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Abstract

Using data from 40 million mobile devices across the US, this paper analyses how state and county governments’ non-pharmaceutical interventions (NPI) aimed at restricting social contact interact with individuals’ physical distancing behavior in response to the COVID-19 pandemic. We use difference-in-differences and instrumental-variable approaches to find that such NPIs lead to a significant uptake in physical distancing. Our estimates show that shelter-in-place policies can increase time spent at home by as much as 39%. Nevertheless, individuals engage in limited physical distancing even in the absence of NPIs, once the virus takes hold in their area. Moreover, we show that governments are more likely to implement lock-down policies if they face a population that does not take physical distancing measures on its own. Our analysis suggests that non-causal econometric approaches studying how the uptake in physical distancing responds to lock-down policies will yield biased results. Exploiting county-level data, we document significant socio-economic heterogeneity in individuals’ responses to the spread of COVID-19 and to lock-downs, and show how state- and county-level policies interact.

Keywords: COVID-19, difference-in-differences, instrumental variables, NPI, community action, physical distancing, big data.

JEL-Classification: I12, H12.

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1 Introduction

The outbreak of COVID-19 has caused a major disruption to the global economic system and an unprecedented healthcare crisis across the world. 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 (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 our best 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 approaches and an instrumental-variable analysis, we show that physical distancing measures pick up after the implementation of government lock-down policies. In particular, in our first approach, we estimate that the introduction of a stay-at-home policy persistently increases time spent at home by 4%, over and above any community action taken. For our difference-in-differences (DiD) instrumental variable (IV) approach, we group states together by the date on which the first within-state COVID death occurs. This analysis suggests that the effect size can be as large as 39% for certain states, once we account for endogeneity due to treatment selection. Nevertheless, we find that communities take action even in the absence of government policies. Moreover, we also find that the more communities take independent action to limit social interactions, the less likely it is that state governments implement restrictive lock-down 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. Moreover, our analysis suggests that non-causal econometric approaches to measure the uptake in physical distancing following lock-down policies will yield biased results, as we provide evidence for a two-way interaction between physical distancing and such policies.

Governments can take two distinctive strategies to mitigate the spread of the epidemic with public health measures according to Ferguson et al. (2020): mitigation and suppression. The former aims at lowering maximum healthcare demand by reducing the contagion rates through non-pharmaceutical interventions (NPIs henceforth), while particularly protecting
individuals at risk. By adapting a number of very restrictive NPIs, the suppression approach attempts to push down the prevalence of new cases to zero. The extant literature suggests that only a mix of suppressive measures such as mandatory home isolation, quarantine and social distancing can be successful in keeping hospitalization rates below capacity and decreasing the number of unnecessary deaths. For many countries, these interventions may need to be complemented with school and university closures as well as bans of mass gatherings (Ebrahim and Memish, 2020; Ferguson et al., 2020; Hellewell et al., 2020). Despite the severity of the problem and early warnings (Wu et al., 2020; Xiao and Torok, 2020), governments in countries such as the UK and many states in the US have been relatively slow to implement NPIs. As a result, the US has experienced record growth rates in reported cases, while the number of undetected cases is likely to be many times higher.¹ Recent research shows that, especially in the early phases of the epidemic, interventions are crucial to slowing down the spread of the disease (Zu et al., 2020; Stoecklin et al., 2020).

Clearly, NPIs are only effective if communities adhere to the rules imposed by governments. Official sources have revealed different behavioral patterns in response to NPIs, ranging from full compliance to so-called ‘corona parties’.² Conversely, individuals may anticipate NPIs and practice social distancing even before suppressive measures have been taken. It is therefore crucial to understand the interaction between actions of governments and communities in response to the COVID-19 epidemic. Our paper is, to our knowledge, the first to provide causal estimates of these interactions using econometric analysis applied to big data on people’s whereabouts and movement patterns.

Our research also adds to several strands of the literature investigating the behavioral impact of major crises such as natural disasters or pandemics. A host of papers studies 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). We add to this literature by providing the first examination of people’s short-term behavioral response to a pandemic in the developed world using real-time data. In addition, our findings shed light on 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 manner (Guiso et al., 2004; Williamson, 2009). Ahlerup et al. (2009) and others suggest a certain degree of substitutability between both aspects based on decreasing marginal effects of social capital in institutional strength. Finally, several studies indicate that informal institutions are vital in promoting behavior to mitigate the spread of infectious diseases (Rönnerstrand, 2013; Chuang

¹See e.g. www.mailman.columbia.edu.
et al., 2015, Rönnerstrand, 2014) or improve disaster management (Barone and Mocetti, 2014). We find support for these earlier findings in that more informed (highly educated and high trust in science) areas respond more strongly to formal policies, while these areas also have a reduced need for such policies in the first place because of independent community action, likely reinforced through informal institutions. Our finding that socio-economically less well-off areas tend to respond less to formal policies suggests an important role for formal institutions in reaching out to such areas through information campaigns and by bolstering the informal institutions in place.

