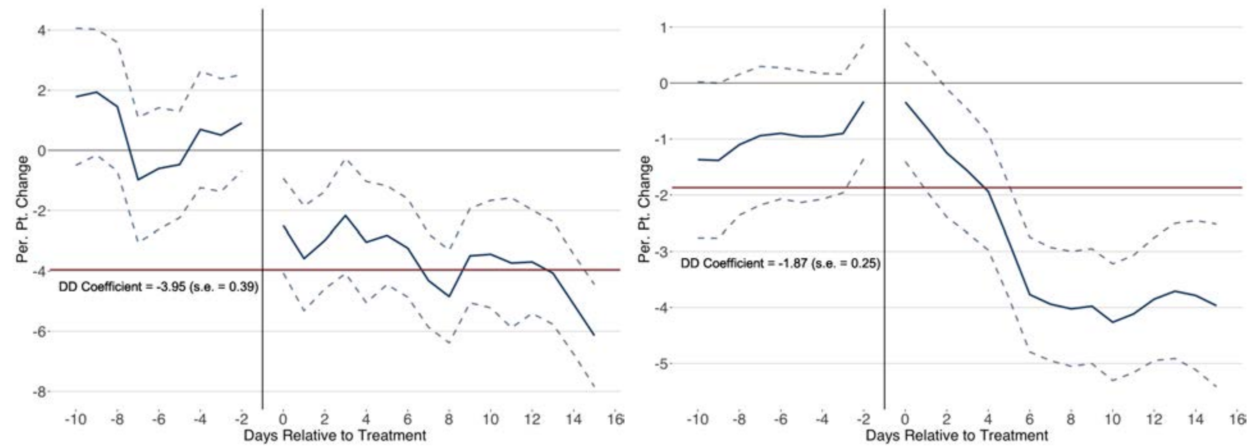
Goodman-Bacon and Marcus (2020) raise substantive issues and provide recommendations when using a difference-in-differences design in the context of Covid-19. Our baseline specification contains several controls to help us address these issues. Namely, we include a control for log cases per capita and a similar event study variable for non-essential business closures.[13] The inclusion of a measure of case incidence helps us to separate the endogenous

[12]For example, if we extended the event times to $k \in [-10, 16]$, South Carolina would not be included in the estimate of $k = 16$ due to the timing of its stay order (recall Table 1). Thus, β_{16} would be partially driven by this change in the composition of the treated group.

[13]Non-essential business closures at the state level are obtained from Raifman et al. (2020).

Figure 3: Effect of Stay-at-Home Orders on the Economy

(a) Hours Worked **(b)** Expenditure



Note: This figure plots the stay-at-home order event study coefficients estimated from Equation 1. Subcaptions represent the dependent variable. Dashed lines represent 95% confidence intervals using robust standard errors. The red line plots the difference-in-difference estimate. Observations are at the state-by-day level. Hours and expenditure data are obtained from Homebase and Affinity Solutions, respectively.

responses of individuals and firms to the risk of infection from the economic impact of stay-at-home orders. Controlling for non-essential business closures helps separate the effect of these complementary policy tools and avoid possible confounding effects of the two.[14]

3 Results: Effect of Stay-at-Home Orders on the Economy

Figure 3 analyzes the effect of stay-at-home orders on the economy. Panel (a) shows that pre-trends in hours worked are slightly elevated eight to ten days before a stay-at-home order, but are flat 1-7 days before a stay-at-home order is enacted. Differences in trends are statistically insignificant at the 95% level using robust standard errors for the entire pre-treatment period. Hours worked declined by 4 percentage points (s.e. = 0.39) on average after a stay-at-home order. These effects vary over time. Immediately after the order, treated states experience over a 2 percentage point decrease in hours. This effect is persistent with a slight downward trend. Fifteen days after the order, treated states experience a trough

[14] Our main results analyze the effect of stay-at-home orders on the economy. Non-essential business closure estimates can be found in Section 3.4 and Appendix A.1.

decline of -6 percentage points. Effects are statistically significant for the entire post-treatment period.

Panel (b) plots the coefficients for statewide expenditure. Expenditures experience a statistically significant 4pp decline five days after the order. This effect remains stable and significant up to fifteen days after the order. We do not find statistically significant differences in pre-trends before a stay-at-home order. However, they do exhibit a 1 percentage point upward trend. It is known that consumers would often stockpile items, such as toilet paper, in the days preceding a stay-at-home order. This upward trend may be representative of the consumer anticipation effect. In this case, our event study estimates for expenditure may be biased towards finding a negative effect and overstate the effect of the order.

Table 2 provides several alternative specifications and further explores the extent of the anticipation effect in the baseline results of Figure 3.[15] T-values are presented to ease comparison across specifications. The baseline results are provided in columns (1) and (4) for reference. Results for employment and businesses open in columns (2) and (3) test the robustness of the benchmark labor market results. Immediately after the order, we see slightly weaker effects of -2.03pp and -2.46pp in employment and businesses open compared to the baseline estimate of -2.50pp in hours worked. Both variables also suggest a slight downward trend with an estimated decrease of about 5.6pp fifteen days after the order is enacted.

The baseline results represent the average effect of statewide stay-at-home orders on treated states' economic activity. However, some counties issued local orders before their state did. These policies could bias the estimates towards not finding a result. To account for this, we also estimate Equation 1 using county-level orders as the basis for treatment. In this case, a county is counted as treated if it issued a stay-at-home order, or if the state it resides in did.[16] As the Homebase dataset does not contain county-level information, this is only

[15] Additional robustness checks for population-weighted regressions, outliers, and composition effects can be found in Appendix Table A.3. These checks have no significant effects on the results.

[16] Data for county-level stay-at-home orders and non-essential business closures and their timing is obtained from `https://ce.naco.org/?dset=COVID-19&ind=Emergency\%20Declaration\%20Types`. Over

Table 2: Effect of Stay-at-Home Orders on the Labor Market and Expenditures

	Labor Variables (Per. Pt.)			Expenditure Variables (Per. Pt.)				
Days	Hours Worked (1)	Employment (2)	Businesses Open (3)	State Level (4)	County Level (5)	Low Income (6)	Middle Income (7)	High Income (8)
-7	-0.98	-0.16	0.37	-0.94	-0.91	0.19	-1.05	-0.39
	(-0.93)	(-0.16)	(0.38)	(-1.49)	(-1.35)	(0.2)	(-1.42)	(-0.55)
-2	0.91	0.88	0.7	-0.33	-0.44*	-0.29	-0.47	0.13
	(1.11)	(1.19)	(0.9)	(-0.63)	(-2.48)	(-0.33)	(-0.81)	(0.19)
0	-2.50*	-2.03*	-2.46*	-0.34	-0.33*	-0.4	-0.35	-0.21
	(-3.12)	(-2.78)	(-3.45)	(-0.63)	(-2.07)	(-0.45)	(-0.61)	(-0.32)
5	-2.81*	-2.59*	-2.62*	-2.83*	-2.72*	-2.41*	-3.12*	-2.52*
	(-3.38)	(-3.53)	(-3.55)	(-5.53)	(-3.89)	(-2.9)	(-5.52)	(-3.92)
10	-3.44*	-3.27*	-4.14*	-4.26*	-3.66*	-4.24*	-4.62*	-3.21*
	(-3.79)	(-4.02)	(-5.22)	(-8.02)	(-3.8)	(-4.59)	(-8.21)	(-4.85)
15	-6.14*	-5.68*	-5.59*	-3.97*	-2.88*	-3.03*	-4.2*	-3.62*
	(-7.08)	(-7.08)	(-6.91)	(-5.36)	(-2.54)	(-2.59)	(-5.84)	(-5.25)
Observations	2,346	2,346	2,346	2,346	73,451	2,254	2,346	2,300

Note: This table presents the stay-at-home order event study coefficients estimated from Equation 1. Columns represent the dependent variable. Observations are at the state-by-day level except column (5) which uses county-by-day observations. Counties begin treatment on the date they issued a stay-at-home order or the date the state they reside in issued an order. T-values are presented using robust standard errors for state estimates and standard errors clustered at the state-level for county estimates. Labor and consumption variables come from Homebase and Affinity Solutions, respectively.
* denotes estimates significant at the 95% level.

estimated for the consumption data from Affinity Solutions in column (5). Furthermore, we expect spending and saving patterns to be related to one's income level. This heterogeneity may lead to differences in consumption smoothing. We test this hypothesis in columns (6)-(8).[17]

Columns (4) and (5) of Table 2 are similar in terms of their coefficient estimates. However, we see that the county estimates are statistically significant at the 95% level two days before

100 counties issued a stay-at-home order before their state did. Appendix Figure A.3 plots counties by their treatment status to gain an understanding of the cross-sectional variation induced by the county orders.

[17]Observations may differ across specifications due to missing values. Specifically, high-income expenditure data is missing for Montana, and low-income data is missing for Alaska and Vermont. Columns (7) and (8) in Appendix Table A.3 re-estimate our baseline results when removing these states from the sample to provide evidence that these differences across income quartiles are not driven by differences in the sample composition.

and the day the order is enacted.[18] This implies that the anticipation effect mentioned earlier is statistically significant. Middle-income earners in column (7) and our baseline estimates have similar upward pre-trends as evidenced by days -7 and -2. However, these upward trends are not as apparent for low (6) and high (8) income consumers. This suggests that the anticipation effect is mostly driven by middle-income consumers. This is further exemplified as all of the significant point estimates are larger in magnitude for middle-income consumers compared to low and high-income estimates. Despite this, we still find statistically significant estimates for both high and low-income consumers 5-15 days after an order is enacted. Lastly, comparing columns (6) and (8), we see that low-income consumers see a trough marginal decline in expenditure of 4.2pp which occurs ten days after the order. High-income consumers see a smaller trough decline of 3.6pp fifteen days after the order.

3.1 Opportunity Insights Employment Measure

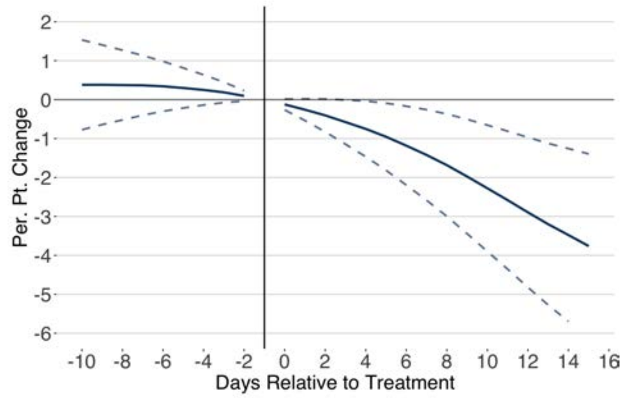
Chetty et al. (2020) mention that the Homebase employment composition skews towards restaurant and retail workers. In contrast, they show that their employment measure more accurately tracks nationally representative statistics such as the Quarterly Census of Employment and Wages (QCEW) and the Occupational Employment Statistics (OES). To explore the possibility that our results our driven by the Homebase composition, we re-estimate Equation 1 using the OI employment measure at the county-level. This serves the dual purpose of accounting for county-level stay-at-home orders.

Figure 4 plots the results using the Opportunity Insights employment measure as the dependent variable. Observations are at the county level. Dashed lines represent 95% confidence intervals using standard errors clustered at the state level. Panel (a) plots the effect of issuing a stay-at-home order on total employment. There is no evidence of differential trends up to ten days before the issuance of an order. There is a statistically significant 1 percentage point decline in employment five days after the order. Similar to the baseline

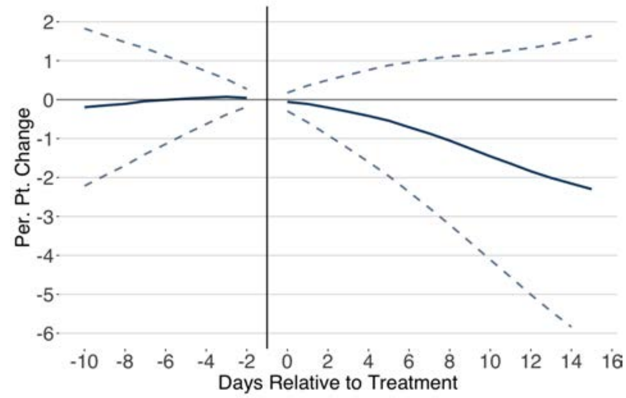
[18] Standard errors for the county-level estimates are clustered at the state level.

Figure 4: Opportunity Insights Employment Measure

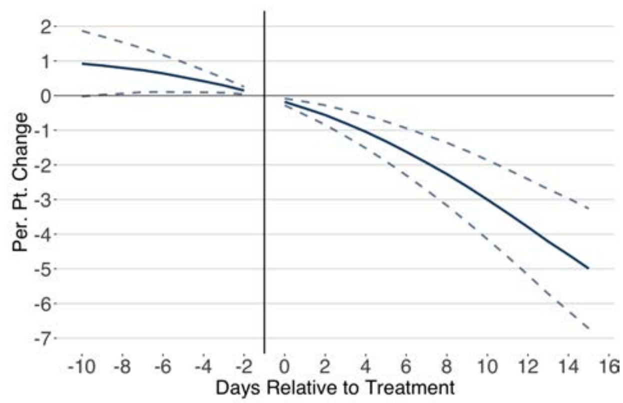
(a) Total Employment



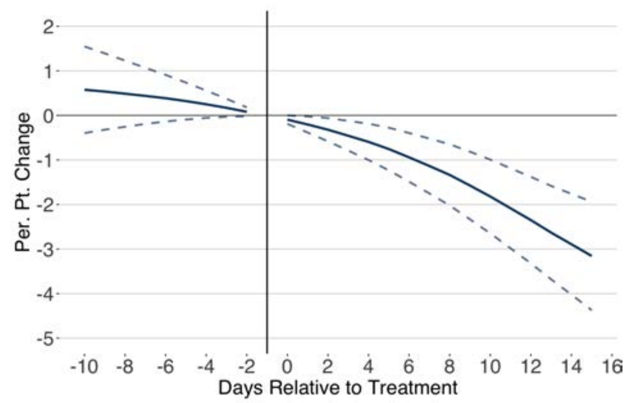
(b) Low Income ($< \$27K$)



(c) Middle Income ($\$27K - \$60K$)



(d) High Income ($> \$60K$)



Note: This figure plots the stay-at-home order event study coefficients estimated from Equation 1. Subcaptions represent the dependent variable. Employment data is obtained from Opportunity Insights. Observations are at the county-by-day level. Counties begin treatment on the date they issued a stay-at-home order or the date the state they reside in issued an order. Dashed lines represent 95% confidence intervals using standard errors clustered at the state level.

estimates, the effect on employment is amplified over time and remains statistically significant for the remainder of the fifteen day post-period. After fifteen days, the effect peaks at -3.8 percentage points. Overall, the Opportunity Insights data provides similar estimates as the Homebase data, but the seven-day moving average has created a significant lag in the employment effects compared with Column two of Table 2. The Day 15 estimate of -3.76pp from the OI data is more similar to the Day 10 estimate of -3.44pp from the Homebase data. The Day 10 and Day 5 estimates are respectively (-2.28pp, -2.81pp).

