Tracking Public and Private Responses to the COVID-19 Epidemic: Evidence from State and Local Government Actions

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<u>*Abstract:*</u>

By early April 2020, the U.S. experienced more confirmed COVID-19 cases and deaths than any other country. Governments, businesses, and individuals have made extraordinary changes in how they function an effort to limit the spread of the virus. In this paper, we conduct a preliminary analysis of near-real-time data related to policy responses, information shocks, and mobility patterns that serve as proxies for social distancing. We also examine population health outcomes that capture the magnitude and severity of the epidemic.

We make two main contributions toward understanding the effects of policies related to the epidemic. First, we develop a typology to organize and group heterogeneous state and local government responses to the epidemic, and assess how those responses have affected social distancing measures in the early stages of the epidemic. Although social distancing has emerged as a major policy goal, very little is known about which policy approaches are effective at producing social distance, or about the importance of social changes relative to nationwide awareness of the epidemic or to state and local information on the progression of the epidemic. We harness several sources of commercial “smart-device” data on mobility and illness patterns and estimate event study regressions to examine the responsiveness of mobility patterns to various mitigation strategies. Second, we use data on confirmed COVID-19 cases and mortality to assess the degree to which governments’ adoption of policies appear to correlate with anticipated growth in the epidemic at a local level. We estimate simple event study models of COVID-19 cases and deaths, and we also discuss some ways that difference in difference and event study models may be interpreted through the lens of a simple Susceptible-Infected-Recovered (SIR) theoretical model.

Using multiple proxy outcome measures, we find large declines in mobility in all states since the start of the epidemic. Even states that did not implement major policy changes have experienced large mobility declines, and other states experienced large changes before the policy actions. This suggests that a substantial portion of the response to epidemic was not induced by specific government policies. However, our event study analysis implies that policy changes and informational events have also led to independent decreases in mobility. We find that five days after a state or county policy change or informational event, there are mobility reductions of between 1% to 18%, although most are in the 1% to 6% range. Our analysis of COVID-19 cases and deaths suggest that state government action often immediately precedes substantial increases in caseloads and deaths. This is logical given efforts to model the direction of the epidemic, but it suggests methodological challenges in estimating the effects of policy changes on the COVID-19 related health outcomes.

Overall, our results suggest that state and local government policy and informational events induced changes in mobility on top of what appears to be a much larger response across all states to the prevailing knowledge and events at both national and international levels. These patterns of response to local policies may, however, shed some light on the likely consequences of proposals to lift various government mandates and about the possible consequences of waning public support for social distancing, which could lead to a decline in the amount of voluntary reductions in mobility.

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Introduction

In only a few months, the COVID-19 epidemic has infected millions of individuals around the world and caused over 130,000 deaths. The World Health Organization (WHO) estimates that the case fatality rate is around 2% (WHO 2020). But the overall burden of COVID-19 remains uncertain, and it is still not clear when and how regular economic and social life will return.

In the U.S., state and local governments have implemented a wide range of policies in response to the epidemic. To provide a preliminary analysis of policy effects across a range of mobility and health outcomes, we combine policy data with near-real-time data streams from smart-device signals ("pings"). We relate policy responses, information shocks, and mobility patterns that serve as proxies for social distancing, finding that policies themselves have relatively limited effects compared to the overall declines experienced across the board in US mobility. We also use these policy and event data to estimate simple event study models of COVID-19 cases and deaths, and discuss some of the ways that difference in difference and event study models may be interpreted through the lens of a simple Susceptible-Infected-Recovered (SIR) theoretical model.

There are two broad categories of policy instruments in play at the moment (Ferguson et al. 2020; CDC 2020). Surveillance policies--such as COVID-19 testing, antibody testing, and body temperature and other symptom checks, in addition to contact tracing--are designed to identify infected individuals and can be used to implement targeted quarantines. Mitigation policies, on the other hand, are designed to reduce the transmission of the virus by limiting physical contact between people. These policies include school closures, stay-at-home orders, restaurant closures, essential business restrictions, and guidelines for group gatherings, hand washing, and face coverings.

The theoretical mechanism supporting most mitigation policies is social distancing. Specifically, governments are adopting policies that they hope will reduce the amount of person-to-person contact in the population. In theory, reducing the frequency of contact means that there will be fewer opportunities for the virus to pass from one person to the next. School closures, stay-at-home orders, and other policies being adopted across the country stand to shape contact rates. Evidence from microsimulation models speaks to the potential of these interventions to decrease the size of the epidemic and to more evenly distribute the number of cases over time (e.g., Ferguson et al. 2020; Peak et al. 2020; Davies et al. 2020; Bento and Teixeira 2020) to reduce the risk that local health care systems will be overwhelmed by surges in demand for health services (Keeling and Rohani 2011).

There is some empirical support for mitigation policies from studies of prior epidemics in the U.S. and other countries, and from studies of the COVID-19 epidemic in China (Correia, Luck, and Verner 2020; Fang, Wang, and Yang 2020; Bootsma and Ferguson 2007; Hatchett, Mecher, and Lipsitch 2007). However, the external validity of pre-COVID-19 case studies is not guaranteed. The current epidemic is much larger than others in recent history, and behavioral responses to an epidemic in the current-day U.S. may differ substantially from the effects of an epidemic in earlier historical periods or in recent years elsewhere. Social distancing is--to put it mildly--an unusual goal for governments in large democracies, which generally have constitutional restrictions on the government's legal authority to restrict personal freedoms related to mobility, assembly, association, and economic activity (Schwartz and Cheek 2017; Porter 1991).

Little research and few data systems are available to measure the quantity of close physical interaction at a level of frequency and detail that would be useful in the context of an ongoing epidemic (Prem et al. 2020). Traditionally, contact surveys are conducted to obtain estimates of the frequency of proximity between different sub-populations (Kremer 1996; Mossong et al. 2008; Rohani, Zhong, and King 2010; Bento and Rohani 2016; Prem et al. 2020), but such survey efforts provide estimates with considerable lags. Contact survey data are used to parameterize sophisticated epidemiological models of disease transmission (e.g., Mossong et al. 2008; Rohani, Zhong, and King 2010; Bento and Rohani 2016; Prem et al. 2020). But point-in-time contact surveys are not a useful way of evaluating the causal effects of mitigation policies adopted during an epidemic, or of monitoring levels of compliance with social distancing guidelines (Fenichel et al. 2011). Finding suitable proxies for the level of social contact is an important basic objective for policy research related to the epidemic.

Beyond simple measurement of the recommended social distancing metric (being within 6 feet of a non-household member), we also lack substantial knowledge about the quantitative magnitude of the policies on mobility, although simulation studies consider their effects (e.g., Jarvis et al. 2020; Prem et al. 2020); researchers are, however, fast filling that gap (Andersen 2020; Painter and Qiu 2020). In addition, little is known about the overall effect of any of these measures on COVID-19 transmission and mortality rates (Kaashoek and Santillana 2020).

For COVID-19, a growing literature uses epidemiological models to investigate how different mitigation policies can impact both transmission and disease burden (e.g., Jarvis et al. 2020; Prem et al. 2020). But identifying the causal effects of public policy changes on first-stage social distancing outcomes and downstream measures of the severity of the epidemic is not a trivial exercise. Governments often pass laws in part because of their own expectations about the local path of the epidemic. For example, in the U.S. and the U.K., the national government's stance on the epidemic seemed to change course in response to the epidemiological simulations presented in Ferguson et al. (2020). In addition, three papers to date examine the partisan angles of U.S.

state policy and mobility (Adolph et al. 2020; Andersen 2020) Painter and Qiu, 2020, and Friedson et al (2020) make progress towards causal identification in the case of California's stay at home laws, using synthetic control. Even if states do not pass policies because of prior knowledge of the disease spread in their region, government policies maybe enacted at the same time as other forces that affect voluntary changes in behavior by businesses, households, and individual people. This kind of private production of social distancing may be at least as important for mitigation as government actions.

As policy makers debate the merits of "opening" the economy by lifting sanctions, it is important to better understand how the policies already in place have affected mobility and transmission. Although these estimates may offer at least some insight into the consequences of lifting certain restrictions, we should not assume that the effects of adopting a policy will precisely mirror the effects of removing restrictions. Moreover, when interventions occur during times of rapid day-to-day national and global news, their impacts can be influenced by timing in ways that are challenging to understand. Simply put, the effect of removing a stay-at-home law in the coming months would not likely be simply the opposite of a policy that was placed during late March; effects of local policies may also depend on the prevailing national and international discourse regarding transmission mitigation strategies.

In this paper, we make several contributions. First, we develop a typology from policy compilations to classify heterogeneous policy responses to the epidemic. We examine both state- and county-level policies and estimate the share of the U.S. population subject to different policy and information events each day for the first months of the epidemic, considering the order in which governments adopted different policy measures. Typologies of policies, mobility proxies, and health outcomes data set the stage for future research on the determinants of behavioral responses and the effects of alternative mitigation strategies.

Second, we study the determinants of social distancing in the early stages of the epidemic, using several sources of commercial smart-device data that proxy mobility patterns. We estimate event study regressions to assess how mobility patterns respond to mitigation efforts that include formal policies as well as information events related to threats likely in the state or county. These regressions provide initial evidence on the first-stage effects of mitigation policies to achieve social distancing.

Third, we use data on confirmed COVID-19 cases and mortality to study the extent to which policies were passed in anticipation of the emergent local spread and severity of the epidemic. With a longer follow-up period and correcting for test numbers, this type of analysis will provide insight into the causal effects of policies on the epidemic. In the methodological section of the pper, we discuss some of the ways that difference in difference and event study research designs may be modified and interpreted through the lens of a simple Susceptible-Infected-Recovery (SIR) theory model.

Our estimates of the incremental effects of public policy on social distancing should be viewed in the context of unprecedented reductions in mobility that occurred nationwide in the month of March and continue to this writing. U.S. Department of Transportation statistics show that various measures of travel and mobility tend to increase substantially in the spring. Our measures of travel outside the state, the county, and the home, show massive declines in mobility occurring during a time of the year when we would normally expect a large rise in mobility. Data from the US Department of Transportation shows that the average number of Vehicle Miles Travelled (VMT) experiences about a 20% typically between February and March (U.S. Department of Transportation 2020). Data for VMT for March 2020 are not yet available, but the index of out-of-state travel we use in our empirical analysis (a measure of the fraction of devices in a state that were detected as being out of state at some point over the past 2 weeks) *fell* by 40% between March 1st and April 2nd for the average state. For the 5 states without SAH orders during this time period (as of April 3rd, these included Arkansas, Iowa, Nebraska, North Dakota, and South Dakota; Vervosh and Healy 2020); the decline was 31%, suggesting that large majority of the decline in mobility could be attributable to the general global, national or local state of knowledge and precautions rather than specific state policies.

States undertook roughly six different types of actions related to COVID19 that might substantially affect mobility: emergency declarations, school closures, restaurant restrictions, gathering restrictions, non-essential business closures, and stay-at-home orders. Although not intended to reduce mobility, local announcements of the first confirmed COVID-19 case also represent an important informational event, as would the first reported death. Due to the close timing of some policy changes (Fig 2.1) and the degree to which they might independently affect mobility, we study effects of first case and death announcements, emergency declarations, school closures, and stay-at-home orders.

We find that state and county stay-at-home orders and other policies, as well as actions of a more informational nature (state emergency declarations and news of first health threats at the state and county levels) lead to declines in mobility. The typical effect sizes, when statistically significant, are 1% to 6% by 5 days after the policy, although some rise to about 18%. These effects typically starting at about 1% to 3% the day of the policy. We supplement our main analysis with several robustness checks related to using the date of policy issue rather than the enactment date, including policies simultaneously versus separately, adjusting the balanced panel nature of our sample restrictions, and whether policies or neighbors influence mobility. These sensitivity analysis do not fundamentally alter our main conclusions.

Our estimates relate to the incremental change in mobility caused by public policy actions and information shocks, and they happen on top of the large reductions in mobility that occur independent of policy changes, as they appear across the board even in states that have not adopted stringent mitigation policies. (see Figure 4 series of time trends of our mobility indices). Some of our measures of mobility capture changes over the past 2 weeks and thus do not display changes immediately. It is possible that larger effects will become more apparent over a longer time horizon.

Our event study estimates also do not shed light on geographic variation in the response to the epidemic. To examine these relationships, we estimated simple cross sectional regression models linking "long differences" (changes from March 1st 2020 to early April 2020) in mobility measures with baseline county characteristics. Across the 2,008 counties included in our regressions, the average long difference in out of county "ping" (signal) rates was a reduction of 49%, making it clear that there was a substantial response to the epidemic in the average county. However, there is substantial variation across counties. The long difference regressions show that mobility rates fell more in counties with larger populations, more density, and larger urban centers. Counties where the recreation and tourism sector is important have experienced much larger declines in mobility. In addition, counties where more people do not have health insurance also seem to have experienced larger falls in measured mobility. The age distribution of the county population also seems to have shaped the response to the epidemic. Counties with a larger prevalence of 75-85 year old males have experienced larger reductions in mobility, which may suggest efforts to protect high-risk groups. Mobility has also fallen more in counties with a larger share of men and women aged 30-34. In contrast, the decline in mobility seems to be smaller in counties with higher prevalence of men and women ages 55-59 and 60-64.

Last, in our examinations of illness-related outcomes at the state and county levels (fevers, COVID19 positive cases, and deaths), our findings suggest that state government action often immediately precede increases in caseloads and deaths. This is surprising, but indicates that while the policies seem mostly to pass the parallel trends assumptions in terms of mobility, they cannot be considered exogenous for later understanding the impacts of policy-induced mobility reductions on disease mitigation.

Evidence on behavioral responses to restrictions adopted by states and counties sheds insight into transmission of COVID-19, as transmission depends on contact overtime. Examining patterns around policy timing and illness prevalence also helps in studying impacts of social distancing on health measures. However, the overall analysis has several notable limitations. Most prominently, we use data sources with unknown sampling frames and representativeness. Insights from these data sources will require multiple researchers analyzing them in different ways to provide a consensus. Second (and related), the data on out-of-state and out-of-county travel are very new and will update over time. Third, none of our mobility measures quantify actual interactions with individuals outside of a person's family; they are meant only as proxies for distancing. In this initial analysis, we do not examine the consequences of the epidemic for labor market outcomes and overall economic output, and we do not attempt to judge the welfare consequences of the epidemic or recent policy changes. However, we believe that our study highlights the usefulness of understanding policy effects on effective social distancing interventions and, as we grasp their effects on transmission, the importance of careful planning for the timing and degree of easing such measures to prevent subsequent waves of COVID-19.

Conceptual Model and Measures

In considering the relationship between state policies and information events, mobility and illness, we expect that states' and counties' actions could impact individual actions through costs imposed, as well as through how individuals update their prior beliefs regarding COVID-19 threats to their own and their community's health. State adoption of stay-at-home laws, for example, conveys information that the virus threat may be higher than a resident's prior. But it also imposes costs (stigma, fines) to free movement. Economic literature establishes that both the amount of a fine and its salience matter for responses to policy (Chetty, Looney, and Kroft 2009) and that it is important to consider behavioral nudges in combination with taxes for reducing the welfare costs of tax policy (Farhi and Gabaix, 2020). In the case of important public health threats, a combination of information and mandates/fines is often used (such as in tobacco policy). There are parallels between workplace smoking bans and quarantine policies in aiming to reduce externalities: in both cases, markets under-protect people against externalities. There are also issues of intergenerational effects present in considering behavior regarding COVID-19 if social interaction is typically greater for younger adults, for whom the health threats of the virus are not as great as for the oldest adults.

While states contemplate attempts to reduce disease transmission through affecting physical proximity, there is also a large personal response to information that is national and international. Epidemiological models integrate evidence of self-adaptive behavior that comes from information even absent policy actions (e.g. research regarding H1N1 ("swine") influenza, such as Fenichel et al. (2011), Fenichel, Kuminoff, and Chowell (2013), and Kremer (1996).

Prior evidence on social distancing policies show evidence of their effectiveness in reducing the spread of illness (e.g. Hatchett, Mecher, and Lipsitch 2007, and Bootsma and Ferguson 2007, for 1918's flu pandemic). The 1918 pandemic led to 675,000 deaths in the United States and 40 million worldwide (Garrett 2008). Although there is strong reasons to believe that state actions will create social distance, there may be less responsiveness detected by a direct comparison of states with and without policy changes by a few days, given personal behavior adaptation to national and international news. Furthermore, we will be unable to disentangle whether some policies act through information avenues that decrease the perceived net benefit of travel or through direct costs imposed on travel through bans (such as school closures, which reduce educational and work travel directly). It is more likely that behavior is changed solely through information avenues for policies such as emergency declarations (Riley, Christophe, and Christl 2003).

This paper focuses primarily on policies that restrict the movement of individuals through suspending activities to which they may travel to supply (as workers) or demand as customers, through broad-based restrictions such as stay-at-home (SAH) policies, or through primarily informational avenues such as state emergency declarations or news of the state's first positive COVID-19 case. These policies can be viewed as sequential in terms of the level of activity affected, and have typically occurred in waves. For example, policies first start at smaller geographic levels (e.g. some school districts closed before a state-wide decision was made), or at different levels of activity (a state school closure law before a SAH order). We track seven state-level and four county-level policies (emergency declarations, school closures, gatherings restrictions, travel quarantines, partial and full non-essential business closures, and SAH policies; the county versions are emergency declarations, school closures, business closures, and SAH policies).

For various reasons, these policies should not be viewed as necessarily exogenous to the virus progression in the regions. States and counties may have started to act more when the threat of the crisis drew closer to home. It is more plausible that policies may be exogenous with respect to mobility. We conduct standard parallel trends tests to investigate whether there were systematically different changes in mobility prior to policy adoption, although we do not have adequate data on prior years to compare, for example, seasonal differences across states that may be correlated with policy adoption.

Primary outcomes we study in this paper are related to whether people travel outside their state, outside their county, and outside their home. There is no clear way to assign a normative judgement to reductions in mobility, as some areas maybe have less access to grocery stores and fewer delivery services, thus requiring individuals to travel more; some may house a greater concentration of essential workers who must travel for work; and some may have greater access to (permitted) socially distant outdoor exercise. Thus, our mobility analysis is not necessarily meant to be normative, although we investigate the role of work related travel where possible.

Our measures related to disease spread by time are also imperfect in that we aim ideally to measure underlying disease prevalence due to the biases associated with COVID-19. First, we measure the extent to which fevers are picked up by users of e-thermometers (data from Kinsa). E-thermometers are not randomly distributed, and not every individual who has an illness will be symptomatic. Fever measures may instead be viewed as a way to detect reduction in several sources of infectious disease that may be associated with fevers. As our second health measure, we look at the number of confirmed COVID-19 cases, while being aware that testing availability is very heterogeneous. Testing coverage depends on many things including availability of tests and testing protocols by state, which could be correlated with state policies. Nevertheless, there is reason to expect that to some degree, testing results may reveal information about disease prevalence. Lastly, we evaluate the number of deaths. Death rates are biased in that COVID-19 mortality has been associated with age and comorbidities, inequalities in availability of and access to health care services. It could also be that deaths are not all accurately reported. Nonetheless, real-time virus-related mortality is used in current simulation modelling research.

Data

Our study focuses on the 1st quarter of 2020, and the outcomes we examine are available in nearly real-time, but they are available only for recent months. We intend to refresh the data when more are available in order to understand medium-term impacts, but emphasis here is on the first 5 days after a policy is enacted. All our data are available at least at the state and county levels, the levels at which COVID-19 policy and information events data are typically available.

State and County Mitigation Policy Data

Using state-level policy data by day collected by Washington University researchers (Fullman et al. 2020) and Boston University researchers (Raifman and Nocka, 2020) as well as policies reported by the National Governors Association, Kaiser Family Foundation, and major national media outlets, we first considered roughly 15-20 separate policies that are tracked. All of the sources we draw on have conducted very detailed primary investigations in order to document the policy changes. However, many of these changes are likely to directly affect mobility (such as state laws banning utility cancellations for non-payment of bills). Some restrictions record different degrees of the same type of policy, such as gatherings restrictions by the size of the group affected, or closures of different types of economic activity.[1] Policy trackers also differ occasionally in whether they follow only mandates or recommendations as well. Given the difficulty of estimating effects of a large number of policies at once, we reduce the number we study through considering their role in our conceptual model and also by examining whether some policies were passed at the same time as other policies, whether a law was passed by a large number of states, and whether there was concordance across multiple sources. This is a building area of research and it is likely that in time there will be more unanimity in which policies are considered the strongest at affecting mobility.

[1] As an example of a policy that varies by degrees, consider the various forms of restrictions of gatherings of different sizes, which represent 10 of the 20 policies of Fullman et al. (2020). We summarize this policy by two of the policy variables available: one for any gatherings recommendation (22 states had such a policy action during our time frame) and one for any gatherings restriction (44 states had such policy actions during our time frame). We decided to further condense the variables to reduce the number of policies tracked, given their likely similarities in terms of implementation and mechanism of action.

Table 1 shows the initial list of six policy actions and two informational events we follow at the state level in this paper. The informational events are the announcement of the state's first COVID-19 case and death; we collect this date through reported data and also by searching news outlets; prior work finds the first state newspaper report of a case lead to substantial online search related to the virus (Bento et al. 2020). For the policy actions, we determined that Fullman et al. (2020) appeared most closely aligned for testing the mobility outcomes, thus this is our primary source for policy data. Fullman et al. describe their sources as the National Governors Association (NGA) and Kaiser Family Foundation (KFF), as well as their own investigations. We track the date of enactment, although we also conduct sensitivity checks with the date of issue. These two dates are on average one or two days apart from each other. Our intention was to see if we could pick up increases in movement in that short period as individuals prepare for reduced mobility.

The six separate state policies we initially track are below, roughly in the order in which they rolled out across states:

1. **Emergency declarations**: These include State of Emergency, Public Health Emergency, and Public Health Disaster declarations. While all states had pursued these policies by March 16th, and the federal government had issued its own emergency declaration on March 13th, we may not expect these alone to restrict mobility in the same way as, say, gatherings restrictions. Rather, states may use these laws in order to pursue other policies such as school closure (ASTHO, 2020) or to access federal disaster relief funds—to make decisions for which they would usually seek legislative approval. By statute, states are able to exercise additional powers when they issue such emergencies. In a typical state, governors are able to declare an emergency, and usually do so for weather-related cases—although some states, such as Massachusetts in 2014, have invoked public health emergencies in order to address addiction-related issues in the state (Haffajee, Parmet, and Mello 2014). In some states, city majors also may issue emergency declarations. In our conceptual framework, this is the earliest form of state policy that might restrict mobility, but it would do so through information and precaution channels.
2. **School closures**: Although some school districts closed prior to state-level actions, by April 7, 2020, 48 states had issued school closure rulings. "Formal closing of (at minimum) public schools" is coded in Fullman et al. (2020). We cross-checked this source against Education Week (2020) and decided to code two states (Iowa and Nebraska) as decided by Fullman et al. (2020) rather than Education Week. While school closure policies would reduce some travel (of children and staff), they could reduce adult mobility as well if parents changed work travel immediately as a result. School closures may also contribute to a sense of precaution in the community. Although many spring

break plans were cancelled, it is possible we might also capture increased travel due to school closures.

3. **Restaurant restrictions** (also including other partial non-essential business (NEB) restrictions): These policies were also fairly widespread, with 49 states having such restrictions by April 7th, according to Fullman et al (2020). This law would directly restrict movement due to the inability to dine at locations other than one's home.
4. **Gatherings recommendations or restrictions:** These policies range from advising against gatherings, to allowing gatherings as long as they are not very large, to cancellation of all gatherings of more than a few individuals. There was a lot of action on this front: 44 states enacted gatherings policies. These laws would reduce mobility in a manner similar to restaurant closings, but would have stronger effects given their universal nature.
5. (all) **NEB closures**: These occur when states have already conducted partial closings and now opt to close all non-essential businesses. Thirty three states acted in this area during our study period. NEB closure laws could have fairly large effects, as they reduce where purchases happen (like malls and restaurants) and reduce work travel.
6. **Stay-At-Home (SAH)**: These policies (also known as "shelter-in-place" laws) are the strongest and the most recent of the policies we track; these laws reduce mobility in very direct and obvious ways. A few states enacted curfews (which specify the hours when individuals can leave their homes), which we do not define as equivalent to SAH policies. A notable set of states have not issued a SAH in any part of the state (Vervosh and Healy 2020); as of April 3rd, these included Arkansas, Iowa, Nebraska, North Dakota, and South Dakota.

We attempt to conduct similar comparisons across county-level policy collections as well. However, there are not as many policy sources at this level. We were able to find data on four different policies at the county level from two sources. First, we obtained K-12 school or school district closure data from files archived by Education Week (2020). We combined these files (in some steps using fuzzy matching techniques) with school- or district-level information from the National Center for Education Statistics (NCES) to calculate the percent of students in a district as well as a county affected by school closures by day. Second, we obtained data on stay-at-home orders, emergency declarations, and business closings at the county-by-day level, from NACo (National Association of Counties 2020). These sources, as well as how we code county policies when state policies take precedence, are described in Table 2. We also created a variable for the date of the first case in the county as reported by New York Times (2020) to examine where this type of salient information may have led to precautionary reductions in movement in the community.

