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HEALTH VS. WEALTH? PUBLIC HEALTH POLICIES AND THE ECONOMY DURING COVID-19

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ABSTRACT

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Abstract: We study the impact of non-pharmaceutical policy interventions (NPIs) like "stay-at-home" orders on the spread of infectious disease. NPIs are associated with slower growth of Covid-19 cases. NPIs "spillover" into other jurisdictions. NPIs are not associated with significantly worse economic outcomes measured by job losses. Job losses have been no higher in US states that implemented "stay-at-home" during the Covid-19 pandemic than in states that did not have "stay-at-home". All of these results demonstrate that the Covid-19 pandemic is a common economic and public health shock. The tradeoff between the economy and public health today depends strongly on what is happening elsewhere. This underscores the importance of coordinated economic and public health responses.

1. Introduction

We study the health and economic impacts of non-pharmaceutical public health interventions (NPIs) to mitigate the spread of Covid-19. Since emerging in December 2019, Covid-19 has spread to nearly all countries in the world. Every state and territory in the USA has reported at least one case to date. Theoretical and empirical literature in epidemiology and public health has argued that NPIs can be important in decreasing peak mortality and cumulative mortality.[1,2,3,4] Countries, states, and cities recently imposed a

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number of NPIs to enhance social distancing with the aim of mitigating the spread of Covid-19. Have these had benefits for public health but at the cost of the economy?

The economic consequences of public health policies during global pandemics is challenging. Global pandemics are rare events.[5,6] New insights combining economic and epidemiological modeling is emerging with new theoretical predictions. The key tradeoff is between public health and the economy.[7] Aggressive NPIs benefit public health and help manage the pandemic with limited medical capacity. NPIs may however damage the economy and create high levels of unemployment. But, even without policy, people pay attention to news and events elsewhere reacting with spontaneous social distancing.[8,9,10] There may also be important economic spillovers to NPIs.[11]

A pandemic can impact an economy in many ways: reductions in people's willingness to work, dislocations in consumption patterns and lower consumption, added stress on the financial system, and greater uncertainty leading to lower investment. These are respectively referred to as (labor) supply shocks, demand shocks, financial shocks and uncertainty shocks. Connected economies and epidemiological communities also move in synch. Even a healthy economy, or an economy that has not mandated a shutdown, may feel the impact of external events. With the exception of the 1918 influenza, recent pandemics have neither had as large of a global impact, nor has there been as much real time data available to empirically assess the economic and public health impact of NPIs. We study outcomes during the Covid-19 pandemic.

We have three main results. First, our analysis shows NPIs may have been effective in slowing the growth rate of confirmed cases of Covid-19 but not in decreasing the growth rate of cumulative mortality. Second, we find evidence of spillovers. NPIs may have impacts on other jurisdictions. Finally, there is little evidence that NPIs are associated with larger declines in local economic activity than in places without NPIs.

The reason we fail to find evidence consistent with a macro-health/economy tradeoff is that epidemiological and economic shocks have been common to the US and indeed to the world. Our results parallel those of a recent contribution which shows that US cities that applied more intensive NPIs in 1918-19 did not suffer greater economic mis-fortune than other cities without such policies.[12] Moreover, economic policies may have un-even impacts on certain economic sectors and types of jobs. We find states with a larger share of employment in jobs that can be done at home have lost fewer jobs after stay-at-home.

We also address the issue of spillovers in NPI policy and public health: do local policies have effects on other jurisdictions and territories? We find they do, at least within the United States. This is not true across borders. In light of this, delaying implementation of NPIs may have little extra economic benefit when significant trade partners have already implemented such policies and when information and disease travels rapidly. This new evidence can account for the lack of a tradeoff between health and the economy.

A relevant comparison to the Covid-19 pandemic is the 1918 influenza pandemic. A significant strand of the literature has developed unique data from this historical pandemic in the United States. In 1918 and 1919, NPIs significantly lowered peak mortality rates. Some weaker evidence shows that these may have reduced total cumulative mortality in US cities.[13] The recent Covid-19 pandemic and associated implementation of NPIs allows us to gauge whether such policies have been effective for public health and if there are economic costs to these policies.

2. Methods

2.1 Data collection

For public health data in US states, we rely on confirmed cases and deaths of Covid-19 reported by the *New York Times* on a daily basis. These data are based on reports from state and local health agencies. Confirmed cases and deaths across countries are from the Center for Systems Science and Engineering (CSSE) at Johns Hopkins University representing a compilation of data reported by the WHO and various countries' public health authorities. We use country and US state-level data beginning in January 2020 up to April 2020. We have data for over 70 countries and 50 US States + the District of Columbia.

Data on NPIs at the country level come from the Oxford Covid-19 Government Response Tracker.[14] These data cover seven policy responses: School closures, workplace closures, cancellation of public events, closure of public transport, public information campaigns, restrictions on internal movement, and international travel bans. This source reports data from over 100 countries. Data on "stay-at-home" orders for US states is from the official orders or announcements made by public health authorities at each state.

Real-time data that helps understand the macro economy is relatively scarce and has only become available in recent decades. Recent research uses real time data from private financial (fin-tech) companies to track consumer spending as well as movement based on privately collected GPS signals from mobile phones. Such data is subject to measurement error, reports for limited and small samples, and cannot be considered as fully indicative of the macroeconomic situation.[15]

We use initial claims for unemployment insurance published by the US Department of Labor (i.e., initial jobless claims) at the state level on a weekly basis. Each state's data are as of the end of the week (i.e., Saturday). We use data which are not seasonally adjusted and which are subject to revision. Initial jobless claims represent a consistent and reliable indicator of the US labor market at the local level, are of reasonable quality, and are often used as a leading indicator for macroeconomic forecasts. These data exclude the self-employed. We also supplement the economic data with information on the employment shares in selected industries we believe may be hardest hit in the recent months such as oil and gas extraction, retail, food processing/restaurants, wholesale and arts, recreation and leisure. We also use information on the share of jobs in a state that can be carried out by telecommuting.[16]

2.2 Data Analysis

Our main dependent variables are the daily growth rates of the (natural) logarithm of cumulative confirmed cases or deaths of Covid-19. We acknowledge considerable debate about measurement error due to variable testing rates across localities. Potential for measurement error also exists for the mortality data. There have been cases of deaths at home from those not admitted to nor tested in hospitals. Using excess mortality is an option but systematic data is not readily available nor directly comparable.

