The COVID-19 Pandemic: Government vs. Community Action Across the United States[*]

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Abstract

Are lockdown policies effective at inducing physical distancing to counter the spread of COVID-19? Can less restrictive measures that rely on voluntary community action achieve a similar effect? Using data from 40 million mobile devices, we find that a lockdown increases the percentage of people who stay at home by 8% across US counties. Grouping states with similar outbreak trajectories together and using an instrumental variables approach, we show that time spent at home can increase by as much as 39%. Moreover, we show that individuals engage in limited physical distancing even in the absence of such policies, once the virus takes hold in their area. Our analysis suggests that non-causal estimates of lockdown policies' effects can yield biased results. We show that counties where people have less distrust in science, are more highly educated, or have higher incomes see a substantially higher uptake of voluntary physical distancing. This suggests that the targeted promotion of distancing among less responsive groups may be as effective as across-the-board lockdowns, while also being less damaging to the economy.

Keywords: COVID-19, difference-in-differences, instrumental variables, NPI, community action, physical distancing, big data.
JEL-Classification: I12, I18, H12, H75, D04.

1 Introduction

The outbreak of COVID-19 has caused an unprecedented healthcare crisis and a major disruption to the global economic system across the world. Political leaders in many countries have taken measures to limit the contagion rates in order to relieve the pressure on health care systems and prevent excess deaths. While epidemiological uncertainty about the virus and its spread remains (Anderson et al., 2020), research on China and South Korea shows that early governmental action and cooperation by the population can stem the uncontrolled spread of the pandemic (Kraemer et al., 2020; Wilder-Smith and Freedman, 2020; Wu and McGoogan, 2020).

In this paper, we provide estimates of how government action influences community behavior along several dimensions, and in turn is itself influenced by decisions made by the population at large. From a policy perspective, understanding whether and how communities respond to government actions is crucial. To the best of our knowledge, we are the first to leverage high-resolution big data on people's movements and whereabouts in combination with causal econometric methods in order to analyze the interdependence between government and community action.

Using staggered difference-in-differences (DiD) approaches and an instrumental-variable (IV) analysis, we show that physical distancing measures pick up after the implementation of government lockdown policies. In particular, in our first approach, we estimate that the introduction of a lockdown policy increases the proportion of people who stay completely at home by around 8%, over and above any community action taken. For our difference-in-differences instrumental variable approach, we group states together by the date on which the first within-state COVID death occurs. The evidence suggests that the effect size can be as large as 39% for certain states, once we account for endogeneity due to treatment selection. However, we document that communities take action even in the absence of government policies. Moreover, we find that the more communities take independent action to limit social interactions, the less likely it is that state governments implement restrictive lockdown measures. Our conclusion is that government policies can further amplify measures already taken at the community level, but that the need for restrictive policies is reduced the more the community takes independent action. Finally, our analysis suggests that non-causal econometric approaches to measure the uptake in physical distancing following lockdown policies will yield biased results, as we provide evidence for a two-way interaction between physical distancing and such policies.

These result shed further light on the role of public health policies in combating the COVID pandemic. In general, governments can take two distinctive strategies according to Ferguson et al. (2020): mitigation and suppression. The former aims at lowering maximum healthcare demand by reducing contagion rates through non-pharmaceutical interventions, while the latter approach adopts very restrictive measures to push down the prevalence of new cases to zero. Most researchers argue that only a mix of suppressive measures such as mandatory home isolation and lockdown policies can be successful in mitigating the spread of the virus. These interventions may need to be maintained over several years (Kissler et al., 2020) and complemented with

school and business closures (Ebrahim and Memish, 2020; Ferguson et al., 2020; Hellewell et al., 2020). Yet, even though lockdown policies have been crucial in slowing down infection rates during the early phases of the disease (Stoecklin et al., 2020; Wu et al., 2020; Xiao and Torok, 2020; Zu et al., 2020), the 'Swedish solution' of voluntary physical distancing has gained increased support in balancing the burden on health systems and the economy in the medium run (Krueger et al., 2020). Our findings reveal that both approaches may help to promote physical distancing. Nonetheless, the evidence we provide for differential outbreak response along socioeconomic lines calls for a more nuanced discussion paired with policies targeted at less responsive groups. Our findings align closely with earlier findings in the epidemiology literature that the distribution of individual infectiousness around the basic reproductive number – R(0) – is often highly skewed in epidemics, making that targeted control measures generally outperform population-wide ones (Lloyd-Smith et al., 2005).

So far, first steps have been taken to analyze social distancing under lockdown policies. Using mobility statistics from Unacast, Engle et al. (2020) find state-wide stay-at-home orders to be correlated with a reduction in mobility of 7.9%. Mediated by perceived risk, this correlation is stronger in counties with lower vote shares of the Republican party, higher population density and relatively more people over age 65. Painter and Qiu (2020) exploit SafeGraph data to show that the introduction of shelter-in-place policies is associated with a 5.1 percentage point increase in the probability of staying home. They also document smaller correlations in Republican states and in case a county is politically misaligned with the governor of the state. Qualitatively similar results have been obtained by Andersen (2020) and Allcott et al. (2020). We add to this literature by providing a detailed examination of the two-way causality and the overlooked endogeneity of lockdown policies.

More broadly, our research speaks to the field studying the behavioral impact of major crises such as natural disasters or pandemics. A host of papers deal with the long-run effects of the Spanish Flu in 1918-19, showing persistent decreases in human capital (Beach et al., 2018), generalized trust (Aassve et al., 2020) and old-age survival (Myrskylä et al., 2013). In terms of economic outcomes, pandemics have been associated with subsequent reductions in returns to assets (Jorda et al., 2020) and slight increases in real wages (Barro et al., 2020). In addition, our findings inform the debate about the role of formal and informal institutions in times of crisis (Stiglitz, 2000). In the past, both types of institutions have been shown to contribute to economic development individually and in a complementary (Guiso et al., 2004; Williamson, 2009) or substitutive manner (Ahlerup et al., 2009). Finally, several studies indicate that informal institutions are vital in promoting behavior that helps mitigate the spread of infectious diseases (Chuang et al., 2015; Rönnerstrand, 2013, 2014).

We find support for these results in that more informed (highly educated and high trust in science) areas react more strongly to the outbreak of the virus itself by voluntarily practicing physical distancing. In contrast to previous studies by Painter and Qiu (2020) and Engle et al. (2020) we do not find evidence for heterogeneity in response to lockdown policies, except for rural and urban areas – neither in that more privileged people show a stronger additional reaction nor

in a catch-up effect of disadvantaged communities. Nevertheless, socioeconomic inequalities may enter the COVID crisis along several dimensions. First, it is likely that disadvantaged groups will be affected more strongly by the crisis due to lower levels of health coverage, higher prevalence of pre-existing health problems, mass lay-offs and unfavorable living conditions.[1] Second, existing income and education differences income and education are likely to be exacerbated through the disruption of economic and educational systems (Armitage and Nellums, 2020; Glover et al., 2020; Van Lancker and Parolin, 2020). Third, tackling inequality could be crucial in mitigating the spread of the virus (Ahmed et al., 2020). Combining data on testing and incidences in New York City with demographic characteristics, Borjas (2020) shows that people residing in poor or immigrant areas were less likely to be tested. Yet, once a test was carried out in these neighborhoods, it was more likely to be positive. These counteracting factors dilute the importance of socioeconomic characteristics, making simple correlations between household income and the number of incidences prone to underestimating the asymmetric effects of the pandemic. Our result that less well-off areas tend to respond less to the outbreak of the crisis implies an important role for formal institutions in reaching out to such areas through welfare programs and information campaigns and for bolstering the informal institutions in place.

The remainder of this paper is structured as follows: Section 2 discusses our data sources, in particular our measures of physical distancing and lockdown policies.[2] Section 3 discusses our empirical approach and presents the results. Section 4 concludes.

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[1]See e.g. www.vox.com

[2]The state-level policy dataset can be accessed here.

2 Data

We compile a dataset on government policies and physical distancing for the period between February 1, 2020 and March 31, 2020 from various sources. In this section, we briefly discuss each of the sources and describe the variables we construct from them.

2.1 SafeGraph Physical Distancing and Foot Traffic Data

Our main dataset comes from SafeGraph, a California-based company that provides data on over 4 million points of interest (POI) across the United States, along with the associated foot traffic at those places, collected from up to 40 million mobile devices. The data was made available to academic researchers by SafeGraph to study the COVID-19 pandemic. Here, we provide a concise discussion of the two main datasets that we use.[3] Both datasets build on SafeGraph's core database of ~4 million POIs in the US, which they compile from thousands of diverse sources in an exhaustive 6-step process designed to guarantee reliability, granularity and accuracy. We aggregate this data to the state and county level to estimate the effect of lockdown policies targeted at reducing social interactions implemented by state governments to combat the spread of the virus.

WEEKLY PATTERNS. A temporary data product especially introduced for the study of the COVID-19 pandemic, Weekly Patterns, provides weekly updates of visitor and demographic aggregations for ~3.6MM POIs across the United States. It is based on an underlying panel of up to 40MM mobile devices with home addresses in all 200,000+ census block groups (CBG) across the United States.[4] Geographic bias of the sample is limited, with the absolute difference between the panel's density and true population density as measured by the US census never exceeding 3% at the state level. The correlation between both densities is 0.98. At the county level, the overall sampling bias is larger, with the correlation dropping to 0.97, although the bias for each separate county drops, to never exceed 1%.[5] In addition to this low geographic sampling bias, the panel also has a low degree of demographic sampling bias. Although device-level demographics are not collected for privacy reasons, average demographic patterns can be studied using panel-weighted, CBG-level Census data. Here again, the frequency of salient race, demographic and income groups in the panel closely tracks the same frequency in the Census. To obtain a measure of **daily state-level foot traffic**, we sum up the total number of visits each day to all POIs in each state. We consider overall foot traffic the best-suited measure to study movement patterns, since it smooths out industry-specific idiosyncrasies in

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[3]For a more detailed exposition of SafeGraph's data products, see `https://safegraph.com`.

[4]CBGs with less than 5 devices are excluded for privacy reasons.

[5]CBGs, expectedly, are marked by larger sampling bias, mostly due to technical errors in determining devices' home locations and so-called sinks. Since we restrict our analysis to the state and county level, this does not pose a serious issue. For a detailed exposition of SafeGraph's panel bias, see here.

foot traffic that arise from the particular nature of the policies imposed – with traffic to airports, for example, temporarily increasing after the travel ban on European countries.

SOCIAL DISTANCING METRICS. To facilitate the study of how people adhere to COVID-related social distancing arrangements, SafeGraph introduced a new data product that provides direct information on the movements of the smartphone devices in its panel.[6] Based on GPS pings from the devices, the common nighttime location of each mobile device over a 6 week period is narrowed down to a Geohash-7 (153m × 153m) granularity, which is denoted the device's home. Aggregate device metrics are then reported at the CBG level.

For our analysis, we further aggregate these metrics to the county and state level. Specifically, we measure, on a daily basis[7]:

- **Median distance traveled from home** for each state and county by taking the median of the same measure for all CBGs.

- **Median home dwell time**, constructed in a similar way.

- **The percentage of devices that spent all day at home** is obtained by summing a count of such devices at the CBG level and dividing it by the total number of devices observed in that CBG.

For the county-level analysis, our preferred measure is the percentage of devices that spent all day at home, because it is constructed from a raw count of the numbers of devices. Thus, this variable exhibits the most detailed variation, and no information is lost by repeatedly extracting moments from it, as is the case for the other variables. At the same time, the measure is less well-suited to state-level analysis, since we expect state-level policies to be more strongly related to movements in the entire state-wide distribution of physical distancing – captured well by the median dwell time –, rather than to the highly detailed movements of single individuals which show up in the percent at home variable.

SafeGraph guarantees privacy preservation of the subjects whose data is collected in at least three ways. First, the data was not collected directly from people's smartphones, but from a secondary source; it contains only aggregate mobility patterns. Second, SafeGraph excluded CBG information if fewer than five devices visited a place in a month from a given CBG so as to further enhance privacy. Third, the data products and maps derived from the mobility patterns are again aggregate results. No human subjects have or can be re-identified using these derived results.

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[6]See here for detailed information on this product.

[7]Detailed descriptions of each variable can be found in Appendix A, Table 5.

2.2 Government Measures

GOVERNMENT MEASURES. Data on government measures implemented to combat the COVID-19 spread has been retrieved from the National Association of Counties (NACO)[8] and the National Governors' Association.[9] For each state and county, we obtained data on whether and when they declared a state of emergency (SOE) and implemented business or school closures and safer-at-home polices.[10] The business closure order requires all non-essential businesses to close down, while the safer-at-home order calls for all citizens to stay at home. Essential needs (such as grocery shopping, exercise and medical emergencies) are the only exceptions to safer-at-home orders. People working in essential businesses are still allowed to go to work. Additionally, all 50 states implemented school closures. The dates for school closures were obtained from the official websites of the administrations of the 50 states and the District of Columbia.

2.3 Instruments and Controls

INSTRUMENTS: WEATHER AND VENTILATORS NEEDED. To account for the potential endogeneity of government policies with respect to the community response measures obtained from the SafeGraph data, we construct an instrument based on the daily number of ventilators required for each state. This variable is based on official estimates from the Institute for Health Metrics and Evaluation (IHME) that publishes COVID-19 projections under the assumption of full social distancing throughout May 2020.[11] While a current or predicted need of ventilators increases pressure on politicians to impose lockdown policies, these information are hardly disclosed to the public, or only with a lag. Therefore, we expect a need of ventilators to influence politicians' decisions without any direct effect on physical distancing in the population.

As to the converse endogeneity, we construct an instrument for community response measures from weather data, based on the deviations of temperature and precipitation from their 10 year-averages in the capital of each state. The data is taken from the National Centers for Environmental Information website.[12] Temperature and precipitation in the capitals are based on measurements from the main weather station of the capital's airport. We match these to our community response measures for the capital cities in question.

HOSPITAL CAPACITY. Data on hospital capacity is provided by Definitive Healthcare[13] through the ESRI's Disaster Response Program[14], which gathers useful data to understand the spread of COVID-19 in the United States. We use daily forecasts on the number of hospital beds and ventilators needed for COVID patients at the state level.

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[8]For details, see `https://ce.naco.org/?dset=COVID-19&ind=State%20Declaration%20Types`. We thank NACO for sharing the underlying data with us.

[9]The underlying data from the NGA can be found under `https://www.nga.org/coronavirus/#states`

[10]In this paper, we use the terms shelter-in-place and safer-at-home interchangeably.