At the state level, we assessed which landmark events of the seven above we should investigate empirically by considering their relationship to mobility, and by examining the timing pattern.

SAH policies may have the strongest effects on mobility in terms of enforcement, but it is possible that people respond to information such as a the first positive case in the state and reduce movement substantially, whereas the SAH policy itself could come at a time when individuals throughout the nation may already have curtailed their activities through private actions or in reaction to national events. The first policy that all states took fairly rapidly was emergency declarations. On January 31st, US DHSS declared a public health emergency, under Section 319 of the Public Health Services Act (42 USC 247d). On March 13th the federal government announced a national emergency declaration.

We also assess more practically the ability to meaningfully separate the effects of different policies given that many happen at the same time. To do this, we enlist the help of two visuals, Fig 2.1 and Fig 2.2. Through the patterns visible in Fig 2.1, we condense the seven events to four and follow those throughout the rest of the paper. The first COVID-19 case in a state is easily set apart in timing from the other policies, as is the first COVID-19 death (Fig2.1). Emergency Declarations also appear separate. However, School Closures, Gatherings Restrictions, and Restaurant/Business Closings are likely too closely related to be separately identified. Thus, we follow School Closures, knowing that to some degree, the effect of the two other policies may be reflected in those results. Similarly, there is a close correlation between activity on non-essential business closures and SAH policies, although there is more policy activity in SAH laws; we select to follow the latter, as it essentially implies businesses would close too.

Fig 2.1 could make it appear that states are passing the different policies together, even if different states drive the action on each. Thus, in Fig 2.2 we examine the timeline of policy adoption for each state. We see that for many states the first COVID-19 case occurred relatively early, followed by emergency declarations. As it appears that the patterns in Fig 2.1 reflect what is happening at an individual state level, the state events we follow henceforth are State First Cases and Deaths, Emergency Declarations, School Closures, and Stay-at-Home laws.

For the county level, we show in Fig 3 that although we gathered data on four policies, there is inadequate variation in the Emergency Declarations and NEB Closures. The two more active ones are SAH laws and School Closures, which affect up to about 15% of the population at the most active point. School closures are measured on different axes in Fig 3 as those decisions are made at the school district level rather than the county level; we aggregate data from school districts to county level and determine a county as having a school closure if more than half of the students schools have closed; we tested sensitivity to 75% and 90% rules and find the results robust in terms of which counties we considered closed (there are very few in the middle range). In both SAH laws and School Closures, the states relevant for these counties all acted later, and so these lines go to zero toward the end of the period.

In Fig 3.2 we show separately the county COVID-19 case initiation pattern. Although the first case was reported on January 25th, there was a fairly long time lag before there was a substantial increase in terms of reported COVID-19 cases within a community, but after March 5th there was a rapid increase. As of March 5th, 1% of the U.S. population lived in counties that had experienced a first case. By March 15th this number was 50%; by March 25th it was about 90%, and this pattern has somewhat flattened since then.

As this section demonstrates, there are some principles we use for o selecting which of the 20 or so different state and local policies currently discussed in the COVID-19 policy literature we should track in our research on mobility. The key decision factor was ensuring close connections to our theoretic framework while considering (non-formally) whether we could plausibly separate the effects of these policies. In further work, we plan to consider further opportunities for investigating the heterogeneity of responses to the policies.

Social Distancing and Mobility Outcome Data

Our aim is to assemble several measures of how much individuals circulate in society, as proxied by detected movement of smartphones, to whom a "home" geographical location is assigned (a location is designated as home if that is where the location is detected primarily during the night). From such data, we can assess how much individuals circulate outside their state, their county, and their home, as well as how long individuals remain each day in their home, and how much their activities may put them in the same close vicinity with other devices. Several companies that use these device signal data for commercial purposes have provided researchers time-limited free access to these data to assist with efforts related to the current crisis. These companies typically receive data from mobile applications that include opt-in features for geolocation tracking. As these data are not collected primarily for research purposes and could have discrete jumps depending on which apps participate, there is value in confirming results across multiple sources. For this research, we use data from PlaceIQ {publicly provided} and SafeGraph {provided upon free research agreement}, and have pending applications with two other companies.[2] Safegraph data have been used in several analyzes so far, including Andersen (2020) and Painter and Qiu (2020). In both our data sources, we aggregate data at the county and state levels, by day.

Below, we describe the measures specifically, but first we outline the approach and the outcomes for which results are presented. We have **five distinct mobility measures**: 1) A measure of **interstate travel**: an index that measures how many devices in a particular state were detected to be out of state during the past 14 days. 2) A measure of **intrastate travel**: An index that

[2] Academics and others working for the public good can access the geolocation Safegraph data used in this paper for free at: www.safegraph.com/covid-19-data-consortium.

similarly measure how many devices in a particular county were detected to be out of the county during the past 14 days. 3) A measure of **community level "mixing":** an index that measures how many other devices where present during the day at locations where my device was present. 4) A measure of **time spent at home** that day: this is the average time a device is located in the home location. 5) A measure of whether devices **leave the home** for different types of activities (work, other) during the day.

PlaceIQ: we used the publicly available anonymized, aggregated location exposure indices (Couture et al. 2020a) from 2020-01-20 to 2020-04-02 (as of April 11th, 2020).[3] Thanks to Couture et al. (2020a), researchers can use PlaceIQ data in the form of N X N matrices at both the state and county levels. The location exposure index (LEX) is a matrix that provides the following information: among smartphones that pinged in a given location (one of 51 U.S. states or one of 2,018 U.S. counties), what share of those devices pinged in each location at least once during the previous 14 days? Although the full PlaceIQ state-level device data is meant to be nationally representative, the publicly available county LEX data are restricted to 2,018 of the more than 3,000 U.S. counties (counties with reasonably large device samples) to protect the identity of counties with few residents and devices in the PlaceIQ data (Couture et al., 2020). A further drawback is that we cannot tell if the same device travelled to more than one state. Couture et al (2020b) create a second measure called a device exposure measure (DEX) which detects for a given day, what was the likely exposure of a device in a county or state to devices in close proximity to the locations that devices visited that day. We consider this a measure of how much society "mixes" in that location.

More specifically, we use these data to construct a measure of out-of-state travel by summing the values across all states other than a home state, for each home state. Let $0 \leq p_{sjt} \leq 1$ be the fraction of cell phone devices in the PlaceIQ sample in state s on date t that were physically located in a different state $j \neq s$ at least once in the previous 14 days. Our index of out of state travel in origin state s on date t is the sum of all of the out of state ping rates. That is, we measure out of state mobility patterns using $p_{st} = \sum_{j \neq s} p_{sjt}$. The aggregate index is the sum of a collection of proportions and therefore it can take on values that are greater than one. Higher values on the out of state mobility index indicate that more people travel to more states. Lower values indicate that fewer people travel to fewer destination states. We construct a similar measure of out of county travel. Specifically, let p_{cdt} be the proportion of cell phones in the county c sample on date t that were physically located in a different county d at some point during the previous 14 days. Our county level aggregate index of out of county mobility is the sum of these dyad travel rates across the set of all possible destination counties: $p_{ct} = \sum_{d \neq c} p_{cdt}$.

[3] We downloaded the version, 0.5 (April 10, 2020,) from the github repository at https://github.com/COVIDExposureIndices/COVIDExposureIndices. Our totaling of the device counts shows that on average, there are 21.7 million devices per day from which data are gathered.

Fig4a shows the PlaceIQ data, representing our index for travel out of state in the last 14 days. Fig 4b shows the equivalent data but for travel outside the county (aggregated to the state level, so it represents the state average of how often devices moved outside their county during a moving average look-back period of 14 days). The separate states trends are recorded through color-coded lines separating out states that as of the end of the period had not enacted stringent policies. The smoothed national average blue line shows that there was a large decline across the board. A simple non-weighted average shows that for states on average, there is a 48% decrease in the out of state travel index over time between March 1st and April 9nd (from an index value of .6638 to .3432), and a 22% decline in the average movement outside of counties, nationally. Although data for March 2020 are not yet available from the U.S. Department of Transportation for (seasonally unadjusted) vehicle miles travelled, data for recent years (2018-2019) shows that the March value is typically 20% higher than February's value (U.S. Department of Transportation 2020).[4] When considering the overall reductions in mobility we observe nationally during March 2020, this places the statistics in stark contrast. The index for "mixing" is shown in in Fig4c and indicates a 73% reduction in this metric from March 1st to April 9th.

It is possible that states without much policy change likely experienced large declines of this type of travel. This is very evident in our results, as states with no SAH policies see declines in movement almost as dramatic as in other states, and states with policies see reductions before policies pass. A simple average of the 5 states with no policies (NYT, April 3rd) shows that their travel-out-of-state index declined by 31%, relative to the national decline of 40%.

SafeGraph: Another company that provides mobility data for COVID-19 research is SafeGraph, which provides research access to their data through free, non-commercial agreements. Safegraph reports that it tracks 35 million unique devices per month. These data provide (among other measures) the number of devices that are detected to be entirely at home during the day, or appear to be at a work location (as judged through prior patterns of location). We measure the "fraction who left the house" by calculation the ration of the number of devices who are detected to leave the house by the total number of tracked devices. The data also classify some devices as having left the house to be at a work location. In sensitivity checks, we separate devices that left the house for work reasons, to calculate the ratio of devices that left a house for a non-work reason.

Fig 4c shows the time trend in devices having left the house, again indicating separate lines for individual states and a national average in blue. From March 1st to April 14th, there was a 14% decrease in leaving the house. In unreported figures, when we excluded work travel, we could

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[4] Unadjusted VMT - U.S. Department of Transportation, Federal Highways Administration, Traffic Volumes and Trends http://www.fhwa.dot.gov/policyinformation/travel_monitoring/tvt.cfm, access date 4/16/2020

that this measure decreased by 23%. Although these seem low, it should be kept in mind that this is over a time when the typical trend is likely an increase, due to warmer weather compared to February. This measure is fairly generous in the meaning of leaving the house, as even 1m move outside the house would count, thus we also consider it one that may not show large adjustments; we have a final measure which records intensity of remaining at home—time spent (measured in hours) on average at home during a day. We illustrate these data in Figure x. There is a 31% increase in time spent at home between March 1st to April 14th.

Other mobility data: Several companies are making mobility data available for COVID19 related monitoring and research. For example, Apple (https://www.apple.com/covid19/mobility) released an index of request intensity for directions from Apple Maps, for walking, transit and driving, starting from January 13th 2020. Data are available for countries, and within countries, for select cities. In an Appendix figure, we analyzed the changes that occurred for several major cities and nationally. Between March 1st and April 15th 2020, there was a 37.6% reduction in requests for driving directions, a 71.6% reductions in transit directions and a 50.7% reduction in walking directions. Klein et al (2020) show with data from another device signal aggregator (Cuebiq.com) that commuting patterns have decreased in several major metropolitan areas in the United States through March 25th They pinpoint the decline to starting between Friday, March 13 and Monday, March 16, 2020, such that by Monday, March 23, 2020, we they find that "most major metropolitan areas in the United States experienced on average a 50% reduction in typical commutes to/from work".

Epidemiological Health Outcomes

We measure influenza-like illness (ILI) by e-thermometer readings in an aggregated and anonymous manner using data from Kinsa[5], a company that has sold these devices since 2013. According to the company's website, there are currently 1.3 million devices that provide data, and each daily reading is estimated to be based on 60,000-160,000 readings (https://content.kinsahealth.com/us-health-weather-map-faq). Kinsa's founder announced allowing the public to access these data (Hu, 2020). Although these data have been used in part for research and at least two papers (Miller et al., 2018 and Ackley et al 2020)) which have conducted validation exercises for older years of the data, we proceed by first conducting a validation exercise for 2020 data. We also caution readers that ILI is not necessarily a predictor of COVID-19, as fevers are not always a sign of the coronavirus; fevers occur for many reasons, and many patients with coronavirus do not have fevers (Hu, 2020). Thus, we consider this change in fever outcome as indicative of infectious diseases, many of which are linked to transmittable diseases. We conduct a few tests to verify data quality and we report these results

[5] We are grateful for these data provided to researchers by https://www.kinsahealth.co/enterprise/kinsa-insights/ "Powered by Kinsa"

in the Appendix. While future research should conduct analysis to consider pros and cons of data on fever symptoms from smart devices, these represent an important source of health-related outcomes absent other data. Miller et al. (2018) find e-thermometer data to correlate with national ILI activity as recorded by the CDC, and Ackley et al. (2020) find a high correlation with ILI data from the California Department of Public Health. In Fig 4c, we demonstrate that the prevalence of fevers has gone down nationally, reflecting the established pattern of the influenza B strain this season.

We next consider the number of COVID-19 cases and deaths, by county, state, and day. We use data directly from the New York Times github site (The New York Times 2020)..

Figure 4d and e shows the rate at which COVID-19 cases (and deaths) have increased by state and nationally, from March 1 onwards; data collected by *The New York Times*, based on reports from state and local health agencies.[6]

Methods

State-Level Event Study

We use event study regression models to examine the way that state-level measures of social distancing and the severity of the epidemic evolve during the period leading up to and following key policy and information shocks. Let E_s be the date of some specified policy or information event in state s. Then $TSE_{st} = t - E_s$ measures the number of days between date t and the event. For example, five days before the event, $TSE_{st} = -5$. Five days after the event, $TSE_{st} = 5$. We set $TSE_{st} = 0$ for states that never experience the event.

We fit event study regression models with the following structure:

$$1)\quad y_{st} = \sum_{a=2}^{15} \alpha_a 1(TSE_{st} = -a) + \sum_{b=0}^{5} \beta_b 1(TSE_{st} = b) + \theta_s + \gamma_t + \epsilon_{st}$$

In the model, θ_s is a set of state fixed effects, which are meant to capture fixed differences in the level of outcomes across states that are stable over the study period. γ_t is a set of date fixed effects, which capture trends in the outcome that are common across all states. ϵ_{st} is a residual error term. α_a and β_b are event study coefficients that trace out deviations from the common trends that states experience in the days leading up to and following a given policy or information event. Specifically, α_a traces out differential pre-event trends in the outcome that are associated with states that go on to experience the policy change or information event examined

6 Available at https://www.nytimes.com/interactive/2020/us/coronavirus-us-cases.html (The New York Times 2020). Last accessed April 11 2020

in the model. β_b traces out differential post-event trends in the outcome that occur after a state adopts the policy or experiences the information shock. Our main specifications are based on a balanced panel of states that are observed across the entire range of dates available for the outcome variable. Standard errors are clustered at the state level. We set the number of pre-treatment leads in the event study to 15 and the number of post-period lags to 5 because a larger set of post-treatment lags leads to substantial changes in the set of states contributing to each of the event study coefficients. Appendix Table A3 records the details of the state-level event study specification for each outcome variable analyzed in the paper, including information on the calendar period covered by the regression, the date of the policy/information event, the sample size, and the collection of states excluded from the balanced panel analysis.

County-Level Event Study

We pursue a similar analysis at the county level, which allows us to examine the effects of policy changes and information events that occur below the state level. At the county level, we let E_c be the date of some specified event in county c. $TSE_{ct} = t - E_c$ measures event time for county c. The county-level event study regression that we use in our main analysis is:

$$2)\; y_{cst} = \sum_{a=2}^{15} \alpha_a 1(TSE_{ct} = -a) + \sum_{b=0}^{5} \beta_b 1(TSE_{ct} = b) + \theta_c + \gamma_t + \sigma_{st} + \epsilon_{st}$$

In this version of the model, θ_c is a county fixed effect that captures time-invariant differences in the level of outcome across counties. γ_t is a date fixed effect that measures time trends that are common across all counties. σ_{st} is a $state \times date$ fixed effect, which allows for a flexible time trend that varies across counties located in different states but is fixed across counties within the same state. This also allows us to compare results in the state model to the county model to understand the response of individuals to state, compared to local, events. As in the state-level model, α_a and β_b trace out differential pre-event and post-event trends that occur during the days surrounding the focal policy or information event. In our preferred specifications, we limit the sample to a balanced set of counties that are observed in all periods and estimate standard errors that allow for clustering at the county level.

Appendix Table A2 records the details of the event study specification for each state- and county-level outcome variable analyzed in the paper, including information on the calendar period covered by the regression, the date of the policy/information event, the sample size, and the collection of states excluded from the balanced panel analysis.

Using Event Study Analysis To Study Transmission Rates

The event studies described so far are standard in the quasi-experimental literature. These models represent a reasonable approach to examine the effects of state and county policies on measures of mobility, but the ultimate goal of most of the mitigation policies we examine is to reduce the transmission rate of the virus and therefore reduce the case rate and the number of deaths. In this section, we attempt to interpret the connection between an event study regression and the parameters of a very simple Susceptible-Infected-Recovered (SIR) epidemiological model. The analysis suggests that an augmented event study regression with the number of new cases or deaths as the dependent variable and that controls for the lagged number infected cases provides one strategy for identifying the effect of policy interventions on the transmission rate parameter.

To illustrate, we consider a simple SIR model in which a homogenous population is partitioned into three compartments. Let N be the fixed size of the population. S_t is the number of people who are susceptible to infection at date t. I_t is the number of people who have active (contagious) infections at date t, and R_t is the number of people who have recovered from the disease and are no longer susceptible. The size of the susceptible population is equal to size of the susceptible population in the previous period minus any new infections. In symbols, $S_t = S_{t-1} - \beta I_{t-1}\left(\frac{S_{t-1}}{N}\right)$, where β is the transmission rate parameter and $I_{t-1}\left(\frac{S_{t-1}}{N}\right)$ is the expected number of contacts between infected people and susceptible people given a random mixing assumption. The number of infected people is equal to the number of infections in the previous period minus the number of new recoveries plus the number of new infections. Specifically, $I_t = (1-\gamma)I_{t-1} + \beta I_{t-1}\left(\frac{S_{t-1}}{N}\right)$, where γ is the recovery rate parameter. Finally, the size of the recovered population at a point in time is given by the stock of recovered people from the previous period plus the number of new recoveries: $R_t = R_{t-1} + \gamma I_{t-1}$. Most of the social distancing policies that state and local governments have been adopting can be viewed as efforts to change the value of the transmission rate parameter, β.

Continuing with the SIR notation, let $m_t = \beta I_{t-1}\left(\frac{S_{t-1}}{N}\right)$ represent the number of new infections on date t. In practice, it seems reasonable to view m_t as a random variable. If we assume that conditional expectation function associated with the number of new cases has an exponential functional form then we can write: $\mathrm{E}\left[m_t \middle| I_{t-1}, \frac{S_{t-1}}{N}\right] = \exp\left[\beta I_{t-1}\left(\frac{S_{t-1}}{N}\right)\right]$. In the early stages of the epidemic, it may be plausible to assume that $\frac{S_{t-1}}{N} \approx 1$. Imposing this restriction and taking logs gives: $\ln\left(\mathrm{E}\left[m_t \middle| I_{t-1}, \frac{S_{t-1}}{N}\right]\right) = \ln(\beta) + \ln(I_{t-1})$. This form suggests that we could estimate a Poisson regression of the number of new cases on date t on an intercept and the log of the lagged number of infected cases:

$$\ln\left(\mathrm{E}\left[m_t \middle| I_t, \frac{S_{t-1}}{N}\right]\right) = \alpha_0 + \alpha_1 \ln(I_{t-1})$$

The SIR model implies that the coefficient on the log of lagged infections is equal to 1. Thus, imposing the restriction that $\alpha_1 = 1$ implies that $\hat{\beta} = \exp(\widehat{\alpha_0})$ is a simple estimator of the fixed transmission rate.

This simple model essentially envisions a time series of data from a single epidemic with a fixed transmission rate that does not vary over time. Quasi-experimental regression models typically examine outcomes in multiple geographic units over time. Likewise, the key intension is to allow the transmission rate to change in response to policy interventions.

For example, suppose that there are $s = 1 \ldots S$ state specific epidemics and that m_{st} is the number of new cases from state s on date t. Let P_{st} be a vector of policy variables that differ over time and across states. Let $\beta_{st} = P_{st}\beta$ be the variable transmission rate parameter. Taking bigger steps away from the simple SIR model, suppose that θ_s is a state fixed effect that describe state specific factors that shape the epidemic. δ_t is a period fixed effect that measures common changes over time across all of the epidemics. An augmented Poisson regression with two way fixed effects is:

$$\ln\left(\mathrm{E}\left[m_{st} \middle| I_{s,t-1}\right]\right) = \mathrm{P_{st}}\alpha_0 + \alpha_1 \ln\left(I_{s,t-1}\right) + \theta_s + \delta_t$$

Imposing the restriction that $\alpha_1 = 1$ implies that exponentiated coefficients on the policy variables measure how the policy changes affect the transmission rate in a state. This model encompasses the event study model because P_{st} to be the vector of policy leads and lags. This framework imposes substantial structure on the epidemics occurring in different geographical areas. Like other difference in difference models, it requires common trend assumptions and strict exogeneity conditions on the adoption of new policies, and it also includes a lagged measure of infections with a parameter restriction. Nevertheless, we believe it is instructive to consider the sense in which these kinds of regressions may be informative about the transmission rate, which is a central parameter in most models of epidemics.

County Cross-Sectional Regressions

The event study models provide a structured way to assess how measures of social distancing and the severity of the epidemic evolved over a critical period surrounding specific policy events. Although it is interesting, the focus on temporal variation in key outcomes may obscure interesting geographical and cross-sectional patterns in the policy response to the epidemic and in the severity of the epidemic itself. To shed light on these patterns we estimate simple OLS regression models linking long differences in county-level measures of social distancing and the severity of the epidemic on a vector of county-level covariates related to the urbanicity, population size, demographic composition, socioeconomic status, and health of the county. As in the event studies, we fit these long difference models for five outcomes of interest: we look at changes between dates A and B in 1) the rate of out-of-county cell phone pings, 2) the likelihood of being home bound (the opposite of the event study outcome of leaving the house), 3) the number of influenza-like illness reports, 4) the cumulative number of confirmed COVID-19 cases per capita as of April 10th, 2020, and 5) the cumulative number of COVID-19 deaths per capita as of April 10th, 2020. The OLS models were relatively parsimonious and included a set of 29 county-level covariates: 16 age × gender prevalence variables and 13 additional county characteristics. We also fit lasso regressions of the same set of outcomes on an expanded set of 57 county-level covariates in the same categories as in the OLS, and use cross-validation to choose the penalty parameter. These models selected a sub-set of covariates from the expanded list and provide some insight into how well county-level social distancing and epidemic patterns can be mechanically predicted by observable county characteristics.

Results

Effects of Policy Changes and Information Events on Social Distancing Measures

State-Level Analysis

One of the primary objectives during the early stage of the epidemic has been to induce higher levels of social distancing in the population. We analyze the impact of state level policy and information events on five proxy measures of social distancing at the state and county levels. All mobility measures are patterns inferred from smart device data.

In Figure 5a, we show the event study coefficients from models, examining the impact of various state policy and information events on out-of-state travel. In Appendix table A2 we present the detailed event study regression results. The four panels in Figure 5a show negligible evidence of differential pre-trends leading up to each of the policy/information events. The event study coefficients trend downwards in the days after the first confirmed COVID-19 case in the state, the date at which the state government declared an emergency, and the date that the state adopted a stay-at-home order.

All three of these effects become increasingly negative over time, suggesting that people did reduce their mobility differentially in response to these events. However, the post event coefficients are significantly different from zero only for the announcement of the first confirmed case and emergency declarations. The effect sizes of the coefficients in Table A2 show that the day after the first confirmed case, there was a 1% reduction in our measure of out-of-state travel over the last 14 days. The 14 day backward-looking window covered by the outcome variable means that it will take time for a change in mobility to show up in the data. Nonetheless, the effect grows over time so that five days after the first case, out-of-state travel (in the last 14 days) had fallen by 4.03% (-0.0266/0.66). Similarly, out-of-state travel fell by 1%

the day after the emergency declaration and by 4.15% five days after the emergency declaration. The magnitude of the estimated effects of stay-at-home policies are even larger, at 3% after one day and 8% after 5 days, but they are noisily estimated and are not statistically different from zero. School closures deviate from the overall pattern of results. The event study coefficients imply that out-of-state travel rates increased in the days after schools were closed. This could be due to families with school-aged children travelling. We cannot assess whether this amounts to a reduction in travel as compared to other years, as our data for mobility measures only cover 2020, but we know from other data that March is usually a month of substantially higher mobility than previous months.

Figure 5b shows state-level event studies for a measure of the percentage of people in a state who travelled out of their home county during the previous 14 days. (We compute state-level averages of the county-level mobility rates, weighting by county population.) The results show a pattern of results similar to some out-of-state travel estimates, but generally these are smaller point estimates. Further, the results here are not statistically significant in any of the cases.

Figure 5c examines the effect of state level events on the index for "Mixing" (concentration of devices in particular locations). The results indicate a statistically significant reaction as large as a 16% decrease (point estimate divided by mean of dependent variable as of March 1st) in response to a state's emergency declaration; however, other policies show statistically insignificant or small effects. In Figure 5d, we examine the responsiveness of time spent at home to state events. This measure also does not display many statistically significant or large effects.

In Figure 5e, we examine our last cell-signal-based measure of mobility: the percent of devices that leave the home. This measure should reflect mobility changes immediately, unlike the measure in Figure 5a and b, which have a 14-day look-back period. The event study estimates from these measures suggest that there are slight decreases in mobility after first cases and after emergency declarations, and then slight increases after school closures. The drop in mobility following SAH policies is larger according to this measure. Some of the event study models are statistically significant, with effect sizes implying around a 4% magnitude.