We also use the logarithm of initial jobless claims at the state-level as a dependent variable. Data are not seasonally adjusted since such adjustments apply to all cross-sectional units (i.e., states) and are captured in period/day intercepts. Initial jobless claims are subject to revision. Our data end with information on the week ending 4 April. The latest revisions apply to weeks before and including the week ending 28 March, 2020.

Country-level NPIs are reported on a scale of 0/1/2. A value of 0 is for "no measure in place". A value of 1 indicates the NPI is recommended, and a value of 2 is the most stringent. We re-code data to take the values of 0 and 1. Here 0 represents both 0 and 1 in the raw data, and 1 is a raw value of 2 the most stringent NPI possible.

State-level NPIs are for so-called "stay-at-home orders". Such rules vary in their particular prescriptions. They typically mandate that people refrain from meeting in groups, limit physical social interaction to within households, and that people frequent only essential businesses. In person work is allowed only for "essential" businesses.

Throughout our paper, we assume that NPIs and their timing are exogenous and uncorrelated with unobservables especially expectations about the future path of mortality and the expected path of economic and social variables of interest. We also allow for leads of NPIs to deal with the issue of reverse causality from mortality to NPIs.

We allow for policy spillovers by measuring the level of policies in all other states. In our international sample, we look at policies of other countries that share a border. Each policy in another state (or country) is divided by the centroid-to-centroid distance. For robustness we also population weighted each other state's distance weighted policy. States with closer proximity to the observation have a bigger potential spillover since we assume economic and social interactions are roughly linear in the log of physical distance with an elasticity of -1. The measure for state i of all other states' NPI policies is $S_{i,-i} = \sum_{j \neq i} \frac{1(\text{Stay-at-Home}_j = 1)}{\text{distance}_{ij}}$. We also introduce the sum of policies in the states which share a border with state i, $S'_{i,-i} = \sum_n 1(\text{Stay-at-Home}_n = 1)$ where n indexes states in the set N of i's neighboring states. Similarly, we can control for the confirmed cases of other states with distance weighting and in neighboring states. For countries we focus on policies only in bordering countries.

In all models we include controls for calendar weeks, state-level fixed effects and event-time trends (linear, quadratic and cubic terms were tested). The event is defined either as the number of days elapsed between the current date and the date a state reached the first death or first confirmed case of Covid-19. We also cluster standard errors of estimated coefficients at the state level.

2. Results

3.1 Policies and Public Health

As of this draft, there were over 2.4 million confirmed cases of Covid-19 worldwide. The United States (765,000), Spain (200,000), Italy (178,972), France (152,000) and Germany (145,000). Reported deaths stood at over 164,000 making this pandemic one of the worst in the last 120 years. The average growth rate of global cases since 1/22/2020

(555 cases) and 4/13/2020 (82 days) was 10.43%. Other reported statistics and information such as case fatality rates and overall infection rates are either too preliminary or mis-measured to be reliable at this stage.

On the international scene, the first countries to impose containment and mitigation strategies were in East Asia near the epicenter of the first outbreak. Mainland China imposed a near total lockdown on Hubei province from late January 2020 and severely limited domestic movement in nearly all other provinces from then until the first week of April. Singapore, South Korea, Hong Kong, and Taiwan all maintained strict international border controls, high levels of contract tracing and testing, and monitoring or closure of international borders.

Western European nations, first with Italy (March 9th), and successively other nations, implemented strict bans on public gathering and domestic and international movement. In the United States, states initiated stay-at-home orders progressively beginning on 19 March (California) through the first week of April. Iran waited 16 days after its first case to put limits on internal/domestic movement. India announced a national shelter-in-place order on 24 March, 53 days after its first official case, and this was initially intended to have a three week duration.

We first test NPIs as determinants of the growth rate of cumulative cases or death rates across countries. On the international scene, in a sample of 73 countries for which we have complete and balanced data, we find that various NPIs had a negative and statistically significant association on the growth rate of (log) confirmed cases. Table 1 column 7 shows that the ordinal sum of the six international NPIs we use could lower the growth rate by about 2 log points (-0.0207, p-value=0.007, 95% C.I. -0.03 to -0.005).

The policies most strongly and statistically significantly associated with slowing the growth rate of (log) confirmed cases in order of magnitude of impact were public transport closures (-0.09, p-value = 0.014, 95% C.I. -0.17 to -0.02), enforced workplace closures (-0.0784, p-value =0.004 , 95% C.I. -0.131 to -0.025), limited domestic travel (-0.-650, p-value = 0.060, 95% C.I. -0.132 to 0.003), and restrictions on international travel (-0.0639, p-value = 0.009, 95% C.I. -0.11 to -0.016). School closures (p-value = 0.387) and limits on public events (p-value = 0.342) are negatively related to growth rates of confirmed cases but were not found to be statistically significant.

For the international sample, five of the six NPIs as well as the cumulative sum of all NPIs are not statistically significant determinants of the growth rate of the cumulative number of deaths. The only NPI that is significant is the closure of public transportation (point estimate: -0.09, p-value = 0.042, 95% C.I. -0.177 to -0.003). In addition the sum of all policies has a negative point estimate of -0.0123 (p-value = 0.226 95% C.I. -0.03 to 0.008), but it is not significant at conventional levels. Since we are recording event time as days since the first death in this table, the sample of countries decreased to 58 from 73 in the sample for confirmed cases. The lack of significance here could be due to our short sample and long lags between implementation of NPIs and effects on death rates.

We also tested for spillovers. Are foreign NPIs associated with lower growth rates of confirmed cases and death rates? We use the total sum of an NPI indicator across countries that share a border as a control in the same regressions as above. We find little evidence of an association for the NPIs of neighboring countries. Six of the seven NPIs, and the summed value of all NPIs in the international data set, are not statistically significant determinants of own-country outcomes for cases and deaths. The only foreign NPI that is a statistically significant of growth in cases is the limitation on internal movement in neighboring countries (point estimate: -0.043, p-value =0.003 , 95% C.I. -0.068 to -0.015).