[11]See www.covid19.healthdata.org for a current version of the data.

[12]The historical weather data is available under www.ncdc.noaa.gov.

[13]www.definitivehc.com

[14]www.coronavirus-disasterresponse.hub.arcgis.com

COVID-19 STATISTICS. The data on COVID-19 cases and deaths in the United States is collected from three different sources: the official US Government COVID-19 dedicated page[15], the Johns Hopkins Coronavirus Research Center[16] and the COVID Tracking Project.[17] We collect measures on positive tests, negative tests and number of deaths; and this for both cumulative count and day-on-day increases.

SOCIO-ECONOMIC STATISTICS. Most demographic variables are sourced from the American Community Survey 2018 (Ruggles et al., 2018), a 1 % random sample of the American population. Population estimates for 2018 come from the official Census Statistics. [18] Next, data on county-level employment and education were drawn from the Quarterly Census of Employment and Wages (2019Q3) and the United States Department of Agriculture's Economic Research Service. As a proxy for belief in science, we leverage data on county-level opinions on climate change from Howe et al. (2015). Data on party vote shares in the 2016 presidential election by county was obtained from the MIT Election Lab.[19] Lastly, we use the institutional health index from the United States Congress Joint Economic Committee.[20] The index combines information on the rate at which citizens cast ballots in the 2012 and 2016 presidential elections; the rate at which residents returned the 2010 census through the mail; and the confidence of adults in corporations, the media and public schools.

3 Results

We now turn to the results of this paper. In particular, we pose four sets of questions. First, to what extent do people practice physical distancing, such as staying at home, in the absence of government policies? Second, how do people adapt their behavior following a policy change? Third, how is the implementation of lockdown policies influenced by previous community behavior? And fourth, to what extent do socioeconomic and political factors matter in shaping community responses to COVID-19?

In order to answer these questions, we first review the data descriptively in subsection 3.1. The descriptive statistics point towards the conclusion that people stay at home even in the absence of lockdowns after the virus takes hold in their area, but that their response is stronger if policies are implemented. Building upon this analysis, in subsection 3.2 we employ a staggered difference-in-differences (DiD) approach and a DiD instrumental variables (IV) methodology in order to estimate the causal effect of lockdown policies. We then turn to the question of reverse causality in subsection 3.3, where we use an IV strategy to show that persistent inaction on the side of the community can trigger the government to implement policies.

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[15] www.COVID19.healthdata.org
[16] www.coronavirus.jhu.edu
[17] www.COVIDtracking.com
[18] www.bea.gov
[19] www.electionlab.mit.edu
[20] www.jec.senate.gov

Finally, we document substantial heterogeneity in community responses across socioeconomic groups in section 3.4. After discovering stark differences between groups in their reaction to the outbreak of the virus, we show that the same heterogeneity is not evident in the additional response to lockdown policies, but is rather due to differences in the degree of voluntary physical distancing. Our analysis suggests that cross-county differences in socioeconomic variables, such as income, belief in science or education, can have as much of an effect on the level physical distancing as the imposition of a lockdown. In the medium run, this implies that governments can decrease the need for damaging lockdowns by expanding cohesive policies.

3.1 Descriptive Statistics

Figure 1 shows the series of events that occurred over the course of February and March 2020 in the United States. The first cases and deaths were confirmed in late February while the spread of the disease officially only gained momentum during the middle of the month. Lockdown policies were imposed across states during the weeks after these incidents. As of end March, all states have adopted school closure policies and roughly half of them have gradually been introducing business closures and shelter-in-place measures, i.e. orders to stay home.

Figure 1: Timeline of Contagion and Lockdown Policies, Feb-Mar 2020
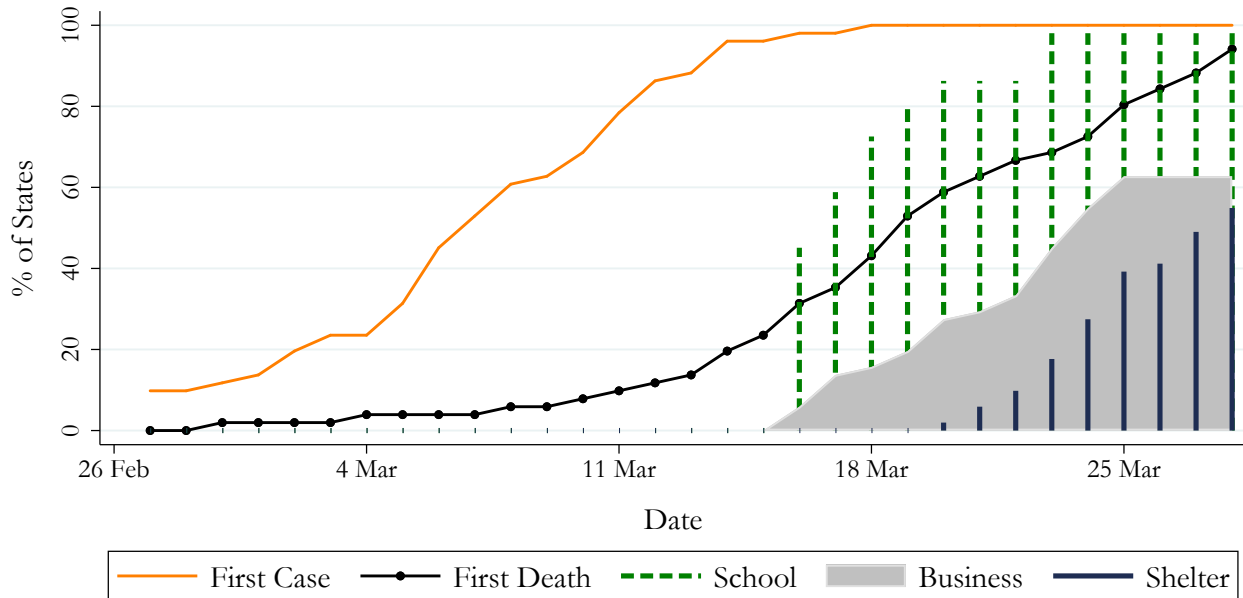
Notes: The orange solid line represents the percentage of states with at least one confirmed case; the black connected line shows the percentage of states that have recorded a first death due to the virus. The dashed (solid) spikes refer to school closures (shelter-in-place policies). he grey shaded area depicts business closures.

Figure 2 and Figure B.4 in the Appendix depict trajectories for selected states from each US region by plotting the percentage of devices that stayed home all day and the foot traffic, respectively, alongside national and state-level lockdown policies. For all states, the variables seem to be stationary until the first week of March, when traffic starts to drop and the percentage

that stayed home increases. The upward trend seems to continue regardless of national and state-level policies, though the nation-wide state of emergency declaration (first dashed grey line) appears to cause a significant acceleration of this trend, as do several of the state-wide policies. Not only does the timing of the outbreak of the pandemic differ across states, but so do the community reaction as well as the timing and scope of lockdown policies. This variation allows us to explore the interplay between community behavior and government action around the time of the enactment of the lockdown policies.

Our analysis reveals that outbreaks of COVID-19 are significantly associated with uptakes in physical distancing. Figure 3 shows how three types of distancing measures respond to the first death in a state, alongside 95% confidence intervals. The estimates illustrate how the variable changes on each day after the first COVID death compared to no death having occurred, where we control for state fixed effects. Note that all three variables change in the expected way: compared to no death having occurred, the percentage of people who stay at home all day and the median dwell time at home go up (panels (a) and (b)), while the median distance from home decreases (panel (c)). Moreover, the estimated effects are large: the percentage of people staying at home all day increases by around 8 percentage points one day after the first death compared to the period before the first death.

At the state-level, the downward trends in traffic observed in Figure B.4 seem to hold both in the presence and in the absence of policy interventions. Figure 4 expands on this point by showing the community response before a policy has been implemented. In the left panel, the y-axis presents the percentage change in traffic between the date of the tenth confirmed case and the enactment of the first lockdown policy, while the x-axis shows the number of days between these two events. The right panel plots equivalently the difference in percentage stayed home. For the majority of states, lockdown policies were implemented several days after the first death as indicated by the positive values on the x-axis. The figure illustrates that once a state is affected by COVID-19, individuals start to reduce their daily foot traffic and spend relatively more time at home even before any lockdown policy is implemented. Hence, we conjecture that calls for physical distancing and information on the virus's spread are taken seriously and individuals voluntarily modify their behaviour even in the absence of fast-moving policies. This association is by no means negligible in size: in our sample, foot traffic decreases by up to 50% 10 days after the tenth confirmed case, while the share of individuals staying home increases by up to 16 percentage points.

**Figure 2:** Percentage Completely at Home and Lockdown Policies in Selected States
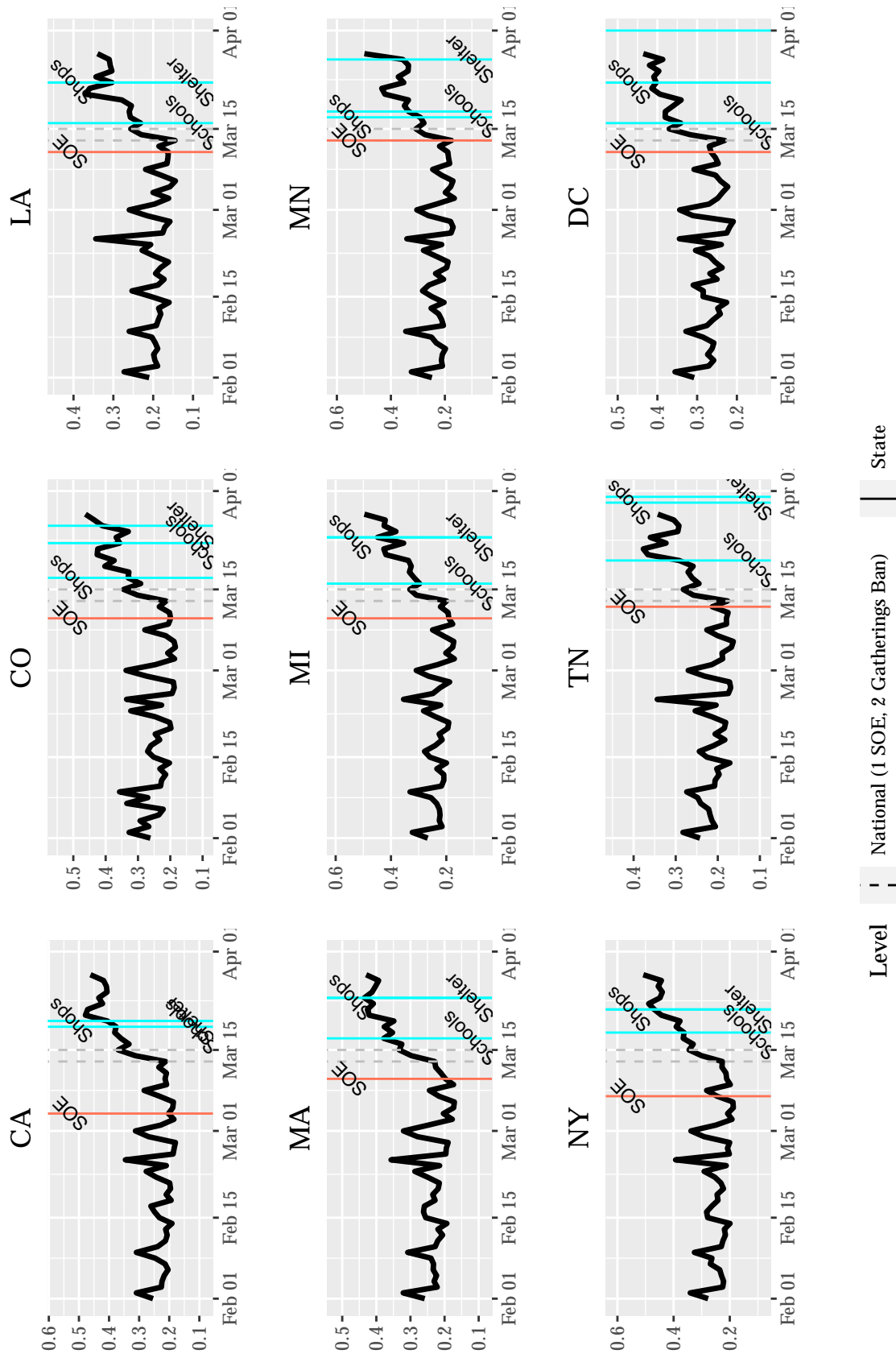
Notes: The plots show daily foot traffic and the percentage of devices that stayed home in selected states over time. The dashed vertical lines indicate national measures (SOE, Gatherings Bans) and the solid lines represent state-level SOEs and lockdown policies.
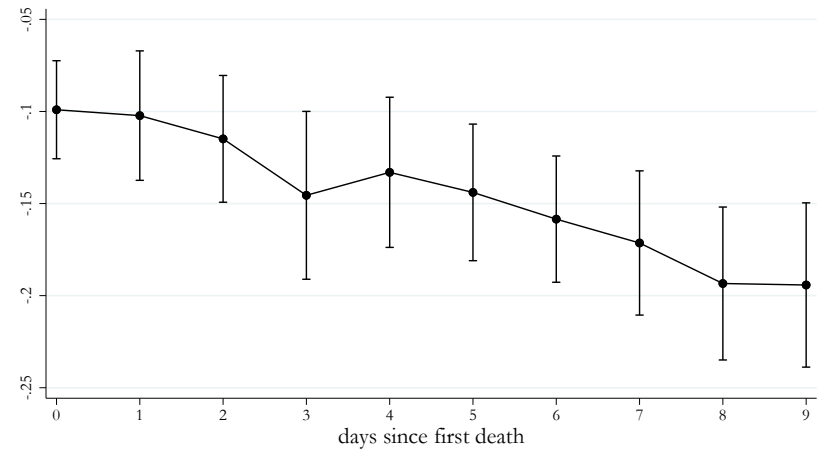
Figure 3: Physical Distancing Change Since First Death from COVID-19, Relative to No Death
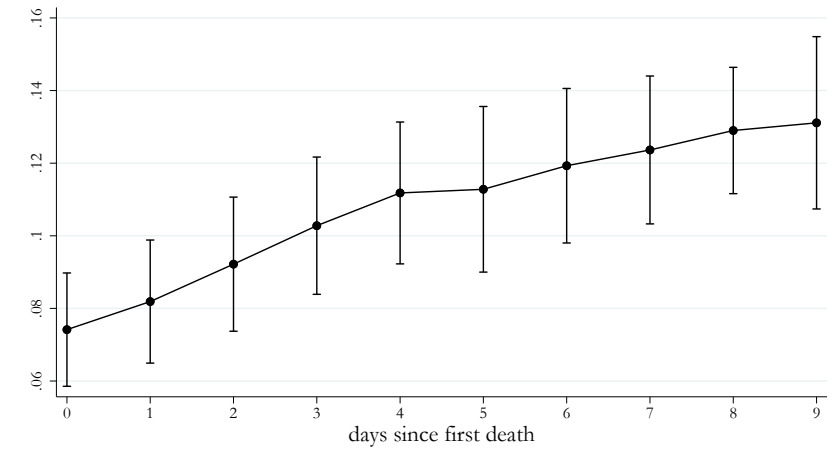
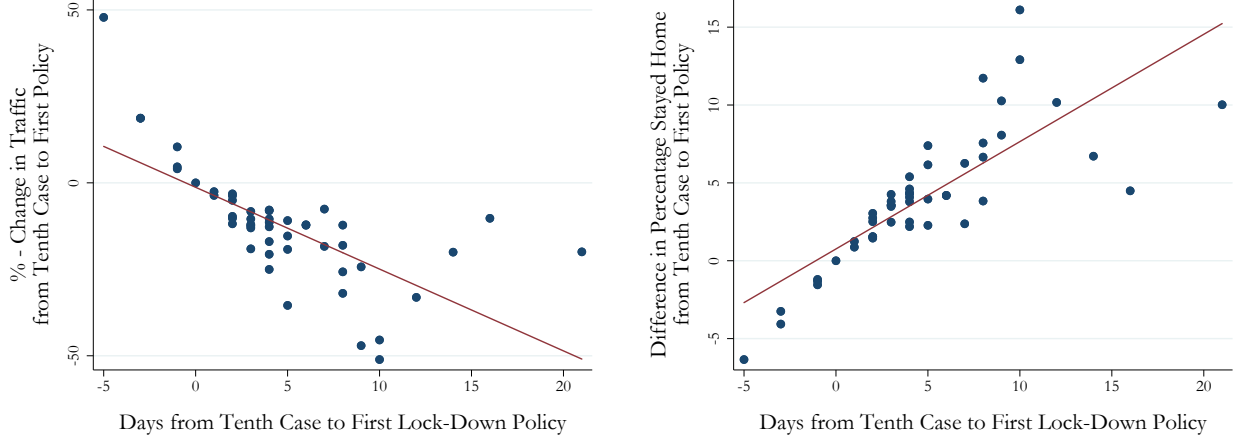(a) Percent Completely at Home
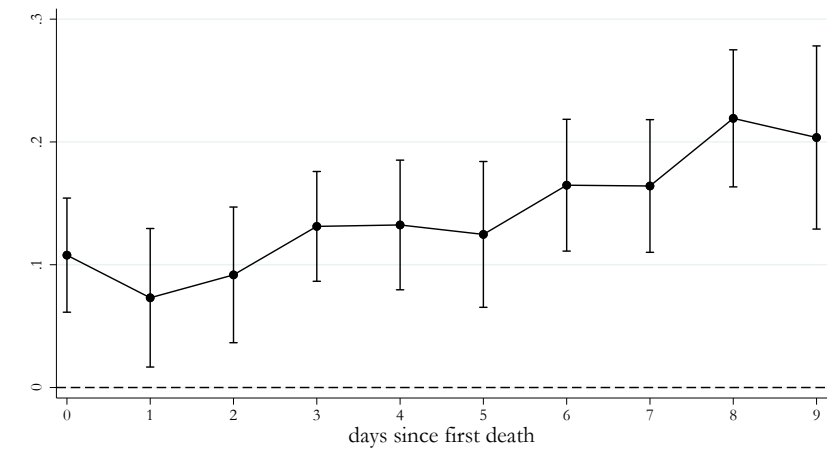
(b) Log of Median Time Spent at Home