County-Level Analysis

Next, we consider responses to county-level policies and information effects, where we examine variation only from county levels, not the state level equivalents that also cover counties. We ensure this by including state by day fixed effects in addition to event study specifications of the county policy. Here, we examine four measures of mobility, as we do not examine whether county policy affects interstate travel (the first outcome of the earlier set of results).

Figure 6a shows that 14-day lagged rates of travel outside of the "home county" fell in the days following the first reported case in a county (3.85% 5 days after the policy), after school closures, and after stay-at-home policies (not statistically significant but the magnitude corresponds to a 21.3% reduction 5 days after the policy). The coefficients for stay-at-home policies are marginally statistically significant, and the coefficients for school closures are statistically significant.

Figure 6b examines the effects on the index for society-wide “mixing”, finding that there are very substantial effects. There are the largest effects found in our analysis, indicating that first cases lead to an 8% decline, that school closures and SAH laws lead to an 18% decline, and first death reports lead to an 8% decline. This suggests that county level policies have been highly effective, and further research should explore the possible reasons. In Figure 4c we find that there are two marginally significant effects on time spent at home, but they are small—which is surprising given the results in Fig 6b regarding the effectiveness on the index mix.

Finally, Figure 6d shows event study estimates of the effects of the county policy and information events on measures of travel outside of the home. The results suggest that movement outside the home fell after each of first four county-level events, but only by about 1-4%. There is some evidence of differential pre-trends in the school closure event study, suggesting that people began reducing their travel patterns several days in advance of actual school closures. The magnitude of the post-event reductions is quite large in some cases. Mobility outside the home fell by 1% the day after schools were closed and by 4% after 5 days. Mobility decreased by 2% the day after stay-at-home orders were issued.

We investigate the robustness of the results through several specification checks of the state- and county-level event studies above. First, we fit augment specifications that include controls for the enactment of other policies. We fit models that remove the event study parameters in favor of a more parsimonious “difference in difference” specification, which imposes the assumption that there are no differential pre-trends and that the policy effects are constant over time. In unreported tables/figures, we find that these results are almost always very similar to the main models.

In another specification, we consider whether there would be a different response to the policy *issue date* as opposed to the *enactment date* (our base specification). We re-estimated all models with the issue date and found that results were quite similar; this was not surprising since the difference between issue and enactment timing is very slight in most cases. For emergency declarations, all but one state issued and enacted its policy on the same date. For school closures at the state level, the average state announced the closure two days before schools were actually closed. For stay-at-home policies, half of the states announced and implemented the policy on the same day. The other half had a gap of between 1 and 3 days.

The collection of states included in the regression models varies across outcomes and events. Because some policies (like SAH) have passed very recently, we may not always have 5 days of follow up for every state. Since we require a balanced panel in our main analysis, we exclude some states. In some cases, we also exclude some states because the outcome data are not available far enough back in time. For example, states' first cases happened early on, as did emergency declarations. Table A3 shows how many states are dropped for each specification. It also shows the time period during which we can estimate effects of law changes. In unreported results, we also consider how outcomes in one state are affected by policy changes in neighboring states. We estimated a DD specification, entering own-state policy variables and a variable for whether any neighboring state enacted a policy. Thusfar, we do not find any statistically significant effects of neighborhood policies, but intend next to explore effects on border counties separately.

Effects of Policy Changes and Information Events On Health Outcomes

The mobility measures considered above are designed to reduce the spread of the virus and are expected to eventually improve health outcomes. However, this cannot be done until sufficient time has elapsed to allow measurable mitigation impacts on transmission, something that is not possible with our current 5-day post-policy period. Instead, in this section, we examine the association between key policy events and the magnitude of the epidemic. We do not approach this exercise expecting causality in the observed relationships given the delay between infection and onset of symptoms (Zhang et al. 2020). The COVID-19 cases and deaths observed today were shaped by social interactions that occurred several weeks ago.

We examine event studies of three outcomes: influenza-like illnesses (ILI), confirmed cases of COVID-19 per population, and COVID-19 deaths per population would shed light on an important placebo check. The effects in the 5 days following policy changes that reduce mobility should be null. To the extent we see trends diverging, it would suggest that states or counties enact policies when they either had been distancing already or when they anticipate that the epidemic is about to grow in the state.

In Figures 7-10, we indeed find evidence of policy anticipation. That is, presence of a pattern in the 5 days after the law suggests that policy-adopting states may have had more community circulation of the virus prior to the policy change compared to other states. We find in Figure 7 that elevated body temperature becomes higher after the first confirmed case in the state. This is reasonable, as a first case suggests that the illness is moving around the community in those states. This result could also simply stem from individuals who experience fevers now being more likely to buy e-thermometers. We also find that fever rises after emergency declarations. There is little change following school closures and SAH laws. In Figure 8 we examine temperature data after county-level policy changes. We do not see much of a pattern after these events.

When we next look at cases, there is an additional reason why we should not expect causal relationships. Cases are strongly correlated to availability of testing (Kaashoek and Santanilla, 2020), so it could be that policy declarations increase state resources. Indeed we see that this is the case in Figure 9a. For school closures and SAH policies, there is a pattern of increase, although it is not statistically significant. There do not appear to be pre-trend violations, but the fact that there is a divergent trend that starts right at enactment *is* consistent with anticipatory policy (policies being passed by areas that expect a surge of cases). In Figure 9b, we observe a similar pattern for deaths, although there is a more obvious increase of deaths before the announcement of SAH policies, suggesting these policies were implemented in states as a consequence of evidence-based forecasts (deaths). In Figures 10a and 10b we look at whether county-level policy appears to be influenced in a similar way. Here, we obtain null results (except for a fall in cases after the school closure dates, although deaths were on the rise afterwards.)

Long Differences in Social Distancing and Health Outcomes

The event study analysis focuses on how various proxies for social distancing and population health change in the days following key policy changes and information shocks. But these analyses obscure the magnitude of the overall changes that have occurred over the past few months. They also make it hard to assess geographical variation in responses to the epidemic.

In this section, we examine cross-sectional variation in responses to the epidemic. We construct a county-level data set and compute the long difference in mobility measures from March 1 to March 31 for each county. In addition, we compute the cumulative number of confirmed COVID cases and COVID deaths in each county; because the epidemic stems from a novel virus, the cumulative cases can be viewed as a long difference changes with a starting point of zero cases and zero deaths. We link each of the county long difference measures with a collection of baseline county characteristics from the Area Health Resource File (AHRF) and the Community Health Rankings database (CHR). We use cross sectional regression models to assess what county-level factors are associated with larger overall changes in social distancing and population health over the first few months of the epidemic.

Across the 2,008 counties with outside county movement in our sample, the average long difference change in out-of-county ping rates was - .49, suggesting that many people did actually reduce their travel and circulation. However, the size of the long-distance change varies substantially across counties. Some counties experienced much larger declines in out-of-county travel than others. About 30% of counties actually experienced an increase in out-of-county travel. Likewise, our measures of the severity of the epidemic (influenza-like illnesses, confirmed COVID-19 cases, and COVID-19 deaths) also vary substantially across counties.

Table 1 shows estimated regression coefficients from models that include measures for county population, population density, metropolitan area type, median age, percent white, percent black, median household income, poverty rates, health uninsurance rates, and indicator variables for whether the county has a recreation-intensive economy and whether the county is a retirement destination area. These simple models predict a substantial share of the variation in some measures of movement. The covariates explain almost half of the variation in the reduction in out-of-county travel. The measure of out-of-home movement is much harder to predict using county-level covariates, and the simple model only explains about 0.7% of the variation across counties. County-level covariates explain about 15% of the variation in COVID-19 cases and about 4% of the variation in COVID-19 deaths.

The regression models of the long difference in out-of-county travel suggest that mobility fell more in counties with larger populations, more density, and larger urban centers. The fall in out-of-county mobility was much larger in counties with recreation-based economies and larger populations of people without health insurance. The 2016 Republican vote share is positively associated with the long difference in out-of-county mobility, which may suggest that counties with more Republican voters have not embraced social distancing as much as counties with more Democratic voters. The age and gender structure of the county is also associated with the long difference in outcomes. Counties with a larger prevalence of men and women ages 30-44 had steeper reductions in out-of-county travel, as did counties with a larger prevalence of men ages 75-84 and women ages 55-59 and 60-64. In contrast, counties with more men ages 55-59 and 60-64 tended to make smaller reductions in out-of-county mobility.

In contrast, county-level characteristics explained very little of the variation in measures of the severity of the epidemic across counties. The e-thermometer-based measures of changes in flu-like symptoms were substantially lower in counties that are classified as retirement destinations. This might reflect weather patterns and the end of flu season. Percent black, median household income, and poverty rates were all predictive of a higher number of confirmed COVID-19 cases.

LASSO

We fit a Lasso regression using an expanded set of 49 regressors and cross-validation to choose a collection of covariates that were strong predictors of the outcomes. The Lasso pruned out all of the covariates in the models for influenza-like illnesses and nearly all of the covariates in the model for of outside-the-home mobility. This suggests that the county-level variation in these outcomes is fairly random at this stage of the epidemic. In contrast, a substantial number of covariates were selected for models of out-of-county migration, COVID-19 cases, and COVID-19 deaths. We hope to explore the predictive value of these models in more detail in later work.

Conclusion and Discussion

Social distancing has emerged as a major intervention during the Covid-19 epidemic. The health threat posed by the virus provides a direct incentive for individuals to avoid physical interactions and, unlike in some other instances such as public smoking bans where the threat of fines and taxes maybe the most effective ways for policy to change public health behavior. However, the private responses of individuals will likely be insufficient to account for externalities and to contain the epidemic. Thus, government policy in this area plays an important role in theory and in practice, during the early months of 2020. State and local governments have embraced this role to varying degrees and have adopted a set of policies that they hope will increase the amount of social distancing beyond the levels that would arise from private responses alone. The federal government also plays an important role, especially in the form of subsidizing social distancing (such as through enhancing unemployment benefits) recognizing the positive externality and sacrifice it represents, however our focus is on state and local policy and news events. In this paper, we used smart device cell signal data products as a proxy for social distancing behavior, and we use event study regressions to identify the incremental change in mobility that is attributable to specific government actions. The estimates we present provide insight into which policies seem to generate the most social distancing in the short run. The short run is important in this case because slowing the pace of the epidemic – flattening the curve – is one way to try to avoid surges in the demand for health services that exceed the capacity of local hospitals and health care systems.

The results from our event study regressions suggest that the timing of most state and county level policy changes are relatively exogenous with respect to mobility. That is, we see little evidence of differential pre-treads in the mobility data leading up to policy events. The analysis suggests that several mitigation policies have enhanced the level of social distancing in local communities, but not as much as has occurred through nationwide responses not spurred by specific state and local actions. The precision of the estimates varies somewhat across different measures of mobility. For example, we find evidence that the 14-day moving average of travel outside the "home county" falls by about 5% in the week after school closures and in the week after stay-at-home orders. Similarly, we find that movement away from the "home address" fell by 1% the day after schools were closed and by 4% after 5 days. Movement away from the home address decreased by 2% the day after stay-at-home orders were issued.

Although they may well be important from a public health point of view, our estimates so far suggest that government policies have mainly served as a small supplement to the voluntary increases in social distancing that individuals, families, and businesses have adopted on their own. We should bear this in mind when contemplating the likely effects of government decisions to retract some or all of their social distancing policies. It is possible that lifting stay-at-home orders and re-opening schools may have differential effects on overall social activity depending on the corresponding change in national or global actions and prevailing attitudes (Cornwall 2020).

While we show that policy changes are relatively exogenous to the outcomes we consider in that our parallel trends tests are met, research increasingly suggests that policy making has been shown to occur on a partisan basis. Adolph et al. (2020) find that "Republican governors and governors from states with more Trump supporters were slower to adopt social distancing policy". Notably, they do not find that caseloads appeared predictive of the enactment of these policies. It is plausible that private responses may also follow a partisan structure. In addition, the ongoing economic costs of the epidemic and of social distancing means that individual people may find it increasingly difficult to maintain a high level of social distancing.

What is learned here compared to earlier epidemics? We contribute to existing papers that show the effectiveness of policies on mobility, although the 1918 epidemic differed in many ways from the current crisis. For one, the 1918 epidemic affect the young more than the current epidemic. If the young are more mobile, and consider health threats as not as severed, policies may face more resistance in attempts to reduce mobility. Indeed we find that the age distribution in the county is correlated with mobility reduction during March 2020.

What should aim for as the optimal amount of social distance reduction, balancing costs and benefits? These policies are judged here only on the extent to which they reduced mobility, but not in a normative sense. There are of course mobility needs that are unobjectionally necessary, and our measure of mobility do not measure whether actual unprotected contact or proximity occurred. Essential workers mobility is captured by our measures, but cannot be separated out. In future work we will attempt to control for the different occupational and industry distributions across geographical areas, as an example. However there is also an economic tradeoff implicitly made between lives saved and economic decline, which Friedson et al (2020) discuss with information on mortality vs. jobs, and which is build into unemployment benefits and other payments being directed at whose jobs are lost in an attempt to socially distance. Barro et al (2020) use data from the 1918-1920 flu deaths to predict that GDP and consumption could decline 6 and 8 percent from the current COVID-19 crisis. These predictions are, however, likely to have large standard errors and be subject to many mobile parts from policies, behavioral responses, but it makes the point that although mobility restrictions are costly, the aim is to lower mobility as much as possible while maintaining the essential services, assessing the benefits in terms of lives saved to be worth the jobs lost, until a vaccine or other pharmaceutical solutions are available.

Several caveats should be kept in mind when considering our analysis. For one, this represents a preliminary analysis, and carried with it the usual caveats that apply to analysis of novel data sources. However, there is a need to examine real-time data and for fast circulation of preliminary research papers. Second, there are different possible ways of coding state and local policies, and there is heterogeneity of implementation even for similarly worded policies. We largely defer to other ongoing efforts, and focus our attention on creating a typology and estimating impacts on mobility. Our results should be interpreted as an effect on average that may mask substantial heterogeneity across states. Third, our measures of mobility come from commercially provided data; although these data have been used in research before and we use data from multiple companies, there is no known universe from which these observations are sampled. Fourth, our measures of the spread of the virus may be poor proxies; the number of positive cases is a function of several things other than the spread of the virus, including availability of testing. It could be that state policies and availability of tests both initiative concurrently --if that is the case, the observed reductions in positive cases may be understated. However, if state policies on social distancing are considered a substitute by states to make more tests available, the results here may be an understatement.

Despite these caveats, we believe that our work contributes to understanding the determinants of both government policy choices and voluntary social distancing behaviors, an important topic for further continued research.

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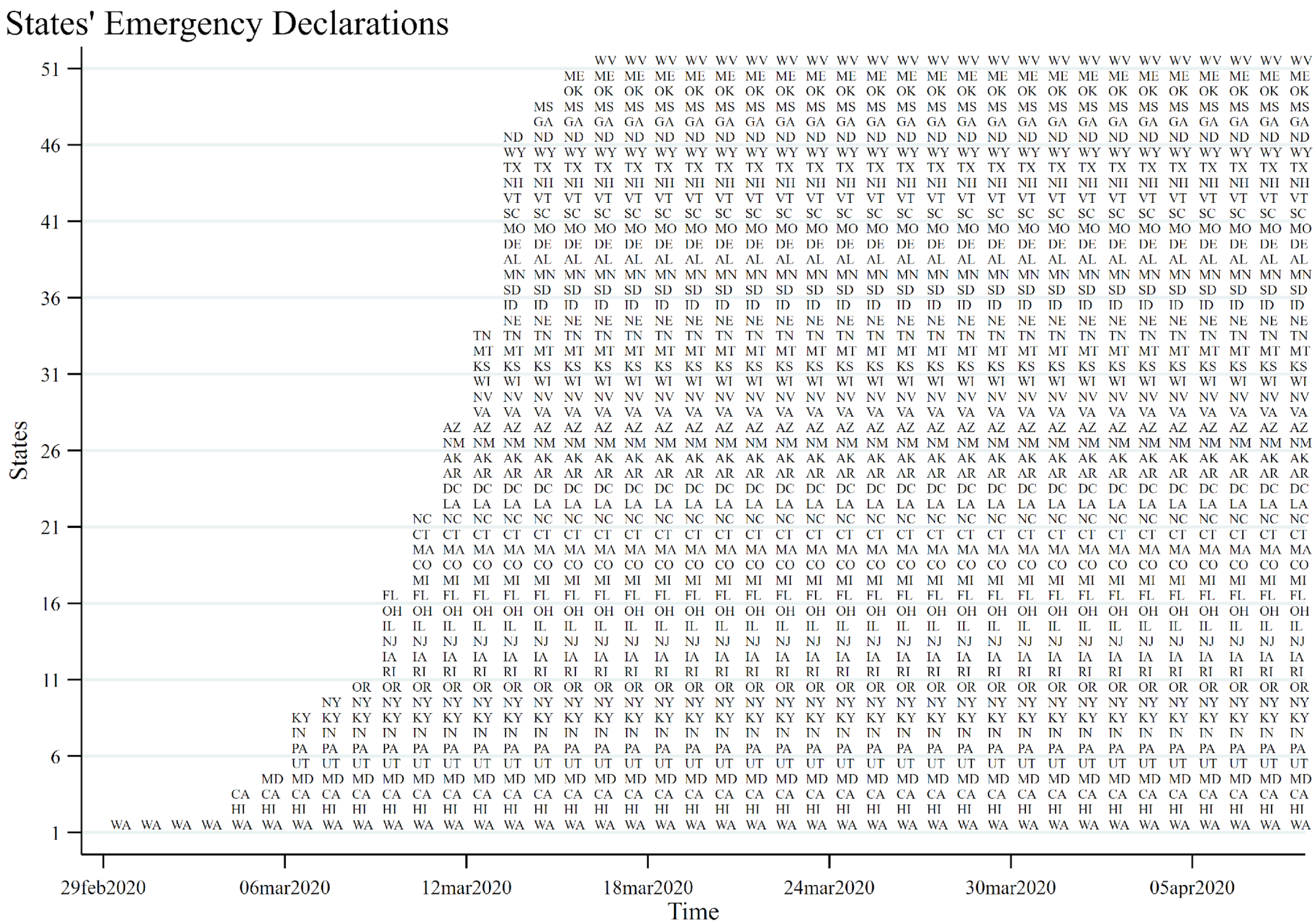
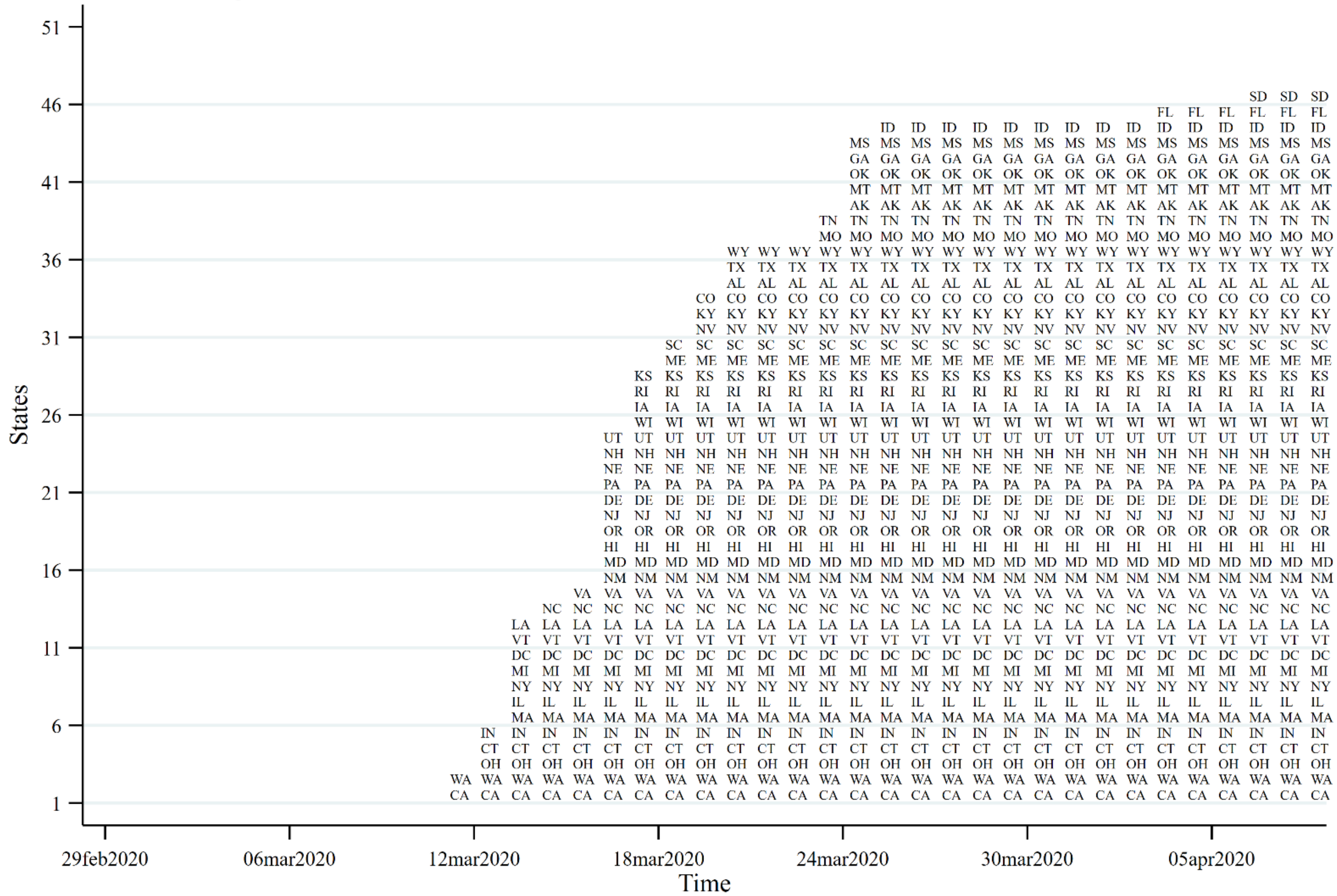
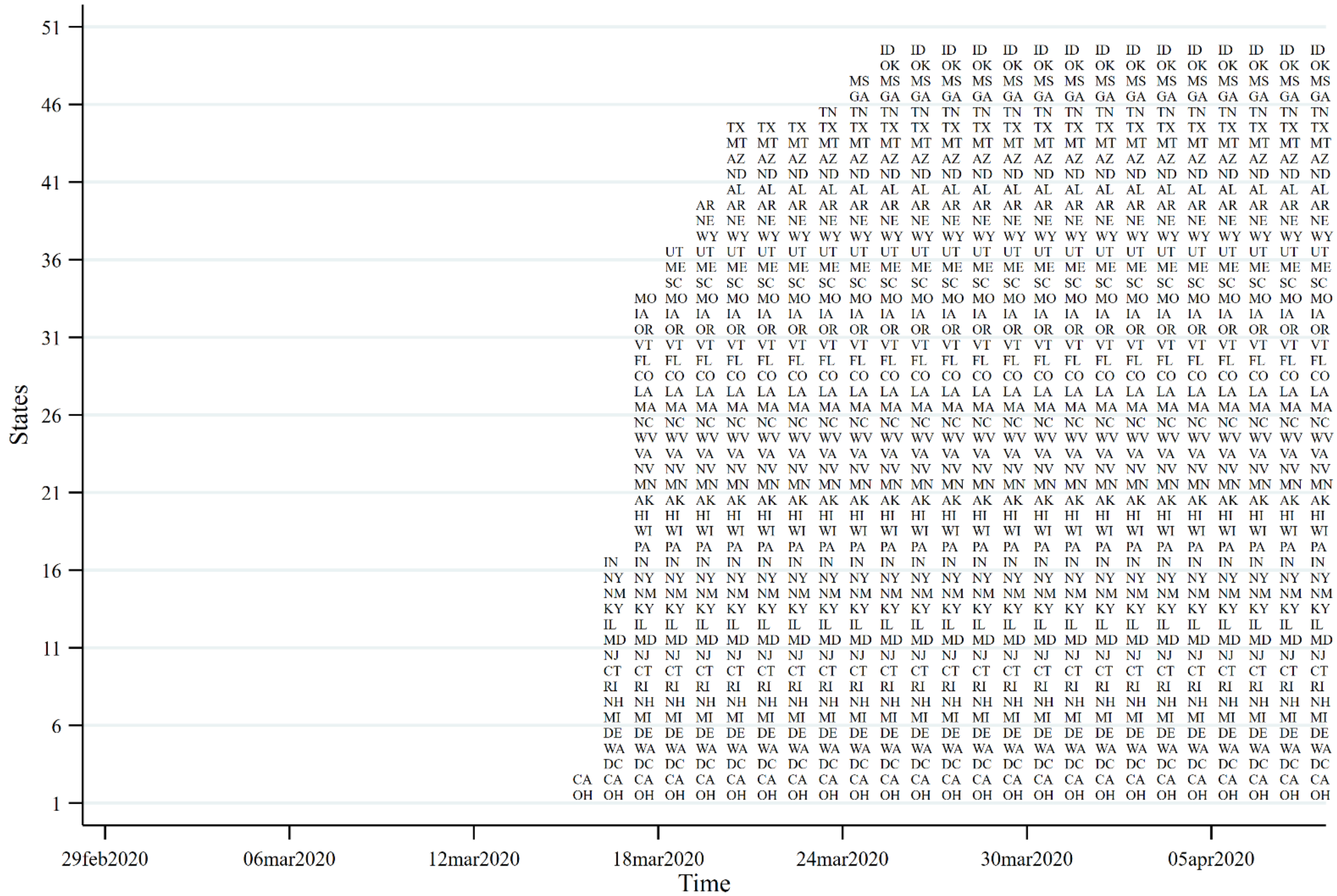
Fig 1—State COVID-19 Policy Enactment and Information Dates (several figures)