NPIs enacted by US states are negatively correlated with the growth rate of confirmed cases of Covid-19. Table 3 shows our regression results. Column 1 of Table 3 shows that a state's own policy was associated with a reduction of the growth rate of 16.9 log points (p-value = 0.000, 95% C.I. -0.20 to -0.13). Figure 1 and Figure 2 show the dynamics. We compare the change in the growth rate in log confirmed cases in each day after the first day of the policy (25 coefficients) and by five-day periods to the pre-policy growth rate. The point estimates are progressively larger in absolute magnitude over time. None of the point estimates for changes in the growth rate of deaths is statistically significant. We also checked for pre-trends and reverse causality by allowing for leads of the NPI. Point estimates of the leads were not individually statistically significant.

We continue our analysis by allowing for policy spillovers between states. Figure 3 shows the path of confirmed cases for five groups of states corresponding to their calendar time adoption of stay-at-home policies. The first group is the first set of states that implemented such a policy during the week ending 21 March, 2020.[17] The following three groups are states that rolled out their stay-at-home orders during the weeks ending 28 March, 4 April, or 11 April. The fifth group (group 0) consists of states that did not have such an order as of April 13, 2020.

Next we demonstrate graphically how NPIs in group 1 and 2 might have affected other groups by plotting changes in trend growth rates of confirmed cases. Figure 3 plots the total confirmed cases within a group against event time (event day 0 is the day of the first confirmed case). We include two trend lines. This first is the average growth rate of confirmed cases since day 0. The second trend is the average growth rate of confirmed cases prior to the week in which the first group, group 1, implemented stay-at-home. If group 1 has an impact on other groups the trend could break here.

Confirmed cases decelerated following the week in which group 1 acted (groups 0, 2, and 3) or after both group 1 and group 2 had acted (groups 1, 4). From these charts, it would appear that there are spillovers, and they may be cumulative. NPI policies in group 1 and group 2 seem to be especially important for determining growth rates of new cases not only in their own states but also in other groups (i.e., 0, 3, and 4).

We test this more carefully in a linear regression in Table 3. In these regressions, we allow for stay-at-home policies in all other states to matter for state *i*. Policies in other states are population and distance weighted. We also allow for differential effects of policies in neighboring states NPIs in other states with a border state indicator dummy variable, and we allow for the level of confirmed cases in other states to affect growth of cumulative cases.

Own state policies are still associated with lower growth rates of confirmed cases after controlling for other state policies. The point is estimate is -0.034 (p-value =0.005, 95% C.I. -0.057 to -0.011). This is one-fifth of the magnitude of the own-state policy in Table 3 when we did not control for other state policies.

Spillovers matter. Policies in other states dating from the week ending March 21st are negatively associated with mortality even in states that had yet to impose a stay-at-home policy. The association between local growth rates of confirmed cases and the first states' policies is the largest. Column 4 shows the point estimate is -14.77 (p-value = 0.056 95% C.I. -29.93 to 0.379). An extra policy (in the first week ending 21 March) at the median distance between states is associated with a decline of about one log point or -0.009 (-0.009 = (1/1688) x -14.777). A new policy by a neighboring state, with the median in-sample centroid-to-centroid distance is associated with a decline of -0.034 (-0.034 = (1/441)*-14.777). This is about the same magnitude as the own-state point estimate. There is no statistically significant differential in the marginal impact of bordering states versus more distant states after accounting for distance between state centroids.

The association for NPI policies in weeks 2, 3 and 4 declines in absolute magnitude and statistical significance in columns 2-4. By the fourth week, the marginal effects of policies in other states are not statistically significant. This is suggestive of the idea that the first wave of stay-at home policies had a bigger impact than later waves.

We also cannot reject the hypothesis that the level of deaths in other cities (weighted by distances between cities) has no relationship with own-city growth rates of deaths *ceteris paribus*.

2.2 Policies and the Economy

Policy has been theoretically predicted to matter for the economy. A high intensity and duration of NPIs is predicted to lower cumulative mortality and peak mortality, but this comes (theoretically) at a greater cost to the economy than had NPIs not been imposed. We find no evidence of this. In

Table 4 we show that applications for unemployment insurance (i.e., jobless claims) rose at the same rate in states that adopted stay-at-home policies as in states without stay-at-home. The point estimate is -0.309 (p-value = 0.108 95% C.I. -0.675 to 0.069). Based on this, there is no evidence that stay-at-home policies led to stronger rises in jobless claims.

The results show some interesting dynamics as well showing in fact that stay-at-home was potentially associated with lower unemployment. In columns 2 (not population weighted) and 3 (population weighted regressions) the association between stay at-home policies and jobless claims is statistically significant and negative two and three weeks after implementation. The coefficient on the first week is not highly statistically significant. We also use six leads of the indicators for stay-at-home. None of these leading marginal effects is statistically significant implying that pre-policy trends are unlikely to account for the post-policy rises in initial jobless claims.

We also interact state-fixed effects with the stay-at-home policy which allows for heterogeneous impacts by state. A potential concern is that the adoption of stay-at-home was economically less costly, and therefore adopted sooner in places where the occupational structure allowed telecommuting or where the structure of employment was less sensitive to the stay-at-home demand shock. This would bias the impact of such policies downwards. For instance, restaurants, retail and other 'in-person' services may have been more vulnerable to the drop in demand from stay-at-home and states that rely on these industries more heavily may have delayed. Figure 4 shows that the association between jobless claims and stay-at-home varies by state. It is difficult to see a clear pattern here however.

We attempt to see where stay-at-home mattered most by checking for a relationship between stay-at-home and industry-level employment-to-population shares as well as an interaction for the share of jobs in a state that were “telecommutable”.[18] We include separate effects for industries that are most likely to be “in-person”. For the main effects, we find jobless claims grew most strongly in states with higher shares of employment in the leisure and recreation industry and in wholesale distribution and smaller where employment shares in retail were higher.

In terms of interactions between industry and stay-at-home there are interesting findings. Stay-at-home had a smaller impact on jobless claims where oil and petroleum sectors were more prevalent and where arts and recreation had a higher share of employment. Other sectors like food preparation, retail sales and wholesale were not differentially affected by stay-at-home orders. This suggests common shocks and cross-state trade may matter. At the very least, there is little straightforward evidence linking stay-at-home to industries that are most obviously in-person like retail, food and leisure.