(c) Log of Median Distance from Home

Note: The graphs plot the coefficients on days-since-first-death dummies, controlling for state fixed effects.

Figure 4: Change in Outcome Variables Before Enactment of Lockdown Policies

Notes: The graphs plot the change in outcome variables over the period between the tenth confirmed case in the state and the implementation of the first lockdown policy against the count of the days between the two events. The left panel shows the percentage change in foot traffic, while the right panel plots percentage point increase in the share of all devices that stayed home. Both variables are smoothed over a 7 day window.

However, it is clear that the community response will differ once states have implemented policies targeted at inducing people to take physical distancing measures. In order to disentangle independent community reaction to COVID-19 from the response to policy measures, we estimate the following model:

$$comm_{i,t} = \alpha_i + \sum_{j=0}^{28} \beta_j \, j\text{-}days_{i,t} + \rho LDP_{i,t} + \sum_{j=0}^{28} \gamma_j \, j\text{-}days_{i,t} \times LDP_{i,t} + u_{i,t}, \tag{1}$$

where $comm_{i,t}$ is the community response variable for state i at time t, i.e. either percent of people who stay at home for the whole day, median time spent at home or median distance from home; $j\text{-}days_{i,t}$ takes the value of 1 if j periods have passed since the first death; $LDP_{i,t}$ is a dummy equal to 1 if the government has implemented the respective lock-down policy at or before period t; and α_i are state-fixed effects. Thus, the coefficient β_j estimates the community response on day j after the first death in case the government action has not been taken, relative to no death having occurred; and $\rho + \gamma_j$ estimates the additional community response if the government action is in place during that period, compared to the case when no death has occurred and no policy action has yet been implemented.

Figure 5 plots the resulting estimates from Equation 1, alongside 95% intervals, for the introduction of lock-down policies. The upper panel illustrates the change in the physical distancing measure in case that no such policy was in place (i.e. β_j for each $j \in \{0, 1, ..9\}$ from Equation 1), while the lower panel shows the additional change in the measure when the policy is in place (i.e. $\rho + \gamma_j$ for each $j \in \{0, 1, ..9\}$). The upper panel documents that even in the absence of government policies, communities take physical distancing actions in response to COVID-19, controlling for differences between states. However, the lower panel suggests that the community responses are stronger if the government also takes action. The estimates in

Figure 5 show that in states with lock-down policies in place, on the day after the first death the percent of people who stay completely at home is approximately 3 percentage points higher than in states without the policy. Similar responses are found for other measures of physical distancing (see Figures B.1 and B.2 in the Appendix). The response is also very similar when using county-level rather than state level data, where we use county instead of state fixed effects (see Appendix Figure B.3). Note that each estimate after the day of the first death pools together both states that have had the policy in place for multiple days as well as states that implemented the policy on the day of the estimate.

We can summarise two main findings so far. Firstly, people do respond to the COVID pandemic even in the absence of state-level policies. Secondly, state-level policies coincide with increased responses of the community in terms of physical distancing measures. However, our estimates do not yet yield insights about the causal response to policies. One issue is that an absence of community action can make the implementation of policies more likely, as we explore in section 3.3. Another issue is that there might be common factors that drive both physical distancing measures as well as the inclination of states to take action – such as the progression of the disease. In order to counter these potential endogeneity issues, in the next subsection we employ causal econometric methods to estimate the effect of shelter-in-place policies on physical distancing.

3.2 Effect of Government Action on Community Action

In order to account for potential endogeneity, we pursue two approaches in estimating the community response to the lockdown policies: first, we estimate a saturated staggered DiD specification where we use a rich set of controls for factors that might influence both community action and the implementation of policies. Moreover, we conduct the analysis at the county level and exclude the capitals of each state, arguing that county-level changes in foot-traffic outside of state capitals do not affect state-level policies. This combats the potential reverse causality problems which are explored in section 3.3. Second, we use a DiD-IV approach for groups of states that have experienced the first death on the same day. As an instrument, we use the number of required ventilators at the state level. We argue that, conditional on controlling for the spread of the virus, our instrument only affects community action by changing the probability of the policy being implemented.

3.2.1 Staggered Difference-in-Differences

As we have shown in subsection 3.1, communities respond to the pandemic even in the absence of any policy. Thus, our identification strategy requires that we control for the progression of the virus in each state in order for the common trends assumption to hold. Put differently, we need to guarantee that, conditional on our controls, counties in states that have not (yet) implemented a policy are a viable counterfactual for those that have. Another potential source of bias comes from non-random treatment assignment or time-varying treatment effects, as several recent

Figure 5: Percentage Point Change in Percent Completely at Home, Conditional on Lockdown

(a) Change if no Lockdown Enacted

(b) Additional Change if Lockdown Enacted
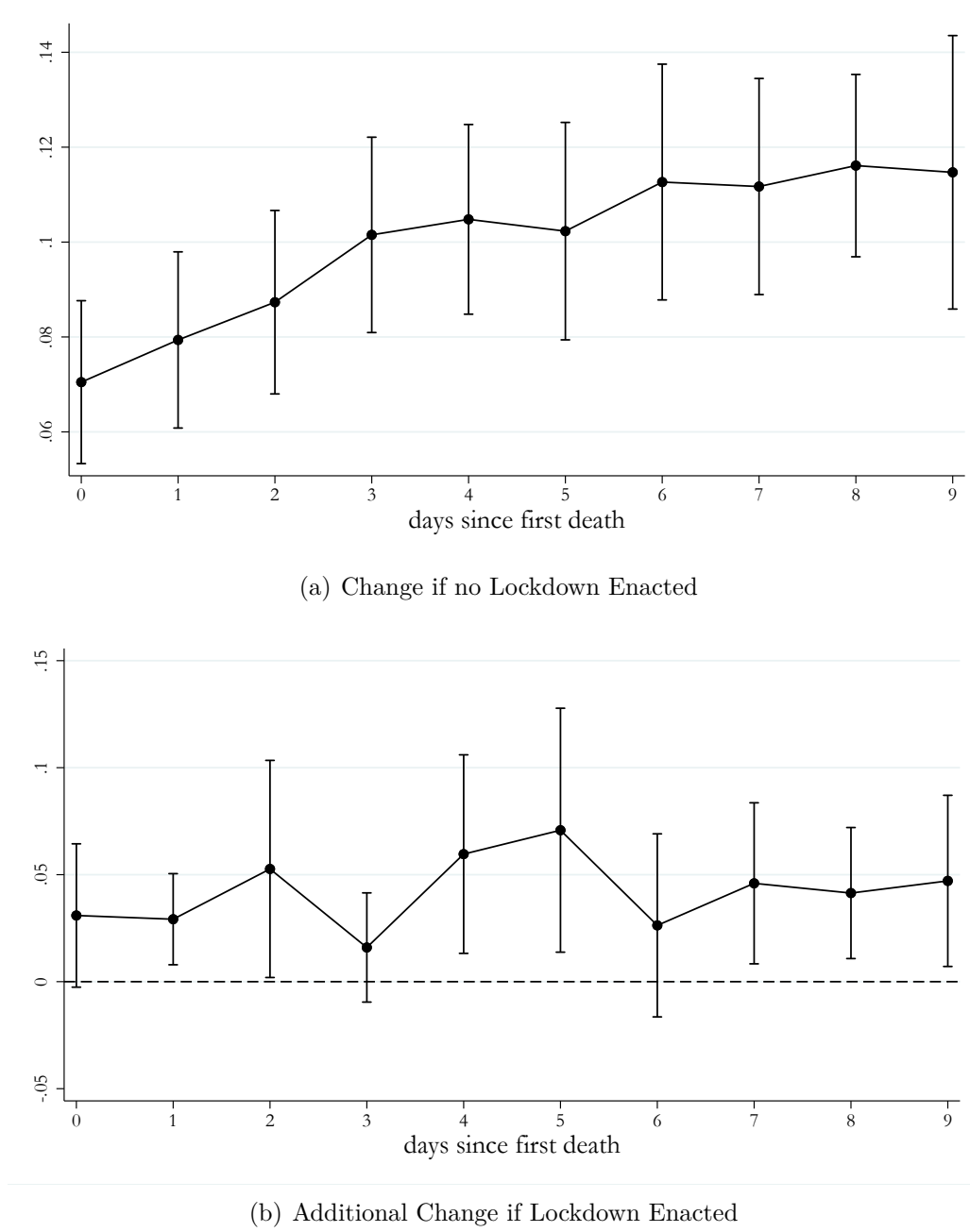
[1] **Top panel**: plots the estimated response of percent of devices at home by day since the first death in the state if no lockdown policy is in place, i.e. the β_js from Equation 1.
[2] **Bottom panel**: plots the estimated additional response of percent of devices at home by day since the first death if a lockdown policy was in place on that day, i.e. the $\rho + \gamma_j$s from the same Equation.

papers have demonstrated (Athey and Imbens, 2018; Goodman-Bacon, 2018). In what follows, we account for the stage of the pandemic each county is in by including days-since-first-case dummies and controls for the number of deaths and cases. We then assume that, conditional on controlling for the progression of the pandemic as well as county and time invariant factors, treatment assignment is indeed random. In that case, the staggered DiD estimates give an unbiased estimate of a weighted average causal effect.

With these caveats in mind, we proceed by estimating the following saturated staggered DiD model:

$$comm_{i,j,t} = county_i + \delta_t + \beta_{i,t} + \sum_{k=-5}^{11} \rho_k P_{j,t+k} + \mathbf{\Psi x_{i,t}} + u_{i,t}, \tag{2}$$

where $comm_{i,j,t}$ is the community physical distancing action under analysis in county i, state j and day t; $county_i$ are county-level fixed-effects; δ_t are day fixed-effects; $\beta_{i,t}$ are day-since-first-case fixed effects[21]; $P_{j,t+k}$ is a state-level policy dummy that is equal to 1 at time $t+k$ and 0 otherwise, where $k=0$ when state j implements the policy at time t; and $\mathbf{x_{i,t}}$ comprises the numbers of deaths and confirmed cases as controls. Note that we include the effect of the policy on previous community response as a placebo check on whether we control sufficiently for pre-policy implementation trends. We also exclude capital counties in order to further combat potential reverse causality issues.

Figure 6 shows the resulting estimates for this specification. The pre-implementation responses are insignificant. Once the policy is enacted, however, there is a marked increase over the subsequent days in the percentage of people who stay at home. We find that the implementation of a shelter-in-place policy increases the time spent at home by approximately 2 percentage points one day after its implementation, once taking account of any community responses due to the county-specific COVID-19 incidence and country-wide developments. Compared to a base of approximately 25.7% at the end of February, this amounts to a 8% increase in the time spent at home. Perhaps more importantly, the effect stays significant for subsequent days.