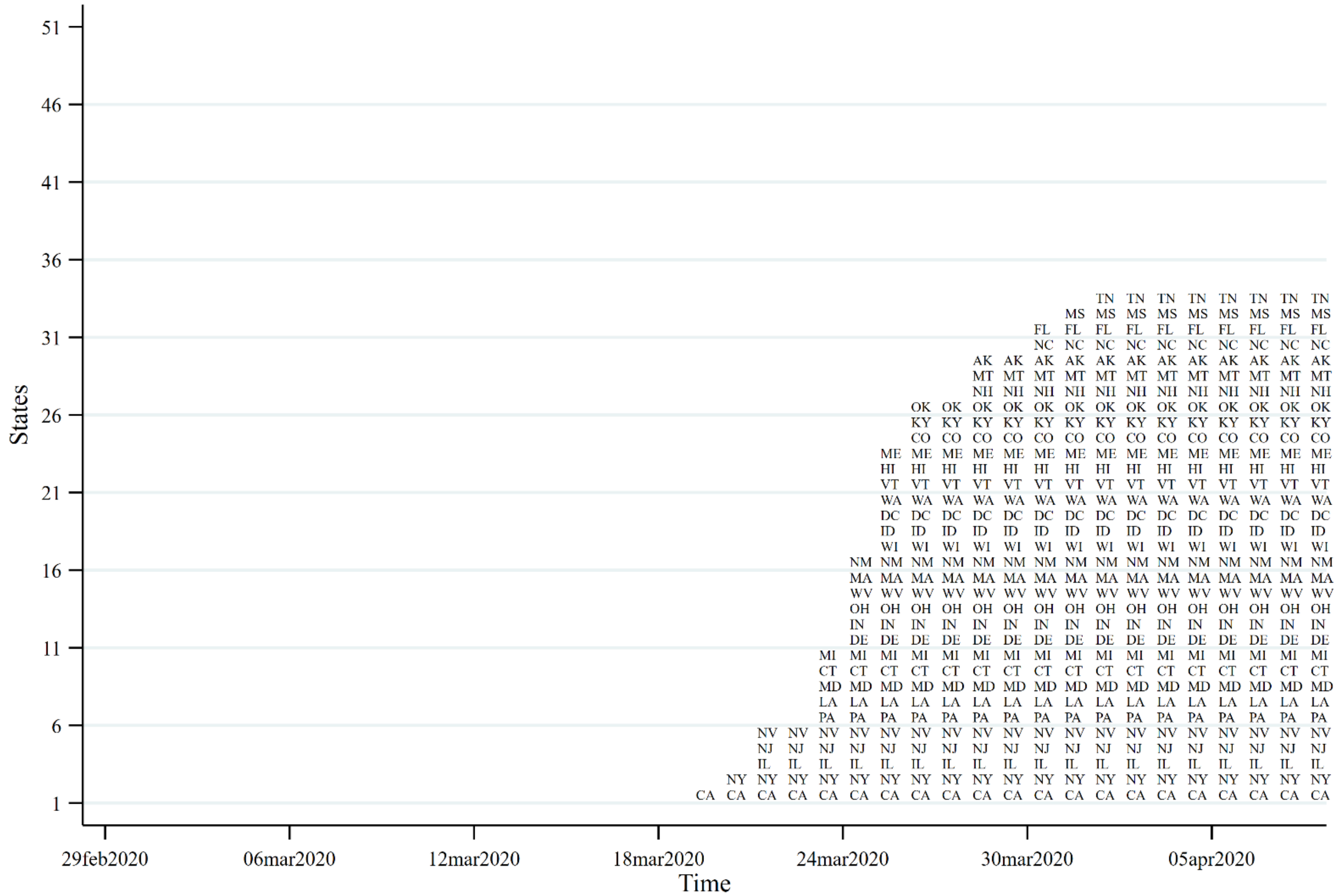
States' Gathering Restrictions



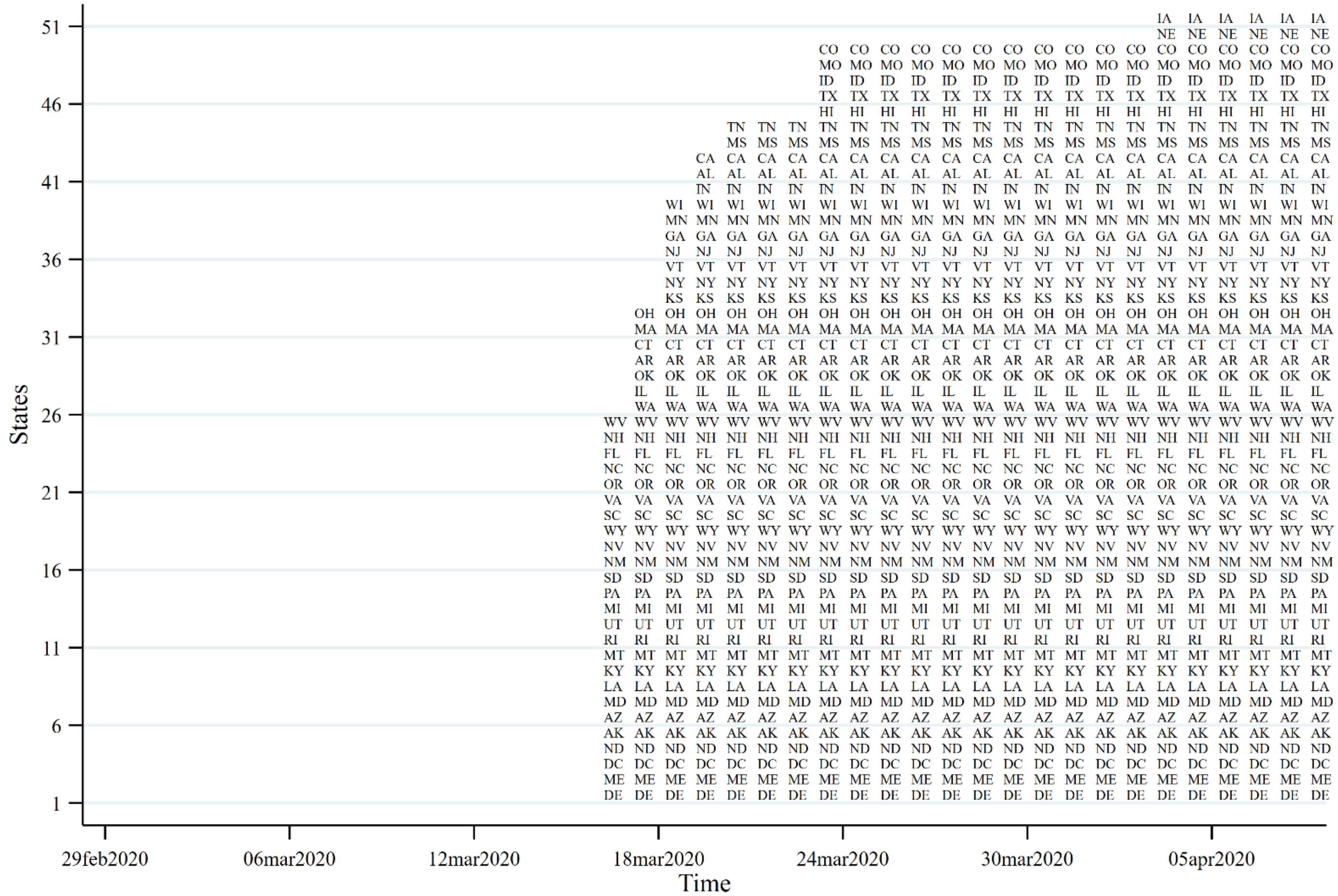
State's Restaurant/Business Closures

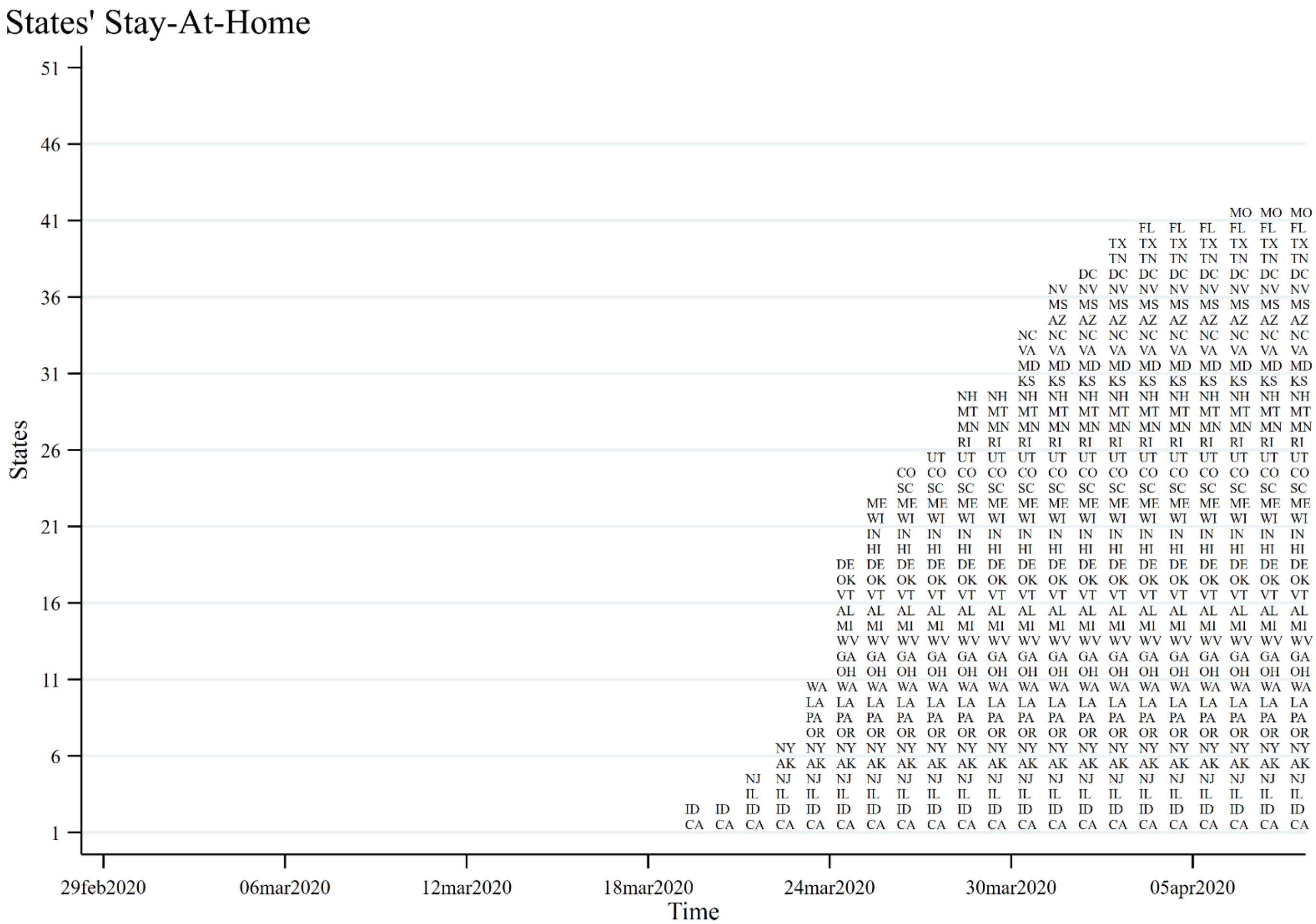


States' Non-Essential Business Closures



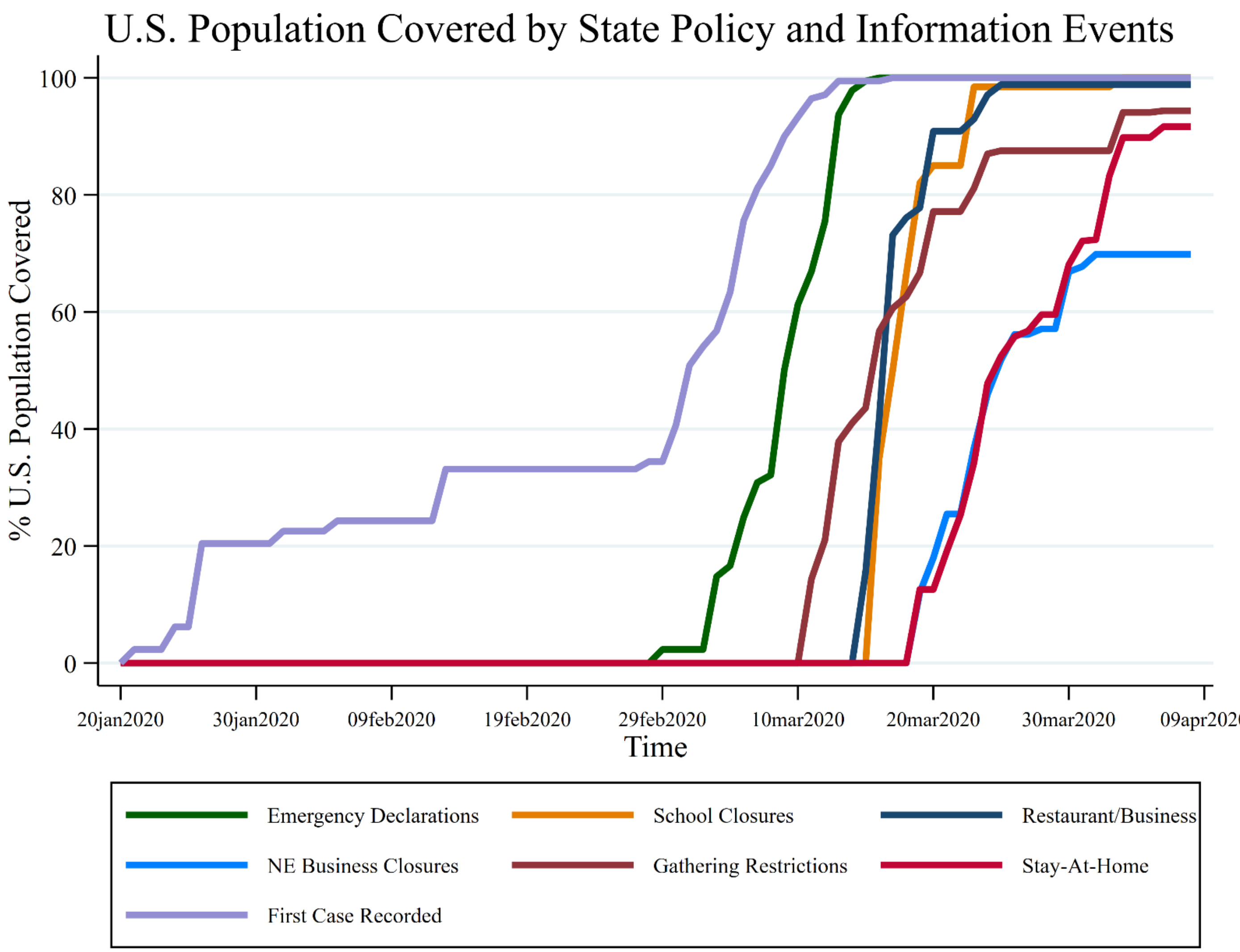
States' School Closures





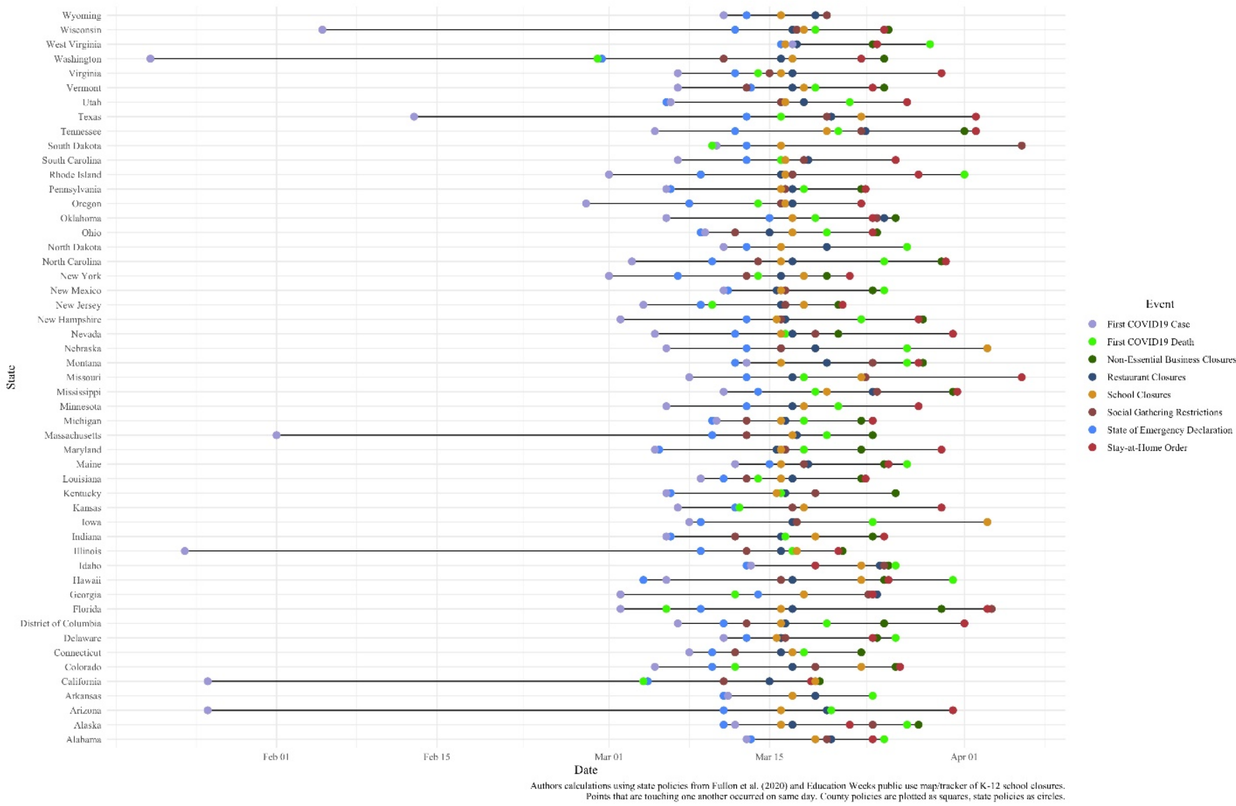
Source for All Fig 1 graphs: Author compilations based on Fullman et al (2020), the public-use map/tracker of K-12 school closures (Education Week, 2020), and author compilations from original sources. For the first figure of state first cases, we hand-collected the timing of the first COVID-19 case announcements from local media reports in each state for Bento et al (2020), cross checking them with other sources including coronavirus.jhu.edu).

Fig 2.1



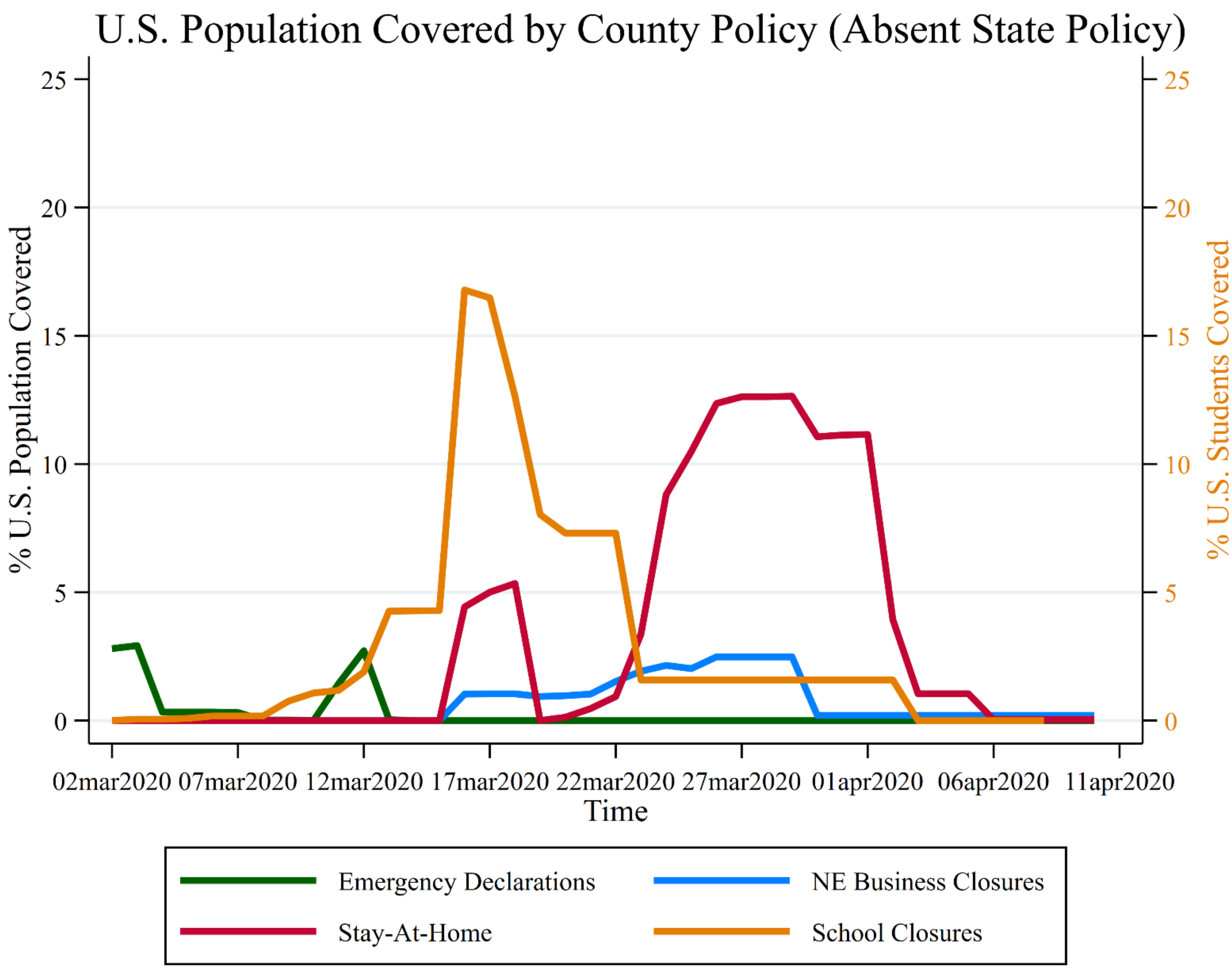
Notes: Please see notes to Figure 1. Each line represents the percentage of the U.S. population exposed to the corresponding state policy or information event between January 20, 2020 and April 9, 2020.

Figure 2.2: State Policy and Information Timelines



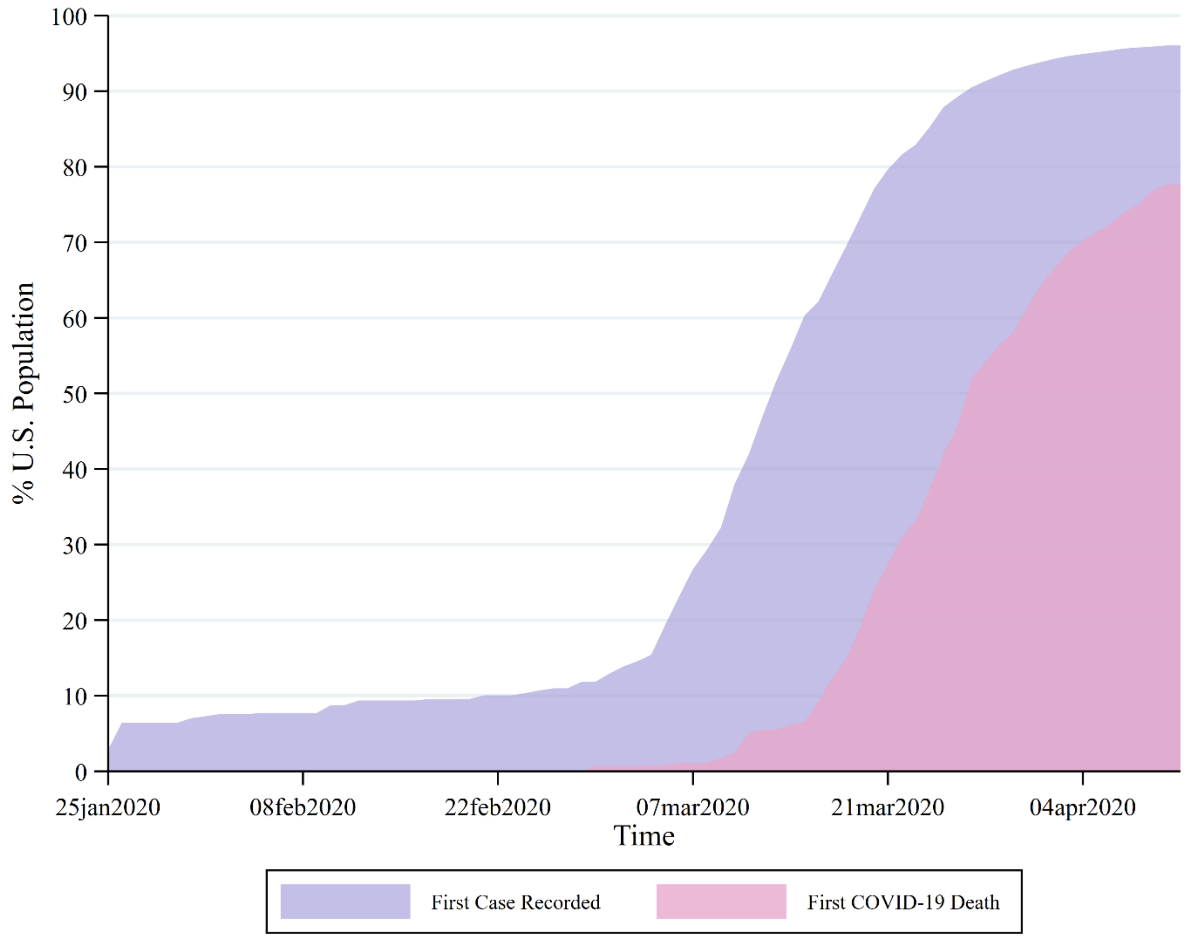
Note: Figure shows for each state, the timeline of their policy and information events shown in the legend; these are all the data presented in Figures 1 and 2.1

Fig 3.1



Notes: See notes to Figure 1. We use the 2018 county populations as the weights. Each line represents the percentage of the U.S. population exposed to their resident county's policy, absent a concurrent corresponding state policy. For school closures, we presented the percentages of students off from schools. The first county emergency declaration was announced in January 25. Note that the right axis refers to the school closure measure, as we denote it by percent of students covered. The left axis measures the percent of the US population represented by the relevant counties.