The rest of this paper is structured as follows: section 2 describes the data, most prominently the big data on physical distancing and the dataset of US lockdown policy actions that we composed.3 Section 3 discusses our empirical approach and presents the results. Section 4 concludes.

3The 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 28, 2020 from various sources. In this section, we briefly discuss each of the sources and describe the general patterns in the data.

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. 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 NPIs 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. 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%. 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

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4 For a more detailed exposition of SafeGraph’s data products, see https://safegraph.com.

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

6 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.
the total number of visits each day to all POIs in each state. We consider overall foot traffic the best-suited measure to study the impact of social distancing on the community, since it smooths out industry-specific idiosyncrasies in 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. 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:

- **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.

- **The percentage of devices exhibiting full-time work behavior** is calculated by summing a count of the devices that spent more than 6 hours at a location other than their home during the period of 8am-6pm in local time and dividing it by the total number of devices observed in that CBG.

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 aggregated 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 aggregated results. No human subjects have or can be re-identified using these derived results.

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7See [here](#) for detailed information on this product.

8Detailed 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)\(^9\) and the National Governors’ Association.\(^{10}\) For each state and county, we obtained data on whether and which date they declared a State of Emergency (SOE) and implemented Business or School Closure and Safer-at-Home policies\(^{11}\). 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 reasons) are the only exceptions to the Stay-at-Home order. 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 States Administration websites of the 50 States and the District of Columbia.

2.3 Instruments and Controls

**Instruments: Weather and Ventilators.** To account for the potential endogeneity of government measures with respect to the community response measures taken from SafeGraph, we construct an instrument based on the 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.\(^{12}\) While a current or predicted shortage of ventilators increases pressure on politicians to impose NPIs, 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 of 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.\(^{13}\) Temperature and precipitation in the capitals are based on measurements from the capitals’ main airport’s weather station. We match these to our community response measures for the capital cities in question.

\(^9\)For details, see https://ce.naco.org/?dset=COVID-19&ind=State%20Declaration%20Types. We thank NACO for sharing the underlying data with us.
\(^{10}\)The underlying data from the NGA can be found under https://www.nga.org/coronavirus/#states
\(^{11}\)In this paper, we use the terms Shelter-in-Place and Safer-at-Home interchangeably.
\(^{12}\)See www.covid19.healthdata.org for a current version of the data.
\(^{13}\)The historical weather data is available under www.ncdc.noaa.gov
Hospital Capacity. Data on hospital capacity is provided by Definitive Healthcare\textsuperscript{14} through the ESRI’s Disaster Response Program\textsuperscript{15}, which gathers useful data to understand the COVID-19 spread in the United States. The dataset contains the number of licensed beds, the current utilization of beds and the potential increases in number of beds in case of emergency for all United States Hospitals; and this for both normal and ICU beds. Additionally, it includes daily forecasts on the number of hospital beds and ventilators needed for COVID patients.

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\textsuperscript{16}, the Johns Hopkins Coronavirus Research Center\textsuperscript{17} and the COVID Tracking Project.\textsuperscript{18} We collect measures on positive tests, negative tests and number of deaths; and this for both cumulative count and day-on-day increase.

Socio-Economic Statistics. Most demographic variables are sourced from the American Community Survey 2018 (Ruggles et al., \textsuperscript{2018}), a 1\% random sample of the American population. Population estimates for 2018 come from the official Census Statistics, and data on employment by state and sector was downloaded from the homepage of the BEA.\textsuperscript{19} 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 moreover leverage data on county-level opinions on climate change from Howe et al. \textsuperscript{2015}. Lastly, data on party vote shares in the 2016 presidential election by county was obtained from the MIT Election Lab.\textsuperscript{20}

\textsuperscript{14}www.definitivehc.com
\textsuperscript{15}www.coronavirus-disasterresponse.hub.arcgis.com
\textsuperscript{16}www.COVID19.healthdata.org
\textsuperscript{17}www.coronavirus.jhu.edu
\textsuperscript{18}www.COVIDtracking.com
\textsuperscript{19}www.bea.gov
\textsuperscript{20}www.electionlab.mit.edu
2.4 Descriptive Statistics

In this section, we provide an overview of the main patterns in our data. 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. NPIs were imposed across states between 7 and 14 days 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.

**Figure 1: Timeline of Contagion and NPIs, Feb-March 2020**

Notes: The orange solid line depicts 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), while the grey shaded area represents business closures.

Figure 2 and Figure B.1 in the Appendix depict a number of trajectories for selected states from each US region by plotting foot traffic and the percentage of devices that stayed home all day alongside the national and state-level NPIs. 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 NPIs. This variation allows us
to explore the interplay between community behavior and government action around the time of the enactment of the NPIs.