As a robustness check, we also estimate Equation 2 at the state level, with state instead of county fixed effects and using days-since-first-death instead of days-since-first-case fixed effects. The results are presented in Figure B.5 in the Appendix with the estimated effects being very similar, albeit somewhat larger.

3.2.2 Difference-in-Differences IV

As a further step to counter endogeneity issues, we group states by the day on which their first death occurred. This allows us to run difference-in-differences IV regressions for states that have experienced the first death on the same day. Grouping states by the incidence of the first death yields the advantage that it explicitly controls for part of the overall evolution of COVID-19 and thus makes the common trends assumption more viable.

We have seen in Figure 1 that there is a lot of variation across states in the timing of the first death caused by the coronavirus. Nevertheless, there are a number of states that share the date of the first death. Table 1 shows the occurrence of the first COVID-related deaths for dates at which at least three states experienced their first death. On six dates we observe a first death for three or more states on the same day. We will concentrate on the first four

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[21]The categorical variable employed here takes the same value for all time periods before the first case. Hence, it is an additional control to distinguish between periods before the outbreak of COVID with those thereafter.

Figure 6: Staggered DiD Estimates of the Policy Impact on Percent of Devices at Home
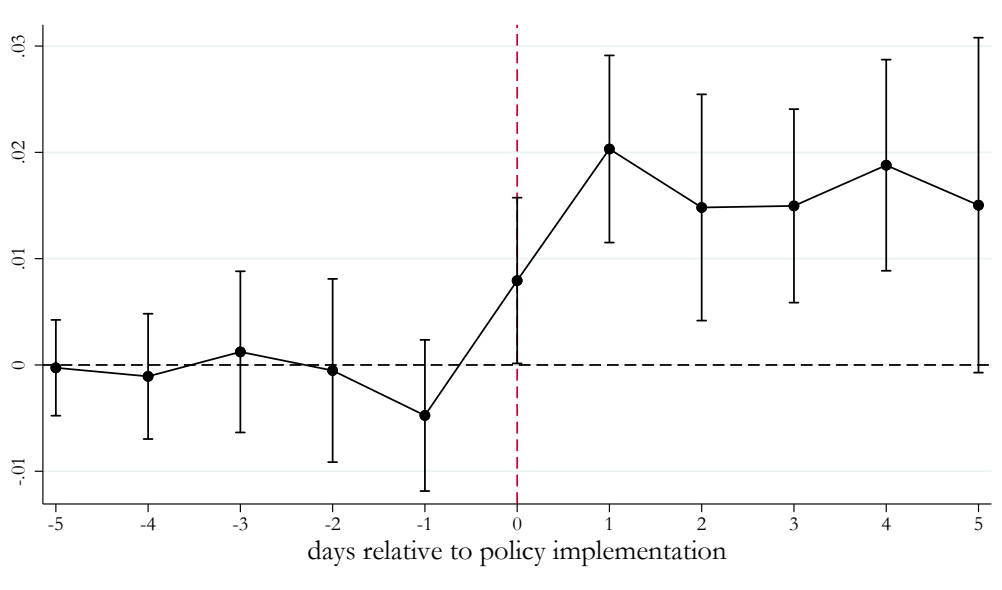
Note: The figure plots the ρ_k-coefficients and 95% confidence intervals from Equation 2 against the days relative to the implementation of a shelter-in-place policy.

of these, since the last two happen too close towards the end of our sample for a meaningful analysis. Note that there are early and late adopters for the groups of states with first deaths on March 14, 16 and 18. In contrast, the two states with first deaths on March 19 that do adopt safer-at-home measures implement those on the same date, March 25.

Table 1: Groups of States by Days since First Death

Date of first death	Number of states	State-ID
March 14	3	LA, NY, VA
March 16	4	IN, KY, NV, SC
March 18	4	CT, MI, MO, PA
March 19	5	MD, MS, OK, VT, WI
March 20	3	MA, OH, TN
March 25	4	AL, IA, NC, NM
Total until March 28	48	

Figure 7 shows the evolution of the median time spent at home since the first death occurred for each group of states. Panels (a) to (c) compare early adopters to never adopters, while in panel (d) both states that adopt a policy did so on the same day. For all groups under analysis, we can draw two broad conclusions: first, even unconditionally, states exhibit parallel trends before the implementation of the policy. Second, within a few days after states impose the shelter-in-place policy, their dwell-at-home time increases relative to states with the same first death date that do not adopt the policy.

Figure 7: Dwell Time at Home (Log), by Date of First Death from COVID-19

(a) First death on March 14

(b) First death on March 16

(c) First death on March 18

(d) First death on March 19
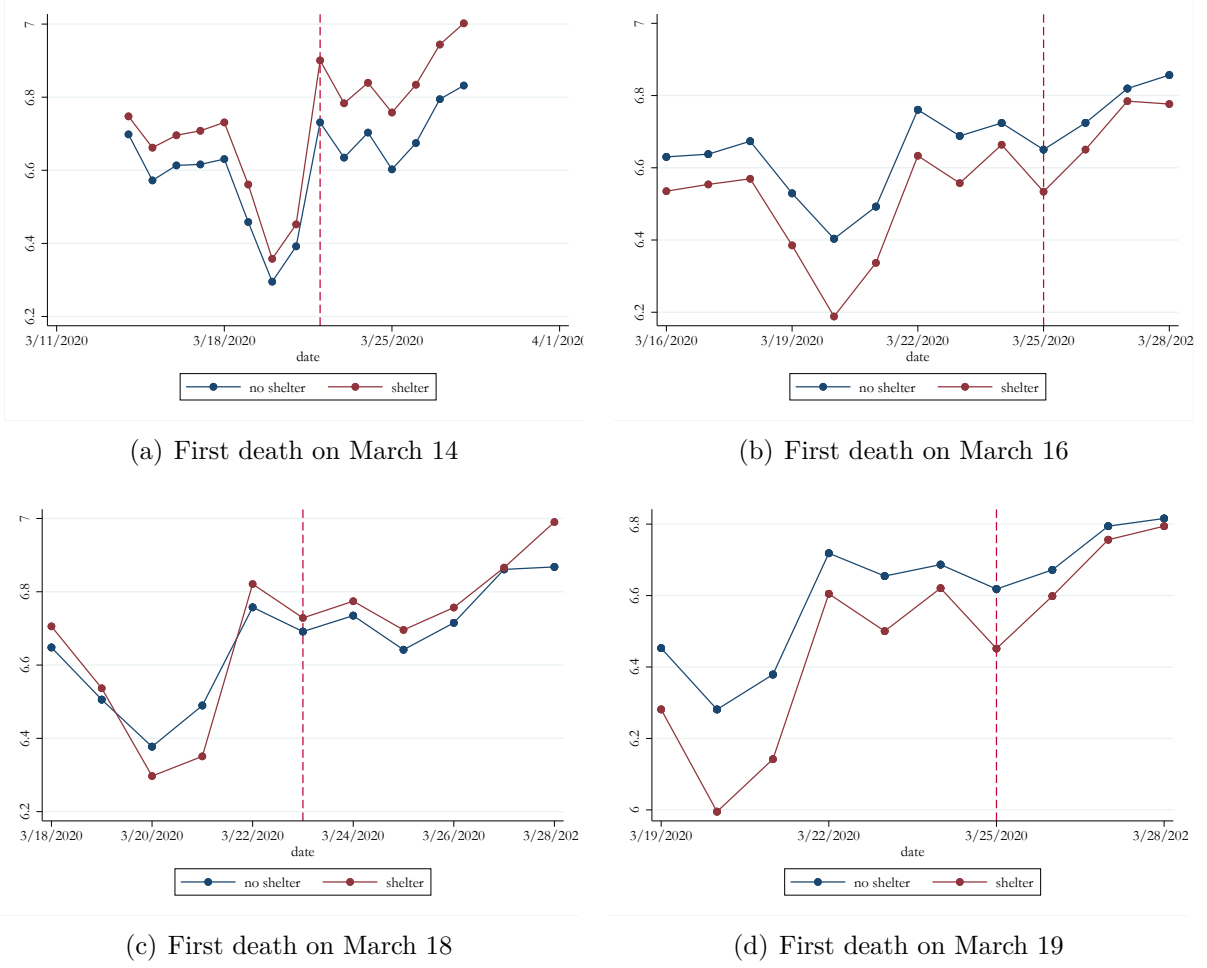
Note: The graph plots the log of median home dwell time for groups of states that had the same date of first death from COVID-19, by whether they implemented shelter-in-place policy early (red) or not at all during the sample. The red vertical lines indicate the first policy implementation date.

We now proceed to a difference-in-differences IV regression for states with the first death occurring on March 19. This date is particularly suitable for our analysis for two reasons. Firstly, it is the date on which most states share their first death. Secondly, the two states that implement a shelter-in-place policy do so on the same date, March 25. This helps to eliminate any remaining potential bias arising from the staggered DiD specification due to time heterogeneity in treatment effects and gives us the unweighted average treatment effect for the treated.

In order to estimate the causal effect of policy adoption, we estimate the following standard DiD specification in the second stage:

$$comm_{i,t} = \alpha_i + \delta_t + \rho LDP_{i,t} + \mathbf{\Psi x_{i,t}} + u_{i,t}, \tag{3}$$

where all variables are defined as before. Thus, ρ is the standard DiD estimate that captures the effect of implementing the policy.[22]

In the first stage, we instrument $LDP_{i,t}$ by the number of ventilators needed in a given state. Note that time-invariant differences across states will be eliminated by the fixed effects α_i. We argue that, conditional on controlling for the number of COVID-related cases and deaths, the number of ventilators needed will be driven by idiosyncratic factors that only affect community action through government measures. This is because the population can observe general statistics capturing COVID-related cases and deaths and respond to them, but cannot in real time observe the number of ventilators needed due to COVID-19. However, a higher need for ventilators will increase the pressure on governors to implement preventive shelter-in-place policies to mitigate the spread of the disease.

Note that for the IV specification, our estimates should be interpreted as those for 'compliers' – i.e. states that will only adopt measures if there is an experienced or projected shortage of ventilators, but would not do so otherwise. Given that there is a long time lag between the COVID-19 outbreak and the implementation of policies for many states, we believe that the existence of 'compliers' is highly likely. Nevertheless, there could also be some 'always takers' – states that would have adopted the policy even if there was no experienced or forecast pressure on their healthcare system.

Tables 2 and 3 report the estimation results for median dwell-time and median distance from home, respectively. In each table, the first column shows the simple DiD result without instrumenting the government action, while the second one contains the baseline IV results. Note that for the latter, the F-statistic in the first stage on the excluded variable is large at 80.6, indicating that our instrument is relevant. Moreover, the sign of the coefficient for our excluded instrument is intuitive: an increase in the amount of ventilators needed increases the probability of introducing a shelter-in-place policy. The number of deaths and of confirmed cases enters our DiD estimation results generally insignificantly; columns 3 and 4 show that our results are robust to excluding either of these controls.

Across both the standard DiD and the IV specifications, there is a large positive and significant effect of the government policy on the community response. According to our favoured IV specification in Table 2, the government response increases the dwell-at-home time by $exp(0.33) - 1 = 39\%$. Without the instrument, the effect is reduced to a still substantial $exp(0.134) - 1 = 14\%$. Given that the median dwell time across states from February 1 to February 15 was 12 hours (including sleep), this would mean that the shelter-in-place policy causes an average increase of 1.7 to 4.7 hours in daily time spent at home.

Note that the size of the results as well as their significance is strikingly similar when analyzing the response of the median distance from home (see Table 3). Again, we find that

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[22]Note that we do not include treatment indicators (whether a government has ever implemented a policy) since these are captured by fixed effects. Moreover, we do not incorporate post-treatment time indicators since we include the more flexible date dummies instead.

Table 2: DiD-IV Estimates of Effect on (Log) Home Dwell Time

	(1) DiD	(2) DiD-IV	(3) DiD-IV	(4) DiD-IV
Lockdown	0.134**	0.329***	0.311**	0.363***
	(0.0412)	(0.0244)	(0.0707)	(0.0736)
COVID deaths	-0.00815*	-0.0140	-0.0130	
	(0.00346)	(0.00790)	(0.00773)	
COVID known cases	2.14e-05	2.34e-05		-3.29e-05
	(9.12e-05)	(9.76e-05)		(0.000198)
State FEs	X	X	X	X
Date FEs	X	X	X	X
First-stage F (excl.)		80.60	14.12	17.99
Observations	285	260	260	260
R^2	0.887			

Note: This table reports various DiD estimates at the state level for those states that experienced their first COVID-related death on March 19, 2020.

Table 3: DiD-IV Estimates of Effect on (Log) Home Distance

	(1) DiD	(2) DiD-IV	(3) DiD-IV	(4) DiD-IV
Lockdown	-0.102***	-0.228**	-0.249**	-0.264*
	(0.0197)	(0.0640)	(0.0665)	(0.104)
COVID deaths	0.0107***	0.0145*	0.0157	
	(0.000772)	(0.00607)	(0.00960)	
COVID known cases	3.71e-05	2.75e-05		8.56e-05
	(0.000106)	(8.43e-05)		(0.000197)
State FEs	X	X	X	X
Date FEs	X	X	X	X
First-stage F (excl.)		80.60	14.12	17.99
Observations	285	260	260	260
R^2	0.921			

Note: This table reports various DiD estimates at the state level for those states that experienced their first COVID-related on March 19, 2020.

the estimated effect is larger once pursuing an IV strategy.[23] Our findings from Tables 2 and 3 suggest that the omission of an IV approach can lead to a downwards bias for the estimated causal effect of a shelter-in-place policy. There are two reasons to expect this result. First, it is likely that there are states which would always implement the policy, regardless of whether ventilators are lacking (i.e. 'always takers'). If such states also have a population that reduces traffic even in the absence of government policies, then the implementation of the policy will appear to have a small causal effect on traffic. In contrast, the IV estimates would be larger since they are only based on states which are 'compliers', and not on those who are 'always takers'.

Second, states with people who do not change their behaviour might be more inclined to introduce shelter-in-place policies. If people who are less likely to take action on their own are also less likely to change their behaviour following government action, then the estimated effect of state action will appear smaller than it truly is. In the following subsection, we will show that there are good reasons to believe that this line of argumentation holds true: states are indeed more likely to implement a policy if their population does not reduce foot traffic on their own.