We do however find a more straightforward interaction with stay-at-home and telecommuting. Stay-at-home has a smaller impact I proportion to the share of jobs that can be done remotely. When we include a control for this and an interaction effect, the un-interacted stay-at-home main effect is associated with higher jobless claims with a point estimate of 2.55 (p-value = 0.064, 95% C.I. -0.159 to 5.27). However, the interaction with the share of jobs that can telecommute is large and negative at -4.93 (p-value = 0.063, 95% C.I. -10.15 to 0.28). The average share of telecommutable jobs is 0.38 implying that states above average and near the top, at a share of say 0.48, felt an impact on jobless claims from stay-at-home roughly 1/3 as large as states at the mean.

3. Discussion and comment

We have studied a range of Non-Pharmaceutical Interventions in the early stages of the global Covid-19 pandemic. We assess the epidemiological and economic implications of these policies. NPIs reduce growth rates of confirmed cases of Covid-19. The reductions apply to local jurisdictions but also “spillover” to geographically proximate units. Spillovers in policy seem to work more strongly domestically (according to US data) than across international borders.

On average, stay-at-home policies are not associated with higher joblessness in the US states that imposed them than in states that did not. We interpret this as evidence that the negative economic shocks were national and not local. There is however some evidence that stay-at-home has sectoral and occupational impacts. States with more jobs that can be done remotely seem to have lost fewer jobs after implementing stay-at-home than states with fewer such jobs.

During Covid-19, NPIs appear to spillover across states in the US data. These spillovers could arise due to direct limitations on contact with infected individuals from other jurisdictions. However, it could also be because of a psychological or expectational effects. We find evidence that policies in the first-moving states matter more for other states than policies from later-moving states. This implies that part of the impact is due to reaction to news of NPIs in other states. Such news may indicate the severity of an outbreak or a pandemic leading to decreases in labor supply and reactive social distancing even without policies in the locality. Reduced demand for other states products and services from places with stay-at-home could spillover to states without policy too. State-to-state trade or shipment data would be required to verify and validate this channel.

The association between own-state policy and growth of new cases of Covid-19 is weakened once accounting for neighboring state policies. This does not imply that local policy is un-necessary or fruitless. Indeed, the opposite may be true. Neighbors of states not implementing NPIs evidently face greater challenges containing and mitigating disease. This implies there is justification for policy coordination if the objective is to mitigate the spread of disease and to reduce mortality. Externalities imply coordination as per standard economic theory.

In terms of the tradeoff between the economy and public health, similar lessons apply. There is no "free lunch" in a connected and open economy. Once a pandemic is underway and some states have implemented NPIs, then the economic spillover is likely to be strong. This occurs as NPIs in one state, region or country reduce local demand as well as demand for goods and services from other localities. NPIs also disrupt supply chains and contribute to a generalized supply shock in an open-economy setting. Information flows between localities means non-local policies could limit economic participation and labor supply even in localities without NPIs.

Could a state or locality do better by not implementing an NPI while others did? Free-riding is tempting, but it may have un-intended impacts. Assume people can move between places. States with NPIs, realizing that the pandemic could be more severe globally due to non-compliance with public health recommendations may be forced to keep their own NPIs in place longer or more intensively. These NPIs reduce the demand for services and products from the non-complier for longer or in greater proportion. The negative impact is in proportion to the level of trade and economic inter-dependence between the two areas. International retaliation with travel bans on non-NPI territories could also limit the economic opportunities of non-complying states. The economic effects would spillover as well. Finally, agents in the non-complying locality may react to information coming from other localities. These reactions will have to be stronger and more intense since the local outbreak would be more intense than if the locality had implemented an NPI.

Table 1 Mitigation Policies and the Growth Rate of Confirmed Cases of Covid-19: Cross Country Evidence

	(1) School Closures	(2) Workplace Closures	(3) Public Events Limits	(4) Public Transportation Closed	(5) Public Information Campaign	(6) Domestic Travel Limited	(7) International Travel Limited	(8) Sum of all policies
Policy	-0.0230	-0.0784***	-0.0233	-0.0953**	-0.0274	-0.0650*	-0.0639***	-0.0207***
	(0.0264)	(0.0267)	(0.0244)	(0.0376)	(0.0357)	(0.0339)	(0.0236)	(0.00751)
Event time	-0.00611**	-0.00563**	-0.00625**	-0.00585**	-0.00633***	-0.00590**	-0.00524**	-0.00452*
	(0.00259)	(0.00239)	(0.00249)	(0.00248)	(0.00234)	(0.00236)	(0.00257)	(0.00247)
(Event time)^2	0.0000984***	0.0000997***	0.000101***	0.0000924***	0.0000991***	0.000101***	0.0000926***	0.0000926***
	(0.0000345)	(0.0000343)	(0.0000342)	(0.0000345)	(0.0000335)	(0.0000341)	(0.0000343)	(0.0000337)
Constant	0.253***	0.251***	0.254***	0.252***	0.270***	0.251***	0.265***	0.269***
	(0.0318)	(0.0311)	(0.0319)	(0.0316)	(0.0434)	(0.0313)	(0.0315)	(0.0307)
Observations	2346	2346	2346	2346	2346	2346	2346	2346
R^2	0.108	0.115	0.108	0.112	0.108	0.112	0.112	0.115
Countries	73	73	73	73	73	73	73	73

Notes: Dependent variable is the daily change in the logarithm of deaths from Covid-19. Estimation is by OLS. All models include country fixed effects and calendar day dummies. Event time is defined as number of days since the first official case of Covid-19. Standard errors in parentheses are clustered at the country level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2 Mitigation Policies and the Growth Rate of Deaths from Covid-19: Cross Country Evidence

	(1) School Closures	(2) Workplace Closures	(3) Public Events Limits	(4) Public Transportation Closed	(5) Domestic Travel Limited	(6) International Travel Limited	(7) Sum of all policies
Policy	0.0340	-0.000391	0.0156	-0.0900^{**}	-0.0496	-0.0414	-0.0123
	(0.0320)	(0.0264)	(0.0279)	(0.0432)	(0.0553)	(0.0315)	(0.0101)
Event time	-0.124^{**}	-0.119^{**}	-0.122^{**}	-0.108^{*}	-0.118^{**}	-0.117^{**}	-0.103
	(0.0581)	(0.0587)	(0.0576)	(0.0563)	(0.0573)	(0.0575)	(0.0624)
$(\text{Event time})^2$	0.0000110	0.00000252	0.00000533	-0.0000169	-0.00000419	-0.000000779	-0.0000230
	(0.0000289)	(0.0000322)	(0.0000282)	(0.0000299)	(0.0000321)	(0.0000289)	(0.0000356)
Constant	2.107^{**}	2.058^{**}	2.086^{**}	1.897^{**}	2.080^{**}	2.049^{**}	1.864^{*}
	(0.906)	(0.918)	(0.902)	(0.887)	(0.896)	(0.901)	(0.962)
Observations	948	948	948	948	948	948	948
R^2	0.303	0.302	0.302	0.306	0.302	0.303	0.303
Countries	58	58	58	58	58	58	58