Panels (b)-(d) plot the employment response by worker's yearly income level. We see heterogeneous effects across the income distribution. Low-income workers ($< \$27K$/Year) experience slightly more than a 2 percentage point decline in employment fifteen days after a stay-at-home order. This estimate is statistically insignificant, as are all other point estimates for low-income workers. We see the strongest effect of -5pp in middle-income workers ($\$27K-\$60K$) fifteen days after an order. All post-period estimates are statistically significant for middle-income workers. However, we do find statistically significant estimates of a downward trend in employment in the ten days leading up to the order. High-income workers ($> \$60K$) experience a significant 3pp decline in employment fifteen days after the order. High-income workers also have a slight downward trend in employment leading up to the issuance of an order, but these estimates are statistically insignificant.[19,20]

3.2 Heterogeneity across Sectors and the Anticipation Effect

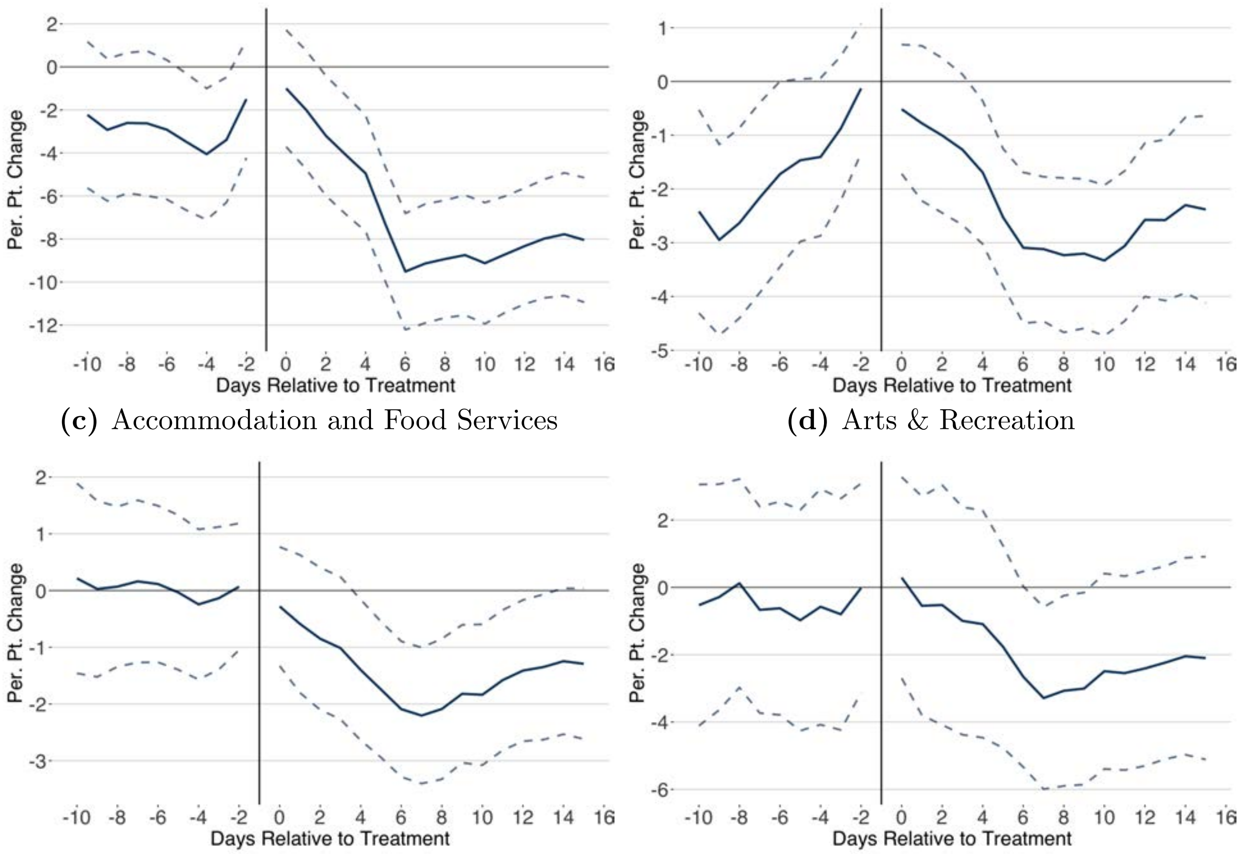
Our baseline expenditure estimates showed some evidence of an anticipation effect by consumers. This is likely the result of stockpiling goods in the days preceding a stay-at-home order. Many consumers shifted their spending from food services, such as restaurants, to grocery and food stores during the pandemic (Cavallo, 2020). These markets and their goods vary significantly in their ability to be stockpiled. An expenditure anticipation effect would be expected for groceries, but not restaurant meals, for example. We now turn to an exploration of the heterogeneous effects in expenditures and the anticipation effect across several consumption categories.

Figure 5 plots the results of Equation 1 for (a) Grocery and Food Stores, (b) Apparel and General Merchandise Stores, (c) Accommodation and Food Services, and (d) Arts &

[19] Workers may shift in the income distribution over time due to wage cuts. As an example, low-income workers may see an increase in employment after a stay-at-home order if middle-income workers on the threshold see a decrease in wages. Chetty et al. (2020) provide evidence that wages for individuals who remained employed during this period of the pandemic were largely unchanged. This suggests, given the significance levels in each panel and the large differences in magnitude across panels, that it is unlikely that shifts in the wage distribution will affect the overall qualitative story presented in Panels (b)-(d). Although, these shifts may affect the quantitative interpretation.

[20] Appendix Table A.4 compares the county-level results in Figure 4 with the event study results at the state level. Additional heterogeneity checks at the state level for employment by NAICS sector are in Appendix Figure A.5.

Figure 5: Heterogeneity across Consumption Expenditures



(a) Grocery and Food Stores

(b) Apparel and General Merchandise

(c) Accommodation and Food Services

(d) Arts & Recreation

Note: This figure plots the stay-at-home order event study coefficients estimated from Equation 1. Subcaptions represent the dependent variable. Expenditure data come from Affinity Solutions. Merchant groups are defined by Affinity Solutions. Observations are at the state-by-day level. Dashed lines represent 95% confidence intervals using robust standard errors.

Recreation as the dependent variable. Observations are at the state level. Dashed lines represent confidence intervals at the 95% confidence level using robust standard errors. All panels suggest similar dynamics in the post-treatment period. There is an initial decline in expenditure which experiences a statistically significant trough decline 6-8 days after a stay-at-home order is issued. After 6-8 days, expenditures begin to recover. However, these effects differ significantly in their magnitudes across sectors. Grocery and food stores exhibit the largest decrease in expenditures of 10 percentage points. In contrast, accommodation and food services only experience a 2pp decline. Trough declines are about 3pp for both apparel & general merchandise stores and arts & recreation expenditures.

Pre-trends also vary significantly across specifications. The anticipation effect seen in our baseline expenditure results appears to be driven primarily by grocery stores and general merchandise stores. Panel (a) of Figure 5 shows that grocery and food stores experienced a statistically significant upward trend of 4pp in the days preceding a stay-at-home order. Apparel and general merchandise stores also see an upward trend that starts at -3pp nine days before the issuance of an order. These upward trends are completely absent in the ten days preceding an order for accommodation and food services as well as arts & recreation expenditures. Panels (c) and (d) provide further evidence that our baseline estimates are not significantly driven by consumer anticipation effects, and that stay-at-home orders had large impacts on the economy.[21]

3.3 Identifying Variation induced by Treatment Timing

Recent work has emphasized the impact that treatment timing can have on difference-in-difference and event study estimates (Goodman-Bacon, 2018; Callaway and Sant'Anna, 2020; Abraham and Sun, 2020). Goodman-Bacon (2018) shows that the baseline DD estimates (red lines in Figure 3) can be expressed as the weighted average of the 2x2 DD estimates for each cohort of treatment timing.[22,23] There are 19 cohorts in our framework ($C_0 = \{ControlStates\}, C_1 = \{CA\}, C_2 = \{IL, NJ\}, ..., C_{18} = \{SC\}$).

Each treated cohort has a DD estimate with the control group for a total of 18 estimates.[24] Figure 6 plots these estimates for hours worked. Cohorts are divided into two groups, those that received treatment before March 29th (Early, blue circles) and those that received

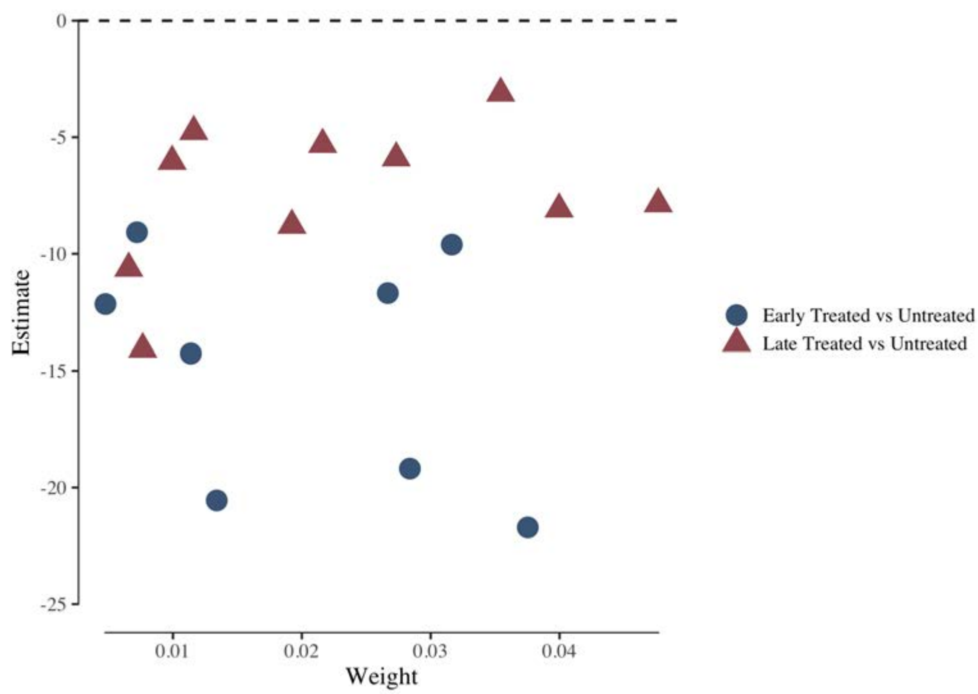
[21] Appendix Figure A.6 contains additional heterogeneity checks for Transportation & Warehousing and Health Care Services.

[22] 2x2 stands for two-period/two-group. Intuitively, the two periods are pre/post treatment. Groups are defined their treatment-timing. DD coefficients are then estimated for each two-group combination, hence 2x2.

[23] Weights rely on three main factors: (1) when units are treated, (2) the size of the treatment cohort, and (3) the length of the sample. Please see Goodman-Bacon (2018) for a full discussion on the estimation of these weights.

[24] The Goodman-Bacon (2018) decomposition also contains DD estimates across treated cohorts. In our case, this results in a total of 18 (treated/control) + 153 (treated/treated) = 171 observations. Appendix Figure A.4 provides the full decomposition for both hours worked and consumption expenditures.

Figure 6: Effect of Treatment Timing on Hours Worked DD Estimate



Note: This figure plots the results of a Goodman-Bacon (2018) decomposition on our difference-in-difference estimate. The dependent variable is hours worked. The y-axis is the treatment-timing cohort DD estimate. The x-axis is the weight for the respective estimates. Cohorts are further divided into two groups: Treatment occured before March 29th (Early, blue circles), Treatment occured on or after March 29th (Late, red triangles). Hours worked data is obtained from Homebase. Observations are at the state-by-day level.

treatment on or after March 29th (Late, red triangles).[25] Each treatment-cohort/control DD estimate is negative, so hours worked decreased after treatment for all cohorts relative to the control group. However, there is a significant difference in the DD estimates across the early/late treatment groups. Early treated groups, on average, experience about twice the relative decline in hours worked (14.8pp) compared to late treated groups (7.5pp).

3.3.1 Refined Event Study Specification

Figure 6 suggests that there may be heterogeneity in the estimated treatment effects across treatment cohorts. We refine the event study specification in Equation 1 to test this hypoth-

[25]Stay-at-home orders occurred over four weeks (recall Table 1). March 29th is used as the determinant for late treatment as it groups the first two weeks (Early) and last two weeks (Late).

esis:

$$y_{it} = \alpha_i + \gamma_t + \Gamma X_{it} + \sum_{g=1}^{G} \sum_{-10 \leq k \leq 15}^{k \neq -1} \beta_k^g I(i \in g) I(StayHome_{it} = k) + \epsilon_{it} \tag{2}$$

where g specifies the group that state i belongs to. Notice that, compared to Equation 1, β_k is now allowed to vary across groups, β_k^g. We again consider the two groups of Early (Before March 29th) and Late (On or After March 29th) treatment. Thus, formally, our specification is

$$\begin{aligned} y_{it} = \alpha_i + \gamma_t + \Gamma X_{it} + & \sum_{-10 \leq k \leq 15}^{k \neq -1} \beta_k^{Early} I(i \in Early) I(StayHome_{it} = k) \\ + & \sum_{-10 \leq k \leq 15}^{k \neq -1} \beta_k^{Late} I(i \in Late) I(StayHome_{it} = k) + \epsilon_{it} \end{aligned}$$

This specification is similar but not equivalent to the specification in Abraham and Sun (2020) as we relax their assumption that groups must belong to the same treatment-timing cohort.[26]

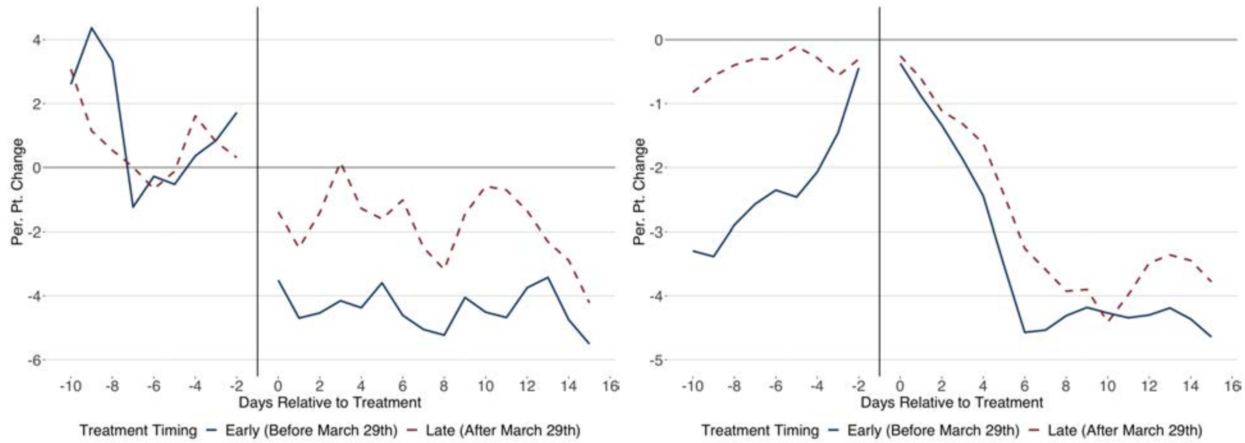
3.3.2 Results

Figure 7 plots the results of Equation 2 with the early treatment estimates in blue and late treatment in red. Similar to Figure 6, Panel (a) suggests that early treated states experience about twice the relative decline in hours worked than later treated states. Early treated states experience about a 4pp decline in hours worked for the entire post-period while late treated states experience a 2pp decline. Panel (b) estimates similar expenditure declines after the issuance of a stay-at-home order in early and late treated states, but significantly different pre-trends. The consumer anticipation effect appears to be driven primarily by early treated states which see a 3pp increase in expenditures before an order is enacted.

[26]This specification is also similar to stratifying the event study by early and late treatment groups.

Figure 7: Effect of Treatment Timing on Event Study Estimates

(a) Hours Worked **(b)** Expenditure



Note: This figure plots the stay-at-home order event study coefficients estimated from Equation 2. Subcaptions represent the dependent variable. Treated states are divided into two groups: Treatment occured before March 29th (Early, blue), Treatment occured on or after March 29th (Late, red). Observations are at the state-by-day level. Hours and expenditure data are obtained from Homebase and Affinity Solutions, respectively.

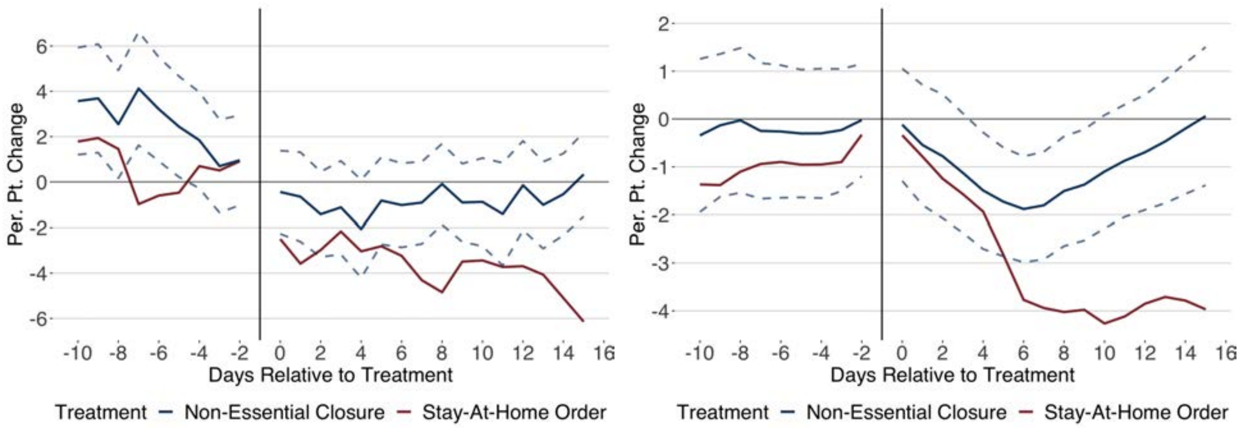
3.4 Non-essential Business Closures

Section 2.3 mentioned that the specification in Equation 1 also includes an event-study covariate for non-essential business closures. The cumulative effect of implemented policies may be of interest in addition to the individual effect of stay-at-home orders. Figure 8 plots our baseline estimates for hours worked and total expenditure side-by-side with non-essential business closures in blue and stay-at-home orders in red. Dashed lines represent confidence intervals at the 95% confidence level using robust standard errors. Standard errors are omitted for stay-at-home orders since they are shown in Figure 3. Panel (a) shows that non-essential business closures did not have a statistically significant effect on hours worked. This may be the result of a break in pre-trends before their issuance.