3.3 Effect of Community Action on Government Action

In this section, we examine to what extent independent community action affects the probability of state governments introducing lockdown policies. From a theoretical point of view, the predicted sign of the effect is rather unclear. As shown in the previous section, people practice physical distancing even before the imposition of restraining measures, be it to minimize individual risk, to limit contagion within the community or because they anticipate the lockdown policies. Under this scenario, governors can introduce extensive measures to suppress the spread of the pandemic at fairly low political cost, yet the additional health effects from these lockdown policies would be comparatively small. The lower political cost would suggest stronger independent community action triggers stronger government action; the smaller effect on public health would suggest the opposite. By contrast, if the population refuses to sufficiently comply with non-compulsory calls for social distancing due to denial, defiance or to take advantage of free movement before an anticipated lockdown, governors' suppressive actions may be more effective in terms of health policy but come at higher political costs.

To evaluate the impact of community action on the probability of governors introducing lockdown policies, we estimate the following equation:

$$LDP_{i,t} = \alpha_i + \delta_t + \beta_j \, comm_{i,t-8} + \mathbf{\Psi x_{i,t}} + u_{i,t} \tag{4}$$

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[23]In contrast, we do not find significant results when employing the percent of devices that stay at home as an outcome variable instead. We conjecture that this is due to the nature of the variable, which is a raw count of the number of devices that stay completely at home, divided by the total number of devices in the state. Thus, this variable will capture some unrelated variation, and potentially be more prone to sampling biases at the CBG level. In contrast, the distance from home and dwell time at home variables capture movements in the median CBG of the median county for each state, and thus are less prone to these problems.

where $LDP_{i,t}$ is 1 on the day of state i's announcement of the lockdown policy in question–after which the state drops out of the sample – and 0 before; and $comm_{i,t-j}$ denotes the jth lag of the physical distancing measure in question. We alternatively capture community action by total foot traffic, percentage of devices staying completely at home, and by median home dwell time, and find similar results across the board. In addition, the regression includes state and days fixed effects, α_i and δ_t, along with a vector of covariates $\mathbf{x_{i,t}}$ that controls for the cumulative number of confirmed cases and deaths by state.

We adopt an Instrumental Variables (IV) strategy to assess how community action affects the probability of lockdown policies being imposed. For policymakers, the most visible indicator for compliance with the call to physical distancing is public foot traffic. However, if the community anticipates the imposition of a lockdown policy, then lagged community action will also be affected by the future imposition of any such policies. As a result, Ordinary Least Squares (OLS) estimates of the effect of independent community action on the imposition of suppressive government-imposed lockdown policies will be biased. To account for this effect, we estimate the J different β_js by means of two-stage least squares, where we instrument the physical distancing measures with the maximum temperature, minimum temperature, precipitation and interaction of max temperature and precipitation in the state capital. If the temperature is high or precipitation low, individuals are more likely to leave their homes. Governmental intervention, though, should not be directly affected by weather, except insofar as it impacts individual behavior. Therefore, the instruments $TMAX$ and $PRCP$ should be relevant and satisfy the exclusion restriction.

To meaningfully interpret the results from Equation 4, we restrict the sample as follows. First, states only enter the panel from the moment they have 10 confirmed cases of COVID-19 onward. We do this because community response before this point is unlikely to affect future government interventions much – or vice versa, for that matter. In other words, we assume that people only start anticipating a state government lockdown policy from the moment the virus has gained foothold in their state. Second, we drop states after they announce the lockdown policy. The measures we consider – school closure, shelter-in-place and business closure – are all implemented for a predetermined period, with a fixed future reevaluation date. Therefore, the decision to implement them is a one-off decision, and leaving states in the panel after announcement would lead to spurious identification, as community action after announcement ceases to affect the probability that governors implement the lockdown policy after that point. Note that in this section, we use the announcement date as the government policy variable, instead of the date of implementation, because we care about the governors' *decision* to implement, not when the implementation is actually followed through.

Table 4 reports estimates for the second-stage regression of the shelter-in-place dummy on our physical distancing measures. As a first caveat, note that we are estimating a linear probability model (LPM). It is well-known that the estimates of a LPM are biased and inconsistent whenever any of the predicted probabilities lie outside the unit interval (Horrace and Oaxaca, 2006). Nonetheless, the marginal effects can be consistently estimated. Moreover, we are not necessarily

Table 4: Second Stage: Regression of Shelter-in-Place on Physical Distancing

	Dependent variable:		
	Shelter in Place		
	~Traffic Capital	~Traffic State	~%Home All Day
	(1)	(2)	(3)
L8.y	2.075**	2.459***	−2.060**
	(0.940)	(0.943)	(0.966)
State FEs	X	X	X
Day FEs	X	X	X
F-Stat 1st Stage	7.526	16.898	13.569

[1] Dependent variable is shelter-in-place dummy, independent variables are various physical distancing measures. Controls include nr. of deaths and number of positive cases.
[2] * $p<0.1$; **$p<0.05$; ***$p<0.01$

interested in the precise magnitude of our estimates, but rather in their sign. That said, we find a significant and positive effect of increased social traffic on the probability that state governments will move to announce a shelter-in-place policy a week later. In other words, if people engage less in physical distancing by themselves, state governments are more likely to impose compulsory measures to that effect. For example, a 1% increase in state-level foot traffic increases the probability of the imposition of a lockdown policy by 2.5%. The magnitude and significance of this coefficient are robust to alternative specifications with different combinations of lags, while the other lag coefficients remain insignificant across various specifications. The week-long lag of community action on government action seems to conform with the delay that usually marks data collection and policy decision-making. We can thus conclude that, not only do state lockdown policies affect people's social distancing behavior, but the prevalence of such behavior before the implementation of a shelter-in-place policy also decreases the probability of that policy being introduced.

3.4 County Analysis: Heterogeneity in Virus Response and Treatment Effects

We now dig deeper into the data by looking at government and community action at the county level. While many county and most state governments similarly recurred to drastic lockdown policies such as shelter-in-place policies as they came under pressure from the rapid spread of the virus, it is less clear that all subgroups of individuals responded to the spread of the virus and the policies implemented in similar ways. In this paragraph, we, therefore, exploit the variation coming from daily data for the more than 3,000 counties to explore the heterogeneity in outbreak and treatment response among different demographic, cultural and economic lines. In what follows, we focus on the percentage of people who stay completely at home as the main measure of social distancing, and shelter-in-place as the lockdown policy of interest. Note that
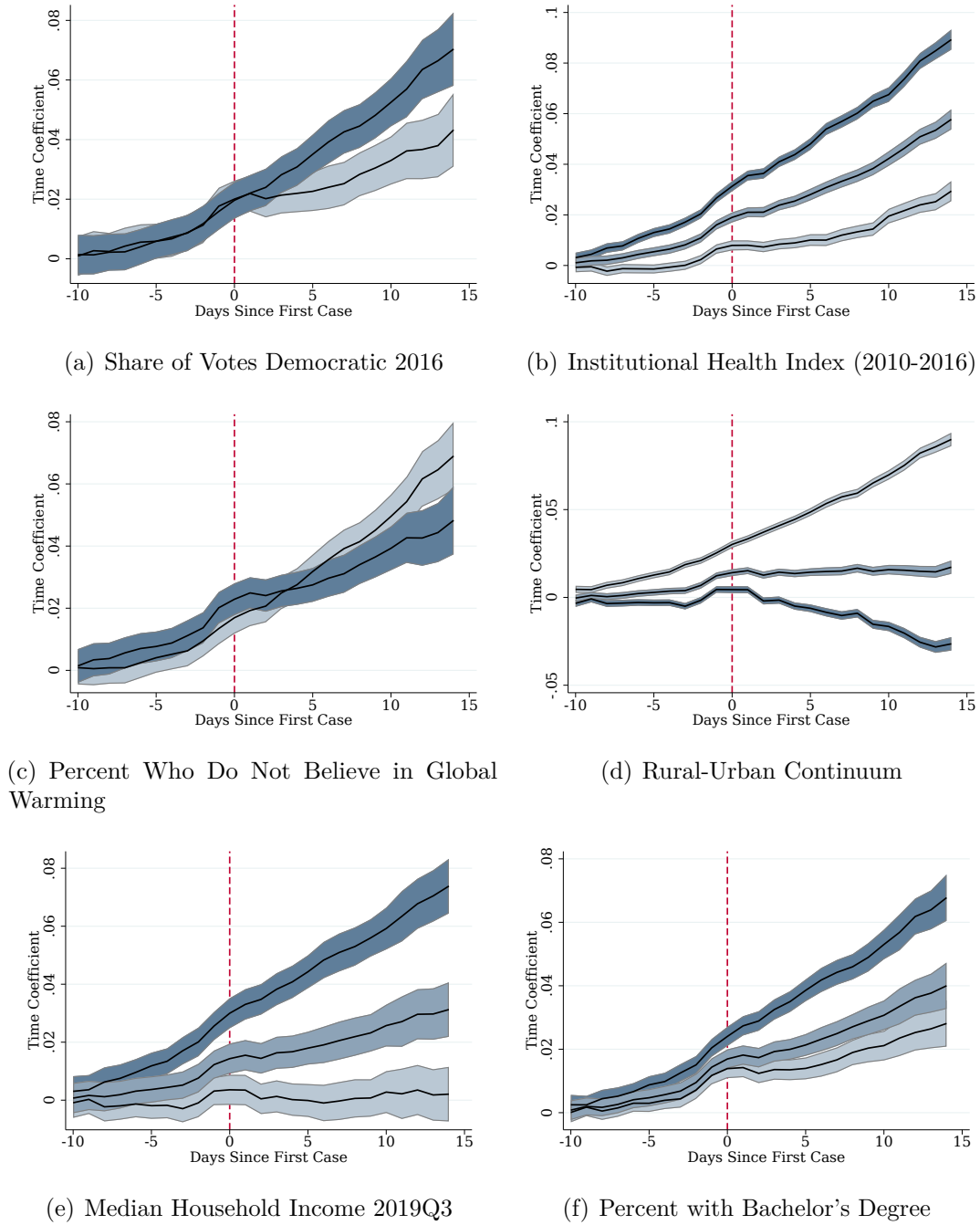
the interpretation of the dependent variable's response is in percentage points (p.p.) throughout the analysis.

Figure 8 plots the evolution of the outcome variable over time, for quantiles of the distribution of several variables of interest. Specifically, it shows the estimates of the regression coefficients on a set of time dummies referring to the 10 days before and 15 days after the first confirmed COVID-19 case in the county, interacted with the variables of interest evaluated at their 10th (light blue), and 90th percentile (dark blue). Also included in the regression are state-day fixed effects and county fixed effects. These allow us to control for state-specific time-varying shocks, as well as any county-level characteristics that remain constant over the period considered, apart from the characteristic we are interacting with. In other words, when considering differences in outbreak response by income group, we are at the same time controlling for time-invariant occupational differences, such that our results cannot be attributed solely to, for example, the fact that lower income groups are less able to work from home (unless the two features exactly overlap). As such, we can neatly disentangle the specific subgroup-level social distancing response to the spread of the virus and to the county-level policies from other factors merely correlated with the social distancing behavior of these groups. Note that we do not control for the policy implementation, so the estimated effects are average effects for counties that did not implement lock-down policies as well as those who did. This means that the reported subgroup heterogeneity can be both due to different voluntary distancing across the subgroups, as well as different frequency of policy implementation. For example, highly educated counties might see residents engage more in voluntary distancing, but might also see county governments implement lockdown policies more often. As a general measure of heterogeneity in outbreak response, both effects are of interest. However, we also further disentangle these effects below.

The figure shows stark differences in the evolution of the physical distancing behavior of the various subgroups as the virus spreads. The top left graph plots this evolution for the cross-county shares of Democratic votes in the 2016 presidential election.[24] The divide between those counties that voted strongly for Clinton and those that did not starts opening up a few days after the first confirmed case. After 15 days, the difference is 3 p.p., which means the percentage of devices that stayed home increases by 8% more for Clinton counties compared to the February mean of ~23% of devices. A similar difference can be observed in the middle left graph when we look at the evolution of counties with different shares of people who do not believe in global warming, which we consider a proxy for distrust in science. While these findings are in line with well-documented dividing lines of trust in science by political party – with 69% of Republicans saying global warming is exaggerated compared with 4% of Democrats (Gallup, 2018) – it is striking how strong of a role they seem to play even during a pandemic whose effects are starkly visible in daily life. At the same time, even counties with very low values of trust in science see social distancing increase by up to 4 p.p., or ~6% compared to the February mean. A further remarkable difference is the one between counties with lower and higher median

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[24]Note that subplot a) and c) only plot the 10th and 90th percentile, as the confidence intervals overlap with the plot for the median.

Figure 8: Heterogeneity in Community Outbreak response Over Time, % At Home

(a) Share of Votes Democratic 2016

(b) Institutional Health Index (2010-2016)

(c) Percent Who Do Not Believe in Global Warming

(d) Rural-Urban Continuum

(e) Median Household Income 2019Q3

(f) Percent with Bachelor's Degree

[1] Light blue: 10th percentile; blue: median; dark blue: 90th percentile - for variable of interest across counties. Dependent variable: percentage of devices fully at home.
[2] Shaded area is 95% confidence interval. Observations: 114,580.
[3] Figure plots estimates of dummies for days since 1st confirmed COVID-19 case interacted with variables of interest in panel regression with state-day fixed effects and county fixed effects.

household incomes. Not only do high-income counties ramp up their physical distancing by up to 8 p.p., or almost 30%, more than low-income counties when the virus gains foothold in the county; they also strongly anticipate the arrival of the virus. The median-income counties respond moderately, while low-income counties barely see an increase in the share of people

staying home. The results in counties with higher and lower shares of college-educated people follow a similar trajectory, though the differences are less stark, and groups with less education still engage in increased physical distancing. As expected, more urban areas experience a much stronger increase than rural areas, with the most rural counties actually seeing a decrease in the percentage of devices staying completely home.[25] A last conspicuous pattern, in the top right graph, is that counties where institutions are in better standing see a markedly larger increase in physical distancing both before and after the virus takes hold.