At the state-level, the downward trends in traffic observed in Figure B.1 seem to hold both in the presence and in the absence of policy interventions. Figure 3 expands on this notion by plotting the change in traffic and the percentage of devices that remained home over the period between the date of the tenth confirmed case in each state and the enactment of the first NPI. Indeed, once a state is affected by COVID-19, individuals start to reduce their daily foot traffic and spend relatively more time at home. Interestingly, this is even the case if no suppressive policies are put in place later on (see also Figure 7). Hence, we conjecture that calls for physical distancing and information on the virus’s spread are on average taken seriously and individuals compensate for the absence of fast-moving policies by voluntarily modifying their behavior. 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.

We now turn towards the developments around the implementation dates of different NPIs. Figure 4 shows the mean of the outcome variables over time, relative to the day school closure and shelter-in-place policies have been introduced. Overall, the descriptive evidence is rather mixed. Once one takes into account the pre-trends before the introduction, the efficacy of the policies seems small. While average foot traffic even stagnates after shelter-in-place policies has been imposed, all other measure seem at least effective in accelerating the pre-policy trend of increased physical distancing.

Finally, Figure 5 shows how these patterns differ by state along political, demographic and socio-economic lines. Panels (1) and (2) focus on the party of the governor and the vote in the 2016 presidential election. Earlier research has shown that states with republican senators and higher shares of Trump voters acted more slowly than democratic states in response to the pandemic (Adolph et al., 2020). Given the early politicization of COVID-19 in the US and strategic considerations related to the virus, governments have responded differently in democratic compared to republican states. Our findings indicate in addition that people in states with a republican leaning react more tentatively to calls to promote physical distancing and to NPIs. Panels (3) and (4) show that states with higher shares of college-educated people and higher population density show a stronger growth in the percentage of individuals that stayed home around the implementation date. In panel (5), we find the degree of physical distancing to be correlated with the share of Asian Americans in the community, while, perhaps surprisingly, panel (6) reveals that people in states with lower levels of health insurance comply less with the measures imposed.
Figure 2: Percentage Completely at Home and NPIs 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 NPIs.
**Figure 3:** Change in Outcome Variables Before Enactment of NPIs

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 NPI against the count of the days between the two events in each state. 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 all day. The change in both variables is with respect to average in the 7 days preceding the tenth case.

**Figure 4:** Evolution of Outcome Variables

Notes: The graphs plot the means in traffic and the percentage of devices that remained home relative to the implementation of the school closure and shelter-in-place policies. The traffic variable is divided by the state’s population and normalized to 100 for the day of the school closure.
Figure 5: Evolution of Percentage Home by State Characteristics

Notes: The graphs plot the mean percentage of all devices that stayed home all day by state characteristics. The x-axis depicts days relative to school closure.
3 Results

3.1 Effect of Government Action on Community Action

We now turn to the main results of this paper. First, we consider the effect of state-level policies on community response. In particular, we attempt to answer two questions. First, to what extent do people adopt physical distancing actions, such as staying at home, in the absence of government policies to that effect? Second, how do people adapt their behavior following a policy change?

In order to tackle these questions, we adopt two approaches. First, we use a staggered difference-in-differences (DiD) approach, where we estimate how various measures of physical distancing react to suppressive NPIs at the state level. As a second approach, we employ a DiD instrumental variables (IV) methodology in order to make our results more robust to potential endogeneity problems. One such problem is the possibility that policies do not only influence community action, but that the reverse is also the case: persistent inaction on the side of the community can trigger the government to implement policies — as we show in section 3.2.

As outlined above, communities can view NPIs as a substitute or as a complement to their own actions. If communities consider them substitutes, they will take more actions in case the government does not act itself. In that case, we would expect the population to practice more social distancing in the absence of government actions. On the other hand, if the two are complements, a lack of government action would coincide with a lack of community response.

To assess how communities react to COVID-19, it is helpful to consider how physical distancing measures evolve after the virus’s outbreak. Figure 6 shows how three types of distancing measures respond to the first death in a state, alongside 95% confidence intervals. The estimates show how the variable changes after the first COVID death compared to the period before the outbreak, where we control for state fixed effects and a time trend. Note that all three variables change in the expected way: compared to no death having occurred, the percent 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 30% compared to the period before the first death.