Panel (b) shows that non-essential closures did have a statistically significant effect on expenditures 4-10 days after their implementation with a trough on Day 6. The significant point estimates have a range from a 1-2 percentage point decline in expenditures. We find no evidence of a break in pre-trends up to ten days before the order. The pre-trends also

Figure 8: Effect of Non-essential Business Closures on the Economy

(a) Hours Worked **(b)** Expenditure



Note: This figure plots the non-essential business closure event study coefficients (blue) and stay-at-home order event study coefficients (red) estimated from Equation 1. Subcaptions represent the dependent variable. Dashed lines represent 95% confidence intervals using robust standard errors. Observations are at the state-by-day level. Hours and expenditure data are obtained from Homebase and Affinity Solutions, respectively.

show that the consumer anticipation effect seen for stay-at-home orders is not evident for non-essential business closures. This may be due to differences in the supply and demand implications between the two policies as non-essential business closures directly targeted the supply side (firms) of the market while stay-at-home orders directly targeted both supply (firms) and demand (consumers). An analog of Table 2 with robustness/heterogeneity checks for non-essential business closures can be found in the appendix. The results are similar to Figure 8 in that we find null effects of non-essential business closures as in Panel (a) or mitigated effects with limited statistical significance compared to stay-at-home orders as in Panel (b). Overall, the inclusion of non-essential business closures suggest that the cumulative effect of mitigation policy in response to Covid-19 is at least as great as our estimates from stay-at-home orders.

3.5 Interpretation of Event Study Results

The event study estimates represent the average effect of issuing a stay-at-home order on hours worked and consumer expenditure for treated states. The previous sections presented these results as percentage point effects (e.g. 4pp decrease in hours worked). These percentage point effects can be used to estimate the absolute and cumulative effects of stay-at-home orders for treated states. Recall that all variables are measured relative to January 4-31. The average amount of consumer spending per day from Jan. 4-31 was $21.82 billion (Chetty et al., 2020). To estimate the absolute effect of a stay-at-home order, we multiply the event study estimates by $21.82 billion. The baseline estimate for consumer spending fifteen days after a stay-at-home order was -3.97%. The absolute effect is then -3.97%*$21.82 = $0.87 billion. The cumulative effect after fifteen days can be found by summing the absolute effect for all of the post-treatment days (0-15). For consumer expenditures, this equates to $10.5 billion. However, this estimate assumes that the entire US was treated. We then multiply $10.5 billion by the percentage of the population that was placed under a stay-at-home order to account for this.[27] The final estimate of the cumulative effect of stay-at-home orders on consumer spending is then 95%*$10.5 billion = $9.94 billion.

Panel A of Table 3 plots the cumulative spending loss for treated states calculated above in Column (1). The bracketed terms are calculated by summing the upper 95% confidence band and the lower band from our baseline expenditure estimates (Figure 3) over the entire post-period. This results in a range from $6.28 billion to $13.61 billion. A similar analysis is conducted for earnings per worker and total earnings for states that issued a stay-at-home order.[28] This suggests a cumulative loss of $80 per worker shown in Column (2). We aggregate this effect over treated states by multiplying the January BLS non-farm employment numbers (152 million) by the percent of the treated population in the US. This suggests about a $15.5 billion loss in earnings in the first fifteen days after a stay-at-home order. Bartik et al. (2020) show that stay-at-home orders had significant effects for almost 30 days

[27]This uses the simplifying assumption that consumer spending per capita is equal across states as we do not have access to the actual percentage of consumer spending by treated states.

[28]We first calculate the average earnings per worker per day in January. To do this, we obtain average hourly earnings ($28.43) and average weekly hours (34.3) from the BLS for all employees on private payrolls in January. Average daily earnings can then be calculated as the hourly earnings multiplied by average daily hours, $28.43*(34.3/7) = $139.31. The cumulative effect for earnings per worker is then calculated by summing our baseline event study estimates for hours worked multiplied by the daily wage.

Table 3: Cumulative and Relative Effects of Stay-at-Home Orders

Panel A: Spending and Hours Worked	Total Spending (1)	Earnings Per Worker (2)	Total Earnings (3)
January Benchmark	\$21.92 billion/day	\$139.31/day	\$26.23 billion/day
Cumulative Effect (Days 0-15)	\$9.94 billion [6.28, 13.61]	\$82.40 [44.19, 120.61]	\$15.51 billion [8.32, 22.71]
Panel B: Employment	Stay-at-Home Orders (1)	BLS Employment (2)	CPS Supplement (3)
Employment Loss	5,797,349	21,909,000	49,839,000
Relative Effect	100%	26.5%	11.6%

Note: This table presents the cumulative effect of stay-at-home orders on consumer spending and hours worked in Panel A. The cumulative effects are obtained by summing the stay-at-home order event-study coefficients of Equation 1 and multiplying this sum by the reported January benchmarks. We then multiply this result by the percent of the population that experienced a stay-at-home order during our sample period. Numbers in brackets present the confidence interval for the calculated cumulative effect by summing the 95% lower and upper confidence bands for the event-study coefficients. The January benchmark for consumer spending is obtained from Chetty et al. (2020). To obtain January benchmarks for earnings per worked and total earnings, we use BLS data on average private hourly earnings (CES0500000003), average private weekly hours (CES0500000002), and total nonfarm employment (CES0000000001). Column (1) of Panel B uses the difference-in-difference estimate for employment to compute the employment loss from stay-at-home orders. The BLS employment loss is computed by subtracting the BLS estimate of total nonfarm employment in April 2020 by the estimate in January 2020. The employment loss in Column (3) is obtained from the supplemental Covid-19 questions used in the May 2020 CPS. The relative effect is then calculated by dividing the employment loss from stay-at-home orders by the total employment loss in the respective columns.

after their issuance. Given this, our results likely serve as a lower bound for the cumulative effect of stay-at-home orders.

Panel B of Table 3 plots the relative employment loss from stay-at-home orders. Unlike consumer spending and hours worked, the effect of stay-at-home orders on employment should not be measured cumulatively. For example, an individual who becomes unemployed one day after a stay-at-home order is likely to be unemployed two days after the order as well. The cumulative effect would then count this individual more than once in the total employment loss. To circumvent this issue, we express the average effect of stay-at-home orders on employment relative to the total employment loss during our sample. The average effect of stay-at-home orders can be found by multiplying the difference-in-difference estimate for employment (-4%) by total employment in January (152 million).[29] As before, this estimate is scaled by the percent of the population under a stay-at-home order during this period which results in -4%*95%*152 million = -5.8 million. We normalize this 5.8 million employment loss relative to the total employment loss to isolate the relative impact of stay-at-home orders. The BLS employment estimate for April (130 million) suggests that 22 million jobs were lost in total. The relative effect is then computed by dividing the employment loss from stay-at-home orders by the total employment loss, $\frac{5.8M}{22M} = 26\%$.

In May, the BLS added several questions to the the Current Population Survey (CPS) to help gauge the effects of the coronavirus on the labor market (BLS, 2020). One of the questions was "At any time in the last 4 weeks, were you unable to work because your employer closed or lost business due to the coronavirus pandemic?"[30] The survey results from this supplemental question suggest 50 million people were unable to work. Column (3) of Panel B uses this estimate as the total employment loss which results in a 11.6% contribution of stay-at-home orders to the total employment loss.

4 Model Framework: Optimal mitigation policy with multiple locations

The empirical results of the previous section showed that stay-at-home orders had a significant effect on the economy. Beyond general concerns about the economic impact of the orders, policy makers often argued about how to best implement them. On April 1st, in regards to issuing a nationwide stay-at-home order, President Trump responded "There are some states that are different. There are some states that don't have much of a problem . . . You have to give a little bit of flexibility." (Press Briefing, 2020) Dr. Anthony Fauci expressed a different opinion the next day saying, "I just don't understand why we're not do-

[29] See Appendix Table A.2 for difference-in-difference estimates. As before, non-farm employment in January is obtained from the BLS.

[30] The CPS is conducted during the week of the 19th. Thus, the four weeks before May 19th slightly abstracts from the sample period of our analysis.

ing that [issuing a federally-mandated stay-at-home order]. We really should be." (Politico, 2020)

This section builds on our empirical results by developing an economic framework to analyze virus transmission and optimal mitigation policy in a fiscal union such as the United States. The model will allow us to address virus transmission across locations. Thus, the economic activity of one location depends upon the spread of the virus in other locations.

4.1 Initial Conditions

We model the United States as a multi-location, closed economy version of the SIR macro model in Eichenbaum et al. (2020). We interpret these locations as states which we denote by $i \in [1, N]$, where N is the total number of states. The initial population in a state as a fraction of the national population is written as $Pop_{i,0} \in (0, 1]$.[31] A fraction of the state population becomes infected at $t = 0$, $T_{i,0} = \epsilon_i$. All agents are initially susceptible, $S_{i,0} = 1 - T_{i,0}$. Thus, active infections in state i at date 0 is $I_{i,0} = T_{i,0}$. This allows states to vary in both initial population and initial infections.

4.2 Virus Transmission

As in Eichenbaum et al. (2020), we assume the virus is transmitted within a state through consumption and employment. We further allow the virus to transfer within and across states through an exogenous variable. For a given Home state, which we denote by H, this transmission mechanism at time t is given by:

$$T_{H,t} = \pi_{s1}(S_{H,t}C^S_{H,t})(I_{H,t}C^I_{H,t}) + \pi_{s2}(S_{H,t}N^S_{H,t})(I_{H,t}N^I_{H,t}) + \pi_{s3}S_{H,t}\sum_{i=1}^{N}(1-\tau^d_{H,i})(I_{i,t}Pop_{i,0}) \quad (3)$$

where $C^S_{H,t}$ is consumption at time t by susceptible agents in the Home state, and $C^I_{H,t}$ is consumption by infected agents. Similarly, $N^S_{H,t}$ and $N^I_{H,t}$ are hours worked by susceptible

[31] The initial population of a state equals one if and only if $N = 1$.

and infected agents in the Home state, respectively. Note that $\tau^d_{H,i}$ represents the travel cost between locations H and i. This yields an interpretation of $1 - \tau^d_{H,i}$ as the percentage of susceptible agents in state H that interact with infected agents in state i. The parameters π_{s1}, π_{s2}, and π_{s3} are the probability of transmitting the virus through each of the three associated mechanisms.

The first two terms of our transmission mechanism, apart from the location indices, are identical to those in Eichenbaum et al. (2020). The substantive difference between the two models is the presence of the third term. This is evident by the fact that our model replicates Eichenbaum et al. (2020) if we assume that the travel cost between states, $\tau^d_{H,i}$, equals zero and infection rates are symmetric, or in the trivial case of modelling one location. The third and novel mechanism is similar in spirit to the empirical model of Adda (2016), but we loosen the restriction that the travel cost is equal between all pairs of states. Our only assumptions are that (1) travel cost within a state is equal to zero, $\tau^d_{H,H} = 0$, and (2) $\tau^d_{H,i} \in [0, 1]$ to prohibit negative transmission across locations.

The spread of the virus in each state can then be modeled by iterating

$$S_{t+1} = S_t - T_t \tag{4}$$

$$I_{t+1} = (1 - \pi_r - \pi_d)I_t + T_t \tag{5}$$

$$R_{t+1} = R_t + \pi_r I_t \tag{6}$$

$$D_{t+1} = D_t + \pi_d I_t \tag{7}$$

where the state index i is suppressed for notational simplicity. T_t follows the transmission mechanism in Equation 3 in each state. R_t and D_t represent the total fraction of recovered and deceased. The parameters π_r and π_d represent the probability an infected person recovers or dies.

4.3 The Economy

We assume that states are closed economies with the exception of virus transmission.[32] This allows us to isolate the economic spillovers of virus transmission across locations while replicating the Eichenbaum et al. (2020) results as a special case. This allows us to focus specifically on the economic effects of the cross-state virus transmission mechanism, and simplifies the solution to the state-level decision problems. In the text that follows, the location index i is suppressed except where needed for clarity of exposition. Individual state economies can be written as follows:

Households We assume agents' discounted lifetime utility depends on their infection status $j \in (s, i, r)$. As infection status follows the transition matrix presented in Equations 3-7, lifetime utility of the various agents can be written as

$$U_t^r = u(c_t^r, n_t^r) + \beta U_{t+1}^r \tag{8}$$

$$U_t^i = u(c_t^i, n_t^i) + \beta \left[(1 - \pi_r - \pi_d) U_{t+1}^i + \pi_r U_{t+1}^r + \pi_d \times 0 \right] \tag{9}$$

$$U_t^s = u(c_t^s, n_t^s) + \beta \left[(1 - \pi_{I,t}) U_{t+1}^s + \pi_{I,t} U_{t+1}^i \right] \tag{10}$$

where c_t is consumption, n_t is hours worked, and $\pi_{I,t}$ represents the probability a susceptible agent becomes infected. In the model, this is found by dividing Equation 3 by S_t which yields

$$\pi_{I,t} = \pi_{s1}(C_t^S)(I_t C_t^I) + \pi_{s2}(N_t^S)(I_t N_t^I) + \pi_{s3} \sum_{i=1}^{N} (1 - \tau_{H,i}^d)(I_{i,t} Pop_{i,0}) \tag{11}$$

The remainder of the economic environment follows Eichenbaum et al. (2020). Prefer-

[32] The closed economy assumption abstracts from endogenous trade fluctuations and international policy such as travel restrictions in response to the virus. Antràs et al. (2020) and Fajgelbaum et al. (2020) develop open-economy models to analyze these avenues. Importantly, these papers provide support that the qualitative results in Section 5 are likely to hold if we relax the closed economy assumption.

ences of agents in each state take the functional form,

$$u(c_t^j, n_t^j) = \ln c_t^j - \frac{\theta}{2}(n_t^j)^2 \tag{12}$$

Consumers maximize utility over an infinite horizon subject to the budget constraint

$$(1 + \mu_{ct})c_t^j = w_t \phi^j n_t^j + \Gamma_t \tag{13}$$

where w_t is the real wage, ϕ is labor productivity, and Γ_t are lump-sum transfers from the government financed by the tax on consumption, μ_{ct}. Intuitively, the consumption tax can be viewed as a proxy for mitigation policies such as stay-at-home orders as it creates a tradeoff between mitigating the spread of the virus and negatively impacting the economy.[33]

Firms In each state, a continuum of representative firms maximize profits

$$\Pi_t = Y_t - w_t N_t \tag{14}$$

where N_t is hours worked and $Y_t = AN_t$ is output.

Government Each state government is subject to a balanced-budget requirement:

$$\mu_{ct}\left(S_t c_t^s + I_t c_t^i + R_t c_t^r\right) = \Gamma_t \left(S_t + I_t + R_t\right) \tag{15}$$

for all t.[34]

[33] The interpretation of the consumption tax as a proxy for mitigation is discussed more in Section 5.3.

[34] Requiring the government budget constraint to hold at the state level implies that fiscal transfers across states are equal to zero. Loosening this restriction to hold at the national level—net fiscal transfers are zero—would create the opportunity to study optimal transfers across locations. This is a possible avenue for future research to study the effect of large fiscal programs such as the Paycheck Protection Program or the Cares Act, but it is not as relevant for our analysis of mitigation policies such as stay-at-home orders.

Table 4: Baseline Model Parameters

	Parameter (1)	Value (2)
Panel A: New Parameters		
State Population (Baseline)	Pop_{i0}	$\frac{1}{N}$
Initial State Infection Rate (Baseline)	ϵ_{i0}	0.001
Travel Cost (Baseline)	$\tau^d_{H,i}$	$\frac{\|H-i\|}{N}$
Panel B: Eichenbaum et al. (2020)		
Initial Country Population	Pop_0^{Agg}	1
Initial Country Infected	ϵ^{Agg}	0.001
Productivity	A	10
Labor Productivity (Susceptible)	ϕ^s	1
Labor Productivity (Infected)	ϕ^i	0.8
Labor Productivity (Recovered)	ϕ^r	1
Disutility of labor	θ	9
Discount Rate	β	$0.98^{1/52}$
Probability of consumption infection	π_{s1}	0.0185
Probability of labor infection	π_{s2}	1.8496
Probability of exogenous infection	π_{s3}	0.2055
Probability of recovery	π_r	$7\times\frac{0.99}{18}$
Probability of death	π_d	$7\times\frac{0.01}{18}$
Consumption Tax	μ_{ct}	0

Note: This table presents the parameters used in our baseline model specification in Section 5. Panel A presents variables specific to our multiple-location model. Panel B presents the baseline parameters used in Eichenbaum et al. (2020) which remain in our model.