To further assess how these different subgroups respond to lockdown policies, we re-estimate the staggered difference-in-differences model with state-day and county fixed effects, where we interact the dummies for days since policy implementation with the variables of interest. This entails the following triple DiD extension of Equation 2:

$$comm_{i,j,t} = county_i + \delta_{j,t} + \delta_t G_{i,t} + \beta_{i,t} + \sum_{k=-5}^{20} \rho_k P_{i,t+k} + \sum_{k=-5}^{20} \rho_k P_{i,t+k} \times G_{i,t} + \mathbf{\Psi x_{i,t}} + u_{i,t}, \ (5)$$
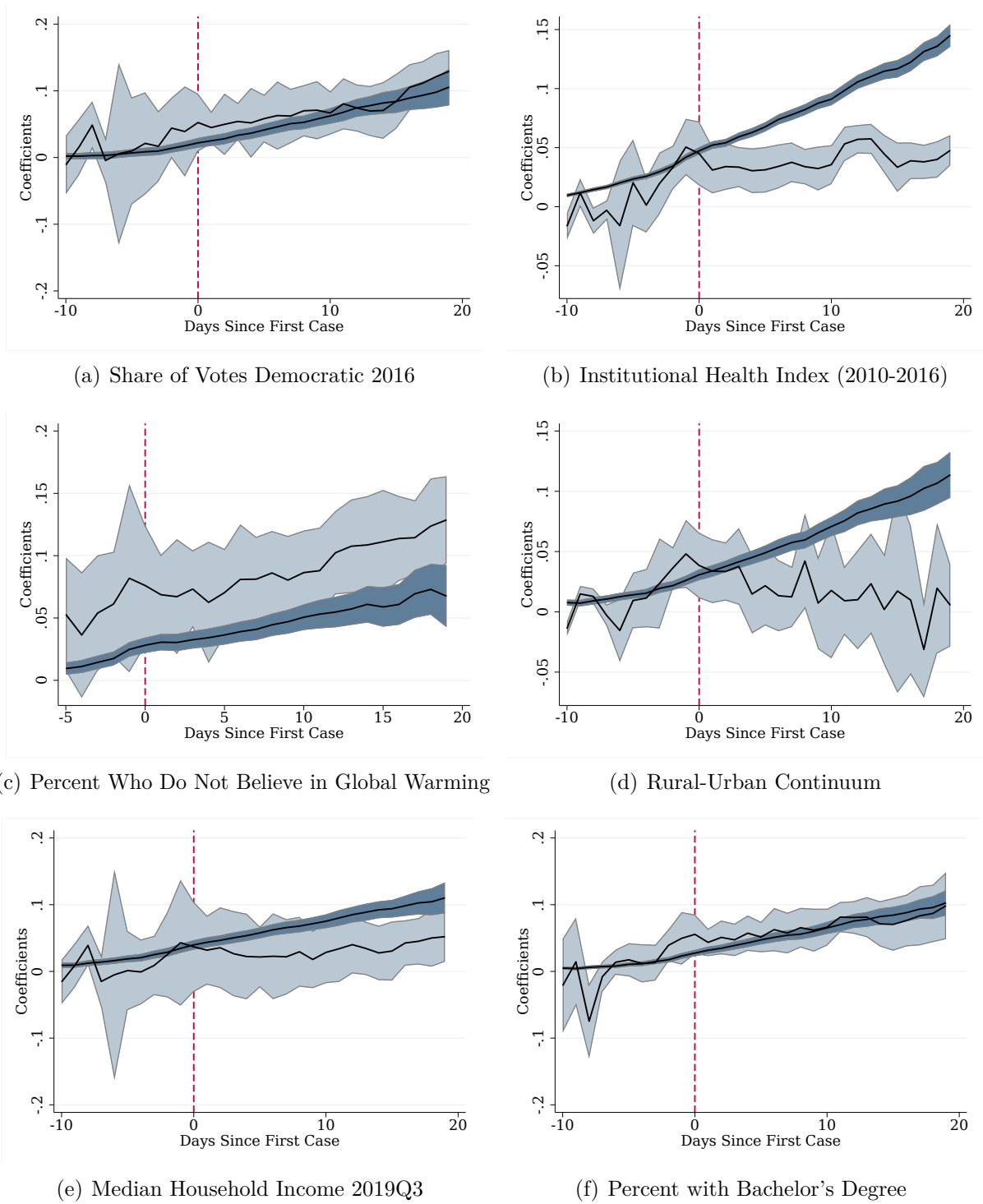
where we add, for county i, state j and day t, the group variable $G_{i,t}$ interacted with day fixed effects and with the staggered DiD dummies $P_{i,t}$, as well as state-day fixed effects $\delta_{j,t}$. We also include a control for whether the county already has a business closure policy in place. The rest of the equation remains the same. Note that the inclusion of day fixed effects interacted with the variable of interest is crucial to recovering the interpretation of the DiD estimate as the counterfactual treatment effect.[26] We also control for cumulative number of confirmed cases and deaths in each county, and double-cluster the standard errors by county and date.

Figure B.6 in Appendix thus shows how the effect of a county-level shelter-in-place policy on physical distancing differs across county subgroups. For most subgroups, we do not find evidence for significantly different responses to a shelter-in-place policy, apart from a marginally significantly stronger response for the poorest county compared to the richest county in Figure B.7 in Appendix. A remarkable exception, however, is that people in highly urbanized areas seem to respond much more strongly to shelter-in-place policies than people in heavily rural areas. This could indicate that the treatment response to such policies largely depends on enforceability – which would also explain why most of the other heterogeneity seems to matter little for treatment response even as it matters a lot for outbreak response. Moreover, these results suggest that most of the heterogeneity documented in Figure 8 is due to subgroup differences in either voluntary physical distancing or in the frequency with which shelter-in-place policies are implemented, not due to different treatment response. Similar results obtain when

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[25]Note that for the rural-urban continuum index, lower values mean more urban. Also note that the decrease for rural areas does not necessarily indicate that they do not engage in physical distancing at all. It might simply indicate that, forced to stay at home, residents in such areas go out for short trips more often than otherwise.

[26]Our analysis suggests that the results obtained by Painter and Qiu (2020) are driven by the fact that they fail to incorporate these additional interactions in the regression. Thus, we argue that the presumed heterogeneity in treatment response along party lines can be largely explained by democratic and republican counties differing in voluntary physical distancing and frequency with which lock-down policies are imposed – as in Figure 8 –, rather than different treatment response to shelter-in-place policies.

Figure 9: Physical Distancing in Counties With and Without Lockdown, by Subgroup

(a) Share of Votes Democratic 2016

(b) Institutional Health Index (2010-2016)

(c) Percent Who Do Not Believe in Global Warming

(d) Rural-Urban Continuum

(e) Median Household Income 2019Q3

(f) Percent with Bachelor's Degree

[1] Light blue: 10th percentile with shelter-in-place policy in place; dark blue: 90th percentile without shelter-in-place policy in place - for variable of interest across counties.

[2] Shaded area is 95% confidence interval. Standard errors are double-clustered by county and date. Observations: 180,295.

[3] Figure plots DiD estimate for days since 1st confirmed COVID-19 case interacted with variables of interest, in panel regression with state x day, variable of interest x day and county fixed effects.

considering state-level shelter-in-place policies, suggesting that the results of Painter and Qiu, 2020 should be interpreted with caution, as explained in Footnote [26].

In Figure 9, we further explore the implications of the above findings by re-estimating the triple DiD with days-since-first-case dummies as the relevant time dimension, as in Equation 1. This allows us to separate the voluntary physical distancing response from the overall response, that is, the sum of voluntary and imposed distancing. We do this to compare the total increase in physical distancing of less-responsive groups to the voluntary increase of more responsive groups. That way, we come to the striking result that subgroups that take more independent action end up increasing their physical distancing in the absence of any policy by nearly as much as their counterpart subgroups do under an imposed lockdown. For example, in highly urban areas (middle right plot), people engage more intensely in voluntary physical distancing in response to the virus than people in heavily rural areas do even when there are lockdown policies in place. Similarly striking results are obtained for counties with institutions in good standing (top right) and for rich counties (bottom left). For the other variables, our findings indicate that the lockdown and voluntary response of both groups are not significantly different from each other. The reason for these findings is as above: for all subgroups at the 90th percentile, the voluntary physical distancing response is very high compared to their counterparts at the 10th percentile, however, the treatment response of both is the same. For example, counties where trust in institutions is high engage in voluntary physical distancing much more than those where such trust is low. Yet, when the government implements an additional lockdown policy, both high-trust and low-trust counties increase their physical distancing by the same additional amount. This means that lockdown policies, even when only implemented in low-trust counties, cannot close the large gap in voluntary response shown in Figure 8. These findings point towards the important conclusion that containment measures targeted at population subgroups that less easily engage in voluntary physical distancing by themselves might be more effective than indiscriminate lock-down policies. Given evidence from the epidemiological literature that the distribution of individual infectiousness in epidemics is often highly skewed around $R(0)$, it seems likely that there are decreasing marginal returns to additional imposed physical distancing as the level of voluntary physical distancing increases (Lloyd-Smith et al., 2005). In other words, it seems likely that there are larger returns to the same increase in physical distancing for those subgroups that barely change their behavior by themselves than for those groups that already heavily engage in voluntary physical distancing. Moreover, not only should such targeted containment policies be more effective from the epidemiological point of view – they should also wreak less economic havoc than full-scale lockdowns. Thus, we conclude that containment policies targeted along socio-economic lines are likely to be more effective at containing the outbreak than total lockdowns, while also leading to less economic damage. Insofar as the groups we identify as less responsive are also typically more exposed to the effects of lockdown policies – with, for example, poor people often being less able to work from home –, this should additionally help avoid a further widening of the socio-economic chasms that drive the differences in response.

Lastly, we investigate how the effect of a county-level shelter-in-place policy differs depending on which state-wide policies are already in place. To this aim, in Figure 10, we plot the responses

of the percentage of people staying fully home to a county-level shelter-in-place policy, for counties where a state-wide policy is already in place (dark blue) and where it is not (lighter blue). Though the confidence intervals are wide, a few patterns can be observed. First, when there is no state-wide school closure in place, the initial response of the county-level share of people staying home to a county-level shelter-in-place policy is much more pronounced, though it seems to decrease quite quickly after. When there is no state-wide business closure policy in place, there is not much difference in initial response to when there is. However, the response seems to decline quicker, possibly because without a state-wide business closure, people are tempted to defy the shelter-in-place order and go out. Finally and expectedly, when there is no state-wide shelter-in-place policy in place, implementing a county-wide stay-at-home order elicits a persistently higher response.

Figure 10: Interaction Between County- and State-Wide Policy, for County Shelter-in-Place

(a) State-Wide School Closure in Place

(b) State-Wide Business Closure in Place

(c) State-Wide Shelter-in-Place Order in Place
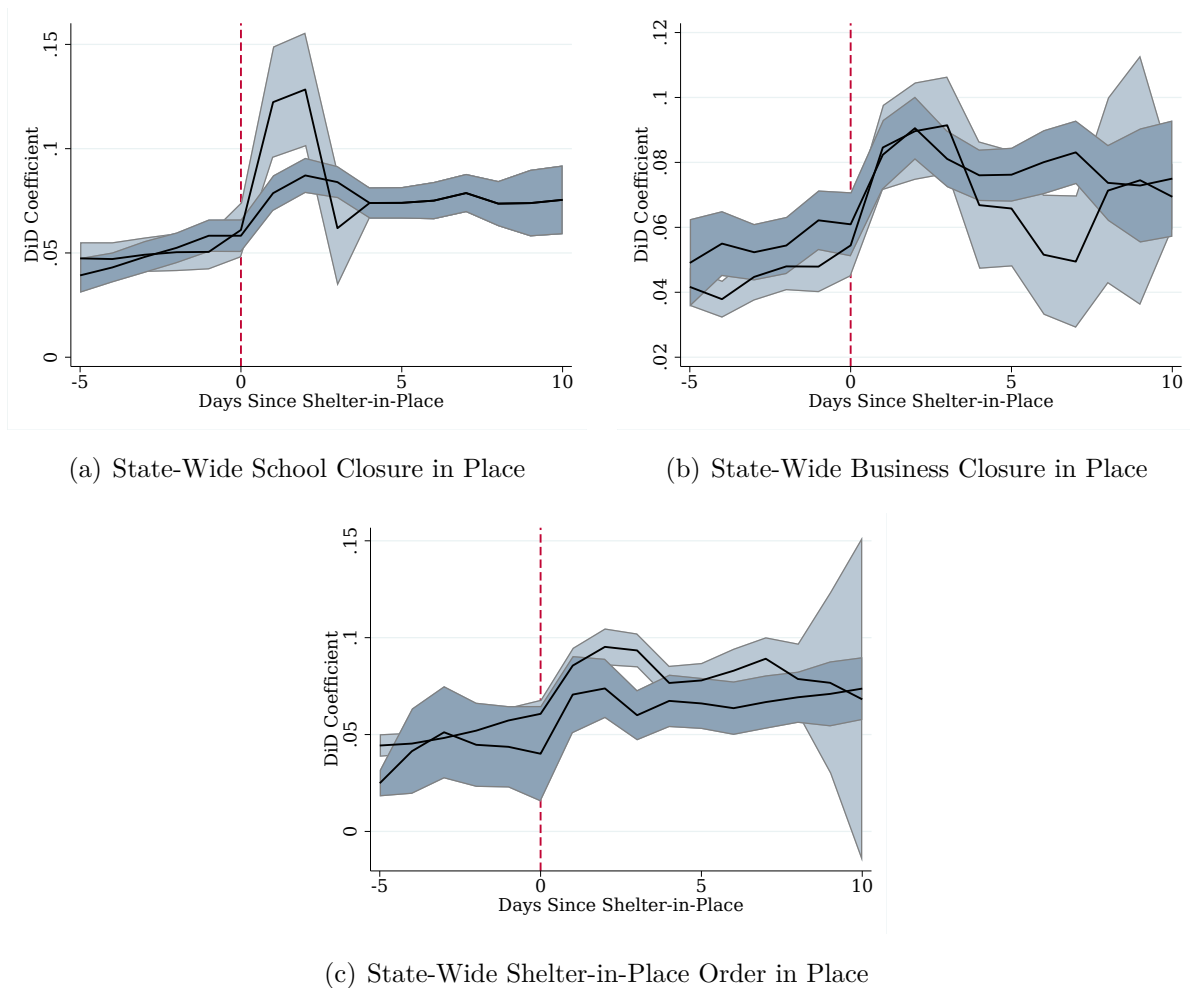
[1] Lighter blue: state-wide policy not in place; darker blue: state-wide policy in place.

[2] Shaded area is 95% confidence interval. Standard errors are double-clustered by county and date. Observations: 180,307.

[3] Figure plots DiD estimate for days since 1st confirmed COVID-19 case interacted with variables of interest, in panel regression with state-day fixed effects and county fixed effects.

4 Conclusion

The outbreak of the COVID-19 pandemic has seen local and national governments around the world scramble to implement policies aimed at constraining social interaction so as to dampen the spread of the virus, relieve the pressure on hospital systems, and save lives. While the drastic nature of such lockdown policies all but guarantees that they reach their desired response, no credible counterfactual estimates of these policies' causal effect on people's social interactions exists so far. This paper aims to fill this gap by studying the interaction between state- and county-level lockdown policies and individuals' physical distancing behavior, using a panel dataset based on 40 million smartphone devices across the United States, combined with detailed data on state- and county-level government policies.