\footnote{For the two figures that follow, we exclude state-day observations more than 9 days after the first state death. Including them does not change the estimated response.}
Figure 6: Physical Distancing Change Since First Death from COVID-19, Relative to No Death

(a) Log of 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 and a time trend.
Of course, the community response will depend on whether states have implemented policies targeted at inducing people to take physical distancing measures. In order to reflect this, we estimate the following model to assess how the physical distancing response depends on whether a government action was implemented:

\[ \text{comm}_{i,t} = \alpha_i + \delta_t + \sum_{j=0}^{9} \beta_j j\text{-days}_{i,t} + pNPI_{i,t} + \sum_{j=0}^{9} \gamma_j j\text{-days} \times NPI_{i,t} + \Psi_h x_{i,t} + u_{i,t}, \]  

(1)

where \( \text{comm}_{i,t} \) is the community response variable, 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; \( NPI_{i,t} \) is a dummy equal to 1 if the government has implemented the policy at or before period \( t \); \( \alpha_i \) are state-fixed effects and \( \delta_t \) is a time trend. Thus, the coefficient \( \beta_j \) estimates the community response on day \( j \) after the first death in case the government action has not been taken, relative to no death; and \( p + \gamma_j \) 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 7 plots the resulting estimates from equation 1 alongside 95% intervals, for the introduction of safer-at-home policies. The upper panel illustrates the change in the physical distancing measure in case that no such policy was in place (i.e. \( -\gamma_j \) for each \( j \in \{0,1..10\} \) 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 \( j \in \{0,1..10\} \)). The upper panel illustrates that even in the absence of government policies, communities take physical distancing actions in response to COVID-19, controlling for differences between states and a common trend across states. However, the lower panel suggests that the community responses are stronger if the government also takes action, providing evidence that government and community responses are indeed complements to each other. The estimates in Figure 7 show that in states with safer at home policies in place, one day after the first death the median time spent at home is approximately \( \exp(0.22) = 25\% \) higher than in states without the policy in place. Similar responses are found for other measures of physical distancing (see Figures B.2 and B.3 in the Appendix). The response is also very similar when using county-level rather than state level data, where we use county-level instead of state-level fixed effects (see Appendix Figure B.4). Note that the pattern of the response in the lower panel of Figure 7 is not easy to interpret, as 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 the implemented the policy on the day of the estimate. Thus, the observed initial increase and later decline in the response can be both
due to a decline in the initial response in physical distancing for early movers as well as a smaller initial response for late movers. However, given that our analysis below indicates a high degree of persistence in the initial response (see Figure 8), we can tentatively conclude that implementing shelter-in-place policies soon (1-2) days after the first death in the state leads to larger overall increases in the time people spend at home.

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 3.2. 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 account for these potential endogeneity issues, we pursue two approaches: first, we estimate a staggered DiD specification where we control for factors that might influence both community action and the implementation of policies. In particular, we do our 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 3.2. 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.

### 3.1.1 Staggered Difference-in-Differences

We know that 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. Several recent papers have also demonstrated that staggered DiD estimates suffer from estimation bias when treatment assignment is non-random, or when treatment effects vary over time (Athey and Imbens, 2018; Goodman-Bacon, 2018). In what follows, we account for the stage of the pandemic each county is by including days-since-first-case dummies and controls for number of deaths and cases. We then assume that, conditional on controlling for the stage of the pandemic as well as county and time fixed...
Figure 7: Change in Median Dwelling Time, Conditional on Safer-At-Home Policies

(a) Proportional Change if no Shelter-At-Home Enacted (Relative to no Death)

(b) Additional Change if Safer-At-Home Enacted (Relative to no Death and no Policy Enacted)

1 Top panel: plots the estimated response of time spent at home by each day since the first death in the state - the βjs from Equation 1.
2 Bottom panel: plots the estimated response of time spent at home by each day since the first death if a shelter-in-place policy was in place on that day - γjs from the same equation.
effects, 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 to estimate the following saturated staggered DiD model:

\[ \text{comm}_{i,j,t} = \text{county}_i + \delta_t + \beta_{i,t} + \sum_{k=-5}^{5} \rho_k P_{j,t+k} + \Psi_h x_{i,t} + u_{i,t}, \]  

where \( \text{comm}_{i,j,t} \) is the community physical distancing action under analysis in county \( i \), state \( j \) and day \( t \); \( \text{county}_i \) are county-level fixed-effects; \( \delta_t \) are day fixed-effects; \( \beta_{i,t} \) are day-since-first-case fixed effects; \( P_{i,t+k} \) is a state-level policy dummy that is equal to 1 at time \( t + k \) and 0 otherwise, where \( k = 0 \) when state \( i \) implements the policy; and \( x_{i,t} \) 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 8 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 time spent at home. We find that the implementation of a shelter-in-place policy increases the time spent at home by approximately 4% on the day of its implementation, once taking account of any community responses due to the county-specific COVID-19 incidence and country-wide developments. Perhaps more importantly, the effect stays around that level 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, and the estimated effects are very similar.

### 3.1.2 Difference-in-Differences IV

As a further step to counter endogeneity issues, we group states by the day of first death. 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 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 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 14th, 16th and 18th. In contrast, the two states with first deaths on March 19th that do adopt safer-at-home measures implement those on the same date, March 25th.

**Table 1: Groups of States by Days since First Death**

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

Figure 9 shows the evolution of the median time spent at home since the first death occurred for each group of states. Panels (a)-(c) compare early adopters to never adopters, while in panel (d) both states who 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 NPI, their dwell-at-home time increases relative to states with the same first death date that do not adopt the policy.