Notes: Dependent variable is the daily change in the logarithm of deaths from Covid-19. Estimation is by OLS. All models include country fixed effects and calendar day dummies. Event time is defined as number of days since the first official death from Covid-19. Standard errors in parentheses are clustered at the country level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3 Change in (log) Confirmed Cases versus Stay-at-Home Orders and Neighboring States' Stay-at-Home Policies.

	(1)	(2)	(3)	(4)
S_i = Stay-at-home	-0.170***	-0.0284**	-0.0335**	-0.0338***
	(0.0197)	(0.0124)	(0.0155)	(0.0114)
$S_{i,-i}$ = (Stay-at-home$_{-i}$) x week ending 21 Mar.		-4.020	-8.018	-14.78*
		(2.952)	(4.909)	(7.538)
$S_{i,-i}$ = (Stay-at-home$_{-i}$) x week ending 28 Mar.		-2.045**	-3.099***	-4.226***
		(0.941)	(0.997)	(1.109)
$S_{i,-i}$ = (Stay-at-home$_{-i}$) x week ending 4 Apr.		-1.527*	-1.684*	-2.385**
		(0.832)	(0.894)	(1.040)
$S_{i,-i}$ = (Stay-at-home$_{-i}$) x week ending 11 Apr.		-0.486	-0.673	-1.379
		(0.892)	(0.935)	(1.070)
$S_{i,-i}$ = (Stay-at-home$_{-i}$) x week ending 18 Apr.		-0.273	-0.532	-1.294
		(0.878)	(0.892)	(1.035)
$S'_{i,-i}$ = (Stay-at-home- border states) x week ending 21 Mar.			0.0452	0.0552
			(0.0457)	(0.0464)
$S'_{i,-i}$ = (Stay-at-home- border states) x week ending 28 Mar.			0.0115*	0.0148**
			(0.00573)	(0.00576)
$S'_{i,-i}$ = (Stay-at-home- border states) x week ending 24 Mar.			0.00222	0.00551
			(0.00563)	(0.00579)
$S'_{i,-i}$ = (Stay-at-home- border states) x week ending 11 Apr.			0.00344	0.00556
			(0.00671)	(0.00729)
$S'_{i,-i}$ = (Stay-at-home- border states) x week ending 18 Apr.			0.00516	0.00712
			(0.00657)	(0.00687)
ln (confirmed cases$_{-i}$/distance)				0.0516
				(0.0461)
ln (confirmed cases, border states)				-0.00881
				(0.0393)
Observations	2175	2175	2175	2175
R^2	0.213	0.282	0.316	0.322
States	49	49	49	49
Week Dummies	NO	YES	YES	YES

Notes: Dependent variable is the daily change in the logarithm of confirmed cases of Covid-19. Estimation is by OLS. All models include state fixed effects. Event time trend and a quadratic term in event time are included. Event time is defined as number of days since the first official case of Covid-19. Week indicators for all weeks after the week ending 28 March are included. The week ending March 21 is the policy reference group. All regressions are weighted by state population. Standard errors in parentheses are clustered at the country level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0$.

Table 4 Initial jobless claims and the Dynamics of Own-State Stay-at-Home Orders

	(1)	(2)	(3)
Stay-at-home	-0.309*		
	(0.179)		
Stay-at-home (3 weeks after)		-0.629***	-0.494***
		(0.230)	(0.164)
Stay-at-home (2 weeks after)		-0.427**	-0.398***
		(0.166)	(0.121)
Stay-at-home (initial week)		-0.304	-0.166**
		(0.188)	(0.0782)
Stay-at-home (2 weeks before)		-0.00315	-0.00453
		(0.124)	(0.122)
Stay-at-home (3 weeks before)		-0.0176	0.0286
		(0.0907)	(0.105)
Stay-at-home (4 weeks before)		0.0356	0.0409
		(0.117)	(0.0853)
Stay-at-home (5 weeks before)		-0.0400	-0.00228
		(0.0509)	(0.0651)
Stay-at-home (6 weeks before)		-0.0658*	-0.0571
		(0.0385)	(0.0448)
N	459	459	459
Number of States + DC	51	51	51
R^2	0.975	0.976	0.977