5 Model Implications: Multiple locations and optimal policy

We illustrate the dynamics of our model through several numerical examples. First, we compare the SIR macro model with one location to our multiple location model. We use the baseline parameters in Eichenbaum et al. (2020) for this exercise (see Table 4). Eichenbaum et al. (2020) study how various parameterizations affect the dynamics of the single-location model, so we do not replicate that sensitivity analysis here. Various changes to their parameters are unlikely to significantly affect the main contribution of our model which is the cross-sectional distribution we obtain. However, our model does contain several new parameters: (1) state population, (2) initial state infection rates, and (3) travel cost across locations. The second subsection develops a procedure to parameterize travel costs across locations which we apply to US Census region data. This application allows us to perform a sensitivity analysis to study the effect that travel cost across locations has on the cross-sectional distribution of infection rates.[35] In turn, we study how the cross-section affects the aggregate dynamics of the model. Lastly, we study optimal mitigation policy when there are multiple locations with asymmetric infection rates.

5.1 Multiple-location model vs single-location model

Figure 9 plots results from our simulated model with one location (blue) and twenty-one locations (red). The grey lines represent the individual states. The red line is the national total computed as the population-weighted average of the grey lines. All states in the country are symmetric in terms of their initial populations and initial infected. Thus, these results are entirely driven by our travel cost which is $\tau^d_{H,i} = \frac{|H-i|}{N}$ where N is the total number of locations in the country.[36] As mentioned earlier, setting travel cost to 0 when initial infection rates are symmetric would result in all states following the blue line.

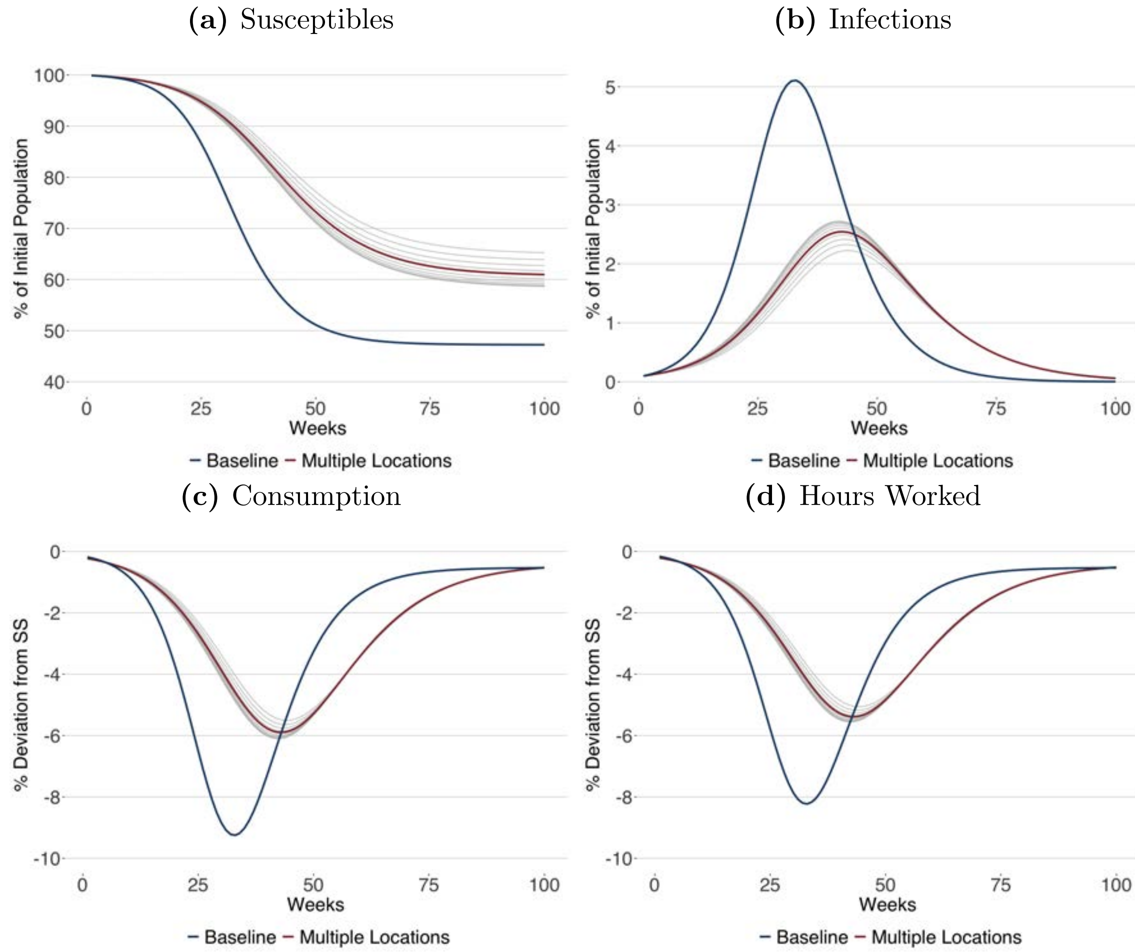
Panel (a) of Figure 9 plots the simulated fraction of susceptibles. With one location, we see that about 45% of the country's population becomes infected by the end of the sample. The multiple-location results suggest that a little less than 30% of the population contracts the virus. Furthermore, the spread of the virus is not symmetric across locations even though the initial conditions are. This creates a cross-section in which 25% of the state population contracts the virus in the least affected state while over 30% contract it in the most severely impacted state.

Panel (b) of Figure 9 plots the simulated infection rates for both models. We see that the

[35]The appendix contains a full sensitivity analysis of state population, state infection rates, and travel cost across locations.

[36]This travel cost is selected as it meets the assumptions that (1) travel cost within a state is equal to 0, and (2) $\tau_{H,i} \in [0, 1]$ for all state pairs. The dynamics of the cross-section will depend on the travel cost selected. Figures 10 and A.8 provide a more in-depth analysis of the effect of different travel costs on the cross-sectional distribution.

Figure 9: Multiple-location Model vs Single-location Model



Note: This figure plots the results of simulations using the baseline SIR macro model with one location (blue) and our extended multiple-location version (red). Grey lines are produced from the multiple-location model and represent individual states. The red line is the national total from the multiple-location model, or equivalently the population-weighted average of the grey lines. Twenty-one locations were simulated.

baseline SIR macro model suggests a peak infection rate of 3.5% at about 30 weeks. With multiple locations, the country-wide peak occurs almost 20 weeks later and is much lower at about 1.5%. There are two interesting results seen in the cross-section of peak infection rates. Not only do peak infection rates differ across states, but they also differ in their timing. The intuition for this can be seen in Panels (c) and (d) which plot the percent deviation of consumption and hours worked from their steady state values. As states experience more infections, susceptible agents endogenously choose to decrease their consumption and hours to lower their probability of being infected. In turn, this contributes to earlier turning points in the infection rate.

5.2 Application to US Census Region Data

The previous subsection showed the capability that our model has to generate a cross-sectional distribution of economic variables and infection rates. We demonstrated the important role played by the travel cost across locations, $\tau^d_{H,i}$. Although our chosen travel cost function is convenient to illustrate the differences between the single and multiple-location models, it is impractical for applied settings. In this section, we develop a procedure to parameterize trade costs across locations. We then apply the procedure to US Census region data to illustrate the advantages of this approach.

5.2.1 Calibration Procedure

Panel A of Figure 10 plots the spread of the virus in the US by Census Region (Midwest, Northeast, South, West) and the national total (dashed line) from May 6th through August 26th. National and Census region data are obtained by aggregating the county-level infection and population data from Section 2. The daily data are transformed to a weekly frequency in accordance with the model. The following steps are followed to generate simulations from the model:

1. Set Number of Locations (N)
2. Set Initial Population of Locations (Pop_0)
3. Set Susceptible Percent of Population (S_t)
4. Set New Infections (T_t)
5. Set Active Infections (I_t)
6. Set Parameters ($\tau^d_{H,i}, \pi_{s1}, \pi_{s2}, \pi_{s3}$)[37]

In this application, the number of locations is set equal to the number of Census regions, $N = 4$. The corresponding vector of populations by region is $Pop_0 = (20.9\%, 17.2\%, 38.1\%, 23.8\%)$

[37] Appendix A.2.2 provides details on why these parameters are chosen to fit the data.

for the Midwest, Northeast, South, and West respectively. The percent of susceptibles at each date is 1 less the cumulative fraction of the population that has been infected up to that date, $S_t = 1 - \sum_{k=0}^{t} T_k$. On May 6th, the susceptible vector is $S_t = (99.7\%, 98.9\%, 99.8\%, 99.8\%)$. The number of new infections per capita for the week ending on May 6th were $T_t = (0.07\%, 0.11\%, 0.04\%, 0.03\%)$. We approximate current active infections as $I_t = T_t + (1 - \pi_r - \pi_d)T_{t-1}$. This approximation produces $I_t = (0.11\%, 0.22\%, 0.06\%, 0.05\%)$. We then jointly set the travel cost across locations as well as the other parameters to minimize the sum of squared errors between susceptible population percentages in the model and the data,[38]

$$\min_{\tau^d_{H,i}, \pi_{s1}, \pi_{s2}, \pi_{s3}} \sum_{i=1}^{4} \sum_{t=May6}^{Aug26} (S^{Data}_{i,t} - S^{Model}_{i,t})^2 \tag{16}$$

As in Section 4, we impose the restrictions on travel cost: $\tau^d_{H,i} \in [0, 1]$ and $\tau^d_{H,i} = 0$ for $H = i$. We also impose the restrictions that the virus transmission parameters are positive $\pi_{s1}, \pi_{s2}, \pi_{s3} \geq 0$. This process yields estimates of $(\pi_{s1} = 0.0009, \pi_{s2} = 1.53, \pi_{s3} = 0.732)$.

5.2.2 Interpretation of Transmission Matrix

Table 5 provides a summary of the initial conditions and the calibrated parameters from this procedure. Among the more interesting estimation results are those pertaining to the travel costs, $\tau^d_{H,i}$. We refer to the set of $1 - \tau^d_{H,i}$ as the transmission matrix as it relates to the interaction between susceptible agents in region H with infected individuals in region i. Specifically, $1 - \tau^d_{H,i}$ represents the probability that a susceptible individual in region H interacts with an infected individual from region i.[39] Rows represent the virus destination (H), and columns the virus origin (i). Thus, the first row represents the vector $1 - \tau^d_{Midwest,i}$. As we imposed the restriction that susceptible and infected agents fully interact within regions, we see that $1 - \tau^d_{Midwest,Midwest} = 1$. The second column produces an estimate of

[38] The appendix provides a more detailed procedure to match both the time series of susceptibles and consumer spending in the Census region data.

[39] In a reduced-form manner, our calibration procedure for $1 - \tau^d_{H,i}$ captures the average real-life interactions across regions over the sample period (e.g. travel to and from college, vacations, visits to care for dependents).

Table 5: Initial Conditions for Census Region Application (May 6)

Panel A: Initial Conditions

	Midwest	Northeast	South	West
Percentage of US Population (Pop_0)	20.9%	17.2%	38.1%	23.8%
Susceptible (S_t)	99.7%	98.9%	99.8%	99.8%
New Infections (T_t)	0.07%	0.11%	0.04%	0.03%
Active Infections (I_t)	0.11%	0.22%	0.06%	0.05%

Panel B: Transmission Parameters

	Parameter	Value
Probability of consumption infection	π_{s1}	0.0009
Probability of labor infection	π_{s2}	1.53
Probability of exogenous infection	π_{s3}	0.732

Panel C: Transmission Matrix $(1 - \tau^d_{H,i})$

	Virus Origin (i)			
Virus Destination (H)	Midwest	Northeast	South	West
Midwest	1.00	0.216	0.030	0.326
Northeast	1.1e-4	1.00	5.6e-4	0.001
South	0.009	0.472	1.00	0.247
West	0.220	0.391	0.264	1.00

Note: This table presents the initial conditions and parameters used in the Census region application. Panel A presents the initial conditions for each Census region on May 6th. Panels B and C present the estimated parameters from solving Equation 16.

$1 - \tau^d_{Midwest,Northeast} = 0.216$. This suggests that, over the sample period of May 6th through August 26th, susceptible agents in the Midwest interacted with 21% of infected agents from the Northeast on average. The third and fourth columns suggest that susceptibles in the Midwest interacted at lower rates with the South (3%) but higher rates with the West (32.6%).

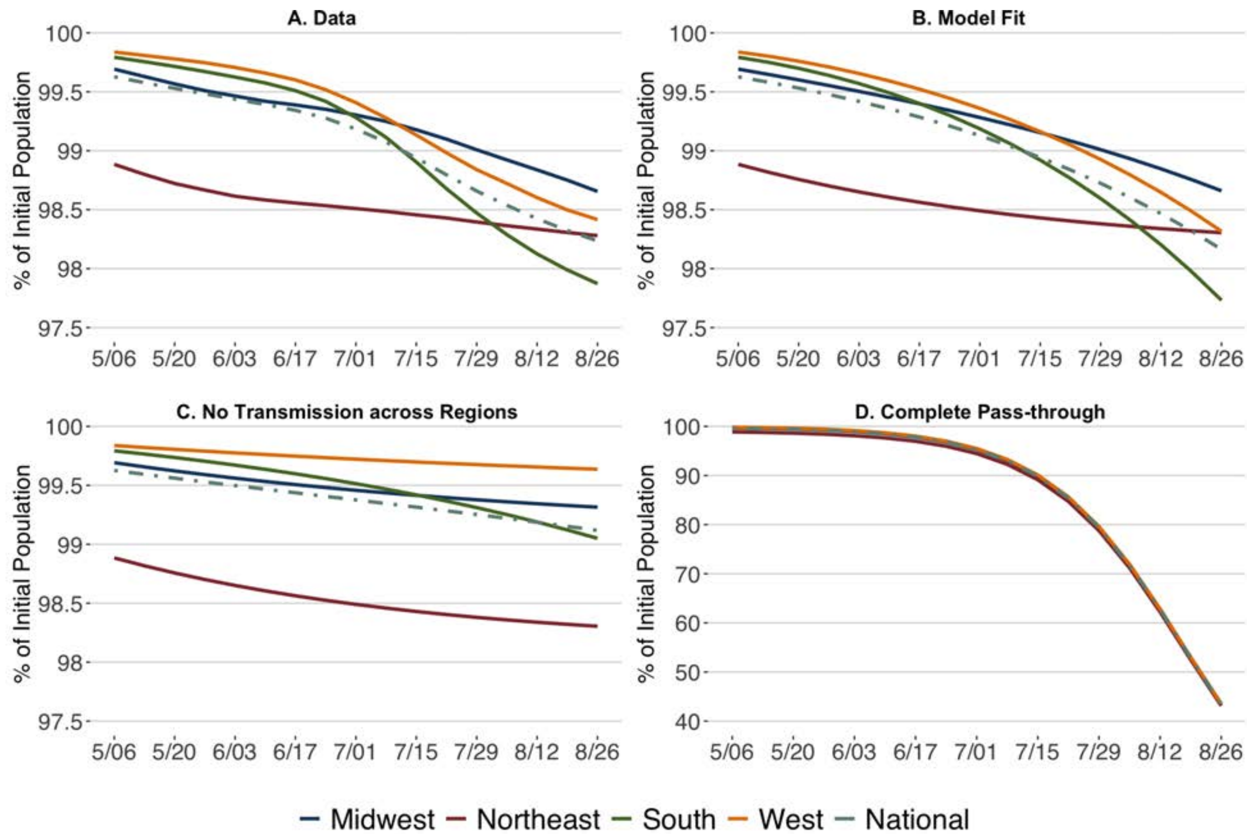
The transmission matrix suggests that there is substantial and highly asymmetric interaction across regions as most elements are non-zero and unequal. One exception is the row for the Northeast region, $1 - \tau^d_{Northeast,i}$, in which the cross-region estimates are all close to zero. This suggests the susceptible agents in the Northeast have not interacted with infected agents of other regions. In contrast, the Northeast contributed significantly to the spread of the virus to other regions. The two largest elements of the matrix are the transmission to the South from the Northeast (47.2%) and to the West from the Northeast (39.1%). It is reassuring that the transmission matrix backed out of this SIR economic model accords with the statement by Yale epidemiologist, Nathan Grubaugh, "We now have enough data to feel pretty confident that New York was the primary gateway for the rest of the country" (New York Times, 2020).

5.2.3 Results

Panel B of Figure 10 plots the results using the above procedure and then simulating our model for 17 weeks in accordance with the length of the sample period. We see that the model performs well in replicating the cross-sectional distribution. The susceptible population in the data for the week starting on August 26 is $S_t^{Data} = (98.7\%, 98.3\%, 97.9\%, 98.4\%)$ for the Midwest, Northeast, South and West. The model produces susceptible estimates of $S_t^{Model} = (98.7\%, 98.3\%, 97.7\%, 98.3\%)$. We also see that the model performs well in replicating the individual time series for each region as well as the crossing patterns across regions. In the data, the South first surpasses the Midwest in their cumulative cases per capita on July 1st and subsequently surpasses the Northeast on August 5th. In the model, these dates are June 24th and August 12th. Lastly, the West experiences higher cumulative cases per capita than the Midwest starting on July 15th, and the model predicts a crossing on July 22nd.