That way, we find that lockdown policies can bring about a counterfactual increase in the time people spend at home of up to 39%, even as our results suggest that individuals also decrease their social interactions to a more limited extent in the absence of any such policies. Moreover, we find evidence that when individuals engage more in such voluntary physical distancing, the likelihood of governments implementing restrictive measures decreases. Furthermore, we weigh in on the debate about the benefits of imposed versus voluntary physical distancing by documenting that in highly urbanized areas that are less distrustful of science, more highly educated, have higher incomes or have stronger institutions, people react more strongly to the outbreak of the virus, even in the absence of lockdown policies. Our results complement earlier epidemiological research that shows that the distribution of individual infectiousness rates in epidemics tends to be highly skewed around the basic reproductive number R(0). Together, these findings strongly indicate that less restrictive containment policies targeted along socio-economic lines are likely to be more effective at containing the outbreak than total lockdowns, while also leading to less economic damage. Lastly, we show that county-level policies tend to have a more pronounced impact when they are implemented with no state-wide policies in place, suggesting that coordination of government response at different levels can further improve outcomes.

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A Tables

Table 5: Description of Key Social Distancing Variables

Product	Variable	Description Raw Data	Aggregation
Social Distancing Metrics	**Home Distance**	Median distance traveled from the geohash-7 of the home by the devices included in the device_count during the time period (excluding any distances of 0). We first find the median for each device and then find the median for all of the devices.	Median of all CBGs in county/state.
	Home Dwell Time	Median dwell time at home geohash-7 ("home") in minutes for all devices in the device_count during the time period. For each device, we summed the observed minutes at home across the day (whether or not these were contiguous) to get the total minutes for each device. Then we calculate the median of all these devices.	Median of all CBGs in county/state.
	Share at Home	Out of the device_count, the number of devices which did not leave the geohash-7 in which their home is located during the time period.	Sum over all CBGs in county/state.
	Percentage at Home	NA (constructed variable)	Sum of Share at Home for all CBGs in county or state / sum of Total Device Count for all CBGs in county or state.
Weekly Patterns	**Traffic**	Number of visits in our panel to this POI during the date range.	Sum of total raw visit counts per day for all POIs in state/county, normalized by total number of unique devices observed in given month.

Note: Description Raw Data replicates the data description provided by SafeGraph here.

Table 6: Summary Statistics

	N	Mean	SD	P5	P25	P50	P75	P95
SafeGraph Data:								
Traffic	2935	13.78	16.97	0.88	2.91	9.12	16.84	46.10
Median dist. from home	2907	6563.37	1434.76	4433.50	5590.00	6450.50	7404.00	8982.00
Time dwelled home	2907	672.07	114.04	470.00	617.00	677.00	730.00	861.00
% devices stayed home	2907	25.37	6.97	16.83	20.02	23.56	29.50	39.09
Share fulltime workers	2907	17.04	3.73	10.87	14.21	17.30	19.75	23.07
COVID-19 Data:								
Deaths JH	2907	3.06	22.61	0.00	0.00	0.00	0.00	10.00
Confirmed cases JH	2907	204.27	1798.36	0.00	0.00	0.00	18.00	626.00
Positive tests CTP	1145	394.18	2311.14	0.00	5.00	32.00	161.00	1328.00
Negative tests CTP	1040	2536.53	7102.81	10.00	73.00	330.00	2008.00	12432.00
Instruments:								
Max. temperature	2901	11.82	8.54	-1.10	5.60	11.10	17.80	27.20
Precipitation	2902	2.63	6.74	0.00	0.00	0.00	1.50	14.50
Inv. ventilation needed	2652	17.06	116.96	0.00	0.00	0.00	0.57	65.29
State-Level Demographics:								
Population in 1000	2907	6415.05	7272.21	702.46	1754.21	4468.40	7535.59	21299.33
Mean age	2907	39.52	1.64	36.80	38.65	39.45	40.38	42.45
Share > 65	2907	15.73	1.92	12.13	14.86	15.69	16.76	19.16
Share of asian-american	2907	5.62	7.94	1.23	2.36	3.75	5.87	11.45
Share college degree	2907	32.61	6.70	23.83	28.13	31.28	35.61	42.73
Unemployment rate	2907	2.79	0.59	2.06	2.37	2.80	3.01	3.64
Labor force participation	2907	63.52	3.82	56.22	61.25	63.70	66.49	68.93

Note: see section 2 for a detailed description of the data.

B Figures

Figure B.1: Proportional Change in Median Dwelling Time, Conditional on Safer-At-Home Policies
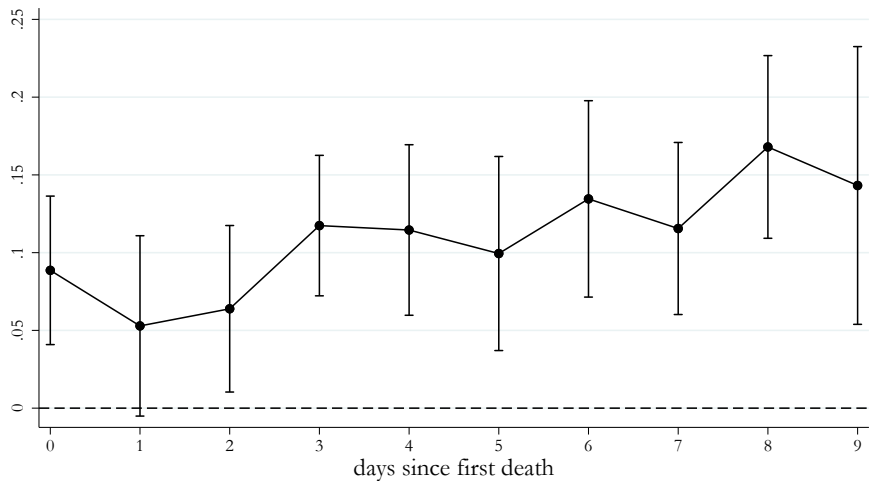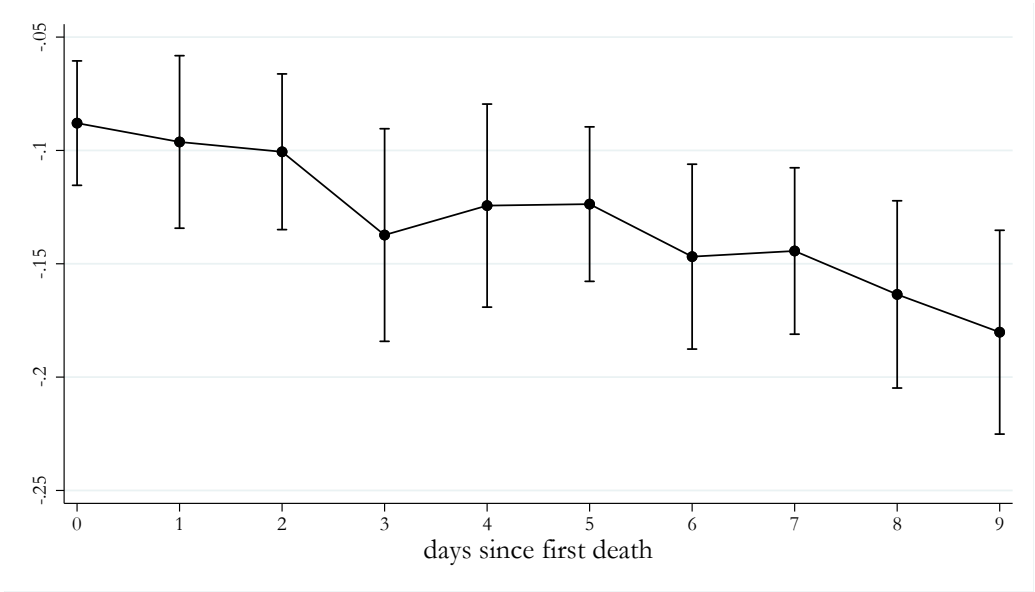

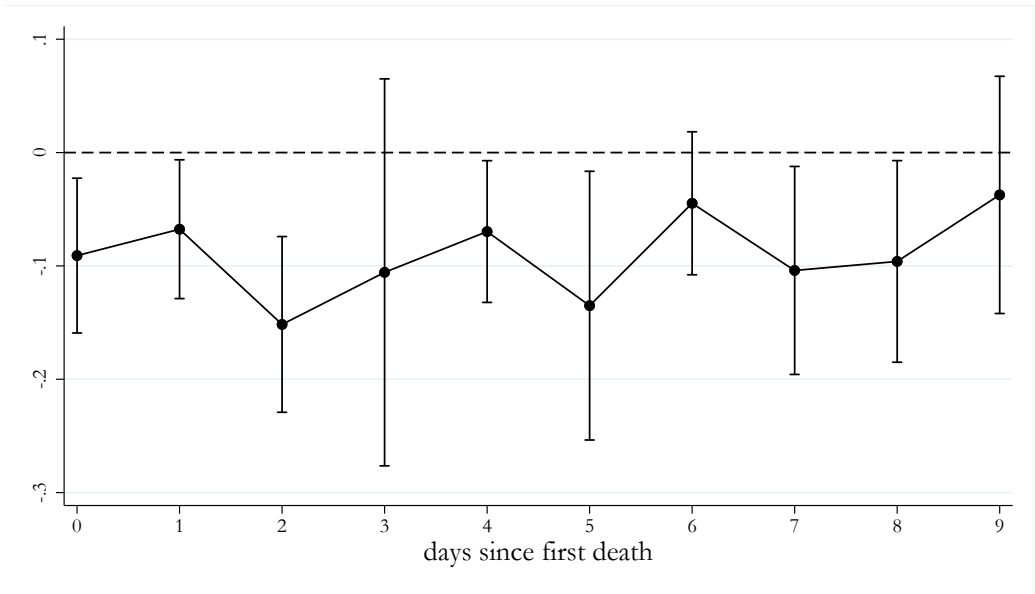(a) Proportional Change if no Lockdown Enacted (Relative to no Death)
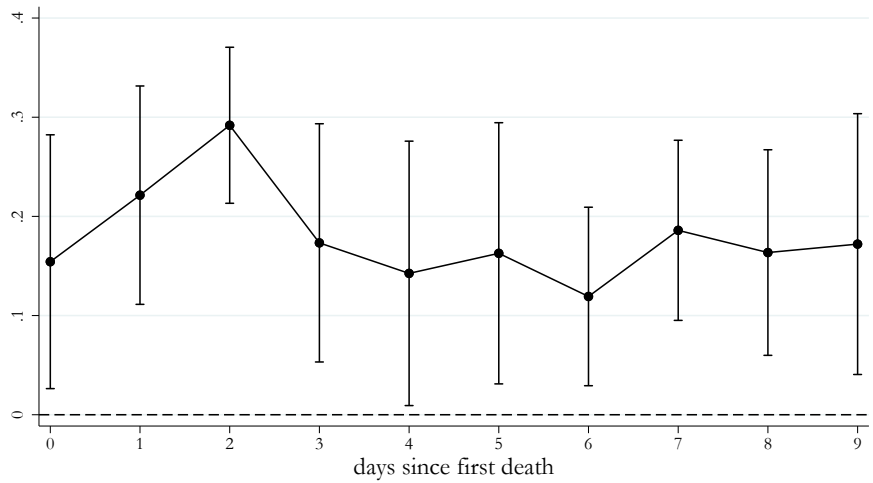
(b) Additional Change if Lockdown Enacted (Relative to no Death and no Policy Enacted

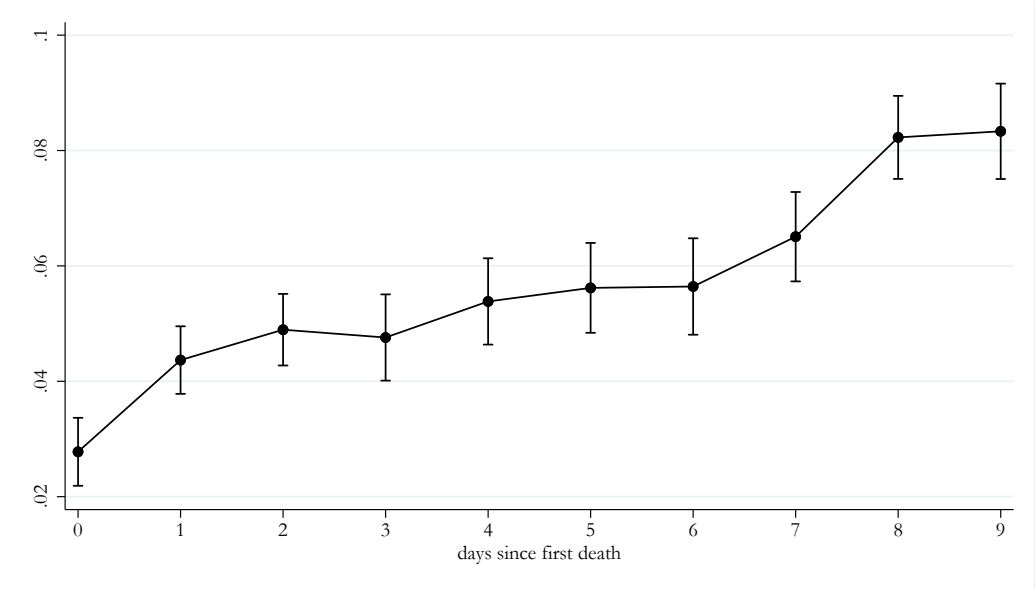**Figure B.2:** Proportional Change in Median Distance from Home, Conditional on Safer-At-Home Policies