We now proceed to a difference-in-differences IV regression for states with the first death occurring on March 19th. 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 do implement a safer-at-home policy do so on the same date, March 25th. This conclusively eliminates any 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 diff-in-diff specification in the second stage:

\[
comm_{i,t} = \alpha_i + \delta_t + \rho NPI_{i,t} + \Psi hX_{i,t} + u_{i,t},
\]

where all variables are defined as before. Thus, \( \rho \) is the standard DiD estimate that captures the effect of implementing the policy.\(^{22}\)

\(^{22}\)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.
Figure 9: Dwell Time at Home (Log), by Date of First Death from COVID-19

In a first stage, we instrument $NPI_{i,t}$ by using the number of ventilators needed in a given state. We argue that, conditional on controlling for the number of COVID-related cases and deaths, the number of ventilators needed will 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 NPIs 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 forecasted pressure on their healthcare system.

Table 2 reports the estimation results for our sample of states with first deaths on March 19th. The first column shows the simple DiD results without instrumenting the government action, while the second one contains the baseline IV results. Note that for the latter, the first-stage F-statistic 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 safe-at-home policy. The number of deaths and of confirmed cases enters our DiD estimation results 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, the government response increases the dwell-at-home time by $exp(0.33) = 39\%$. Without the instrument, the effect is reduced to a still substantial $exp(0.134) = 14\%$. Given that the median dwell time across states from 1 to 15 February 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 time spent at home.\textsuperscript{23}

Our analysis suggests 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 would be based only 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 at home 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

\textsuperscript{23}Note that our non-instrumented estimates here are similar to those for the state-level DiD in Figure 2.5. Any difference that remains can be either due to bias from time heterogeneity in treatment effects in the staggered DiD (though we try to account for that) or, more probably, due to the different samples.
Table 2: DiD-IV Estimates of Effect on (Log) Home Dwell Time, for States With Common First Death

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>DiD</td>
<td>DiD-IV</td>
<td>DiD-IV</td>
<td>DiD-IV</td>
</tr>
<tr>
<td>Shelter-in-place</td>
<td>0.134**</td>
<td>0.329***</td>
<td>0.311**</td>
<td>0.363***</td>
</tr>
<tr>
<td></td>
<td>(0.0412)</td>
<td>(0.0244)</td>
<td>(0.0707)</td>
<td>(0.0736)</td>
</tr>
<tr>
<td>COVID deaths</td>
<td>-0.00815*</td>
<td>-0.0140</td>
<td>-0.0130</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.00346)</td>
<td>(0.00790)</td>
<td>(0.00773)</td>
<td></td>
</tr>
<tr>
<td>COVID known cases</td>
<td>2.14e-05</td>
<td>2.34e-05</td>
<td>-3.29e-05</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(9.12e-05)</td>
<td>(9.76e-05)</td>
<td>(0.000198)</td>
<td></td>
</tr>
<tr>
<td>State FEs</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Date FEs</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>F statistic</td>
<td>80.60</td>
<td>14.12</td>
<td>17.99</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>285</td>
<td>260</td>
<td>260</td>
<td>260</td>
</tr>
<tr>
<td>R²</td>
<td>0.887</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

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

are indeed more likely to implement a policy if their population does not reduce their traffic on their own.

3.2 Effect of Community Action on Government Action

In this section, we examine to what extent independent community action affected the probability of state governments introducing NPIs. From a theoretical point of view, the predicted sign of the effect is rather unclear. As shown in the previous section, people have practiced social 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 NPIs. 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 NPIs would be comparatively small. The lower political cost would suggest stronger independent community action triggers stronger government action; the smaller health effects would suggest the opposite. On the other hand, 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 lock-down, 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 NPIs we estimate the following equation:
\[ NPI_{i,t} = \alpha + \gamma_i + \delta_t + \sum_{j=1}^{J=8} \beta_j comm_{i,t-j} + \Psi x_{i,t} + u_{i,t} \] (4)

where \( NPI_{i,t} \) is 1 on the day of state \( i \)'s announcement of the NPI in question, \( t_o \geq t - \) after which the state drops out of the sample, and 0 before; and \( comm_{i,t-j} \) denotes the \( j^{th} \) lag of the community action variable 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, \( \gamma_i \) and \( \delta_t \), along with a vector of covariates \( x_{i,t} \) that controls for the cumulative number of confirmed cases and deaths by state.

As the aim of this paper is to disentangle the interaction of the actors, we adopt an Instrumental Variables (IV) strategy to assess how community action affects the probability of NPIs being imposed. For policymakers, the most visible indicator for compliance with the call to decrease social contacts is public foot traffic. In our main specification, we therefore instrument daily foot traffic in the state capital with the daily weather in that city. Implicit in this specification is the assumption that foot traffic in the state capital is a salient measure of community action for the governor of the state, and affects the decision to impose a state-wide policy. At the same time, if the community anticipates the imposition of a suppressive NPI, 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 NPIs will be biased.