Comparative Statics Panels C and D emphasize the importance of modelling the interaction of virus transmission across locations. We again present the two extreme cases of no transmission across regions ($\tau_{H,i}^d = 1$ for all $H \neq i$) and complete pass-through of infections across locations ($\tau_{H,i}^d = 0$). The remaining parameters are the same as in Panel B. Thus, differences across panels are due entirely to differences in the transmission matrix, $1 - \tau_{H,i}^d$.

Figure 10: US Census Region Data vs Model (Susceptibles)



Note: This figure plots the susceptible percentage of the population in the United States and each Census region. Panel A plots the observed time series in the data. Panel B plots the model fit after jointly optimizing the travel distance across locations and the transmission parameters $(\tau^d_{H,i}, \pi_{s1}, \pi_{s2}, \pi_{s3})$. Panels C and D present comparative static exercises using π_{s1}, π_{s2}, and π_{s3} from Panel B. Panel C allows no virus transmission across Census regions. Panel D allows complete pass-through of the virus across regions. Infection data is obtained from *USA Facts*.

Panel C plots the results for no transmission across regions. This underestimates the infection rates of all regions compared to the data. The percentage of susceptible individuals predicted by the model on August 26 are now $S_t^{Model} = (99.3\%, 98.3\%, 99.1\%, 99.6\%)$. The model now predicts only one cumulative infection rate crossing. Cumulative infections in the South exceeds the Midwest on July 22nd, several weeks past the equivalent data moment.

Panel D plots the results when there is complete pass-through of infections across locations. This provides the opposite result and significantly overestimates the spread of the virus. The model now predicts that all regions have converged with ∼57% of their population contracting the virus by August 26th. Table 6 summarizes the results of our Census

Table 6: US Census Region Data vs Model Prediction (August 26)

Panel A: Cumulative Infections

	Aggregate	Midwest	Northeast	South	West
Data	1.8%	1.3%	1.7%	2.1%	1.6%
Calibrated Model	1.8%	1.3%	1.7%	2.3%	1.7%
No Transmission	0.9%	0.7%	1.7%	0.9%	0.4%
Complete Pass-through	56.5%	56.4%	56.8%	56.4%	56.4%

Panel B: Crossing Patterns

	South > Midwest	South > Northeast	West > Midwest
Data	July 1	August 5	July 15
Calibrated Model	June 24	August 12	July 22
No Transmission	July 22	NA	NA
Complete Pass-through	NA	NA	NA

Note: This table presents the results of our Census region application. Panel A plots the cumulative infections on August 26th. Panel B plots the dates in which a given Census region passed another region in cumulative infections. Both panels present the results seen in the data and three model simulations: (1) the calibrated model using the parameters in Table 5, (2) allowing no virus transmission across locations, and (3) allowing complete pass-through of the virus across locations.

region application for ease of comparability across model specifications. Overall, the model simulations suggest that the virus was transmitted partially, but not fully, across locations in the US.

5.3 Optimal Mitigation Policy

The previous subsection showed that there is substantial interaction across regions in the US. Importantly, the Census region application also suggests that there is not complete pass-through of the virus across locations. This section builds on this finding by analyzing optimal mitigation policy when initial infection rates are asymmetric and the virus is partially, but not fully, transmitted across locations.

5.3.1 Planner's Problem

The equilibrium conditions in the appendix show that infected agents do not internalize the negative externalities they impose on other agents in the economy. This creates the role for government intervention to help mitigate virus transmission through the consumption tax of the model, μ_{ct}. This tax can be viewed as a proxy for containment measures as the government can reduce the consumption and hours worked of infected agents by increasing μ_{ct}.[40] However, like stay-at-home orders, the consumption tax does not specifically target infected agents. This creates a tradeoff between decreasing transmitted infections, T_t, and negatively affecting the economy.

To formalize this in a multiple-location model, assume that a social planner sets μ_{ct} to maximize the population-weighted average of the discounted lifetime utility of all agents:

$$\max_{\mu_{ct}} U_0 = \sum_{i=1}^{N} Pop_{i0} \left(S_{i0} U_{i0}^S + I_{i0} U_{i0}^I + R_{i0} U_{i0}^R \right) \tag{17}$$

where lifetime utility of an agent of type $j \in (s, i, r)$ follows Equations 8-10. This problem best describes a national government which maximizes the above welfare function with the ability, but not the requirement, to restrict state governments to follow the same policy rule.[41] This allows us to evaluate the tradeoffs that exist between national mandates as suggested by Dr. Fauci and allowing state-level discretion as implied by President Trump.

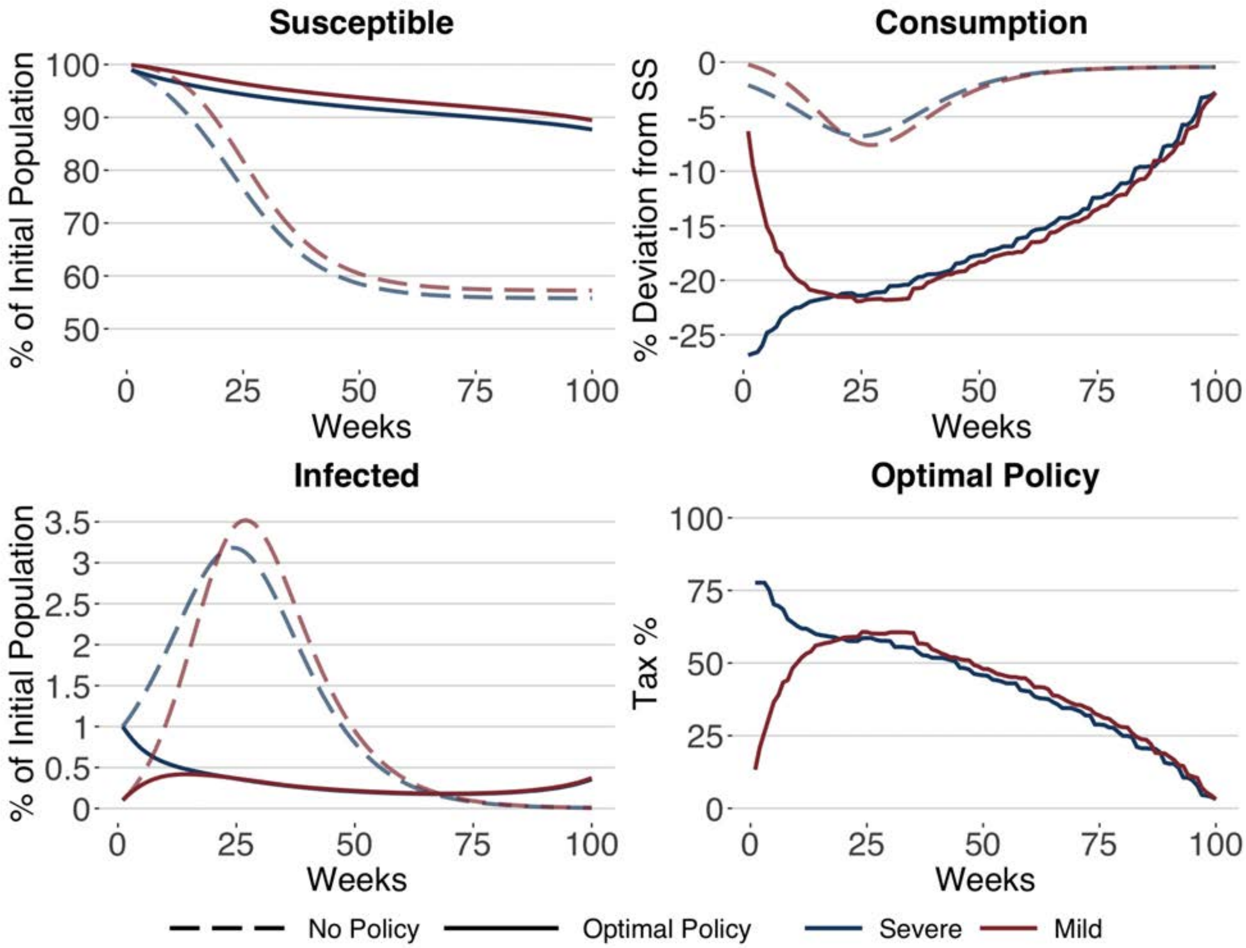
5.3.2 Results

Figure 11 plots the results of a two-location simulation using the optimal policy rule derived from the welfare function in Equation 17. Both states have an initial population of $\frac{1}{2}$. We consider one state to be severely infected (blue) with an initial infection of 1%, and the other to have a mild infection (red) of 0.1%. As before, we set $\tau_{H,i}^d = \frac{|H-i|}{N}$. The dashed lines illustrate the baseline consumption tax rate of zero in all periods. The optimal policy of the severely and mildly infected states differs for almost 20 weeks. During this period, the

[40] Importantly, the event study results in Section 3 showed that stay-at-home orders did indeed reduce spending and hours worked which provides empirical support for the use of the consumption tax as a proxy for mitigation policy.

[41] State governments are likely to place different weights on the welfare of their constituents and the welfare of the rest of the country. Our planning problem allows us to abstract from this complication. It is within this context that our model is that of a fiscal union.

Figure 11: Optimal Mitigation Policy



Note: This figure plots the results of a 2-location SIR macro model with (solid lines) and without (dashed lines) policy intervention. Both locations have an initial population of $\frac{1}{2}$. Initial infections to 1% and 0.1% for the severe (blue) and mild (red) infected locations, respectively.

severely infected state sets a higher consumption tax to mitigate their infection rate. After twenty weeks, the infection rates of the two states converge which leads to similar mitigation policies.

These results suggest that it is not optimal to set a nationwide mitigation policy. Appendix Figure A.9 plots an analogous simulation when restricting both states to the same policy rule to understand this result. A common, national policy leads to similar virus transmission dynamics in both states compared to Figure 11. However, the mildly infected state experiences significantly larger declines in consumption at the onset of the pandemic. Simply put, a nationwide mandate is too restrictive for the economy of the mildly infected state and does not help contribute to the mitigation of the virus relative to local mandates. These results suggest that policy should be set at a local level contingent on sufficient asymmetries in infection rates.

6 Conclusion

We show that stay-at-home orders reduced consumption and employment by 4 percentage points using an event study framework. Our estimates suggest that stay-at-home orders contributed to the total employment loss by almost 6 million employees. This accounts for 10%-25% of the total employment decline that occurred during our sample period. Cumulatively, stay-at-home orders contributed a loss of about $10 billion in consumer spending and $15 billion in total wages.

We pair our empirical findings with an economic SIR model to evaluate virus transmission and optimal mitigation policy in a fiscal union. We show that travel costs are important for both the aggregate and cross-sectional estimation of the virus spread and economic equilibrium. The extended model is shown to fit both the infection transmission and consumption time series at the Census region level. Lastly, we find that it is optimal for states to set their own mitigation policies when infection rates are asymmetric. Our model, combined with our simulation approach, demonstrates the value of data-driven public policy decisions that factor in the economic and health consequences of Covid-19 as well as the value of local and state policy discretion.

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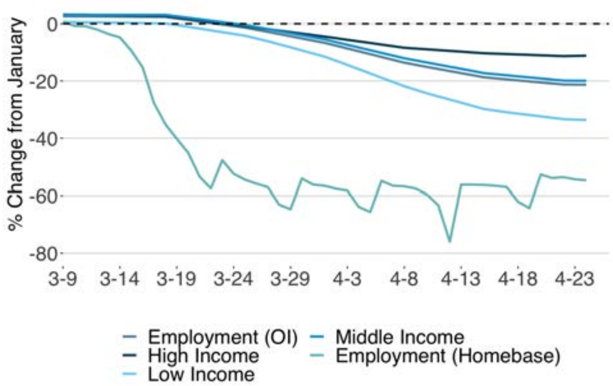
A Appendix

A.1 Data

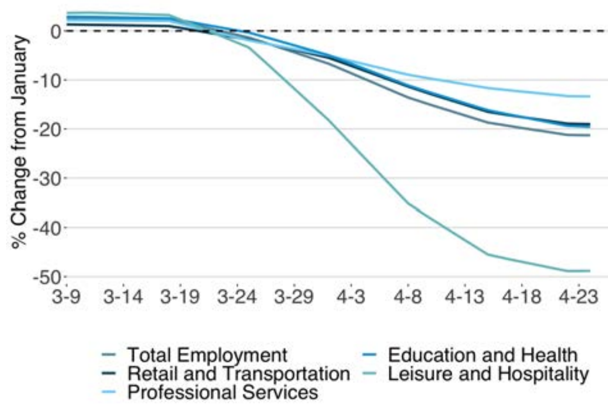
A.1.1 Summary Statistics

Figure A.1: Heterogeneous Response to Covid-19 by Income and Sector

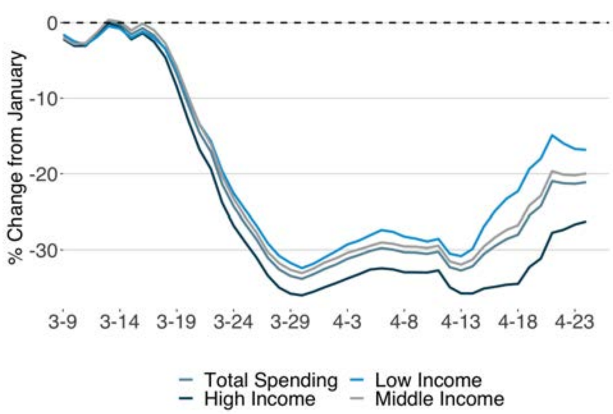
(a) Employment by Income Level



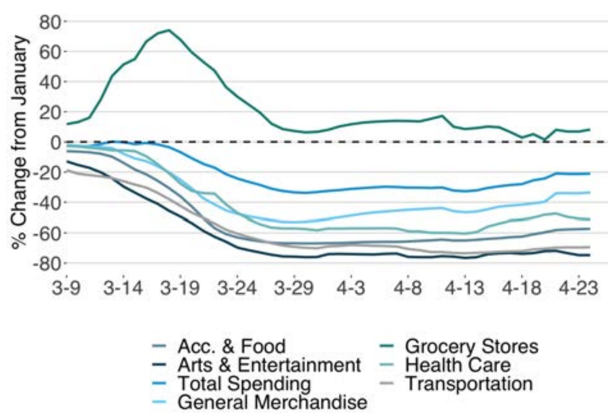
(b) Employment by Sector



(c) Expenditure by Income Level



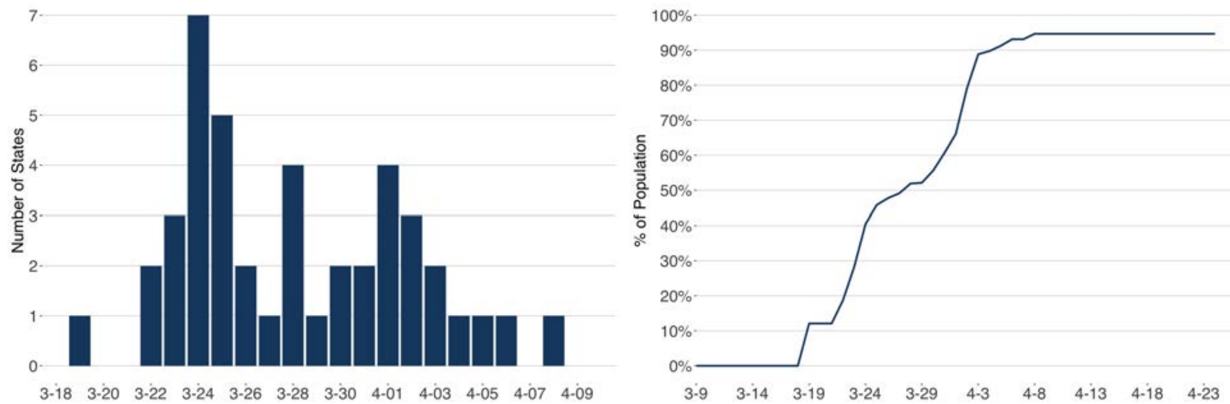
(d) Expenditure by Sector



Note: This figure plots the aggregate time series of employment and consumption expenditures from March 9th to April 24th. Employment data is obtained from Homebase and Opportunity Insights. Expenditure data is sourced from Affinity Solutions. Panel (a) plots the employment response by income level. Panel (b) plots the employment response by sector. Panel(c) plots the expenditure response by income level. Panel (d) plots the expenditure response by sector. All variables are measured relative to their respective counterparts from January 4th-31st.