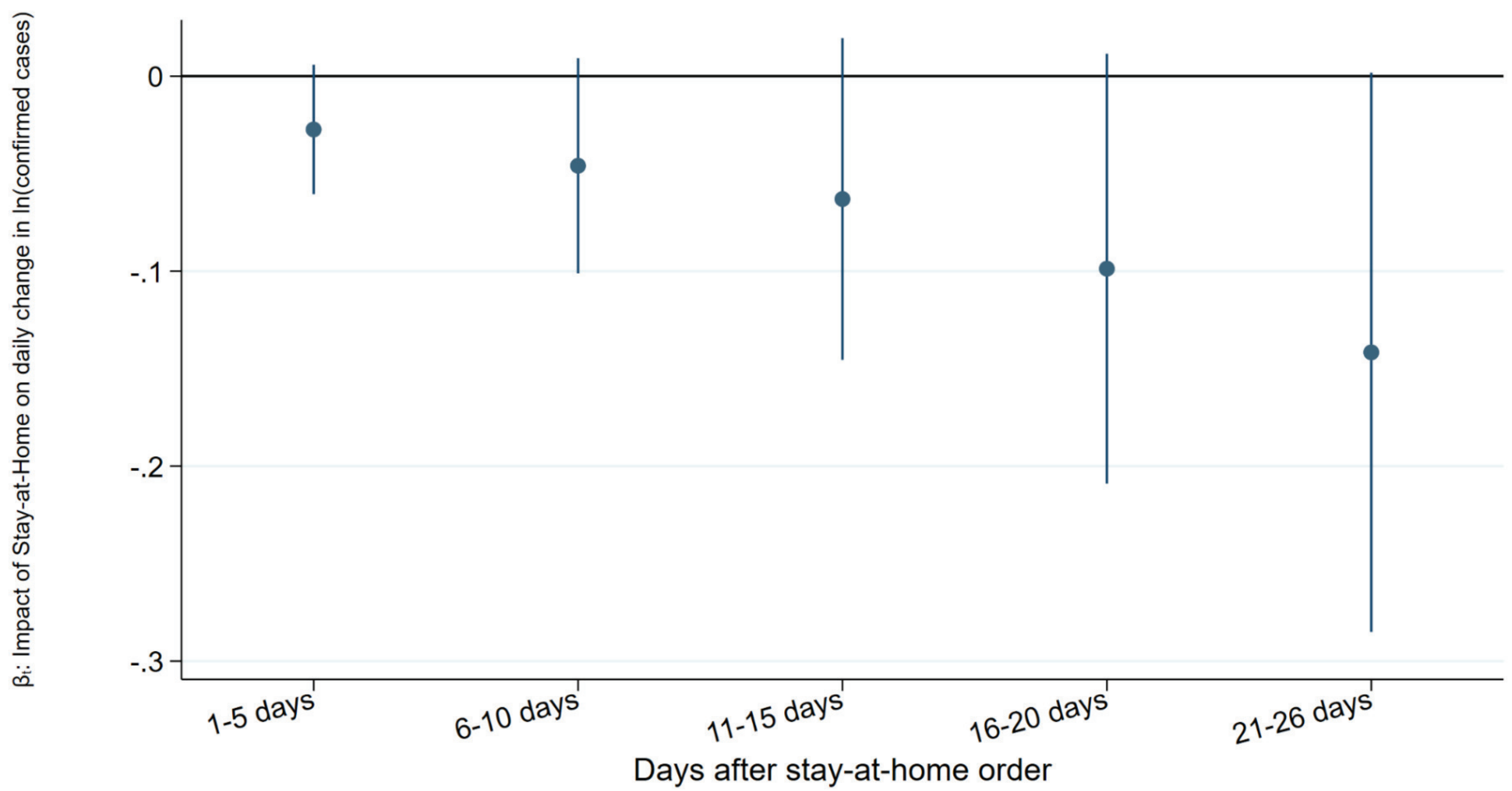
Notes: Dependent variable is the logarithm of initial jobless claims in the previous week (not seasonally adjusted). Estimation is by OLS. Data is a panel of states + District of Columbia by week. All models include state fixed effects and calendar week fixed effects. Regressions (1) and (2) are weighted by state population. Column (3) is an unweighted regression. In columns (2) and (3) week t is the first week for the stay-at-home order. Week t − 3 denotes three weeks after stay-at-home was initiated, t − 2 two week etc. The week prior to initiation of the stay-at-home order is the reference group. Standard errors in parentheses are clustered at the country level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 5 Initial jobless claims, Stay-at-Home Orders and Sectoral Employment

	(1)	(2)	(3)	(4)
Stay-at-home	-0.303	0.537	2.403*	2.559*
	(0.185)	(0.367)	(1.355)	(1.353)
Average Share of Jobs-at-home		4.644	6.315	0.0706
		(3.689)	(3.974)	(4.373)
Stay-at-home x Average Share of Jobs-at-home			-4.927	-4.937*
			(3.559)	(2.599)
Share of Jobs in Oil & Gas				55.43
				(169.0)
Share of Jobs in Arts, Rec. and Entertainment				255.8***
				(86.32)
Share of Jobs in Food & Accommodation				-10.73
				(17.72)
Share of Jobs in Retail				-189.4***
				(38.91)
Share of Jobs in Wholesale				149.9***
				(41.93)
Share of Jobs in Oil & Gas x Stay-at-home				-316.3***
				(89.42)
Share of Jobs in Arts, Rec. and Entertainment x Stay-at-home				-124.7**
				(55.04)
Share of Jobs in Food & Accommodation x Stay-at-home				-2.579
				(10.13)
Share of Jobs in Retail x Stay-at-home				9.512
				(17.31)
Share of Jobs in Wholesale x Stay-at-home				-6.138
				(27.31)
N	267	267	267	267
R^2	0.971	0.662	0.663	0.849