To account for this effect, we estimate the \( J \) different \( \beta_j \)'s by means of two-stage least squares, where we instrument each of the \( J \) lags of \( comm_{i,t-j} \) in the first stage regression as follows:

\[ comm_{i,t-j} = \alpha + \gamma'_i + \delta'_{t-j} + \beta'_1 TMAX_{i,t-j} + \beta'_2 PRCP_{i,t-j} + \Psi' x_{i,t-j} + u'_{i,t-j} \] (5)

where \( TMAX_{i,t} \) is the maximum temperature in Fahrenheit in the capital of each state \( i \) on day \( t \), \( PRCP_{i,t} \) is the same for precipitation in millimeters, and the rest of the regression is identical to the second stage equation 4. If the temperature is higher than normal or precipitation lower, 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 as well as 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 NPI from the moment the virus has taken foothold in their state. Second, we drop states after they announce the NPI. The NPIs 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 NPI after that point. Note that in this section, we use the date of NPI announcement 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 3 reports the first stage regression results for our main specification with (log) foot traffic in the state capital as the dependent variable, as well as alternative specifications with (log) foot traffic in the state as a whole and the (log) number of smartphone devices that stay at home all day as dependent variables.

Table 3: First Stage: Regression of NPI on Physical Distancing

<table>
<thead>
<tr>
<th>Dependent variable:</th>
<th>Foot Traffic Capital</th>
<th>Foot Traffic State</th>
<th>Home All Day</th>
</tr>
</thead>
<tbody>
<tr>
<td>Max Temperature</td>
<td>0.003***</td>
<td>0.003***</td>
<td>-0.002***</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.0005)</td>
<td>(0.0004)</td>
</tr>
<tr>
<td>Precipitation</td>
<td>-0.001</td>
<td>-0.002***</td>
<td>0.003***</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>State FEs</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Day FEs</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Observations</td>
<td>280</td>
<td>280</td>
<td>280</td>
</tr>
<tr>
<td>R²</td>
<td>0.997</td>
<td>0.998</td>
<td>0.998</td>
</tr>
<tr>
<td>Adjusted R²</td>
<td>0.996</td>
<td>0.998</td>
<td>0.998</td>
</tr>
<tr>
<td>Residual Std. Error (df = 228)</td>
<td>0.073</td>
<td>0.056</td>
<td>0.050</td>
</tr>
<tr>
<td>F Statistic (df = 51; 228)</td>
<td>1,470.294***</td>
<td>2,917.787***</td>
<td>2,479.997***</td>
</tr>
</tbody>
</table>

1 Dependent variables in logs, temperature in Fahrenheit, precipitation in mm. Controls include nr. of deaths and number of positive cases.
2 * p<0.1; **p<0.05; ***p<0.01

The first thing to note from the table is that the coefficients in all three specifications are jointly significant, with F statistics well above 100 for all three specifications. This supports the conjecture that weather is a relevant instrument for foot traffic. Moreover, the coefficients are nearly all significant at the 1% level, and the coefficient signs make intuitive sense: higher
temperature leads to more foot traffic and less people staying completely home, while more precipitation leads to less foot traffic and more people staying home. The magnitudes are also reasonable, with, for example, a 2-degree F increase (~1.1 C increase) in temperature leading to a 0.6% increase in foot traffic and a related 0.6% decrease in the number of people that stay home all day. In the actual two-stage regression, we additionally include the interaction between temperature and precipitation as an instrument. The result of that regression can be seen in Table 4, which reports estimates for the effect of foot traffic and people staying at home on the imposition of a state-wide shelter-in-place policy.