Figure A.2: Timeline of Stay-at-Home Orders

(a) New States with Order **(b)** Percent of Population with Order



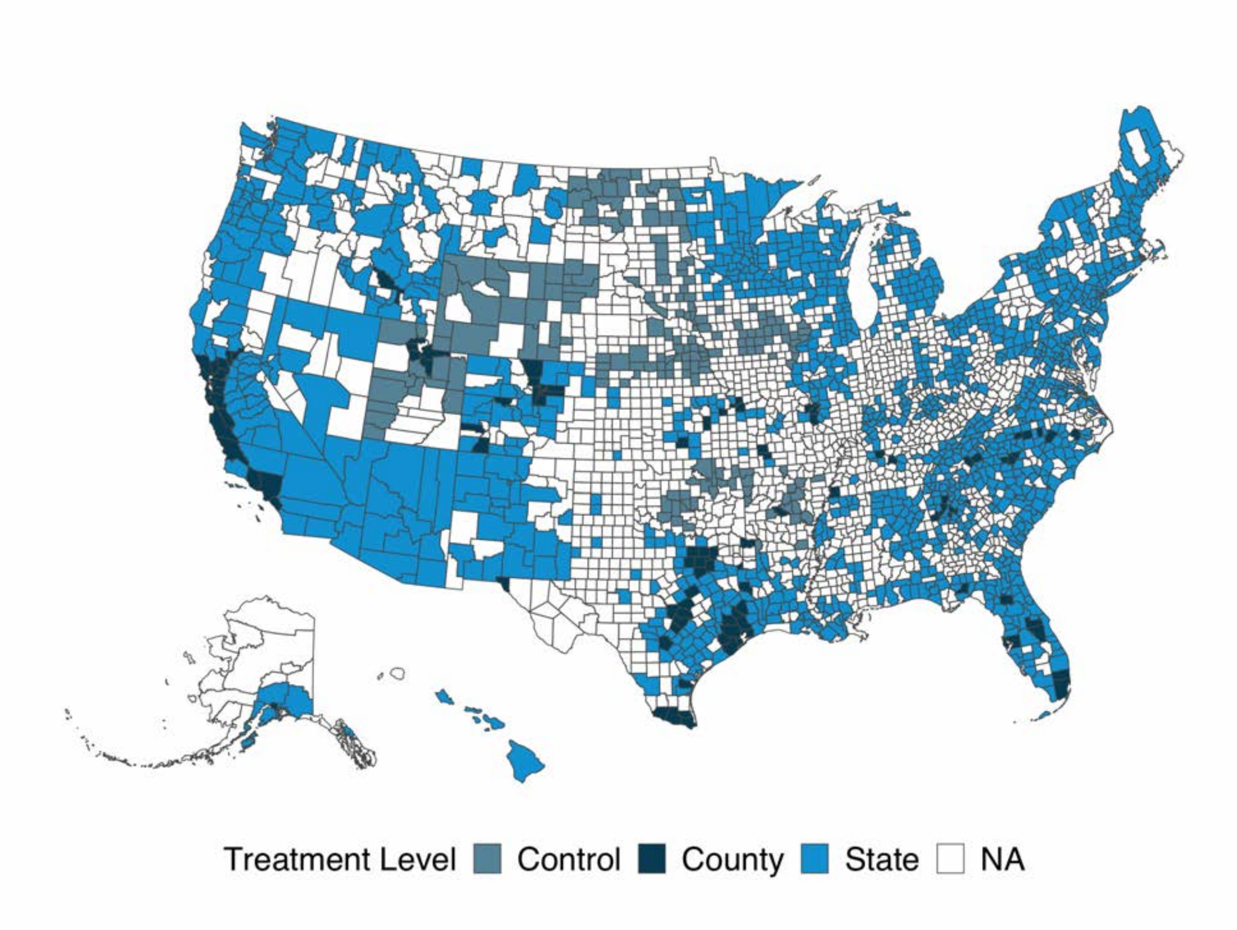
Note: This figure plots the number of new states that issued a stay-at-home order from March 18th through April 9th in Panel (a). Panel(b) plots the percent of the total US population under a stay-at-home order from March 9th through April 24th. Dates of stay-at-home orders are obtained from the *New York Times*.

Table A.1: Balance across states

	March 18 (Pre-treatment)			April 23 (End of Sample)		
	(1) Treatment Mean/SD	(2) Control Mean/SD	(3) Difference (1) - (2)	(1) Treatment Mean/SD	(2) Control Mean/SD	(3) Difference (1) - (2)
Cases	208.95	29.38	179.58	19,644	2,256	17,388
	(506.52)	(18.4)		(42,255)	(1,290)	
Population (in millions)	7.20	2.18	5.03	7.20	2.18	5.03
	(7.74)	(1.31)		(7.74)	(1.31)	
Cases per 10,000 Residents	2.46	1.51	0.95	238	109	129
	(3.25)	(0.76)		(279)	(51)	
Businesses Open	-0.22	-0.14	-0.08	-0.46	-0.29	-0.17
	(0.08)	(0.06)		(0.08)	(0.07)	
Employment	-0.34	-0.23	-0.12	-0.55	-0.37	-0.18
	(0.1)	(0.1)		(0.09)	(0.1)	
Hours Worked	-0.34	-0.23	-0.11	-0.54	-0.35	-0.19
	(0.1)	(0.1)		(0.1)	(0.11)	
Expenditures (Total)	-0.03	0.00	-0.03	-0.20	-0.15	-0.05
	(0.04)	(0.04)		(0.08)	(0.08)	
Expenditures (Low Income)	-0.04	-0.01	-0.03	-0.17	-0.12	-0.04
	(0.09)	(0.08)		(0.12)	(0.12)	
Expenditures (Middle Income)	-0.03	0.01	-0.03	-0.20	-0.14	-0.06
	(0.04)	(0.04)		(0.07)	(0.08)	
Expenditures (High Income)	-0.04	-0.03	-0.01	-0.26	-0.19	-0.07
	(0.05)	(0.06)		(0.06)	(0.06)	
Number of observations	43	8	51	43	8	51

Note: This table presents means and standard deviations for outcomes of interest and other observable characteristics at the state level by treatment status for March 18th and April 23rd. Treatment is defined by the issuance of a statewide stay-at-home order. March 18th is the day before the issuance of the first statewide stay-at-home order. April 23rd is the day before the first lifting of a stay-at-home order. The third column reports the difference in means between the two groups. Low income expenditure is missing for Alaska and Vermont. High income expenditure is missing for Montana. This affects the number of observations for these variables.

Figure A.3: Counties by Treatment Status



Note: This figure plots counties that remain in our final sample by their treatment status. Stay-at-home data at the county level is obtained from the National Association of Counties. Statewide orders are obtained from the *New York Times*. Counties that issued a stay-at-home order before their state are shaded in dark blue. Counties that did not issue a stay-at-home order but reside in a state that did are shaded in light blue. Counties that did not experience a county-level or state-level stay-at-home order are shaded in grey-blue. Counties missing from our combined dataset are not shaded and labelled as NA.

A.1.2 Difference-in-difference Estimates

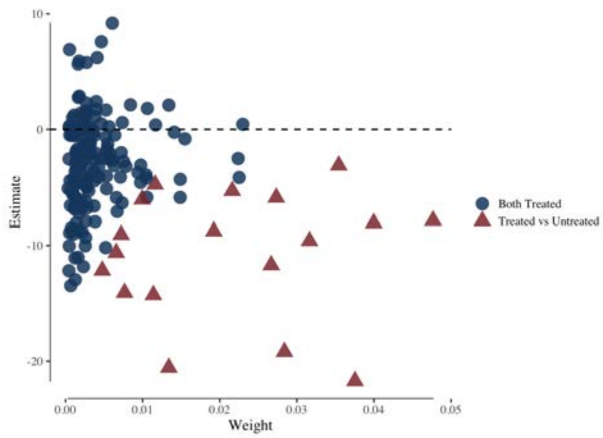
Table A.2: Difference-in-difference Estimates

	Labor Variables (Per. Pt.)			Expenditure Variables (Per. Pt.)			
	Hours Worked	Employment	Businesses Open	State Level	Low Income	Middle Income	High Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Stay-Home Order	-3.954^{***}	-4.009^{***}	-4.719^{***}	-1.869^{***}	-1.964^{***}	-1.959^{***}	-1.913^{***}
	(0.394)	(0.386)	(0.359)	(0.253)	(0.370)	(0.277)	(0.266)
Business Closure	-3.103^{***}	-2.983^{***}	-3.690^{***}	-0.216	-0.771^{*}	-0.460	-0.384
	(0.411)	(0.406)	(0.383)	(0.267)	(0.411)	(0.283)	(0.271)
Observations	2,346	2,346	2,346	2,346	2,254	2,346	2,300

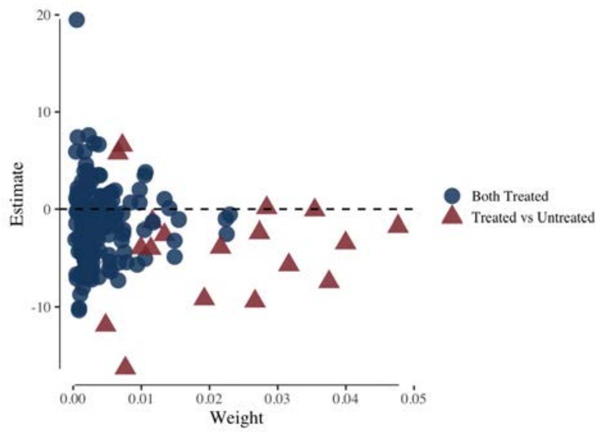
Note: This table presents difference-in-difference estimates for stay-at-home orders and non-essential business closures. Coefficients are jointly estimated in the same regression. Column names represent the dependent variable. Robust standard errors are reported. All observations are at the state-by-day level. Labor market and expenditure variables come from Homebase and Affinity Solutions, respectively.
*p<0.1; **p<0.05; ***p<0.01

Figure A.4: Goodman-Bacon (2018) Decomposition

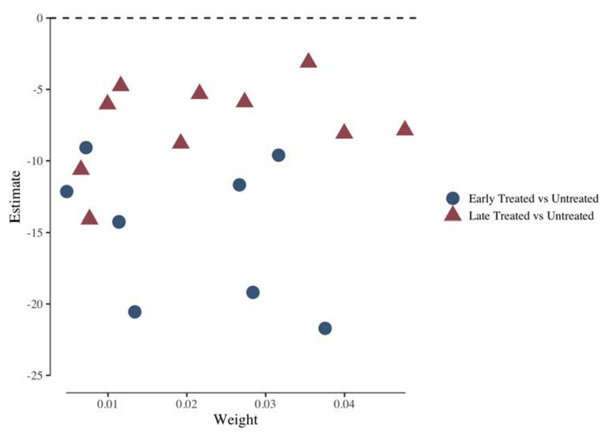
(a) Hours Worked (All 2x2)



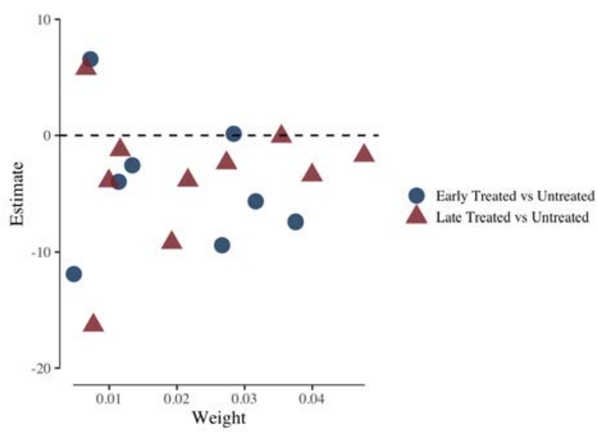
(b) Expenditure (All 2x2)



(c) Hours Worked (Early vs Late)



(d) Expenditure (Early vs Late)



Note: This figure plots the results of a Goodman-Bacon (2018) decomposition on our difference-in-difference estimate for hours worked in Panel A and expenditure in Panel B. The dependent variable is hours worked. The y-axis is the treatment-timing cohort DD estimate. The x-axis is the weight for the respective estimates. The top panel plots all 2x2 difference-in-difference coefficients. Estimates are divided into two groups: Both Treated (blue circles) and Treated vs Untreated (red triangles). In the bottom panel, only the treated vs untreated DD estimates are plotted. Estimates are further divided into two groups: Treatment occured before March 29th (Early, blue circles), Treatment occured on or after March 29th (Late, red triangles). Hours worked and expenditure data is obtained from Homebase and Affinity Solutions, respectively. Observations are at the state-by-day level.

A.1.3 Additional Event Study Estimates

Table A.3: Effect of Stay-at-Home Orders on the Economy (Composition Effects)

	Hours Worked (Homebase, State, Per. Pt.)			Total Expenditure (Affinity Solutions, State, Per. Pt.)				
Days	Drop California (1)	Drop New York (2)	Pop. Weighted (3)	Drop California (4)	Drop New York (5)	Pop. Weighted (6)	Drop Alaska/Vermont (7)	Drop Montana (8)
-7	-1.17	-0.82	-0.57	-0.92	-0.92	-0.63	-0.91	-0.9
(t-value)	(-1.1)	(-0.77)	(-0.64)	(-1.43)	(-1.43)	(-1.09)	(-1.53)	(-1.44)
-2	0.89	0.9	0.91	-0.34	-0.34	-0.45	-0.26	-0.27
	(1.09)	(1.07)	(1.03)	(-0.63)	(-0.63)	(-0.83)	(-0.48)	(-0.52)
0	-2.61*	-2.29*	-2.06*	-0.37	-0.37	-0.23	-0.3	-0.34
	(-3.25)	(-2.84)	(-2.55)	(-0.66)	(-0.66)	(-0.41)	(-0.53)	(-0.63)
5	-2.88*	-2.84*	-2.59*	-2.98*	-2.98*	-2.25*	-2.78*	-2.8*
	(-3.43)	(-3.37)	(-3.28)	(-5.86)	(-5.86)	(-4.14)	(-5.29)	(-5.53)
10	-3.59*	-3.39*	-3.23*	-4.38*	-4.38*	-3.25*	-4.17*	-4.18*
	(-3.93)	(-3.69)	(-3.83)	(-7.99)	(-7.99)	(-5.75)	(-7.94)	(-7.95)
15	-6.31*	-6.09*	-5.36*	-4.03*	-4.03*	-2.83*	-4*	-3.87*
	(-7.25)	(-6.97)	(-5.79)	(-5.28)	(-5.28)	(-3.65)	(-5.28)	(-5.21)
Observations	2,300	2,300	2,346	2,300	2,300	2,346	2,254	2,300

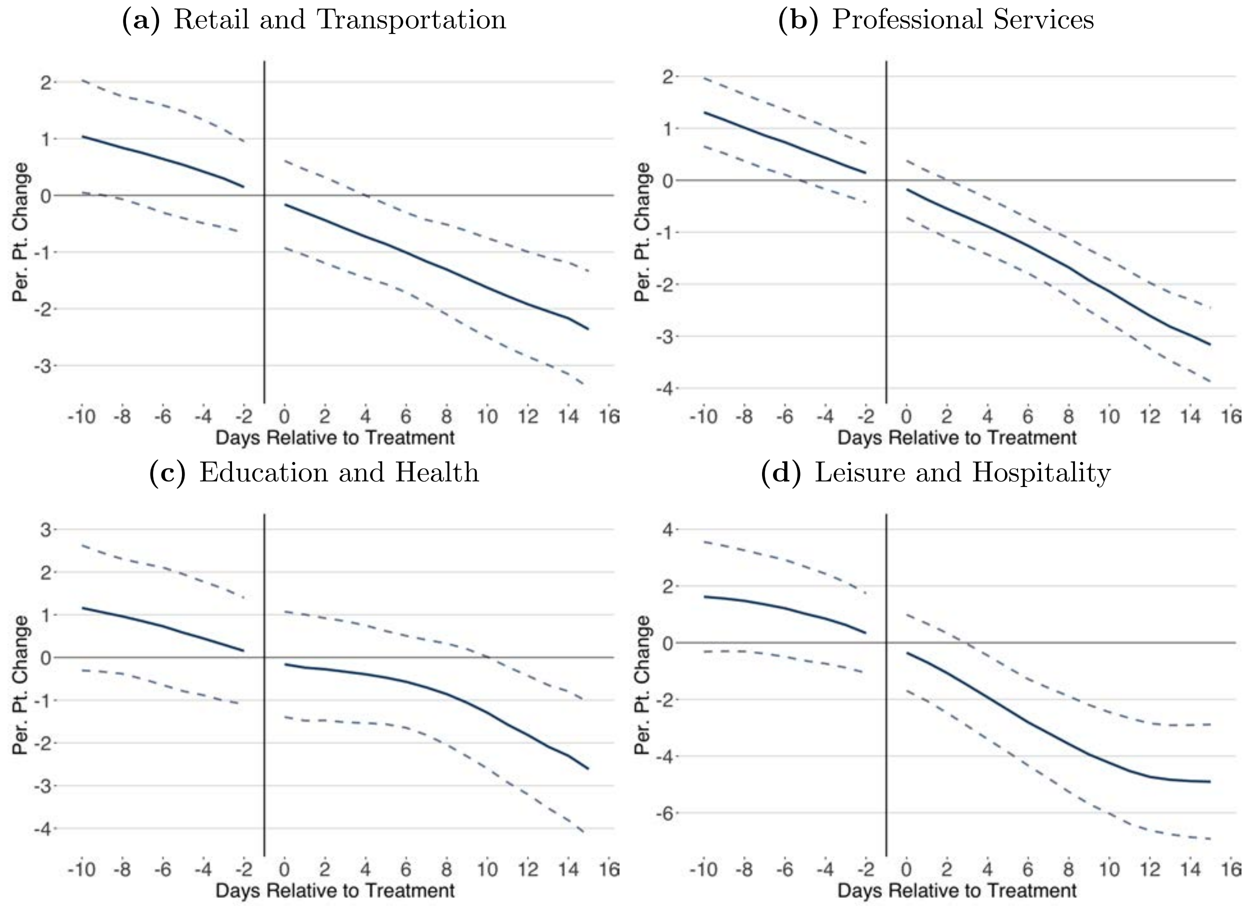
Note: This table presents estimates of equation 1 for several specifications. All estimates are conducted at the state level. T-values are presented using robust standard errors for state estimates. Hours worked and total expenditure variables come from Homebase and Affinity Solutions, respectively. Specifications (1) and (4) drop California from the sample. Specifications (2) and (5) drop New York from the sample. Specifications (3) and (6) weight regressions by state population. Specification (7) shows that composition effects are not driving our results in Column (6) of Table 2. Similarly, specification (8) shows that Column (8) of Table 2 is not driven by composition effects.
* denotes estimates significant at the 95% level.