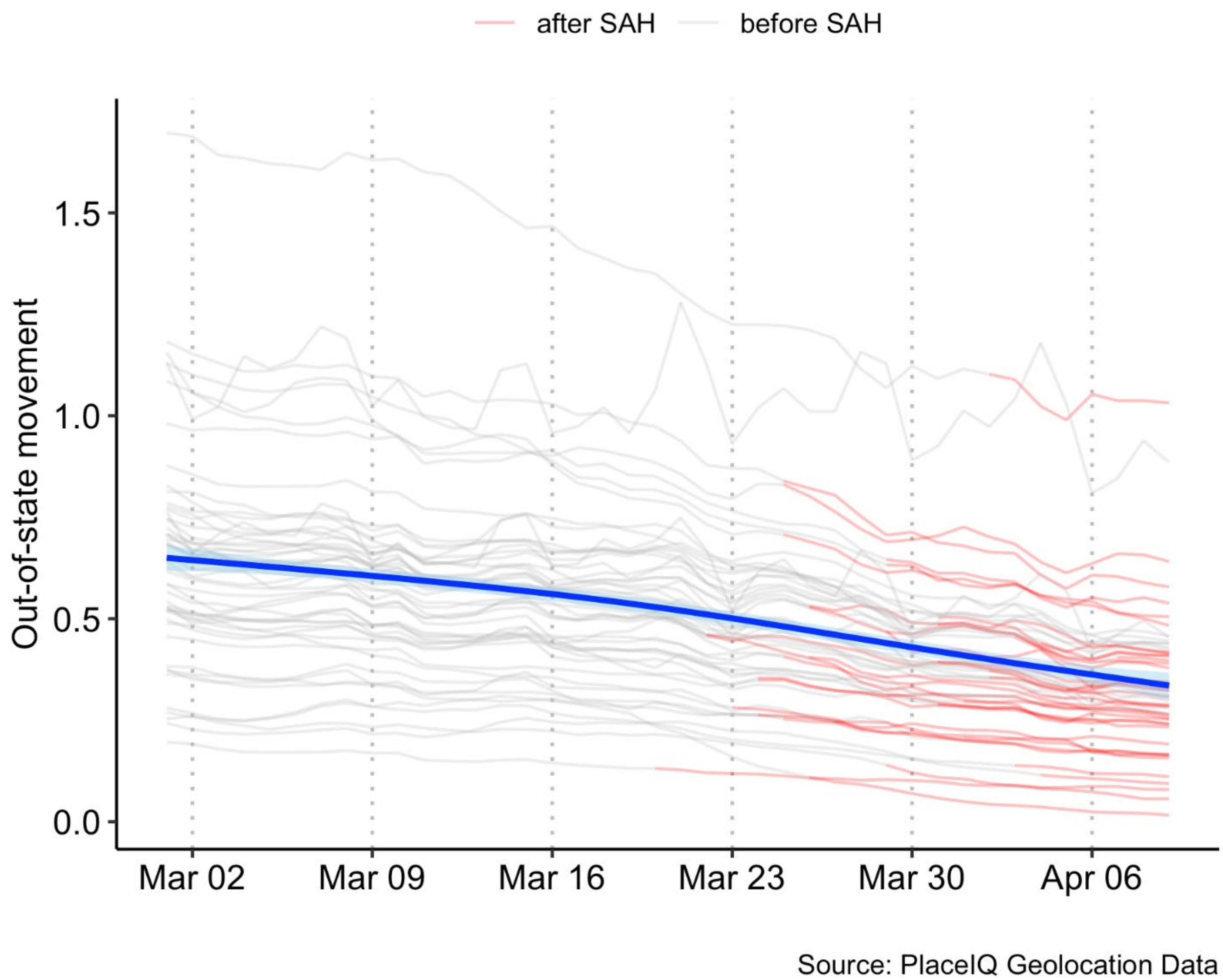
Fig 3.2 Timeline of U.S. Counties Experiencing First Positive Case



Sources: Data from The New York Times, based on reports from state and local health agencies, available at https://www.nytimes.com/interactive/2020/us/coronavirus-us-cases.html. Last accessed April 11 2020

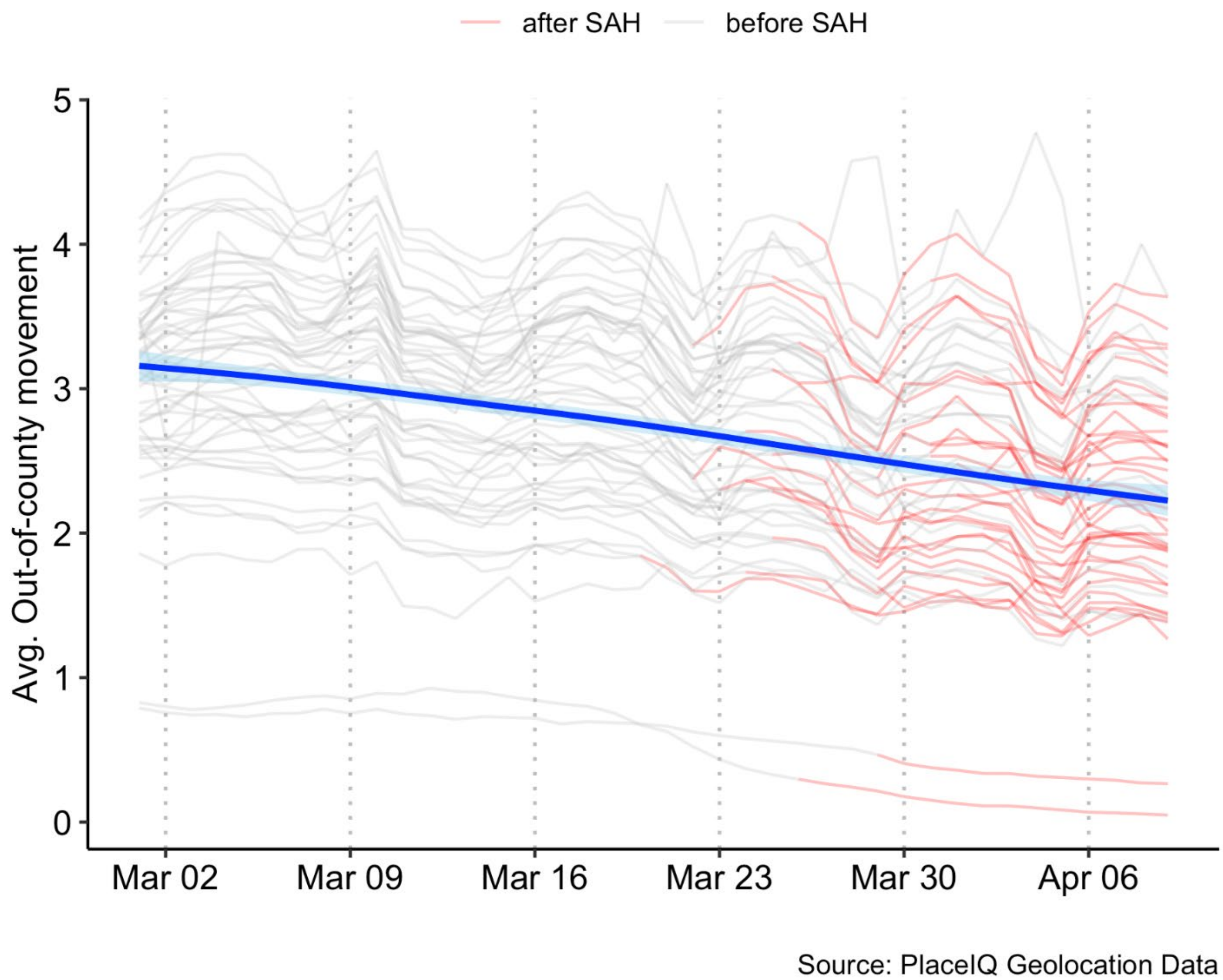
Fig 4: National and State Time Trends in Outcomes

<u>Fig 4a: Index for Leaving the State (in last 14 days), by Day by State (March 1- April 9th 2020)</u>



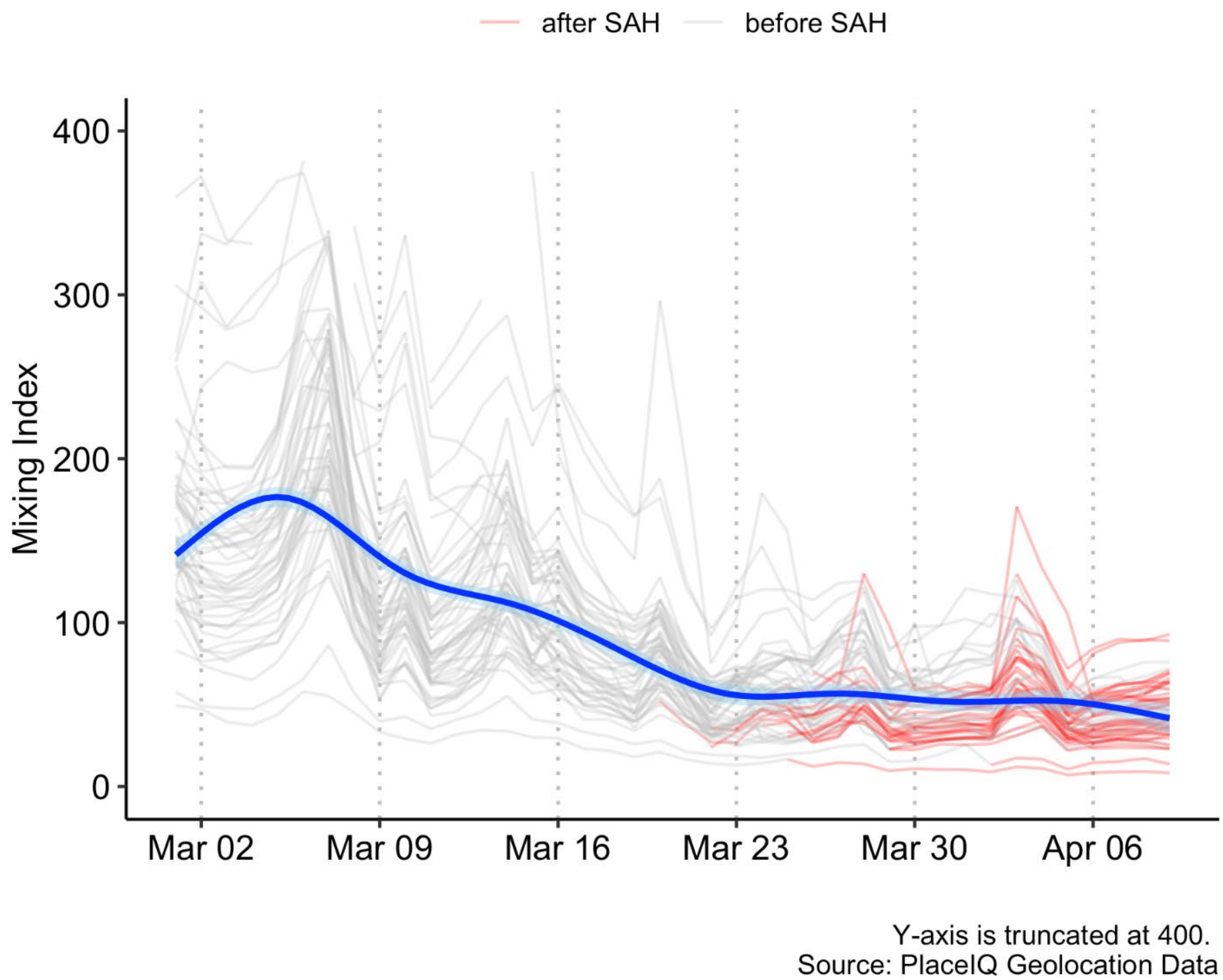
Note: Each thin line represents a state, and shows the sum of the percent of cell phones detected out of state in the last 14 days. Red lines represent states with SAH laws, for the portion after the laws are in effect. The thick blue line represents a "smoothed" national local average (a generalized additive model (GAM)) of the states.

Fig4b: Index for Leaving the County (in last 14 days), by State by Day (March 1- April 9th 2020)



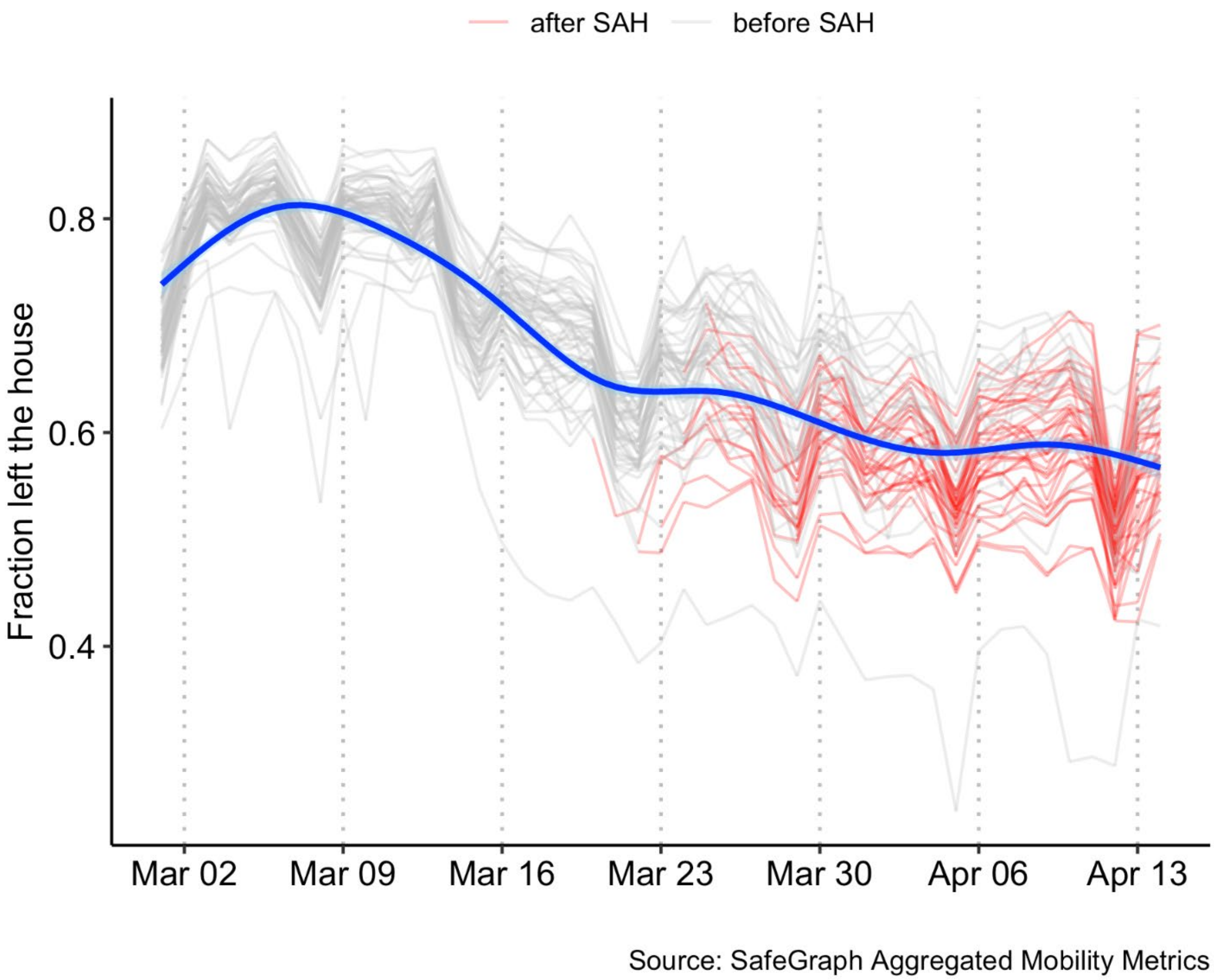
Note: Each grey line represents a state, and shows the sum of the percent of cell phones detected out of the home county, in the last 14 days, population weighted average at the state level. Thus, this is the state's average of people's movement out of county. Red lines represent states with SAH laws, for the period after the law is in effect. The thick blue line represents a "smoothed" national local average (a generalized additive model (GAM)) of the states.

Fig4c: Mixing Index, by State by Day (March 1- April 9th 2020)



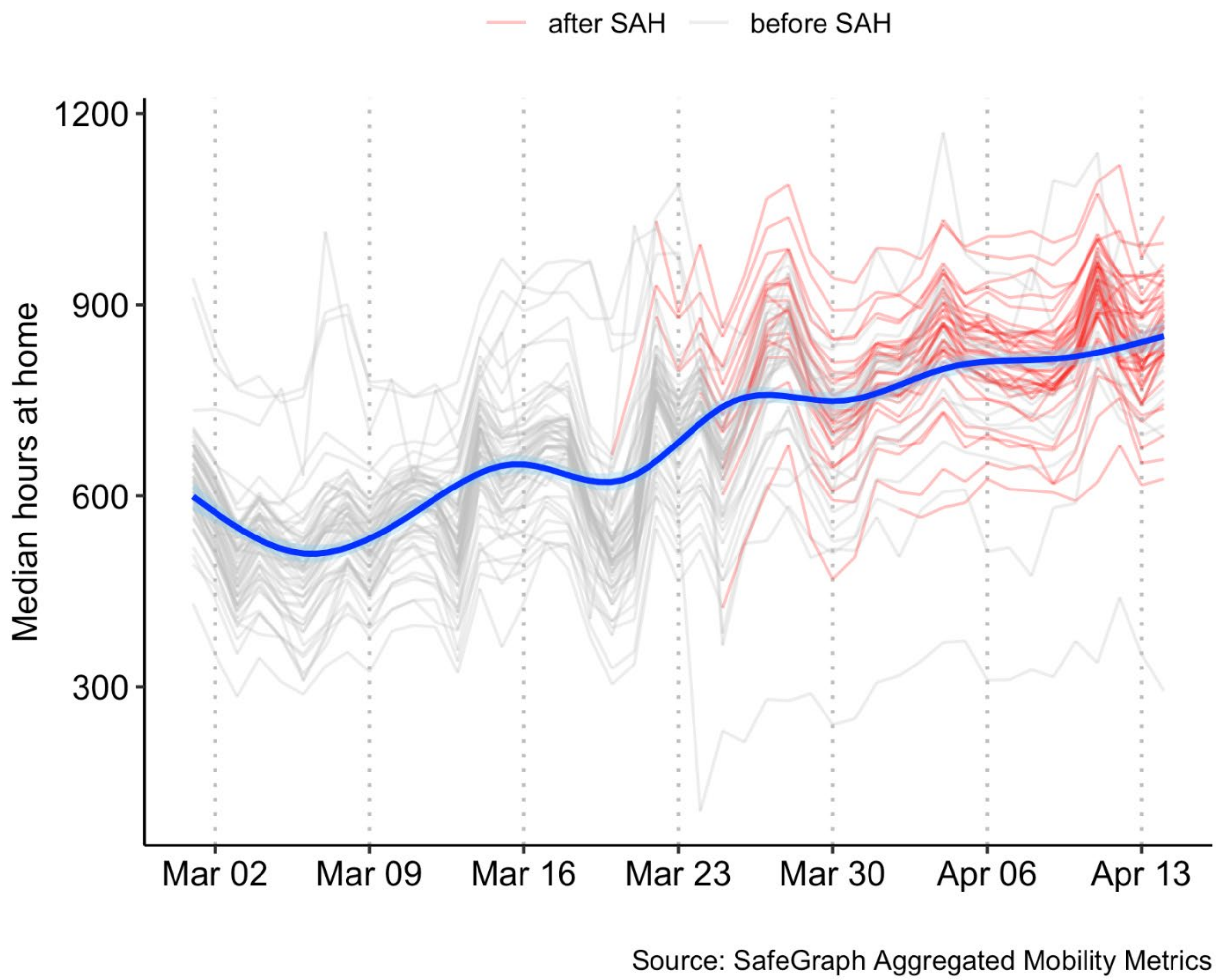
Note: Each grey line represents a state, and shows the value of an index for the amount of mixing of device-owners that happens in a state on that day. Red lines represent states with SAH laws, for the period after the law is in effect. The thick blue line represents a "smoothed" national local average (a generalized additive model (GAM)) of the states.

Fig4d: Percent leaving the House, by Day by State (March 1- April 14th 2020)



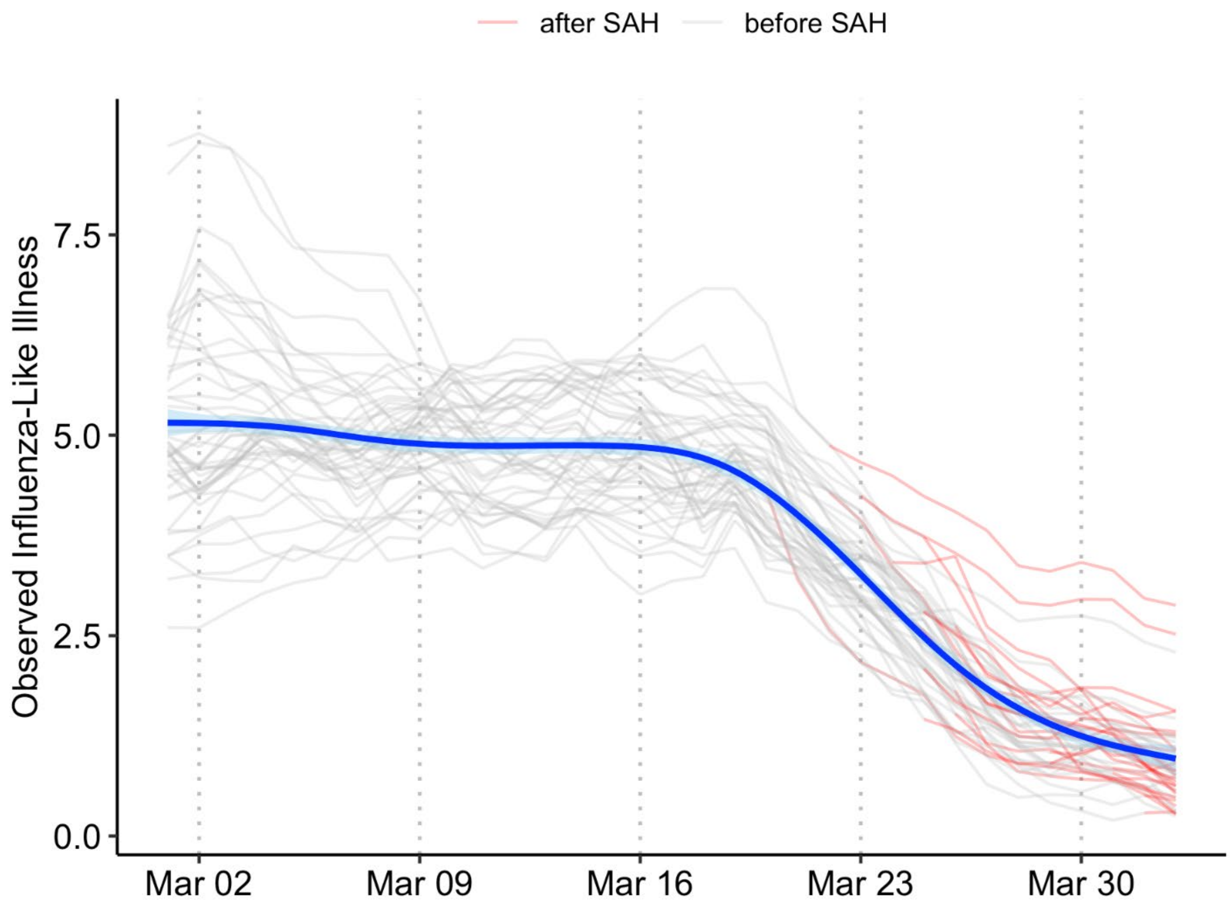
Note: Each grey line represents a state, and shows the percent of cell phones detected out of the house at some point during the day. Red lines represent states with SAH laws, for the period after the law is in effect. The thick blue line represents a "smoothed" national local average (a generalized additive model (GAM)) of the states.