Table 4: Second Stage: Regression of NPI on Physical Distancing

<table>
<thead>
<tr>
<th>Dependent variable:</th>
<th>Shelter in Place</th>
<th>~Traffic Capital</th>
<th>~Traffic State</th>
<th>~All Day</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td></td>
</tr>
<tr>
<td>L1.y</td>
<td>−0.284</td>
<td>−0.076</td>
<td>−0.127</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.749)</td>
<td>(0.706)</td>
<td>(0.695)</td>
<td></td>
</tr>
<tr>
<td>L2.y</td>
<td>−0.522</td>
<td>−0.799</td>
<td>0.539</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.969)</td>
<td>(0.784)</td>
<td>(0.768)</td>
<td></td>
</tr>
<tr>
<td>L3.y</td>
<td>0.197</td>
<td>0.080</td>
<td>−0.200</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(1.055)</td>
<td>(0.790)</td>
<td>(0.694)</td>
<td></td>
</tr>
<tr>
<td>L4.y</td>
<td>0.140</td>
<td>0.816</td>
<td>−0.514</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(1.033)</td>
<td>(0.785)</td>
<td>(0.677)</td>
<td></td>
</tr>
<tr>
<td>L5.y</td>
<td>−0.551</td>
<td>−0.645</td>
<td>0.685</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.904)</td>
<td>(0.792)</td>
<td>(0.707)</td>
<td></td>
</tr>
<tr>
<td>L6.y</td>
<td>−0.871</td>
<td>−0.557</td>
<td>1.023</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.883)</td>
<td>(0.800)</td>
<td>(0.744)</td>
<td></td>
</tr>
<tr>
<td>L7.y</td>
<td>0.617</td>
<td>0.701</td>
<td>−0.160</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.968)</td>
<td>(0.909)</td>
<td>(0.774)</td>
<td></td>
</tr>
<tr>
<td>L8.y</td>
<td>1.380*</td>
<td>1.625**</td>
<td>−1.537**</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.801)</td>
<td>(0.823)</td>
<td>(0.771)</td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Control</th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>State FEs</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Day FEs</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Observations</td>
<td>276</td>
<td>276</td>
<td>276</td>
</tr>
<tr>
<td>R²</td>
<td>0.384</td>
<td>0.413</td>
<td>0.466</td>
</tr>
<tr>
<td>Adjusted R²</td>
<td>0.223</td>
<td>0.259</td>
<td>0.327</td>
</tr>
<tr>
<td>Residual Std. Error (df = 218)</td>
<td>0.308</td>
<td>0.300</td>
<td>0.286</td>
</tr>
</tbody>
</table>

1 Temperature in Fahrenheit, precipitation in mm. Independent variables are in logs. Controls include nr. of deaths and number of positive cases.
2 * p<0.1; **p<0.05; ***p<0.01

As a first caveat, note that we are effectively estimating a linear probability model (LPM). It is well-known that the estimates of an 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 interested
in the precise magnitude of our estimates, but rather in the direction of the sign. That said, the estimates obtained from the second-stage regression are generally not statistically significant, with the exception of the 8th lag of the social distancing measures; traffic in state capital and state, and percent of people staying home all day. The coefficient on the 8th lag for all these variables indicates that an increase in independent community social distancing increases the probability of the imposition of an NPI by around a 1-1.5 ratio. That is, a 1% increase in state-level foot traffic 8 days ago increases the probability that the state will announce a shelter-in-place policy by 1.5% today. 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 NPIs affect people’s social distancing behavior, but the prevalence of such behavior before the implementation of an NPI also changed the probability of it being introduced.

3.3 County Analysis: Heterogeneity in 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 NPIs 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 virus 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 NPI of interest. Figure 10 plots the evolution of this percentage over time, for sections of the distribution of several variables of interest. Specifically, it plots 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 minimum (light blue), median (blue), and maximum (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 differences that remain constant over the period considered. Thus, we can neatly disentangle the specific subgroup-level social distancing response to the spread of the virus and the county-level policies from other factors merely correlated with the social distancing behavior of these groups. Note that the evolution over time shown here includes any potential effects of county-level NPIs introduced over the
period – but not the effects of state-level NPIs. As such, the effects shown are a mix of individuals in county subgroups behaving differently and of counties within such subgroups implementing more or less restrictive policies.

The figure shows stark differences in the evolution of the social 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 total county votes in the 2016 presidential election. While the increase in the percentage of people staying completely home is relatively similar for those counties that are at the minimum and the median, it is starkly higher for the county that had the highest share of Clinton voters. The divide between those counties that voted more for Democrats and others starts opening up a few days after the first confirmed case. A similar pattern can be observed in the middle left graph when we look at the evolution of counties with differing population percentages who believe in climate change, which we consider a proxy for trust 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 this pandemic. Nonetheless, even those counties with the lowest share of people who trust in science still see the share of people at home increasing by 2%.

Another remarkable difference in evolution is between counties with lower and higher median household incomes. Not only do high-income counties ramp up their social distancing up to 20% more than low-income counties when the virus takes foothold in the county, they also strongly anticipate the arrival of the virus. The median-income counties respond moderately, while low-income counties actually see a decrease in people staying home that predates the arrival of the virus. The share of people staying at home in counties with higher and lower shares of college-educated people follows a similar trajectory. A last conspicuous pattern, in the top right graph, is that counties where a large share of employees work in the manufacturing sector in fact see a decrease in people staying fully at home, likely because of the essential nature of manufacturing and possibly because of increased hiring in essential sectors.