Table A.4: Opportunity Insights Employment Measure (State vs County)

	State Employment (Per. Pt.)				County Employment (Per. Pt.)			
	Total	Low	Middle	High	Total	Low	Middle	High
Days	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
-7	0.71	0.23	0.92	0.68	0.37	-0.04	0.73*	0.44
(t-value)	(1.55)	(0.33)	(1.83)	(1.8)	(0.94)	(-0.06)	(2.27)	(1.36)
-2	0.15	0.09	0.19	0.12	0.1	0.04	0.15*	0.08
	(0.39)	(0.15)	(0.44)	(0.37)	(1.39)	(0.38)	(2.69)	(1.59)
0	-0.17	-0.13	-0.21	-0.13	-0.12	-0.06	-0.18*	-0.09
	(-0.46)	(-0.22)	(-0.49)	(-0.44)	(-1.73)	(-0.49)	(-3.61)	(-1.95)
5	-1.02*	-0.74	-1.29*	-0.76*	-0.95*	-0.54	-1.32*	-0.76*
	(-2.82)	(-1.35)	(-3.05)	(-2.61)	(-2.17)	(-0.75)	(-4.52)	(-3.09)
10	-2.01*	-1.41*	-2.62*	-1.69*	-2.28*	-1.46	-3.00*	-1.82*
	(-4.38)	(-2.00)	(-5.18)	(-4.89)	(-2.75)	(-1.07)	(-5.15)	(-4.31)
15	-2.67*	-1.56	-3.67*	-2.75*	-3.76*	-2.3	-4.99*	-3.16*
	(-5.05)	(-1.88)	(-6.46)	(-6.29)	(-3.12)	(-1.15)	(-5.65)	(-5.08)
Observations	2,346	2,300	2,300	2,346	34,937	23,912	26,558	16,415

Note: This table presents estimates of equation 1 for the Opportunity Insights employment data. Specifications (1)-(4) are estimated at the state level, and columns (5)-(8) at the county level. T-values are presented using robust standard errors for state estimates and standard errors clustered at the state-level for county estimates. Low-income workers make $< \$27K$ per year. Middle-income workers make $\$27K - \$60K$. High-income workers make $> \$60K$ per year.
* denotes estimates significant at the 95% level.

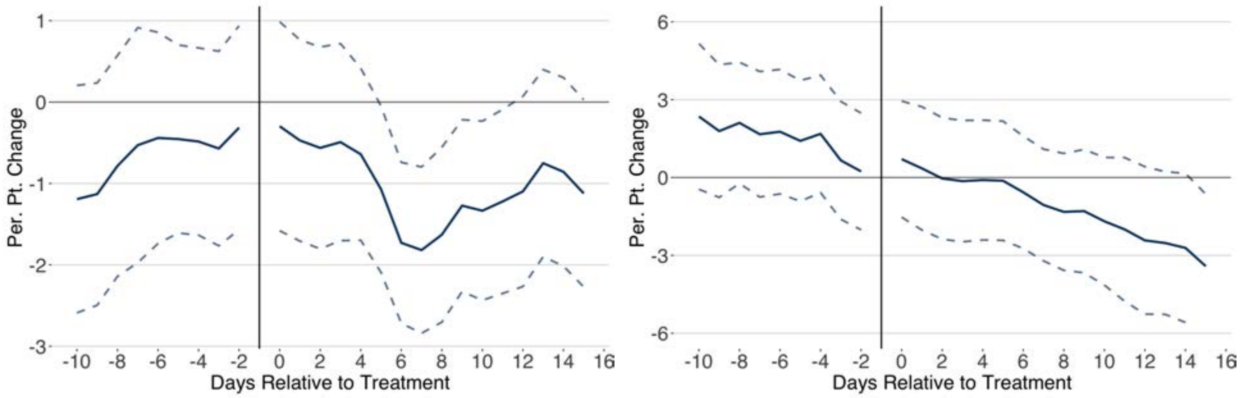
Figure A.5: Opportunity Insights Employment Measure by NAICS Sector



Note: This figure plots the results of our event study presented in Equation 1. Employment data is obtained from Opportunity Insights. Observations are at the state level. Dashed lines represent 95% confidence intervals using robust standard errors. Retail and Transportation is NAICS supersector 40, Professional Services (60), Education and Health (65), Leisure and Hospitality (70).

Figure A.6: Heterogeneity across Consumption Expenditures (Extra)

(a) Transportation and Warehousing **(b)** Health Care Services



Note: This figure plots the results of our event study presented in Equation 1. Expenditure data come from Affinity Solutions. Merchant groups are defined by Affinity Solutions. Observations are at the state level. Dashed lines represent 95% confidence intervals using robust standard errors.

Table A.5: Effect of Non-essential Business Closures on the Economy

	Labor Variables (Per. Pt.)			Expenditure Variables (Per. Pt.)				
Days	Hours Worked (1)	Employment (2)	Businesses Open (3)	State Level (4)	County Level (5)	Low Income (6)	Middle Income (7)	High Income (8)
-7	4.12*	3.45*	3.74*	-0.25	-0.26	-1.71	-0.22	-0.31
(t-value)	(3.23)	(2.94)	(3.28)	(-0.34)	(-0.37)	(-1.56)	(-0.27)	(-0.41)
-2	0.96	0.71	1.11	-0.02	-0.06	-0.31	-0.03	-0.2
	(0.94)	(0.79)	(1.22)	(-0.04)	(-0.29)	(-0.3)	(-0.04)	(-0.26)
0	-0.44	-0.27	-0.85	-0.12	0.06	-0.31	-0.08	-0.42
	(-0.48)	(-0.33)	(-0.96)	(-0.2)	(0.4)	(-0.3)	(-0.11)	(-0.63)
5	-0.82	-0.76	-1.6	-1.73*	-1.42*	-2.54*	-1.93*	-1.93*
	(-0.84)	(-0.89)	(-1.76)	(-3)	(-2.08)	(-2.75)	(-2.92)	(-2.72)
10	-0.88	-0.58	-1.74	-1.1	-1.51	-1.95	-1.16	-1.99*
	(-0.89)	(-0.65)	(-1.92)	(-1.83)	(-1.49)	(-1.91)	(-1.84)	(-2.59)
15	0.33	0.14	-1.78	0.06	0.01	-1.37	-0.11	-0.64
	(0.35)	(0.16)	(-1.97)	(0.08)	(0.01)	(-1.13)	(-0.16)	(-0.88)
Observations	2,346	2,346	2,346	2,346	73,451	2,254	2,346	2,300

Note: This table presents estimates of equation 1 for several specifications with non-essential business closures as the covariate of interest. All estimates are conducted at the state level except column (5) which provides county level estimates. T-values are presented using robust standard errors for state estimates and standard errors clustered at the state-level for county estimates. Labor and consumption variables come from Homebase and Affinity Solutions, respectively.
* denotes estimates significant at the 95% level.

A.2 Model

A.2.1 Equilibrium Conditions

Our model can be summarized by the following first-order conditions:

Susceptible

$$\frac{1}{c_t^s} - (1+\mu_{ct})\lambda_{bt}^s + \lambda_{\pi_{It}}\pi_{s1}(I_t C_t) = 0 \tag{18}$$

$$-\theta\phi^s n_t^s + A\lambda_{bt}^s + \lambda_{\pi_{It}}\pi_{s2}(I_t N_t^I) = 0 \tag{19}$$

$$\beta(U_{t+1}^i - U_{t+1}^s) - \lambda_{\pi_{It}} = 0 \tag{20}$$

Infected

$$\frac{1}{c_t^i} - (1+\mu_{ct})\lambda_{bt}^i = 0 \tag{21}$$

$$-\theta\phi^i n_t^i + A\lambda_{bt}^i = 0 \tag{22}$$

Recovered

$$\frac{1}{c_t^r} - (1+\mu_{ct})\lambda_{bt}^r = 0 \tag{23}$$

$$-\theta\phi^r n_t^r + A\lambda_{bt}^r = 0 \tag{24}$$

Market Clearing

$$S_t c_t^s + I_t c_t^i + R_t c_t^r = C_t \tag{25}$$

$$S_t n_t^s + I_t n_t^i + R_t n_t^r = N_t \tag{26}$$

A.2.2 Parameters chosen to Optimize Model

In Section 5.2, we optimize $(\tau_{H,i}^d, \pi_{s1}, \pi_{s2}, \pi_{s3})$ to best fit the data. We chose these parameters as they summarize the endogenous labor responses by susceptible agents in the model without policy intervention. Recall that the probability of being infected is

$$\pi_{I,t} = \pi_{s1}(C_t^S)(I_t C_t^I) + \pi_{s2}(N_t^S)(I_t N_t^I) + \pi_{s3}\sum_{i=1}^{N}(1-\tau_{H,i}^d)(I_{i,t}Pop_{i,0}) \tag{27}$$

Optimizing the parameters $(\tau_{H,i}^d, \pi_{s1}, \pi_{s2}, \pi_{s3})$ directly controls the probability of being infected. Furthermore, the equilibrium conditions show that the lagrange multiplier for this constraint is

$$\lambda_{\pi_{It}} = \beta(U_{t+1}^i - U_{t+1}^s) \tag{28}$$

In the steady-state, this can be re-written as

$$\lambda_{\pi_I} = \beta(U^i_{ss} - U^s_{ss}) \tag{29}$$

where

$$U^s_{ss} = \frac{1}{1-\beta} u^s_{ss} \tag{30}$$

$$U^i_{ss} = \frac{1}{1-\beta(1-\pi_r-\pi_d)}(u^i_{ss} + \beta\pi_r u^r_{ss}) \tag{31}$$

However, in the steady-state, $u^s_{ss} = u^r_{ss}$. Substituting and simplifying, Equation 29 can be re-written as

$$\lambda_{\pi_I} = \left(\frac{\beta}{1-\beta(1-\pi_r-\pi_d)} u^i_{ss} + \frac{\beta^2\pi_r}{1-\beta(1-\pi_r-\pi_d)} u^s_{ss}\right) - u^s_{ss} \tag{32}$$

$$\lambda_{\pi_I} = (u^i_{ss} - u^s_{ss}) + c \tag{33}$$

where c is a constant that depends on the discount factor and the probabilities of recovering and dying. Substituting for steady-state utility, we obtain

$$\lambda_{\pi_I} = [(\log(c^i_{ss}) - \frac{\theta}{2}(n^i_{ss})^2) - (\log(c^s_{ss}) - \frac{\theta}{2}(n^s_{ss})^2)] + c \tag{34}$$

$$\lambda_{\pi_I} = \log(c^i_{ss}) - \log(c^s_{ss}) + c \tag{35}$$

$$\lambda_{\pi_I} = \log\left(A\phi^i \left(\frac{1}{\theta}\right)^{1/2}\right) - \log\left(A \left(\frac{1}{\theta}\right)^{1/2}\right) + c \tag{36}$$

$$\lambda_{\pi_I} = \log(\phi^i) + c \tag{37}$$

Thus, the endogenous response by susceptible agents to decrease their probability of infection can be summarized by the loss in labor productivity and c. One could potentially treat ϕ^i and β, which affects c, as free parameters. However, the other components of c, π_r and π_d are set to match their empirical counterparts and should not be treated as free parameters.

A.2.3 Extended Model Calibration Procedure

Section 5.2.1 provided a procedure to calibrate our model in applied settings. This procedure focused solely on matching the time series of susceptibles in the model to that in the data. We extend this procedure to match both the time series of susceptibles and consumption in the data. The following steps are followed to generate simulations from the model:

1. Set Number of Locations (N)

2. Set Initial Population of Locations (Pop_0)

3. Set Susceptible Percent of Population (S_t)

4. Set New Infections (T_t)

5. Set Active Infections (I_t)

6. Set Parameters ($\mu_{ct}, \tau^d_{H,i}, \pi_{s1}, \pi_{s2}, \pi_{s3}$)

The first five steps are identical to the procedure in Section 5.2.1. Step six now includes the consumption tax, μ_{ct}, in the calibration procedure. This increases the number of calibrated parameters by $N * T$, where N is the number of locations and T is the number of time periods in the application. Due to the increase of parameters, we use the following four step procedure for calibration:

Step One Jointly set $(\tau^d_{H,i}, \pi_{s1}, \pi_{s2}, \pi_{s3})$ to match the empirical time series of susceptibles.

$$\min_{\tau^d_{H,i}, \pi_{s1}, \pi_{s2}, \pi_{s3}} \sum_{i=1}^{4} \sum_{t=May6}^{Aug26} \left(S^{Data}_{i,t} - S^{Model}_{i,t} \right)^2 \tag{38}$$

Step Two Jointly set μ_{ct} to match the empirical time series of consumption holding the calibrated transmission parameters fixed $(\tau^d_{H,i}, \pi_{s1}, \pi_{s2}, \pi_{s3})$ fixed.

$$\min_{\mu_{ct}} \sum_{i=1}^{4} \sum_{t=May6}^{Aug26} \left(C^{Data}_{i,t} - C^{Model}_{i,t} \right)^2 \tag{39}$$

Step Three Repeat Step One holding the calibrated consumption taxes, μ_{ct}, fixed.