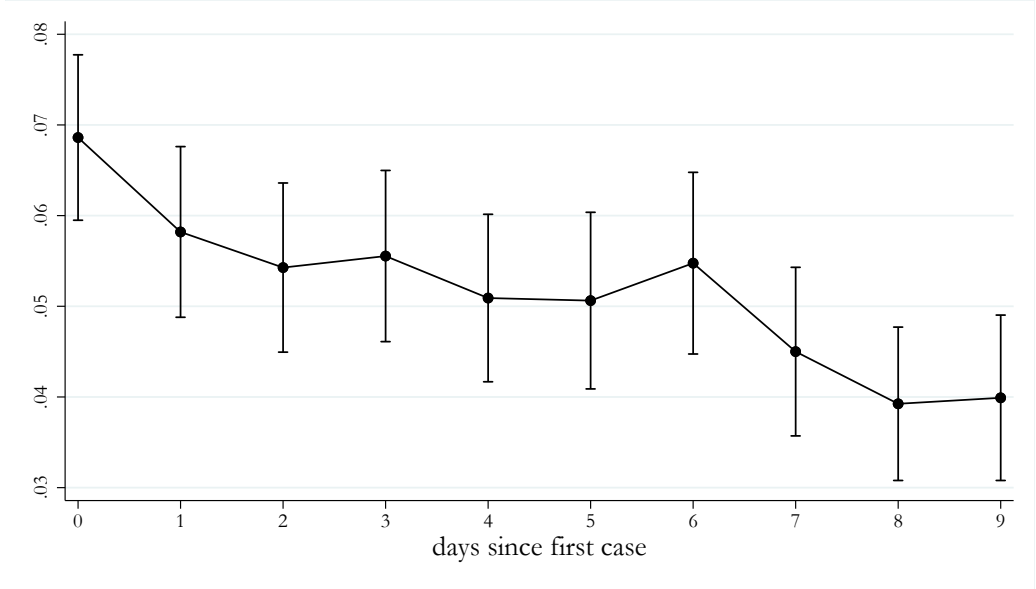
(a) Proportional Change if no Safer-At-Home Enacted

(b) Additional Change if Safer-At-Home Enacted

Figure B.3: Percentage Point Change in Percent Completely at Home at the County Level, Conditional on Safer-At-Home policies

(a) Percentage Point Change if no Lockdown Enacted (Relative to no Death)

(b) Additional Change if Lockdown Enacted (Relative to no Death and no Lockdown Enacted)

Figure B.4: Foot Traffic and Lockdown Policies in Selected States
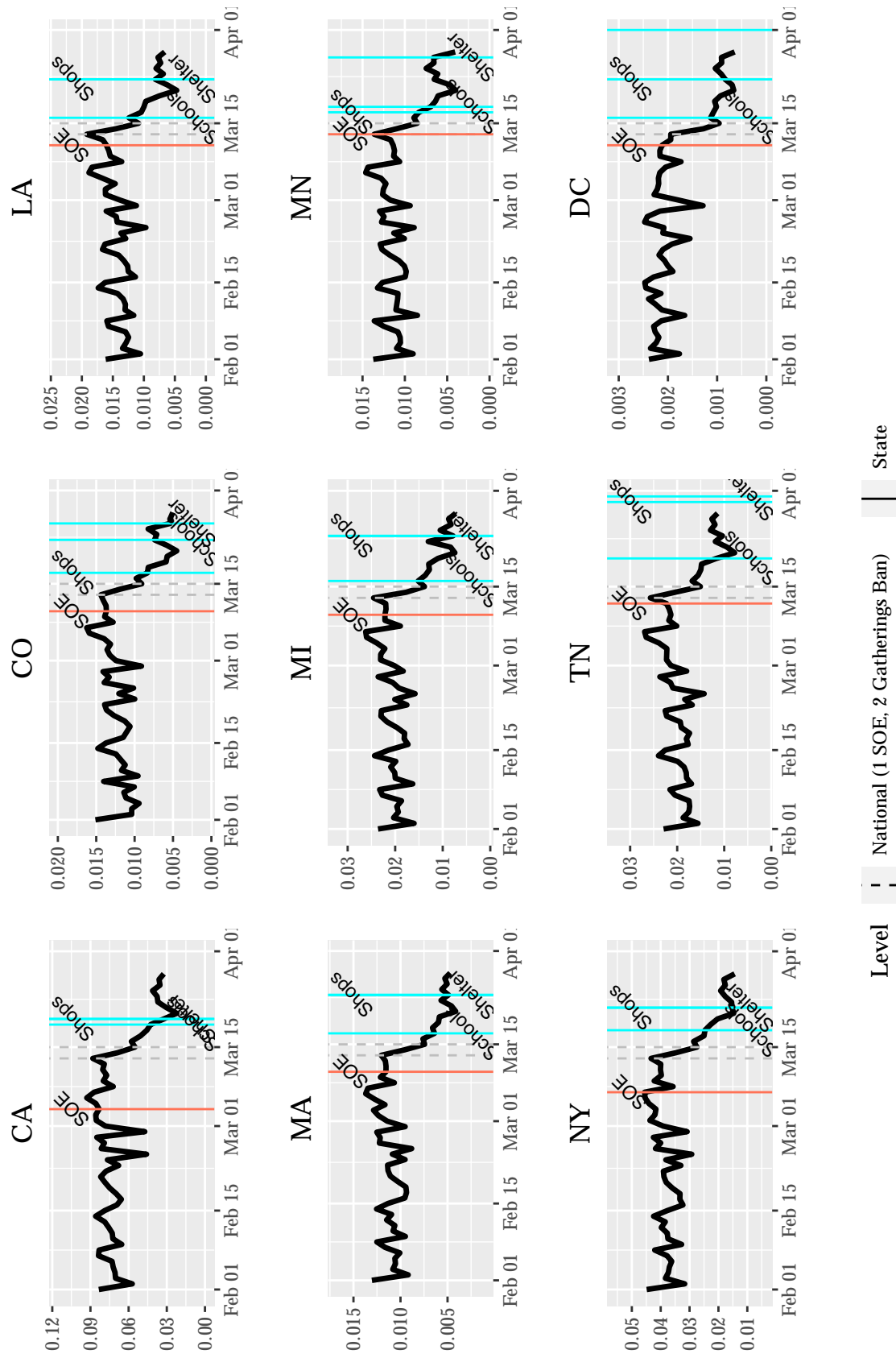
Notes: The plots show daily foot traffic and the percentage of devices that stayed home in selected states over time. The dashed vertical lines indicate national measures (SOE, Gatherings Bans) and the solid lines represent state-level SOEs and lockdown policies.

Figure B.5: Staggered Diff-in-Diff Estimates of the Policy Impact
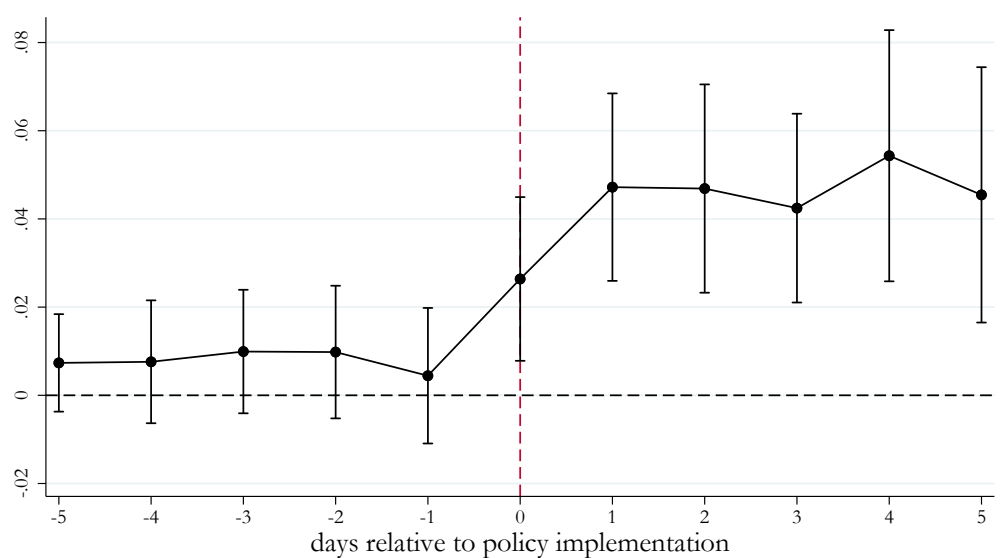
Figure B.6: Heterogeneity in Counterfactual Shelter-in-Place Response
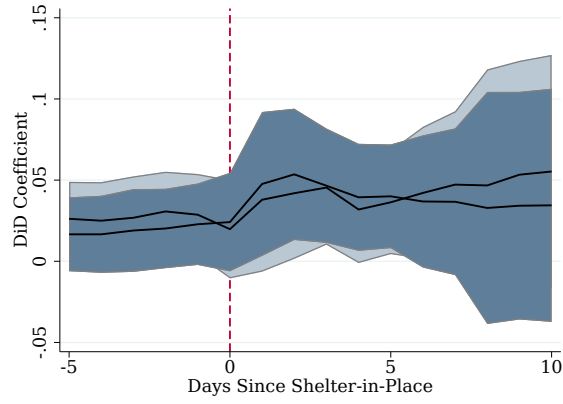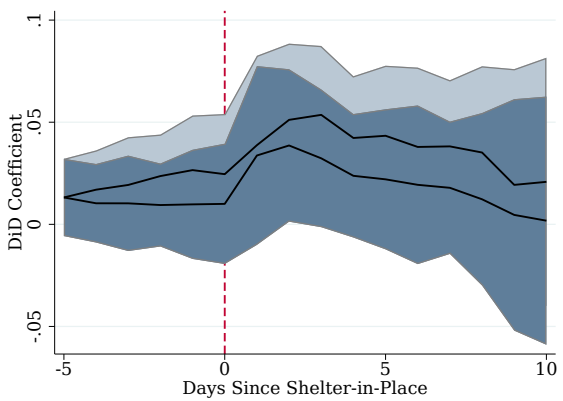

(a) Share of Votes Democratic 2016
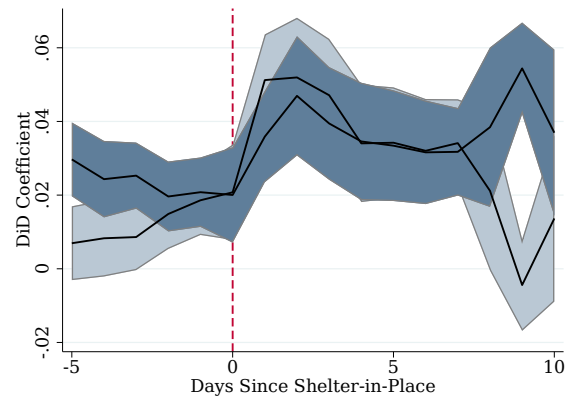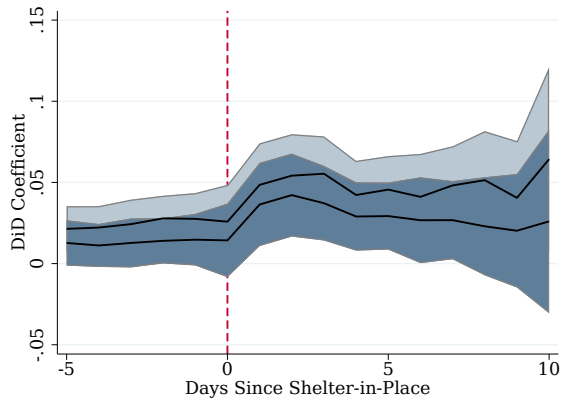
(b) Institutional Health Index (2010-2016)
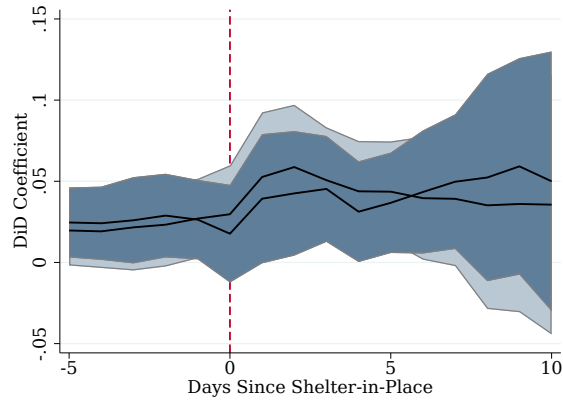
(c) Institutional Health Index
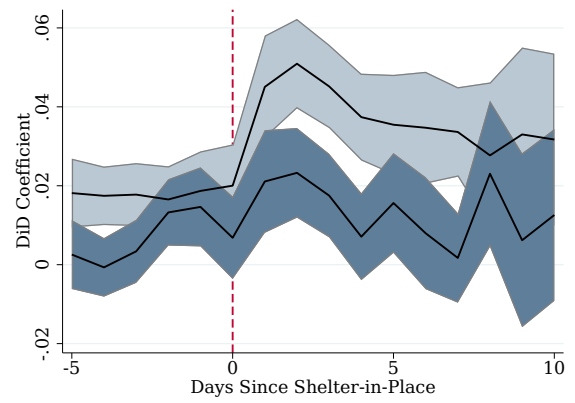
(d) Rural-Urban Continuum

(e) Median Household Income 2019Q3

(f) Percent with Bachelor's Degree

[1] Light blue: 10th percentile; dark blue: 90th percentile - for variable of interest across counties.

[2] Shaded area is 95% confidence interval. Standard errors are double-clustered by county and date. Observations: 180,295.

[3] Figure plots DiD estimate for days since 1st confirmed COVID-19 case interacted with variables of interest, in panel regression with state x day, variable of interest x day and county fixed effects.

Figure B.7: Counterfactual Shelter-in-Place Response, for Poorest and Richest County
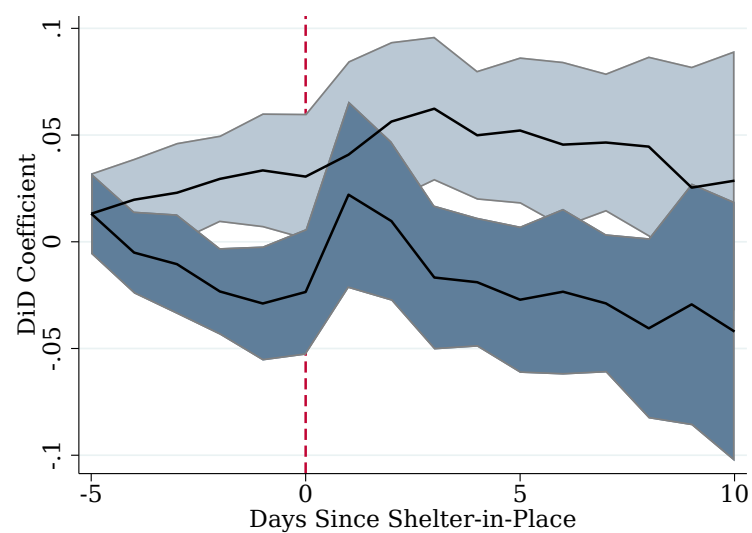
[1] Light blue: min; dark blue: max.

[2] Shaded area is 95% confidence interval. Standard errors are double-clustered by county and date. Observations: 180,295.

[3] Figure plots DiD estimate for days since 1st confirmed COVID-19 case interacted with variables of interest, in panel regression with state x day, variable of interest x day and county fixed effects.