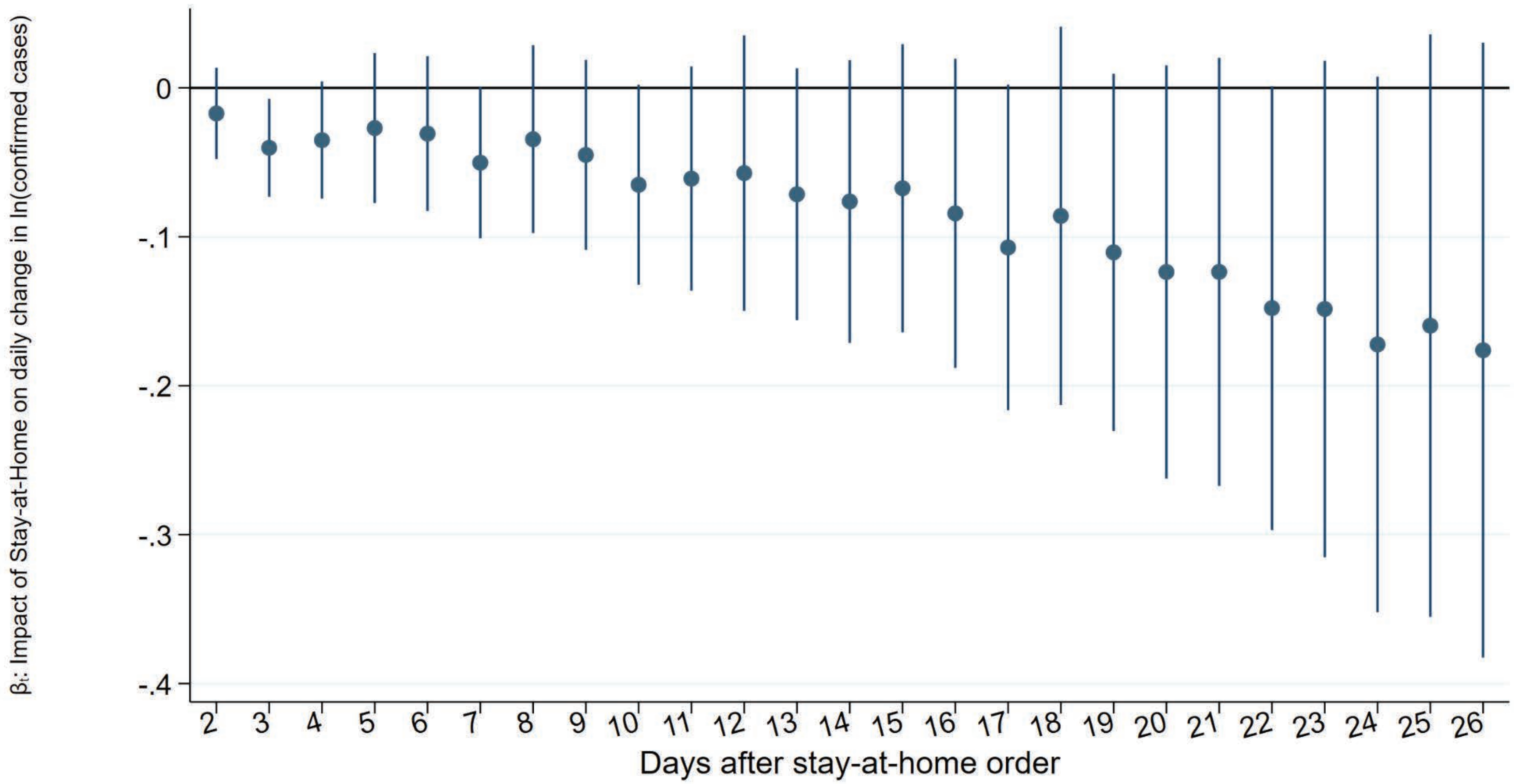
Notes: Dependent variable is the log of initial jobless claims (not seasonally adjusted). Estimation is by OLS. Data is a panel of states + District of Columbia by week. Column (1) includes state fixed effects and all models have calendar week fixed effects. Regressions are weighted by state population. Standard errors in parentheses are clustered at the country level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 1 Stay-at-Home and the Growth Rate of Cumulative Cases of Covid-19: Dynamics Post-Policy



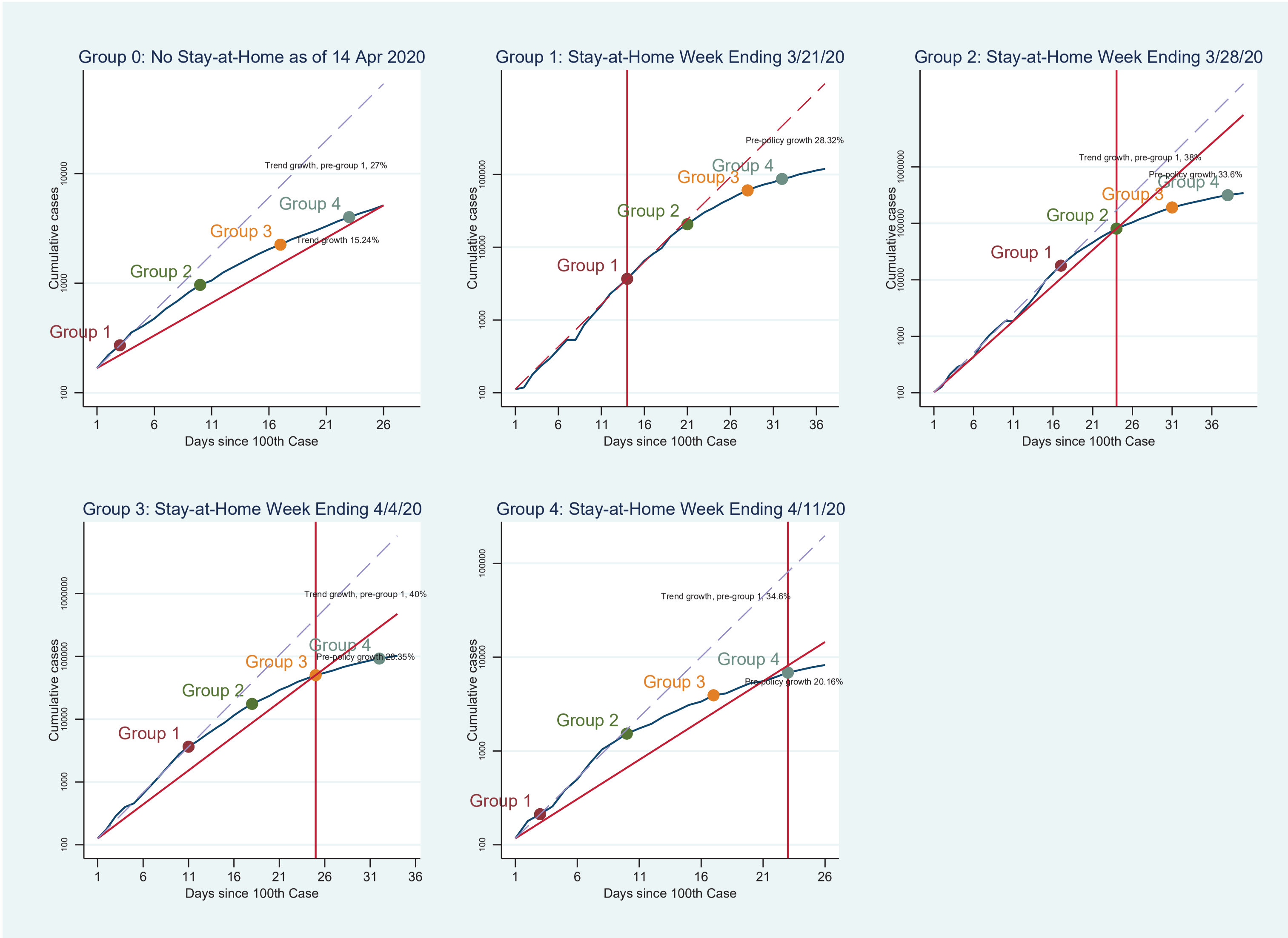
Notes: Chart shows the average level of the daily change in the log of confirmed cases of Covid-19 in periods after implementing a stay-at-home order with 95% confidence bars. The levels (dots) are the coefficients from OLS regressions where the dependent variable is the logarithm of confirmed cases. Regressions include state fixed effects, event time trend and quadratic effect and calendar day dummies. Event time is counted in days since the first case of Covid-19 within a state. Standard errors are clustered at the state level.