Step Four Jointly set $(\mu_{ct}, \tau^d_{H,i}, \pi_{s1}, \pi_{s2}, \pi_{s3})$ to match both the inverse-variance weighted time series of susceptibles and consumption.

$$\min_{\mu_{ct}, \tau^d_{H,i}, \pi_{s1}, \pi_{s2}, \pi_{s3}} \frac{\sum_{i=1}^{4} \sum_{t=May6}^{Aug26} \left(S^{Data}_{i,t} - S^{Model}_{i,t} \right)^2}{var\left(S^{Data}_{i,t} \right)} + \frac{\sum_{i=1}^{4} \sum_{t=May6}^{Aug26} \left(C^{Data}_{i,t} - C^{Model}_{i,t} \right)^2}{var\left(C^{Data}_{i,t} \right)} \tag{40}$$

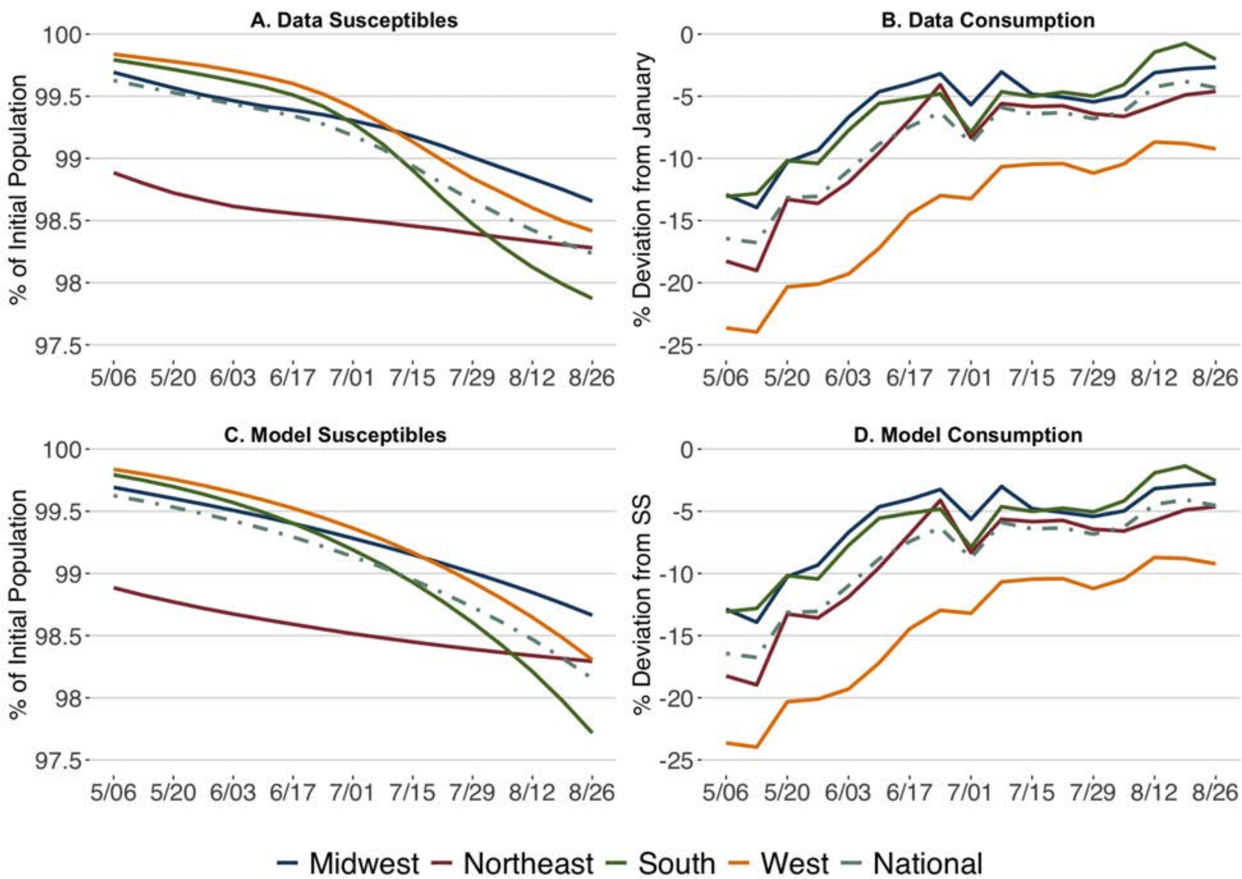
subject to

$$(\mu_{ct}, \tau^d_{H,i}, \pi_{s1}, \pi_{s2}, \pi_{s3}) \geq 0.9 * (\mu_{ct}, \tau^d_{H,i}, \pi_{s1}, \pi_{s2}, \pi_{s3}) \tag{41}$$

$$(\mu_{ct}, \tau^d_{H,i}, \pi_{s1}, \pi_{s2}, \pi_{s3}) \leq 1.1 * (\mu_{ct}, \tau^d_{H,i}, \pi_{s1}, \pi_{s2}, \pi_{s3}) \tag{42}$$

As in Section 5.2.1, we impose the restrictions on travel cost: $\tau^d_{H,i} \in [0, 1]$ and $\tau^d_{H,i} = 0$ for $H = i$ and that the virus transmission parameters are positive $\pi_{s1}, \pi_{s2}, \pi_{s3} \geq 0$ for all steps in the calibration procedure. Figure A.7 plots both the time series for susceptibles and consumption in the data and the model after following this extended procedure. Overall, the results are similar to those in Figure 10 with the addition of matching the time series of consumption in the data.

Figure A.7: Extended US Census Region Application



Note: Panel A plots the observed time series of susceptibles in the United States and each Census region from May 6 through August 26. Panel B the observed consumption paths. Panels C and D plot the equivalent model fits for susceptibles and consumption, respectively, after jointly optimizing the consumption taxes, the travel distance across locations, and the transmission parameters ($\mu_{ct}, \tau^d_{H,i}, \pi_{s1}, \pi_{s2}, \pi_{s3}$). Infection data is obtained from *USA Facts*. Consumption data is obtained from Affinity Solutions.

A.2.4 Additional Comparative Statics

Figure A.8 plots the results of a three-location SIR model under different parameterizations. Columns represent different initial conditions. The first column uses initial populations of $\frac{1}{3}$ and initial infections of 0.1% for all states. The second column uses the initial population across states, $Pop_0 = (36.6\%, 56.5\%, 6.9\%)$, while maintaining the same initial infections. The third column randomizes the initial infections of locations, $T_0 = (0.16\%, 0.05\%, 0.16\%)$, using the randomized populations from the second column.[42] Rows represent the travel cost used. We require that aggregate initial population and infections are equal 1 and 0.1%, respectively, for all simulations. Thus, differences in the aggregate must be attributed to the cross-sectional interactions rather than initial aggregate conditions. All other parameters

[42] These population and infection rates were determined using a random draw. The random draws are then scaled to maintain the same initial aggregate conditions as our baseline simulation. The actual dynamics will depend on the specific random draw, but will not change the qualitative comparative static results.

remain the same as in Table 4.

No Transmission across Locations The first row plots the results using no interaction across locations, $\tau^d_{H,i} = 1$ for $H \neq i$. Panel (a) shows that, with symmetric locations and no transmission across locations, all locations have the same time series for the fraction of susceptible agents. Panel (b) shows that virus transmission within a state is increasing in its population. The intuition is that individuals are more likely to catch the virus from a larger number of people in more populous states. Panel (c) shows that different initial infection conditions can create crossing patterns in the cross-section. For these particular randomized population and initial infection draws, we see that State 2 takes about twenty and forty weeks to surpass States 3 and 1, respectively, in cumulative infections. This differs from Panel (b) where states with higher population always experience higher cumulative infections than lower populated states.

Partial Transmission The second row simulates the same scenarios using $\tau^d_{H,i} = \frac{|H-i|}{N}$. Comparing Panels (1a) and (2a) show that asymmetric travel costs across locations can lead to cross-sectional heterogeneity of outcomes even when initial population and infections are symmetric. Panel (2b) shows that, as in Panel (1b), initial population affects both the cross-sectional and aggregate time series. Panel (2c) shows that allowing for virus transmission across locations mitigates, but does not eliminate, the effect that initial infections has on the cross-sectional crossing patterns.

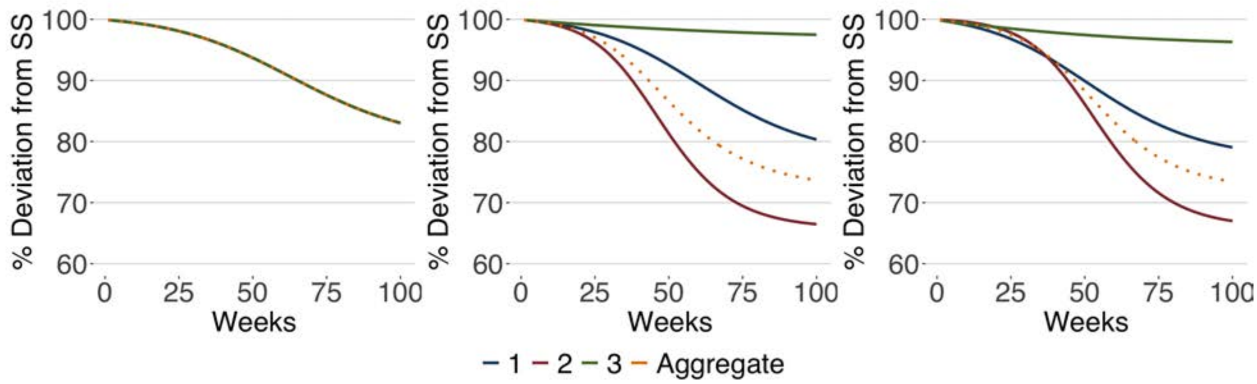
Full Transmission The third row plots the results using no travel cost across locations, $\tau^d_{H,i} = 0$. Panel (3a) shows that when there is full transmission of the virus across locations that our model replicates Eichenbaum et al. (2020) when locations are symmetric. This is also the case when locations have different populations in Panel (3b). Our transmission mechanism in Equation 3 shows that idiosyncratic initial infection rates are needed to generate a departure from Eichenbaum et al. (2020) when travel cost is zero. However, Panel (3c) shows that the consequences of idiosyncratic infection rates are small when there is complete virus pass-through across locations even with asymmetric population sizes.

Overall, Panels (1a)-(1c) show that both initial population and initial infection rates can have significant consequences for the dynamics of our model. Comparing these panels to their counterparts in Panels (2) and (3) show that the extent of these consequences depends heavily on the modelling of virus transmission across locations. In the limiting case of no travel cost across locations, our model converges to the single-location model of Eichenbaum et al. (2020) regardless of initial population and infection symmetry. Figure A.8 also shows that cross-sectional assumptions have important implications for aggregate dynamics. In the most severe cases, over 50% of the total population becomes infected after 100 weeks as seen in Panel 3 with full transmission across locations. However, Panel (1a) suggests that less than 20% of the nation becomes infected when there is no transmission across locations. Our results suggest that our model is not only important for modelling cross-sectional behavior, but also aggregate dynamics even when holding aggregate initial conditions fixed.

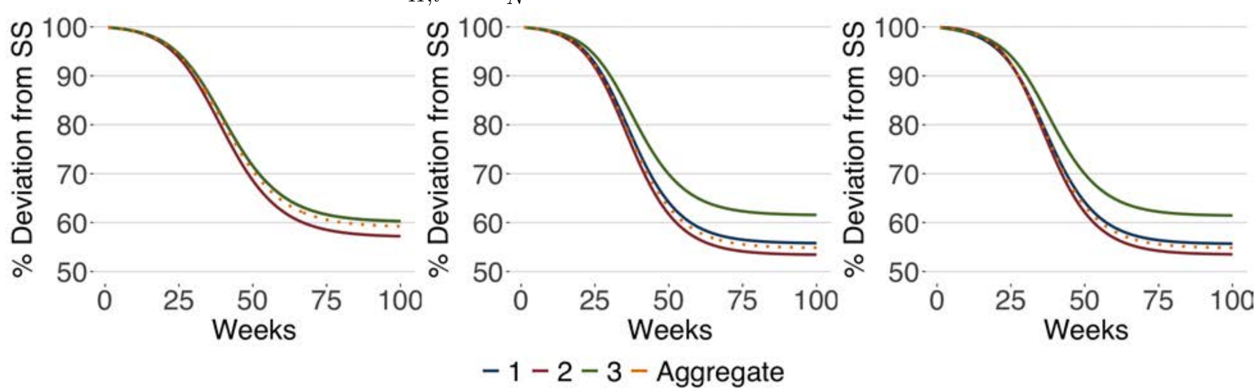
Figure A.8: Comparative Statics (Susceptibles)

1. No Transmission, $\tau^d_{H,i} = 1$ for $H \neq i$

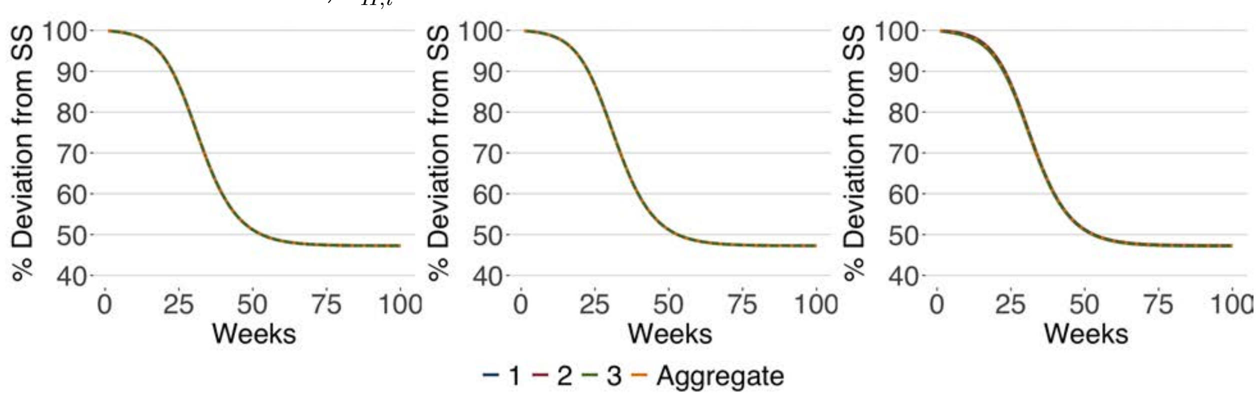
(a) Pop = (33.3%, 33.3%, 33.3%) Infections = (0.1%, 0.1%, 0.1%) **(b)** Pop = (36.6%, 56.5%, 6.9%) Infections = (0.1%, 0.1%, 0.1%) **(c)** Pop = (36.6%, 56.5%, 6.9%) Infections = (0.16%, 0.05%, 0.16%)



2. Partial Transmission, $\tau^d_{H,i} = \frac{|H-i|}{N}$



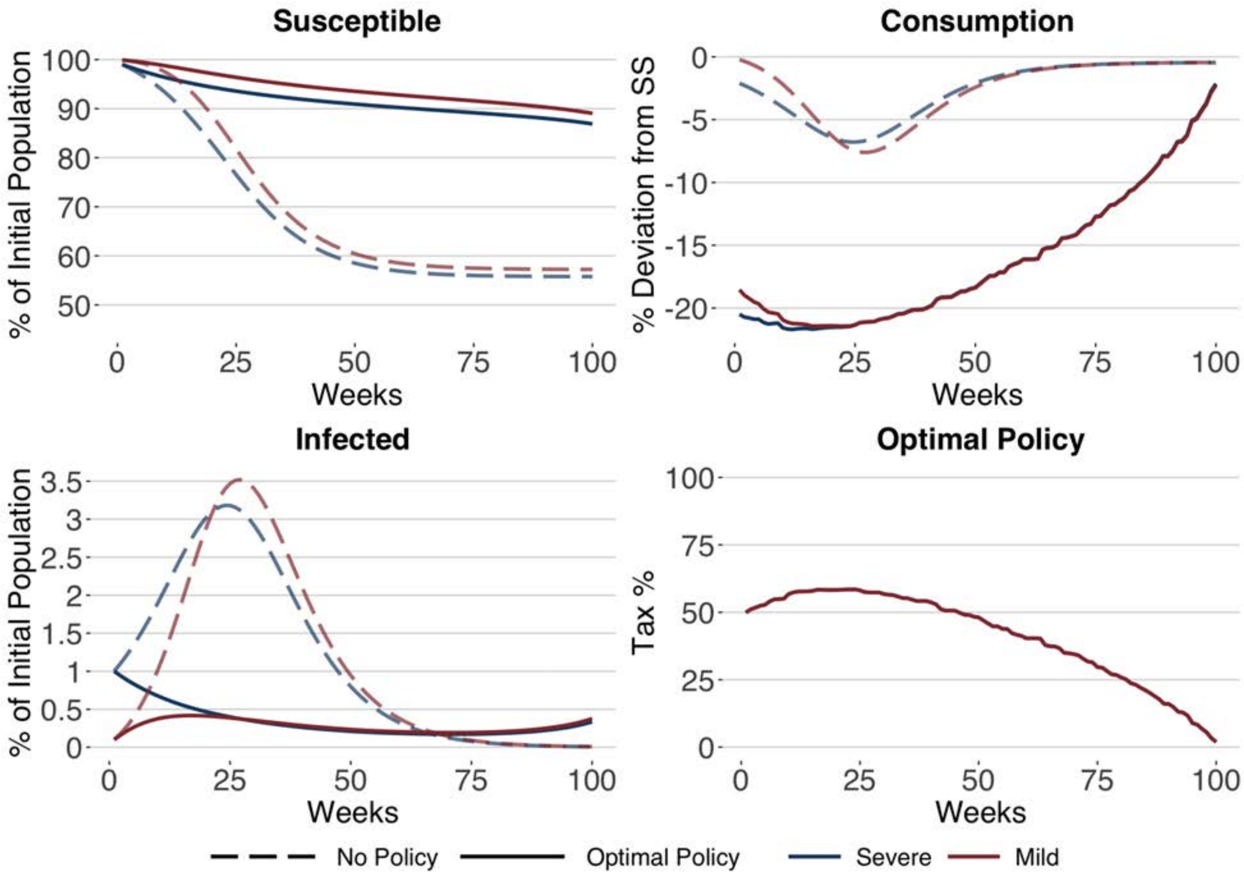
3. Full Transmission, $\tau^d_{H,i} = 0$



Note: This figure plots the path of susceptibles using a 3-state SIR macro model. In all specifications, aggregate initial infections are equal to 0.001. Columns represent different initial conditions. The first column presents results when locations are symmetric. The second column randomizes the population of locations. The third column randomizes the initial infections of locations using the populations from the second column. Rows represent the travel cost used. The first row plots the results using no interaction across locations. The second row uses $\tau^d_{H,i} = \frac{|H-i|}{N}$. The third row plots the results using no travel cost across locations.

A.2.5 National Mitigation Policy

Figure A.9: Optimal National Policy



Note: This figure plots the results of a 2-location SIR macro model restricted to a common, national policy. Both locations have an initial population of $\frac{1}{2}$. Initial infections to 1% and 0.1% for the severe (blue) and mild (red) infected locations, respectively.