Fig4e: Mean Hours at the House, by Day by State (March 1- April 14th 2020)



Note: Each grey line represents a state, and shows the mean number of minutes a device spent in total in the house during the day. Red lines represent states with SAH laws, for the period after the law is in effect. The thick blue line represents the population weighted average of the states' values.

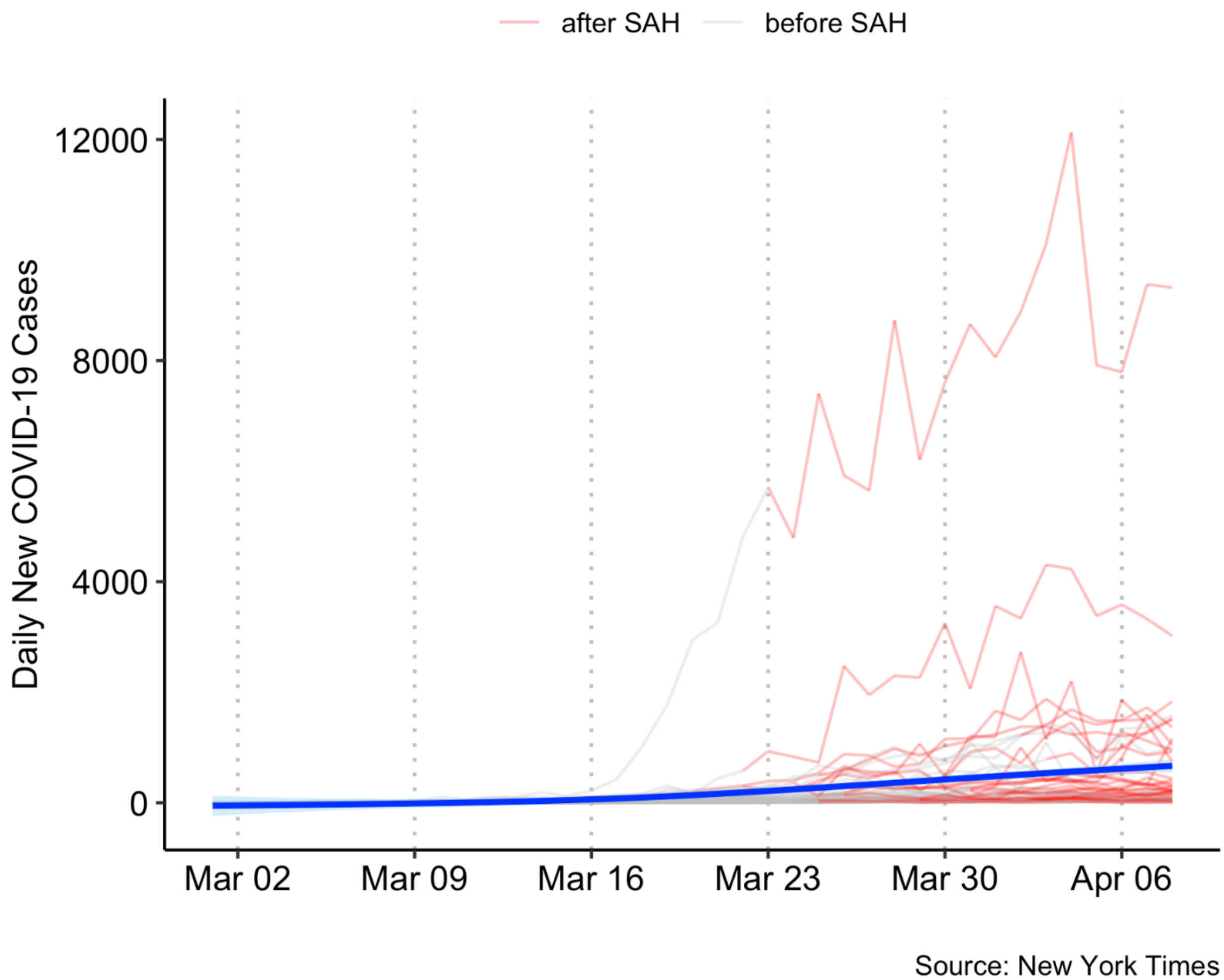
Fig4f Percent with Feverish Symptoms, by State by Day(March 1-April 2nd)



Source: Kinsa Thermometer Network Data

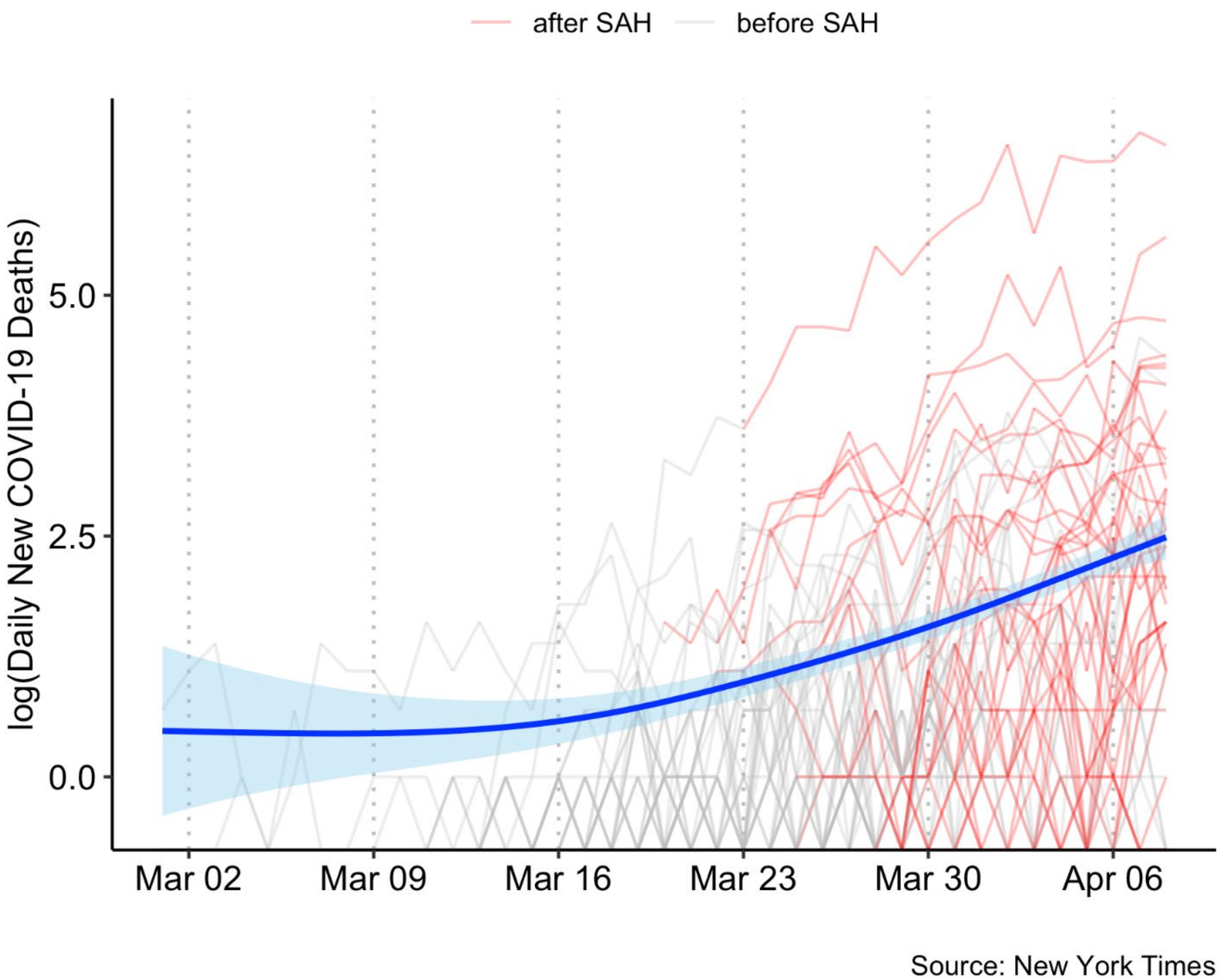
Note: Each grey line represents a state, and shows the percent of observations detected with feverish symptoms (ILI) that day. Red lines represent states with SAH laws, for the period after the law is in effect. The thick blue line represents a "smoothed" national local average (a generalized additive model (GAM)) of the states.

Fig4g: COVID New Cases by Day (Data through April 7th)



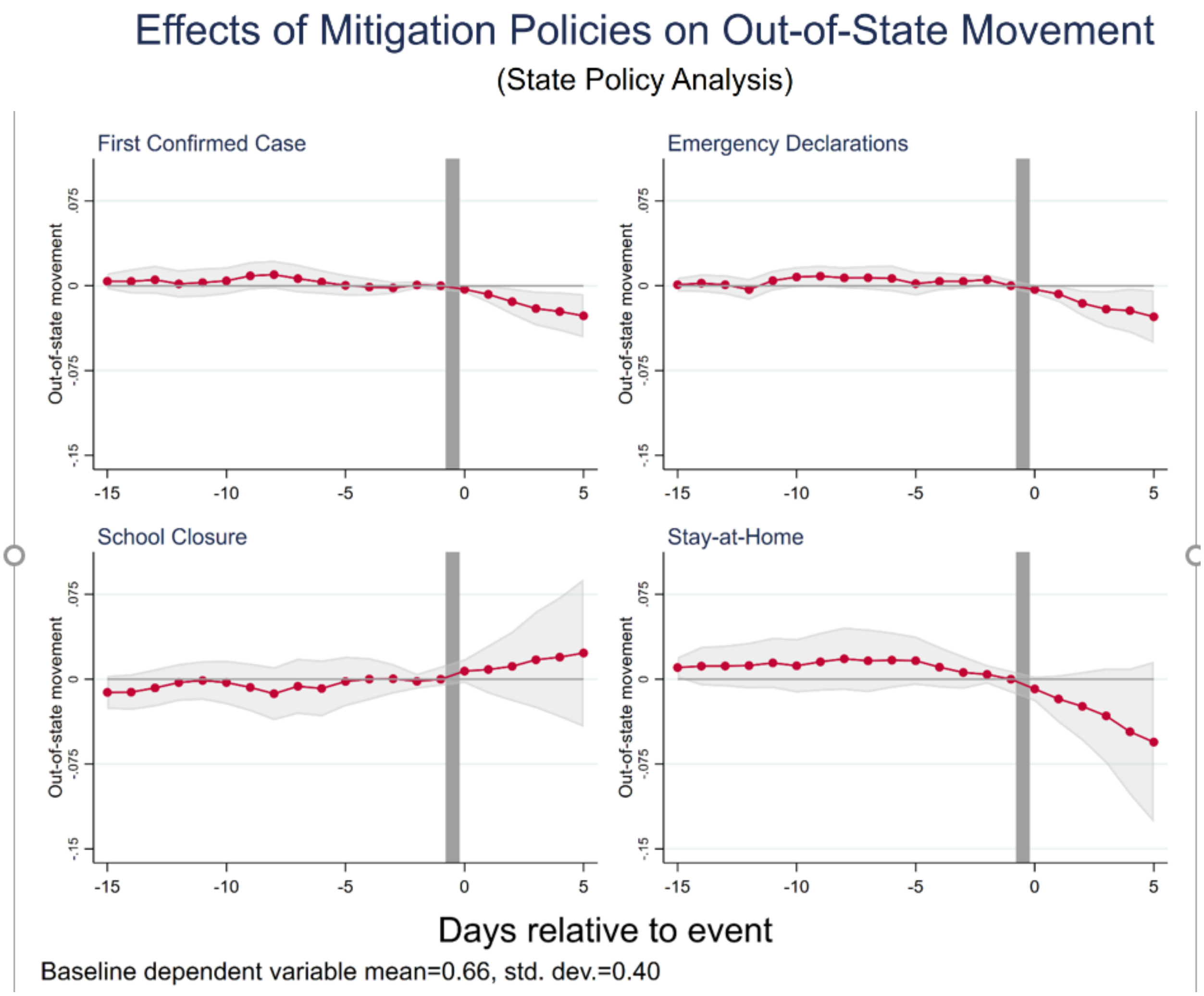
Note: Each grey line represents a state, and shows the number of new COVID-19 positive cases reported that day. Red lines represent states with SAH laws, for the period after the law is in effect. The thick blue line represents a "smoothed" national local average (a generalized additive model (GAM)) of the states.

Fig4h: Log of COVID Deaths by Day (Data through April 7th)



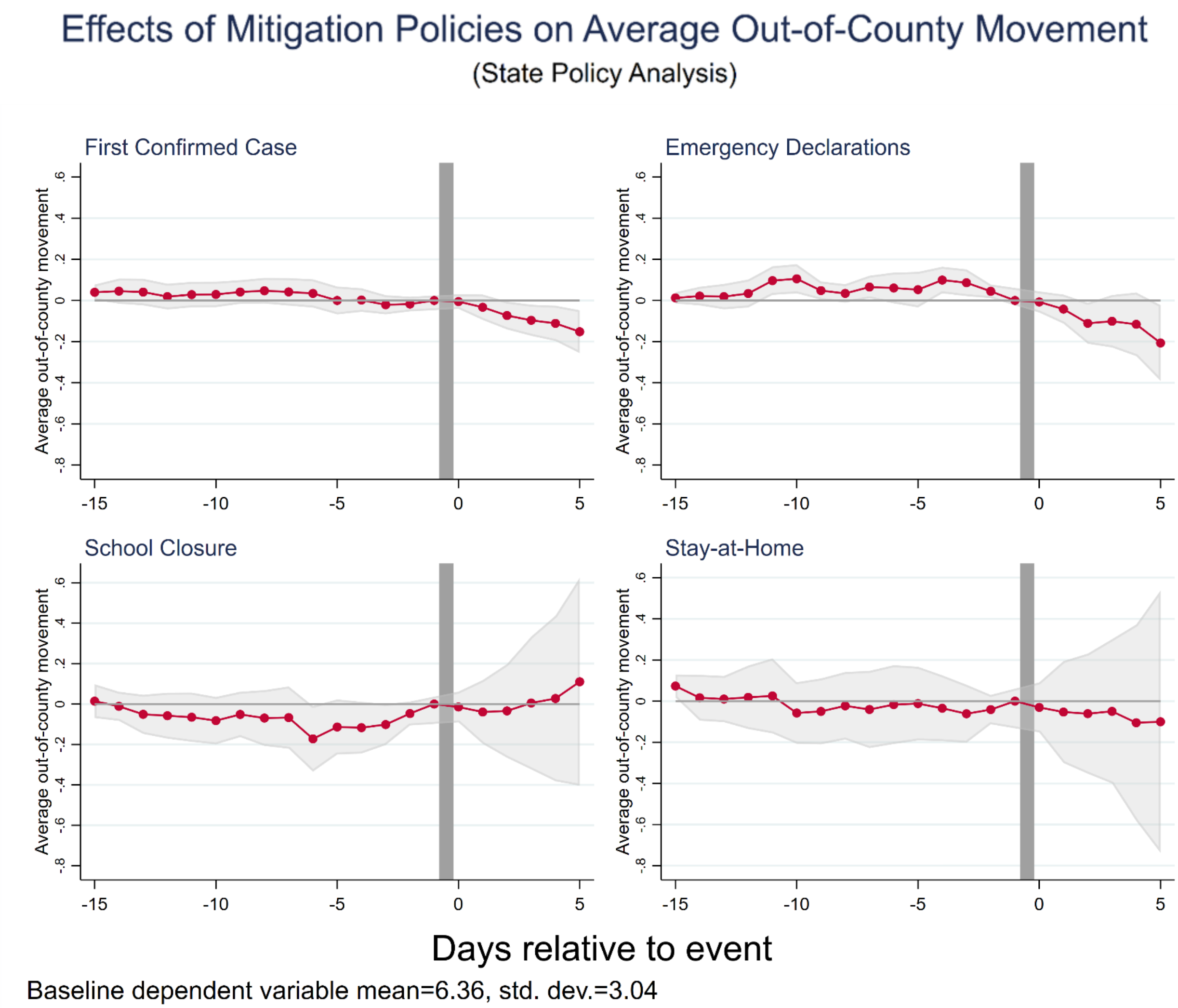
Note: Each grey line represents a state, and shows the number of COVID-19 deaths reported that day. Red lines represent states with SAH laws, for the period after the law is in effect. The thick blue line represents a "smoothed" national local average (a generalized additive model (GAM)) of the states.

Regression Results (Coefficients and 95% Confidence Intervals)
Fig 5a: *Leaving the State (in last 14 days), State Policy and Information Events*



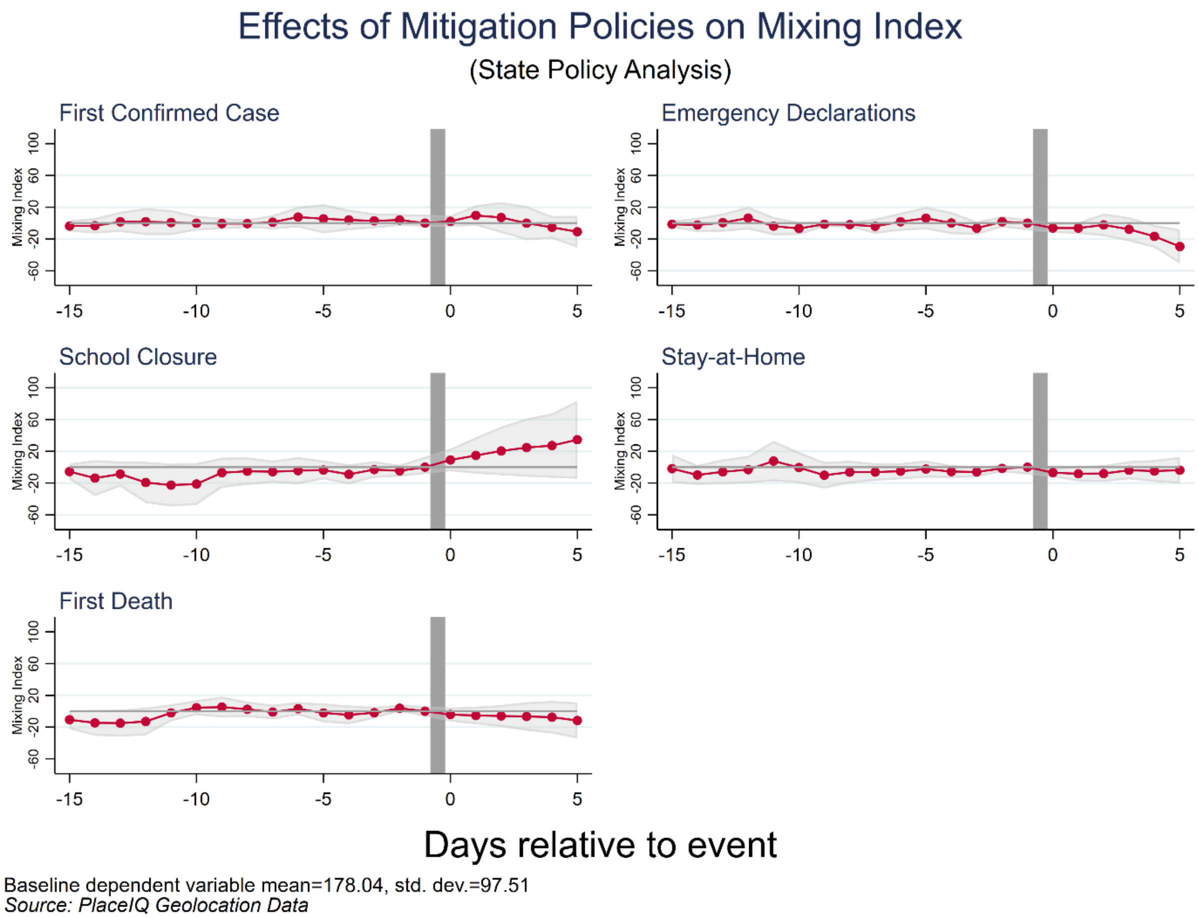
Notes: The dependent variable shows sum of the percent of cell phones detected out of state in the last 14 days. All regressions are estimated as a balanced panel. Standard errors are clustered at the state level. See Appendix for full event study estimates.

Fig 5b: *Leaving the County (in last 14 days), State Policy and Information Events*



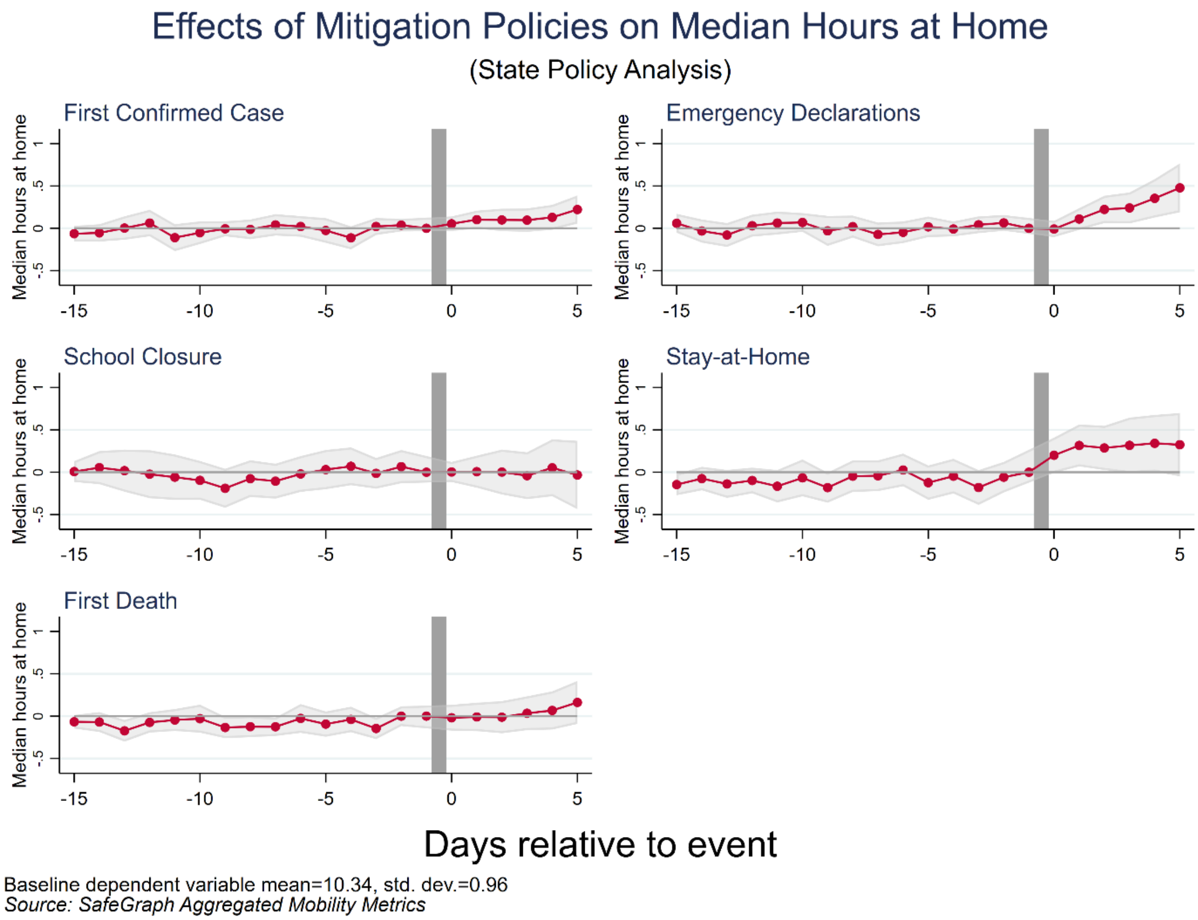
Notes: The dependent variable shows sum of the percent of cell phones detected out of the home county in the last 14 days, and population-weighted averaged to the state level. All regressions are estimated as a balanced panel. Standard errors are clustered at the state level. See Appendix for full event study estimates.