Next, we re-estimate the staggered difference-in-difference model with state-day and county fixed effects, where we interact the dummies for days since policy implementation with several variables of interest. We also control for cumulative number of confirmed cases and deaths in each county. Figure 11 thus shows how the effect of a county-level shelter-in-place policy on social distancing differs across county subgroups. Also plotted are the pre-trends. Note that these are generally not equal to zero, because we report the sum of the pure DiD estimate
Figure 10: Heterogeneity in Community Virus Response Over Time

(a) Share of Votes Democratic 2016

(b) Share of Employment in Manufacturing 2019Q3

(c) Percent Who Believe in Climate Change

(d) Rural-Urban Continuum

(e) Median Household Income 2019Q3

(f) Percent with Bachelor’s Degree

1 Light blue: min; blue: median; dark blue: max - for variable of interest across counties.
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.
plus the interacted DiD estimate. As a matter of fact, many of the pre-trends are markedly different from zero, probably because shelter-in-place policies were often highly anticipated. For example, in counties in metropolitan areas with more than 1MM inhabitants (light blue, middle left graph), we see a very high degree of anticipation of the shelter-in-place policy, in line with anecdotal evidence of the lockdown in New York. The response of people in more urban areas to the policy once it arrives also seems relatively larger.

The other most prominent findings are that in counties with a high share of 2016 Democratic voters, people who believe in climate change (as a proxy for trust in science), a high median household income, and a high share of college-educated, people respond more strongly to the imposition of a shelter-in-place policy in terms of staying in their homes.

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 12, 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.

24 The light-blue-shaded line gives the closest estimate of where the non-interacted pre-trends would be if the interacted variable were zero, since it is the minimum of the variable, and all reported variables are positive. Note that for all plots but panels (b) and (c), the pre-trends for the minimum are indeed not significantly different from zero.
Figure 11: Heterogeneity in Counterfactual Shelter-in-Place Response

(a) Share of Votes Democratic 2016
(b) Percent Who Believe in Climate Change
(c) Rural-Urban Continuum
(d) Median Household Income 2019Q3
(e) Percent with Bachelor’s Degree

1 Light blue: min; blue: median; dark blue: max - for variable of interest across counties.
2 Shaded area is 95% confidence interval. 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-day fixed effects and county fixed effects.
Figure 12: 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. 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 'lock-down' 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 aimed to fill this gap by studying the interaction between state- and county-level non-pharmaceutical interventions aimed at limiting social contact.
and individuals' physical distancing behavior, using a massive panel dataset from 40 million smartphone devices across the United States, combined with detailed data on state- and county-level government policies.

That way, we find that NPIs can effect a counterfactual increase in the time people spend at home of up to 39%, even as we show that individuals also decrease their social interactions to a more limited extent in the absence of such NPIs. Moreover, we show that when individuals engage more in such independent physical distancing, the likelihood of governments implementing restrictive measures decreases. This suggests that effective information campaigns that incentivize community action to pandemics can help reduce the need for prohibitive lock-downs that hurt the economy and are potentially unsustainable for long periods of time. Relatedly, we document that in areas where people are more science-minded, when such lock-downs are implemented, they are better adhered to by the population. Additionally, more highly-educated and richer areas also respond stronger to lock-down measures, emphasizing the need for governments to reach out more to individuals who are socio-economically disadvantaged so as to increase the effectiveness of such measures and save lives. 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 improve outcomes.
References


# Tables

## Table 5: Description of Key Social Distancing Variables

<table>
<thead>
<tr>
<th>Product</th>
<th>Variable</th>
<th>Description</th>
<th>Raw Data</th>
<th>Aggregation</th>
</tr>
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<tr>
<td>Social Distancing</td>
<td>Home Distance</td>
<td>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.</td>
<td>Median of all CBGs in county/state.</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Home Dwell Time</td>
<td>Median dwell time at home geohash-7 (&quot;home&quot;) 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.</td>
<td>Median of all CBGs in county/state.</td>
<td></td>
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<tr>
<td></td>
<td>Share at Home</td>
<td>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.</td>
<td>Sum over all CBGs in county/state.</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Percentage at Home</td>
<td>NA (constructed variable)</td>
<td>Sum of Share at Home for all CBGs in county or state / sum of Total Device Count for all CBGs in county or state.</td>
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<tr>
<td>Weekly Patterns</td>
<td>Traffic</td>
<td>Number of visits in our panel to this POI during the date range.</td>
<td>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.</td>
<td></td>
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*Note: Description Raw Data replicates the data description provided by SafeGraph here.*
### Table 6: Summary Statistics

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<td>Time dwelled home</td>
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<td>617.00</td>
<td>677.00</td>
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<td>% devices stayed home</td>
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<td>6.97</td>
<td>16.83</td>
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<td>Share fulltime workers</td>
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<td>Max. temperature</td>
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<td>Share &gt; 65</td>
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<td>14.86</td>
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<td>7.94</td>
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</table>

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

Figure B.4: Proportional Change in Median Dwelling Time at the County Level, Conditional on Safer-At-Home policies

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

(b) Additional Change if Safer-At-Home Enacted (Relative to no Death and no Policy Enacted)
Figure B.1: Foot Traffic and NPIs 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 NPIs.
Figure B.2: Proportional Change in Percent Completely at 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: 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.5: Staggered Diff-in-Diff Estimates of the Policy Impact