Figure 2 Stay-at-Home and the Growth Rate of Cumulative Cases of Covid-19: Daily Dynamics Post-Policy



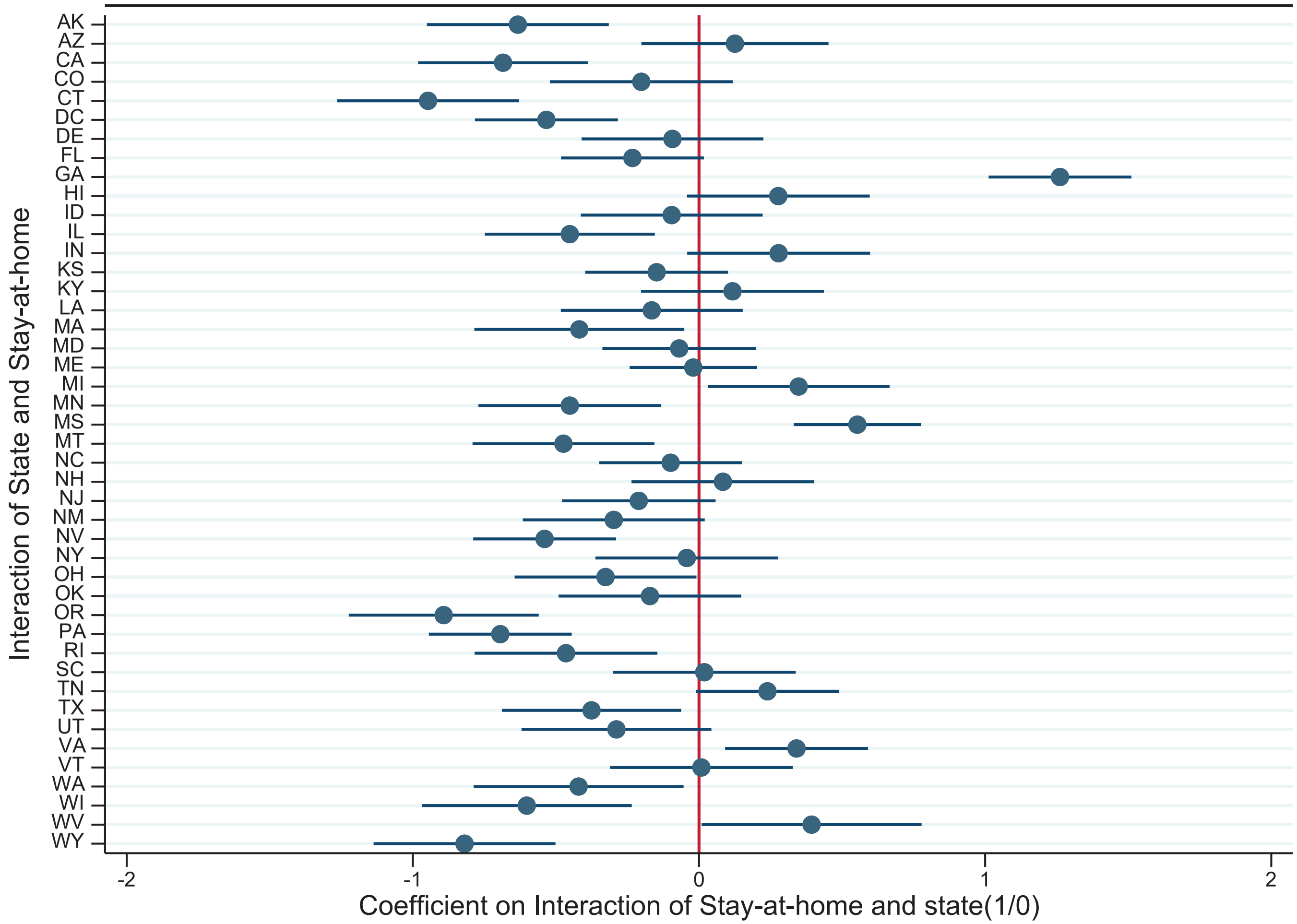
Notes: Chart shows the average level of the daily change in the log of confirmed cases of Covid-19 in days after implementing a stay-at-home order with 95% confidence bars. The reference category is the first day of the stay at home order. The levels (dots) are the coefficients from OLS regressions where the dependent variable is the logarithm of confirmed cases. Regressions include state fixed effects, event time trend and quadratic effect and calendar day dummies. Event time is counted in days since the first case of Covid-19 within a state. Standard errors are clustered at the state level.

Figure 3 Cumulative Cases of Covid-19 and Stay-at-Home Orders



Notes: Figures plot cumulative cases of Covid-19 for five groups of states. Group 1 -4 implemented stay-at-home orders in successive weeks. Group 0 did not initiate stay-at-home within the sample. Data are plotted on a logarithmic scale. Vertical line denotes the end of the week in which all states in the group implemented stay-at-home. Dotted trend line is the average rate of growth of conformed cases prior to week ending 3/21. Solid line is the trend growth rate prior to implementation of own-group policies.

Figure 4 Impact on Initial Jobless claims of Stay-at-Home Orders by State



Notes: Chart shows the average level of the log of initial jobless claims after implementing a stay-at-home order with 95% confidence bars. Data is a panel of states + District of Columbia by week. All models include state fixed effects and calendar week fixed effects. The regression is weighted by state population. Standard errors are clustered at the state level.

Endnotes and References

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[17] A data appendix available upon request shows the timing for each state and their group. Group 1 includes California, Illinois, New Jersey and Maryland. Group includes 27 states including New York, Washington, Louisiana, Massachusetts , and Michigan. Group 3 includes 13 states such as Florida and Texas. Group 4 includes Alabama and Missouri. The non-adopters were: Arkansas, Iowa, Nebraska, and the Dakotas.

[18] These data are from Dingel and Neiman downloaded from https://github.com/jdingel/DingelNeiman-workathome on April 17, 2020.