Fig 5c: *Mixing Index, State Policy and Information Events*



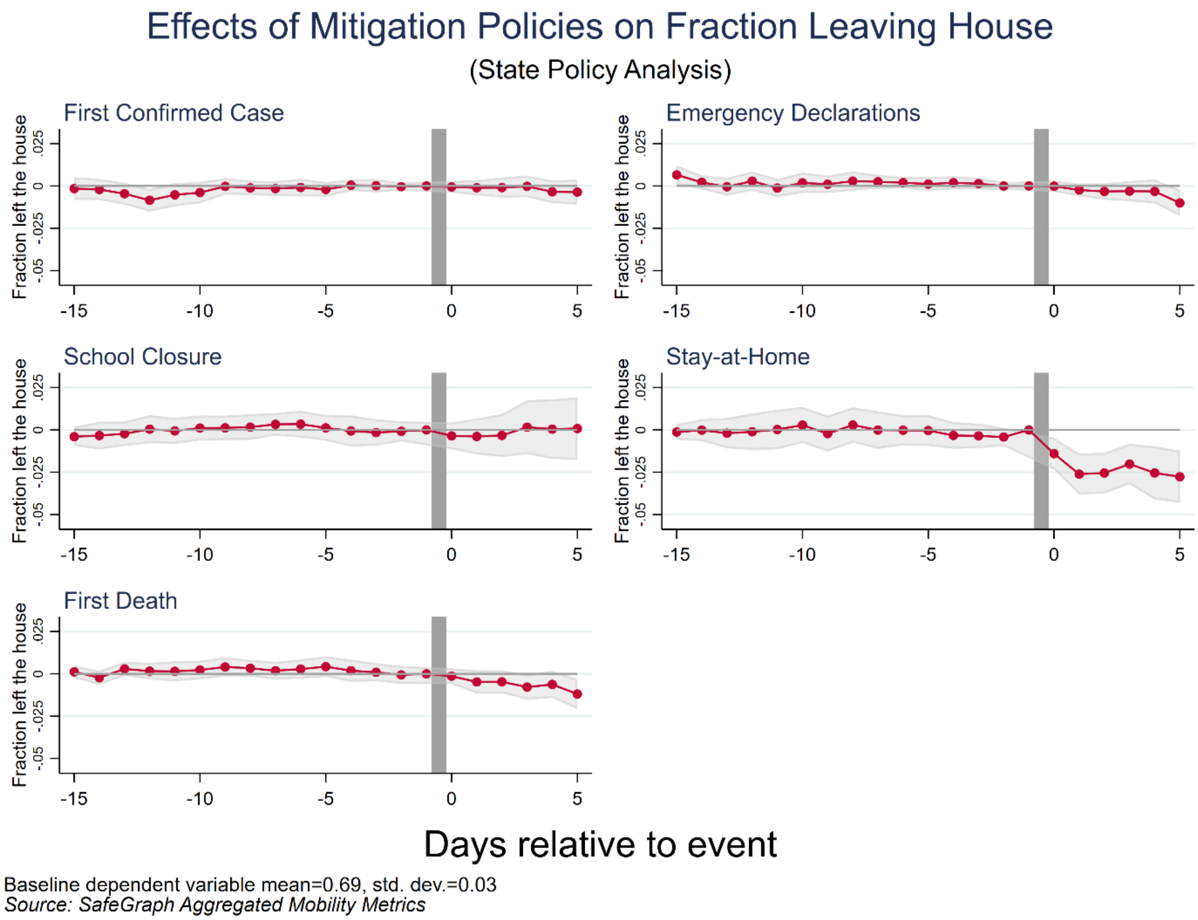
Notes: The dependent variable shows the index for mixing. All regressions are estimated as a balanced panel. Standard errors are clustered at the state level. See Appendix for full event study estimates.

Fig 5d: *Mean Hours at Home State Policy and Information Events*



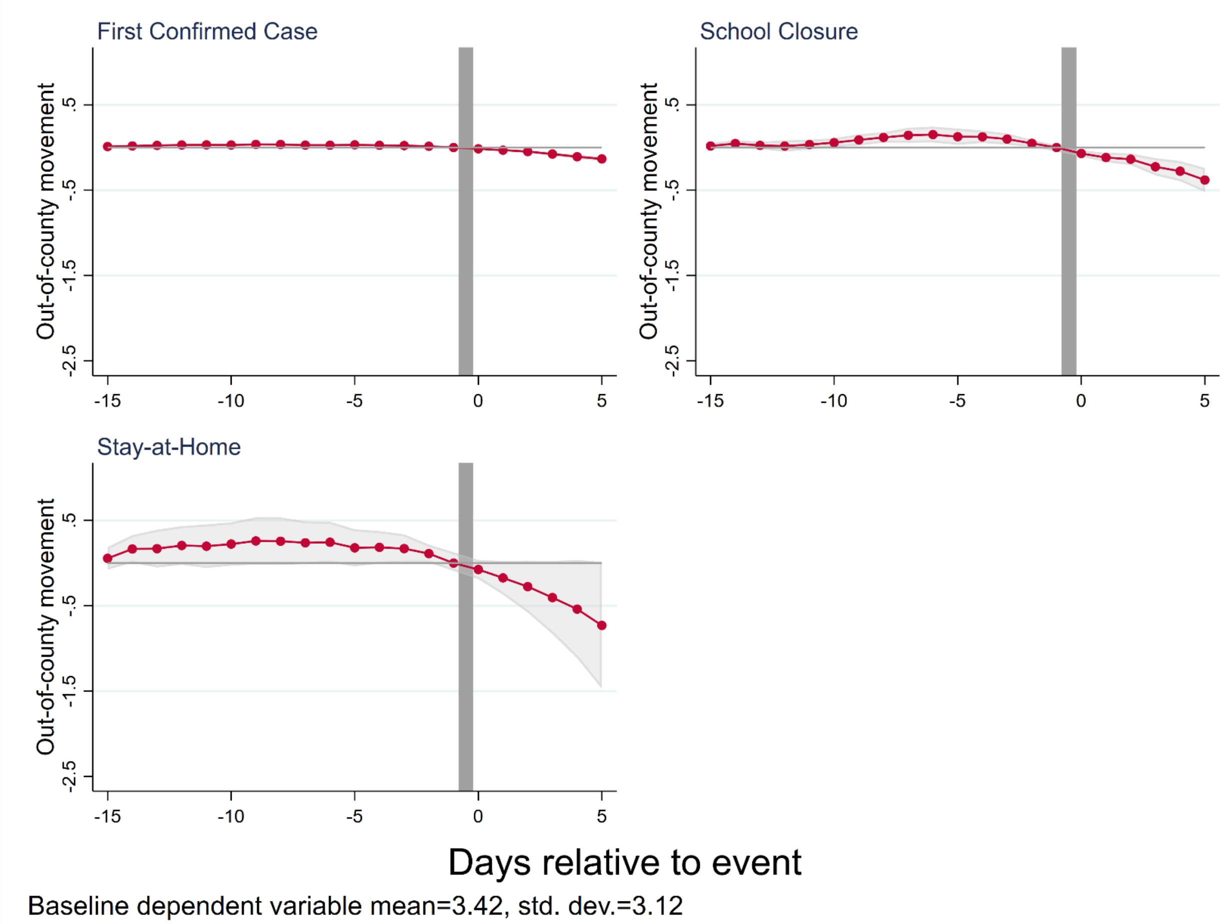
Notes: The dependent variable shows the mean dwell time. All regressions are estimated as a balanced panel. Standard errors are clustered at the state level. See Appendix for full event study estimates.

Fig 5e: *Leaving the House, State Policy and Information Events*



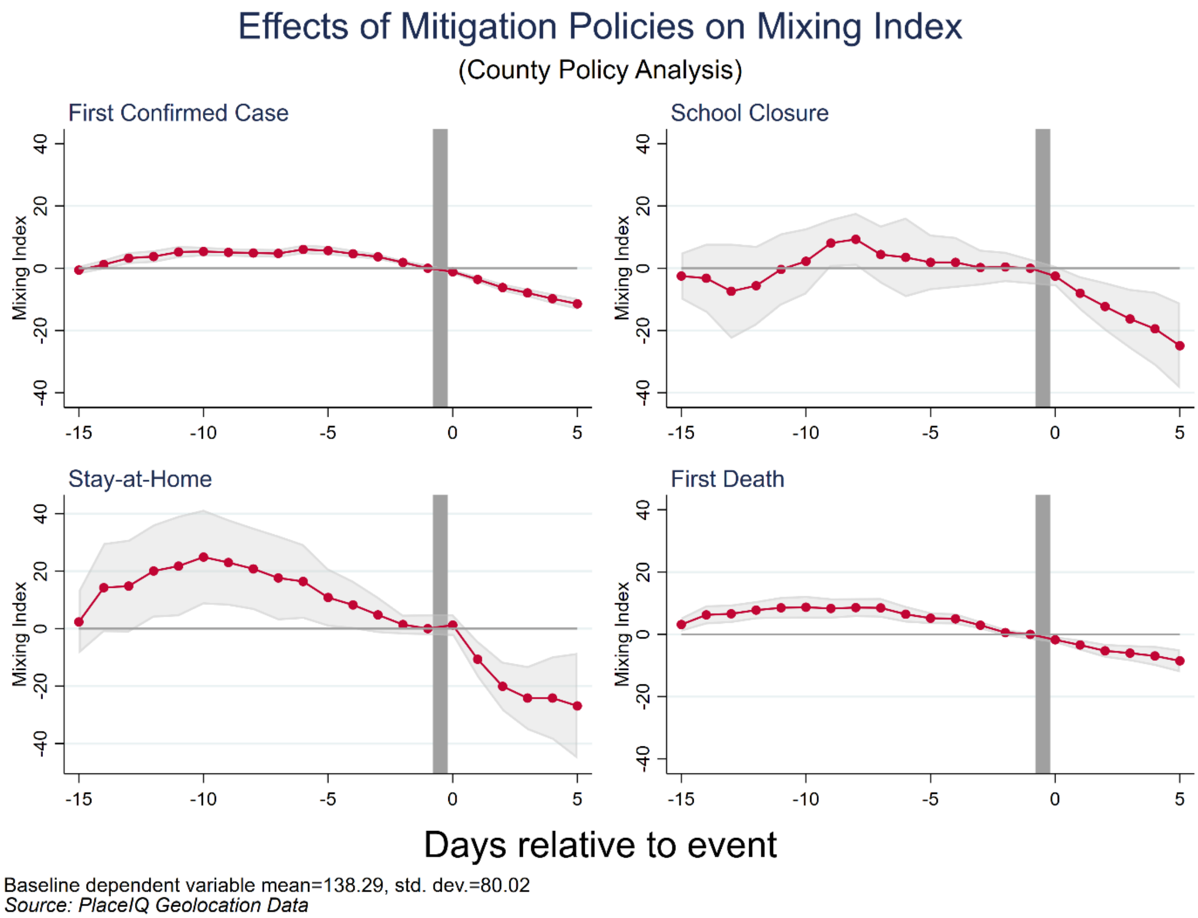
Notes: The dependent variable shows the percent of cell phones detected out of the home at some point during the day, as a share of devices that did not go to a work location that day. All regressions are estimated as a balanced panel. Standard errors are clustered at the state level. See Appendix for full event study estimates.

Fig 6a: *Leaving the County (in last 14 days), County Policy and Information Events*



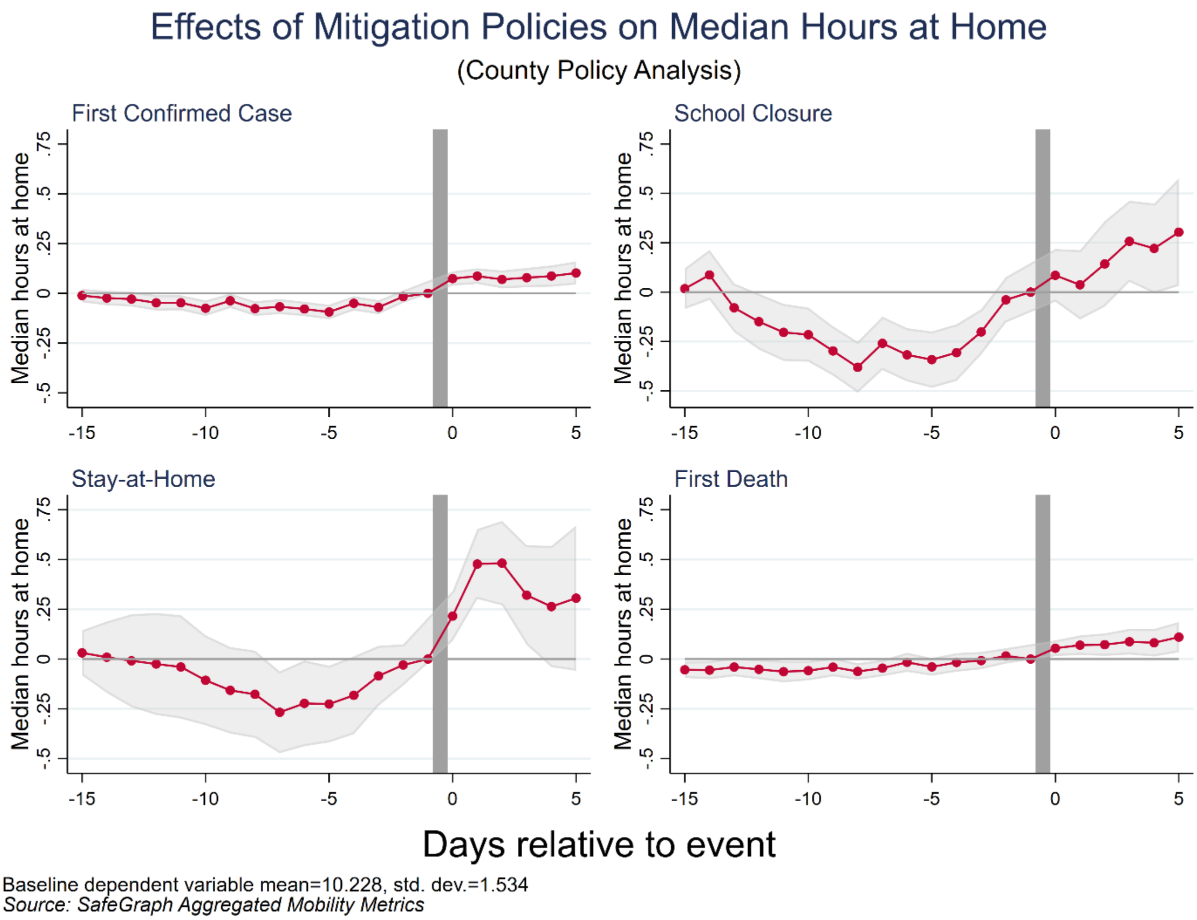
Notes: The dependent variable shows sum of the percent of cell phones detected in a different county in the last 14 days. All regressions are estimated as a balanced panel. Standard errors are clustered at the county level. See Appendix for full event study estimates. The county policies tracked here are ones where the relevant state did not have a policy at the time.

Fig 6b: Mix Index, County Policy and Information Events



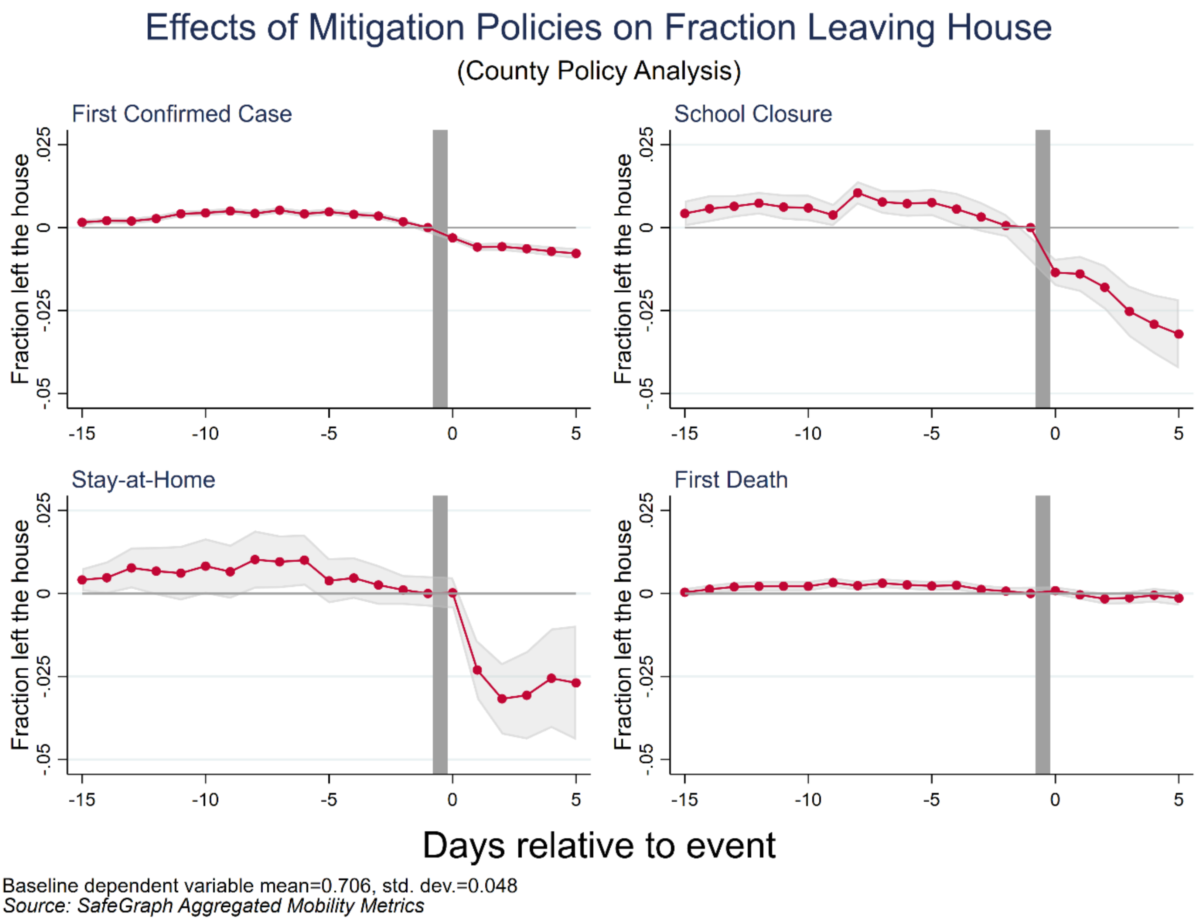
Notes: The dependent variable shows the mean dwell time at home, in that county. All regressions are estimated as a balanced panel. Standard errors are clustered at the county level. See Appendix for full event study estimates.

Fig 6c: Mean Hours at Home, County Policy and Information Events



Notes: The dependent variable shows the mean dwell time at home, in that county. All regressions are estimated as a balanced panel. Standard errors are clustered at the county level. See Appendix for full event study estimates.

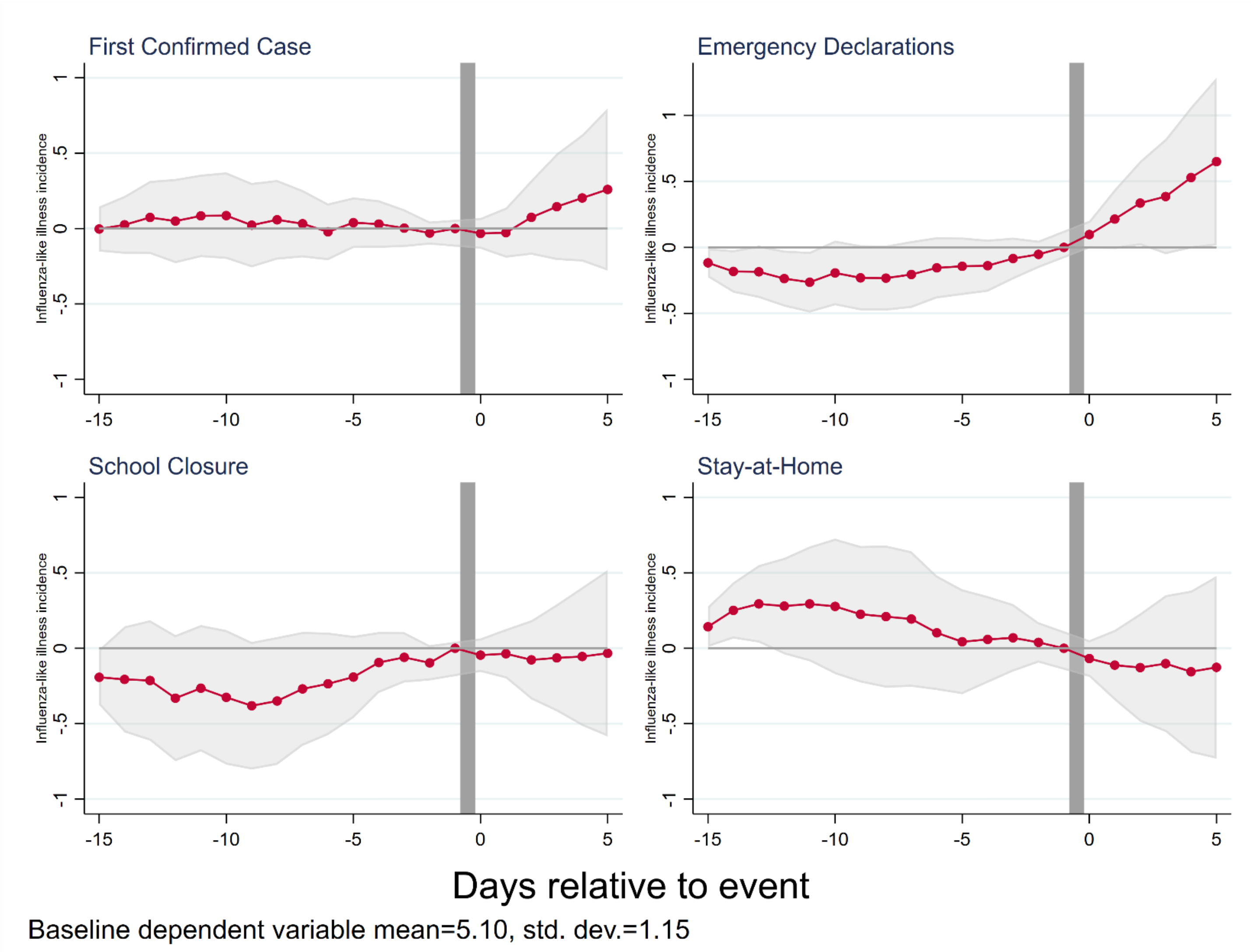
Fig 6d: Leaving the Home, County Policy and Information Events



Notes: The dependent variable shows the percent of cell phones detected out of the home at some point during the day, as a share of devices that did not go to a work location that day, in that county. All regressions are estimated as a balanced panel. Standard errors are clustered at the county level. See Appendix for full event study estimates.

EVIDENCE OF POLICY ANTICIPATION—NOT INTERPRETABLE AS CAUSAL EFFECTS

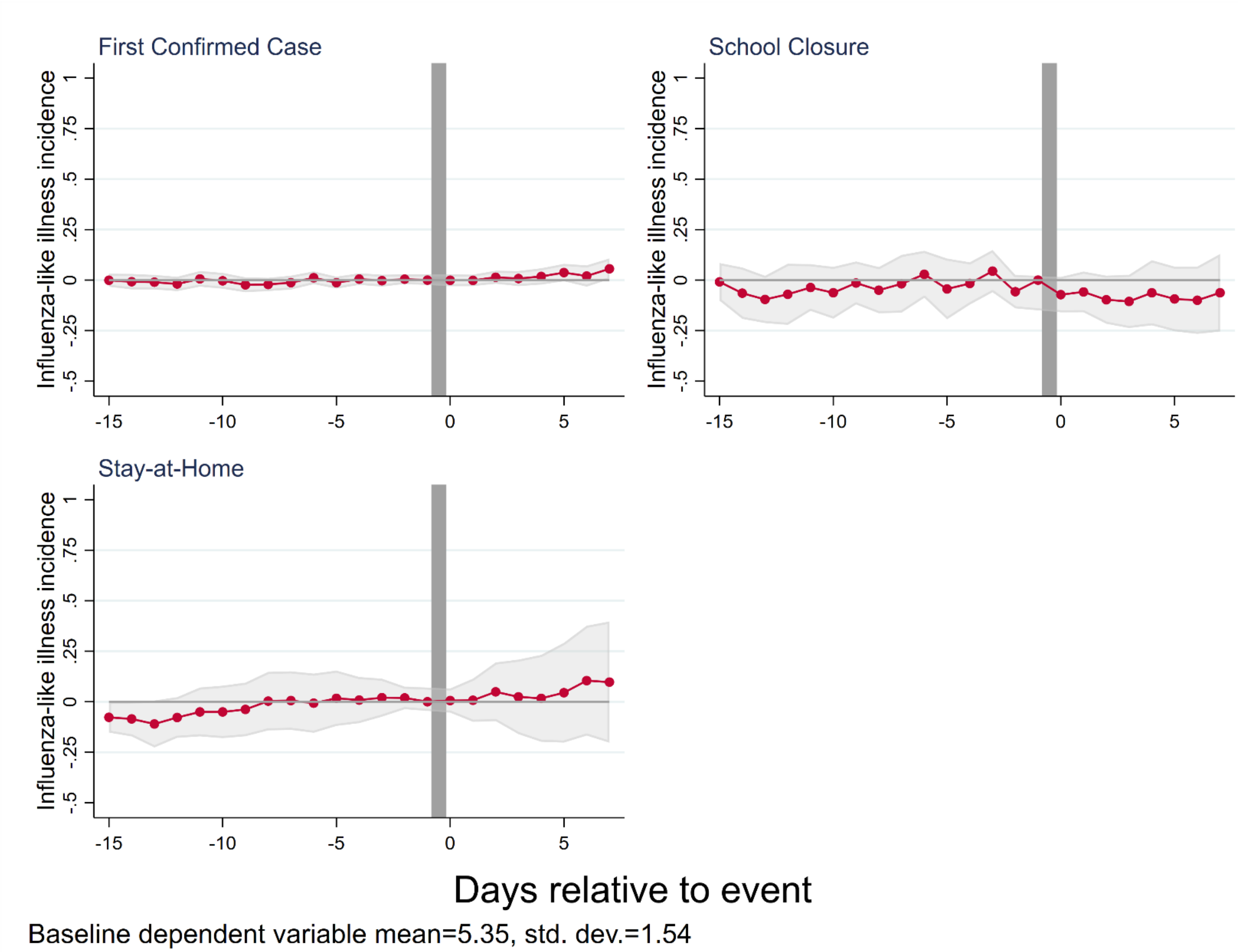
Fig 7: Feverish symptoms, State Policy and Information Events



Note: Dependent variable represents the percent of observations detected with feverish symptoms (ILI) that day.
All regressions are estimated as a balanced panel. Standard errors are clustered at the state level. See Appendix for full event study estimates.

EVIDENCE OF POLICY ANTICIPATION—NOT INTERPRETABLE AS CAUSAL EFFECTS

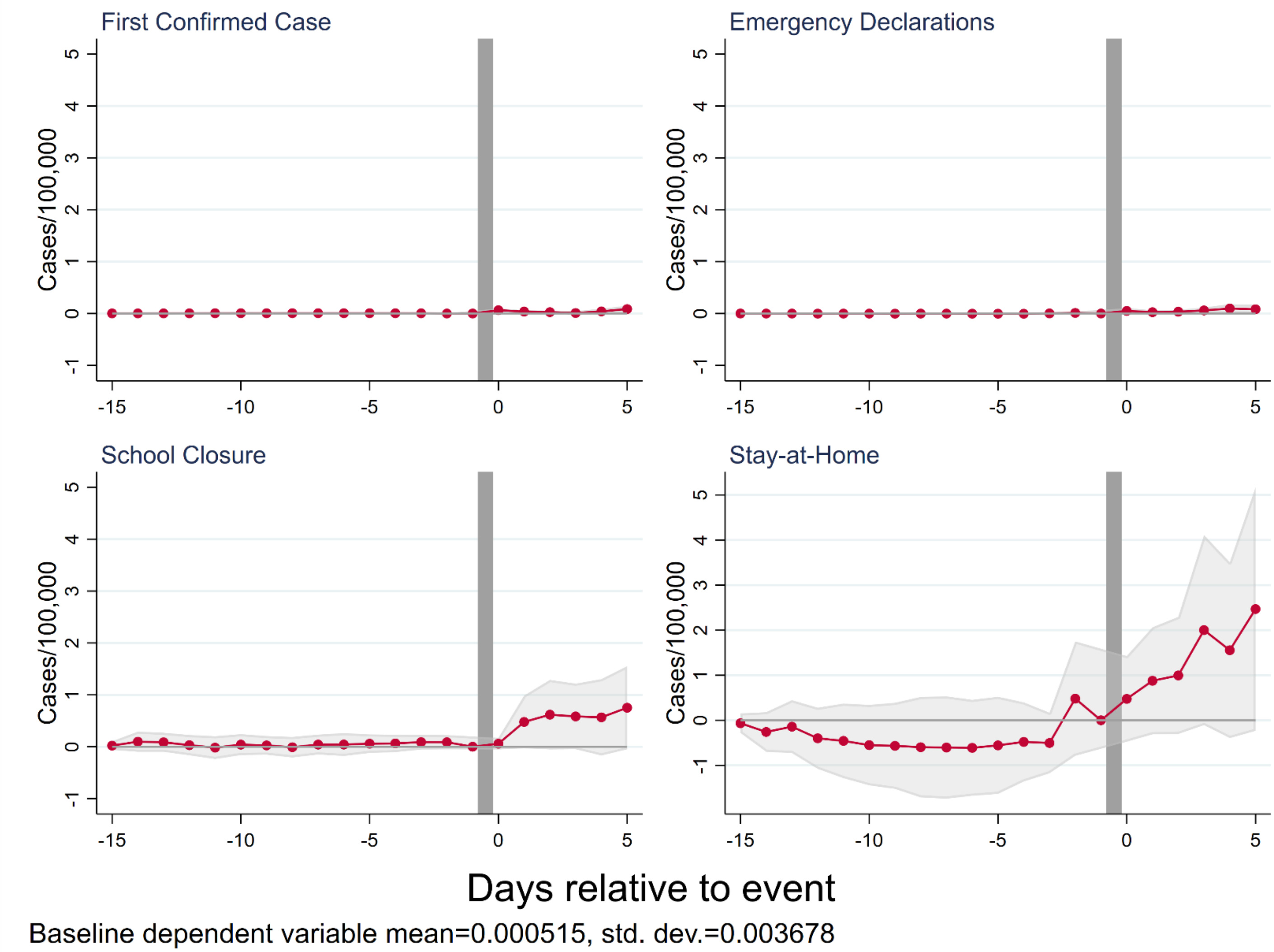
Fig 8: Fever symptoms, County (Absent State) Policy and Information Events



Note: Dependent variable represents the percent of observations detected with feverish symptoms (ILI) that day, in that county.All regressions are estimated as a balanced panel. Standard errors are clustered at the county level. See Appendix for full event study estimates.

EVIDENCE OF POLICY ANTICIPATION—NOT INTERPRETABLE AS CAUSAL EFFECTS

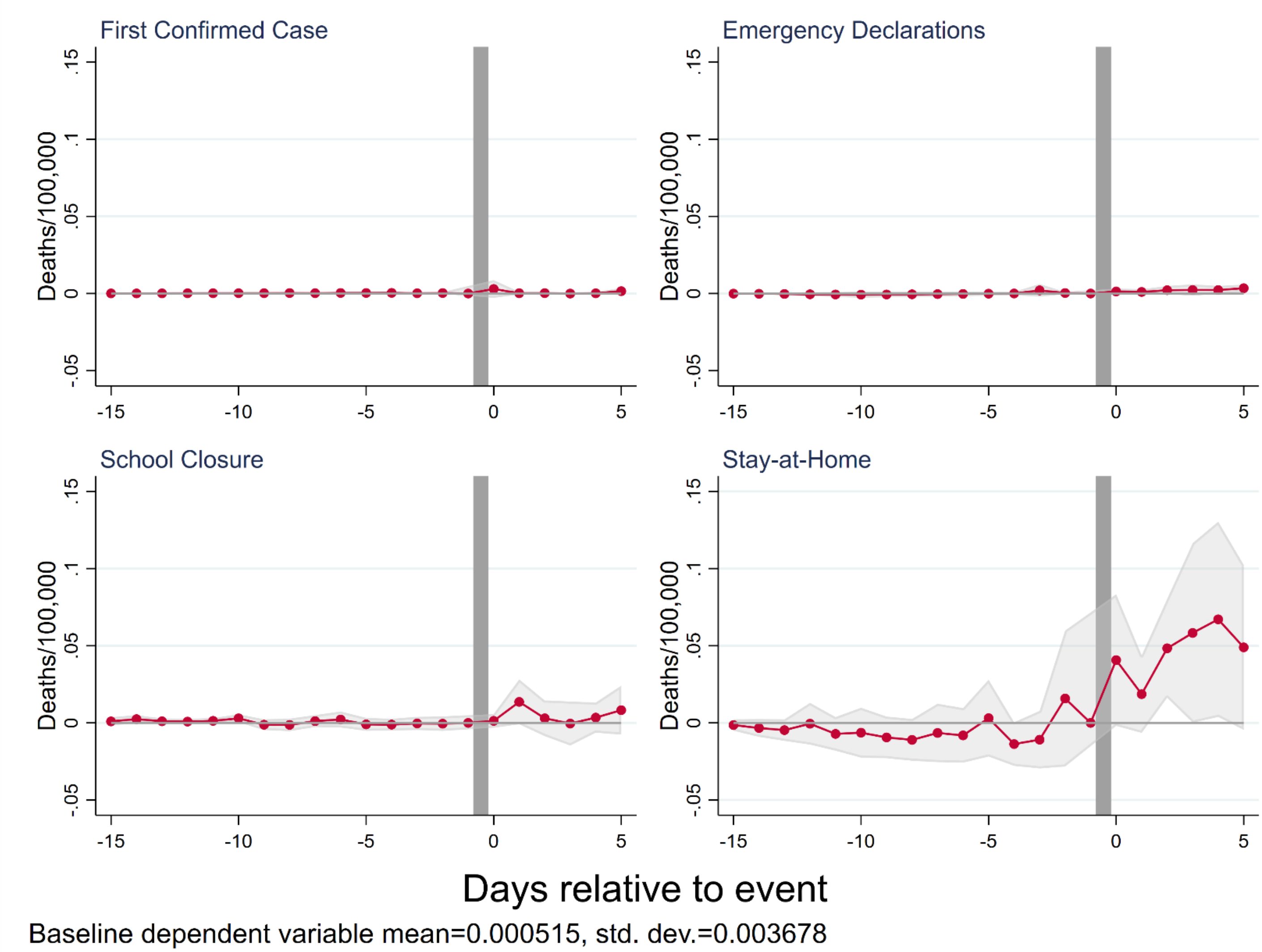
Fig 9a: COVID New Cases, State Policy and Information Events



Note: Dependent variable represents the he daily count of new COVID-19 positive cases from The New York Times .All regressions are estimated as a balanced panel. Standard errors are clustered at the state level. See Appendix for full event study estimates.

EVIDENCE OF POLICY ANTICIPATION—NOT INTERPRETABLE AS CAUSAL EFFECTS

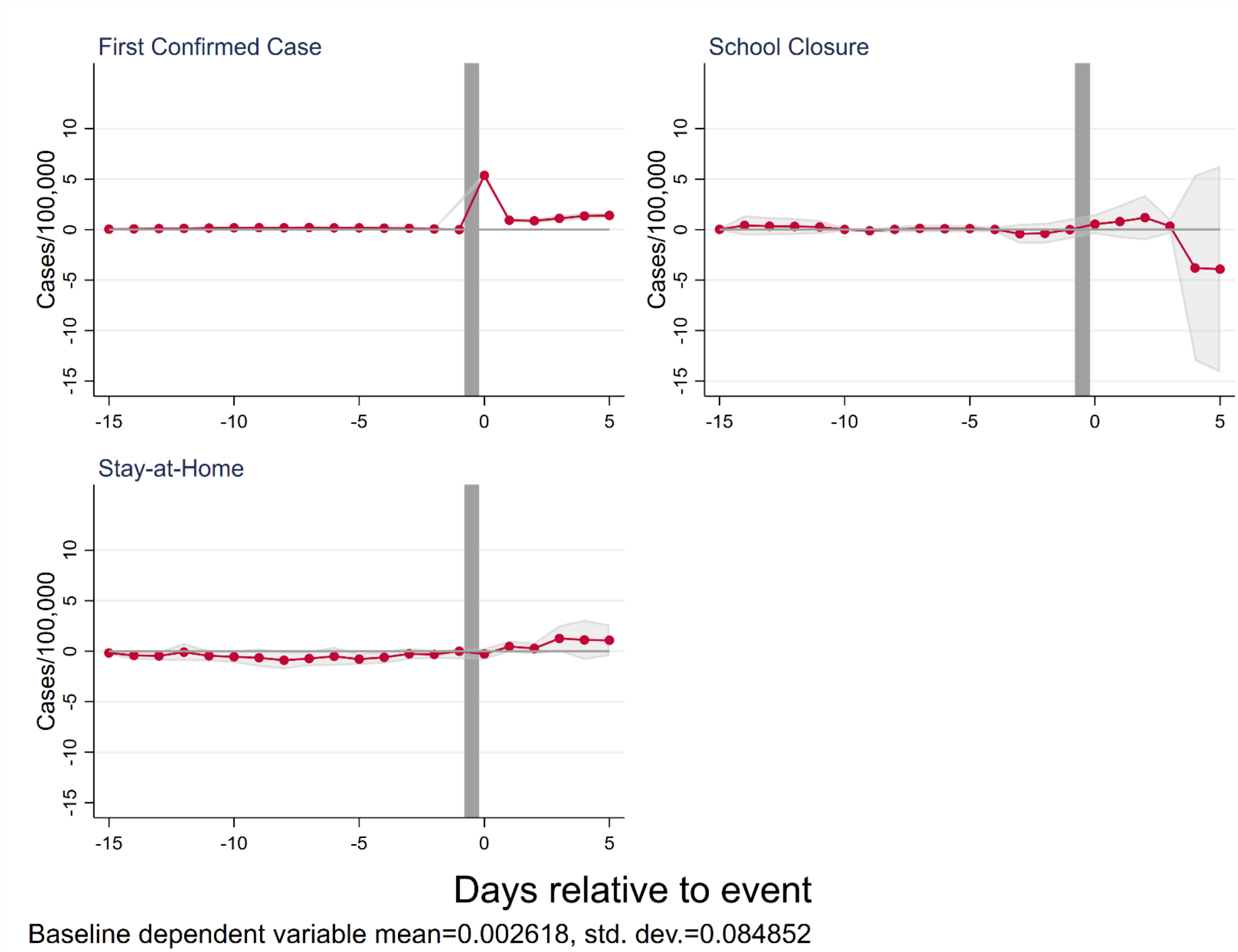
Fig 9b: COVID-19 Deaths, State Policy and Information Events



Note: Dependent variable represents the he daily count of COVID-19 deaths from The New York Times .All regressions are estimated as a balanced panel. Standard errors are clustered at the state level. See Appendix for full event study estimates.

EVIDENCE OF POLICY ANTICIPATION—NOT INTERPRETABLE AS CAUSAL EFFECTS

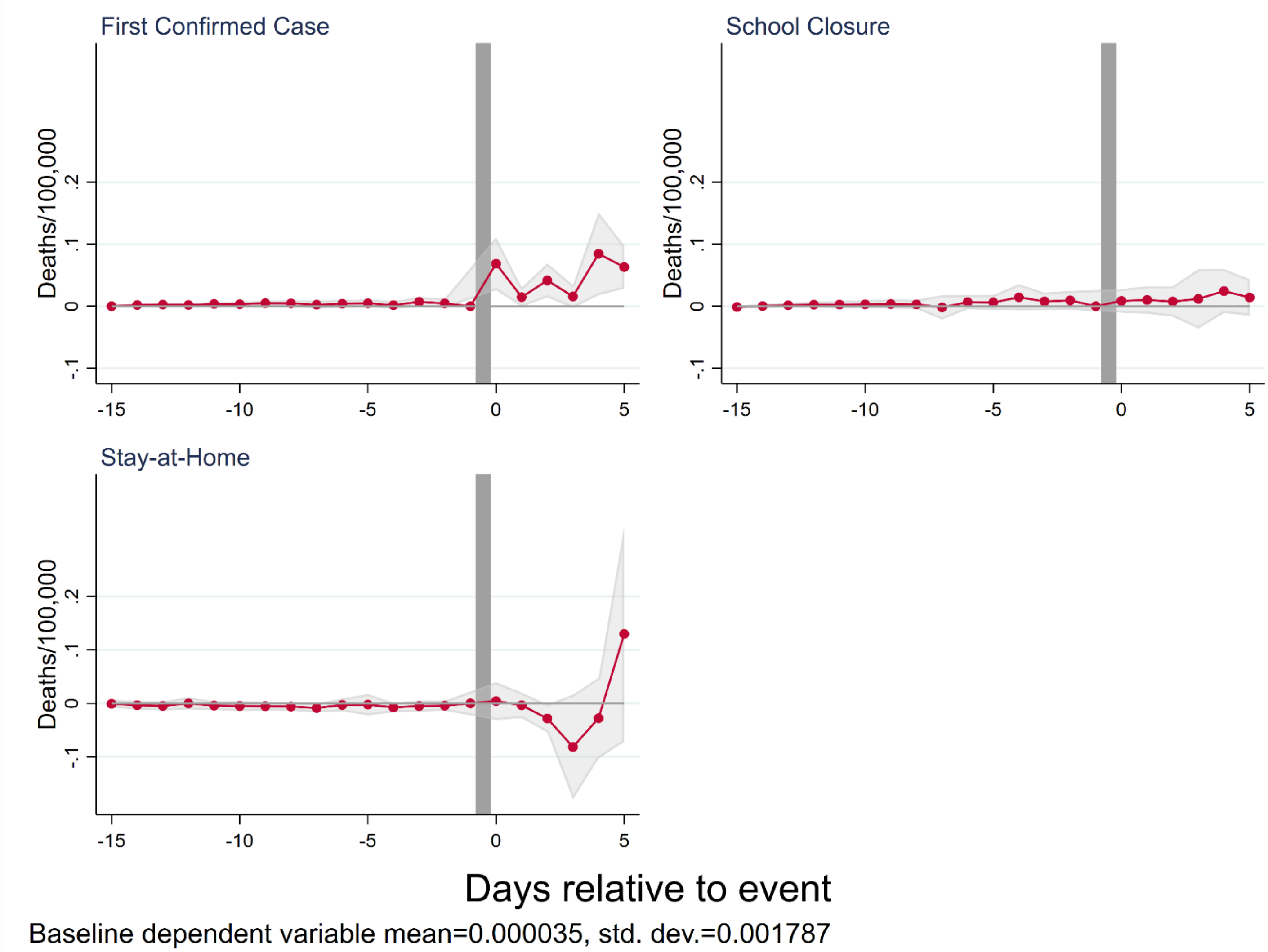
Fig 10a: Cases, County (Absent State) Policy and Information Events



Note: This shows, the daily count of COVID-19 positive cases from The New York Times, based on reports from state and local health agencies, available at https://www.nytimes.com/interactive/2020/us/coronavirus-us-cases.html.

EVIDENCE OF POLICY ANTICIPATION—NOT INTERPRETABLE AS CAUSAL EFFECTS

Fig 10b: Deaths, County (Absent State) Policy and Information Events



Note: This shows the daily count of COVID-19 deaths per 100,000 population. Data from The New York Times, based on reports from state and local health agencies, available at https://www.nytimes.com/interactive/2020/us/coronavirus-us-cases.html.

Table 1: OLS and LASSO Estimates: County Correlates of Changes in Outcomes, March 1 2020-Present

Table 1a:OLS Outcomes (Change from March 1-present)

Variable	Leaving County	Leaving Home	Flu Like Symptoms	+ Cases Per Capita	Deaths Per Capita
Population/1000	-0.001**	0.000	0.001***	0.000	0.000
Pop Density	0.000	0.000	0.000	0.002*	0.000
Metro Area > 1 Million	-0.573***	-0.146	0.135	0.736	-0.013
Metro Area 250k to <1 Million	-0.195***	0.018	0.518	-0.571	-0.027
Metro Area LT 250k	-0.092*	-0.004	0.468	0.105	0.012
Republican Vote Share 2016	1.733***	-0.589*	0.4	2.019	-0.094
Percent White	-0.017***	0.001	0.018	-0.026	-0.005
Percent Black	-0.016***	-0.002	0.018	0.074**	0.003
Median HH Income	-0.008	0.002	-0.003	0.337***	0.008
Poverty	-0.005	-0.004	0.044	0.440***	0.008
Uninsured	-1.845**	0.067	-1.225	-9.120*	-0.651
Recreation County	-0.326***	-0.064	0.374	0.017	0.03
Retirement Destination	0.064	0.1	0.303	-0.694	-0.058
Age and Gender Composition					
Percent Male 30-34	-0.299***	0.11	0.265	0.777	-0.096
Percent Male 35-44	0.053	-0.008	-0.092	0.129	0.058
Percent Male 45-54	-0.024	-0.067	-0.046	-0.652	0.097
Percent Male 55-59	0.330**	0.04	0.08	0.414	-0.078
Percent Male 60-64	0.438***	0.082	0.167	0.578	0.000
Percent Male 65-74	0.020	0.051	0.3	-0.772	-0.148*
Percent Male 75-84	-0.361**	-0.055	-0.345	-0.995	0.006
Percent Male GT 84	0.004	0.085	0.35	-1.883	0.199
Percent Female 30-34	-0.706***	-0.131	-0.288	-2.248*	-0.073
Percent Female 35-44	0.084	0.078	-0.165	0.565	-0.028
Percent Female 45-54	-0.024	-0.006	0.268	1.572*	0.002
Percent Female 55-59	-0.390**	0.015	-0.199	-2.815*	-0.084
Percent Female 60-64	-0.408***	-0.197	-0.609	0.554	0.175
Percent Female 65-74	0.09	0.05	-0.149	0.562	0.038
Percent Female 75-84	0.142	0.031	0.434	2.513*	0.09
Percent Female GT 84	-0.153	-0.033	-0.204	0.519	-0.14
Constant	3.719***	-0.047	-1.892	-23.099***	0.008
N	2008	3097	3102	2606	2606

r2	0.484	0.007	0.011	0.153	0.038

Note: Specification: simple OLS using cross-sectional data at county level. Each column represents results from a separate regression, where the dependent variable is the outcome listed. Sources of county characteristics: Area Health Resource File (AHRQ 2020) and County Health Rankings (CHR2020); we use the latest year available in each original source. Please see Appendix X for variable definitions.

LASSO Results

	Leaving County	**Leaving Home**	**Flu Like Symptoms**	**Cases Per Capita**	**Deaths Per Capita**
Population	-0.001	--	--	-0.001	--
Pop Density	0.000	--	--	0.001	0.000
Metro Area > 1 Million	-0.377	--	--	0.251	--
Metro Area 250k to <1 Million	-0.005	--	--	-0.521	--
Metro Area LT 250k	0.049	--	--	--	--
Republican Vote Share 2016	0.463	-0.001	--	3.093	-0.228
Percent White	-0.001	--	--	--	-0.002
Percent Black	-0.006	--	--	0.104	0.002
Median HH Income 2010	0.009	--	--	0.309	0.006
Percent Poverty	-0.019	--	--	--	--
Percent Uninsured	-2.706	--	--	-12.516	--
Recreation County	-0.131	--	--	-0.615	--
Retirement Destination County	0.149	--	--	-0.369	--
Percent SNAP 2010	--	--	--	--	--
Percent Disabled 18-64	-0.005	--	--	-0.100	--
Percent HS Grad	0.110	--	--	2.303	-0.231
Percent Some College	-2.282	--	--	-7.438	--
Unemployment Rate	3.289	--	--	7.576	--
Children in Poverty	--	--	--	13.442	--
Income Inequality	-0.053	--	--	1.156	0.077
Children in Single Parent HH	-0.005	--	--	--	--
Social Associations	-0.001	--	--	-0.078	--
Violent Crime	-0.001	--	--	0.002	0.000
Air Pollution	-0.006	--	--	0.186	--
Severe Housing Problems	1.478	--	--	37.994	0.099
Driving Alone To Work	--	--	--	-15.316	--

Long Commute Driving Along	-0.606	--	--	--	0.040
Percent Agric-Fish-Hunt-Mine	--	--	--	--	--
Percent Construction	0.006	--	--	--	--
Percent Education-Health-Soc	--	--	--	0.064	--
Percent Manufacturing	--	--	--	--	--
Percent Other Industry	-0.018	--	--	--	--
Primary Care Physicians	-545.265	--	--	--	--
Poor or Fair Health	2.974	--	--	0.211	--
Poor Mental Health Days	-0.097	--	--	--	--
Adult Smoking	-0.482	--	--	--	--
Adult Obesity	0.674	--	--	-0.910	--
Food Environment	--	--	--	1.071	--
Physical Inactivity	0.365	-0.408	--	8.532	--
Access to Exercise	-0.452	--	--	0.338	--
Excessive Drinking	-2.850	--	--	-12.745	--
Percent Male 30-34	-0.228	--	--	0.089	--
Percent Male 35-44	--	--	--	0.150	--
Percent Male 45-54	--	--	--	0.194	0.039
Percent Male 55-59	0.059	--	--	--	--
Percent Male 60-64	0.056	--	--	--	--
Percent Male 65-74	-0.087	--	--	-0.499	--
Percent Male 75-84	-0.172	--	--	--	--
Percent Male GT 84	--	--	--	--	--
Percent Female 30-34	-0.546	--	--	-0.531	--
Percent Female 35-44	-0.084	--	--	--	--
Percent Female 45-54	--	--	--	0.993	0.012
Percent Female 55-59	--	--	--	-1.580	--
Percent Female 60-64	--	--	--	--	--
Percent Female 65-74	--	--	--	--	--
Percent Female 75-84	--	--	--	1.339	--
Percent Female GT 84	--	--	--	1.162	--
Constant	**6.117**	**-0.112**	**0.115**	**-27.802**	**-0.356**

Notes:

Appendix

Table A1 - State Policy Enactment and Information Event Dates

State	Emergency Declaration	School Close	Restaurant/Other Restrict	Gathering Restrict Any	NE Business Close	Stay At Home	First confirmed case
AK	11-Mar-20	16-Mar-20	17-Mar-20	28-Mar-20	24-Mar-20	22-Mar-20	12-Mar-20
AL	13-Mar-20	19-Mar-20	20-Mar-20		20-Mar-20	24-Mar-20	13-Mar-20
AR	11-Mar-20	17-Mar-20	19-Mar-20				11-Mar-20
AZ	11-Mar-20	16-Mar-20	20-Mar-20			31-Mar-20	26-Jan-20
CA	4-Mar-20	19-Mar-20	15-Mar-20	19-Mar-20	11-Mar-20	19-Mar-20	26-Jan-20
CO	10-Mar-20	23-Mar-20	17-Mar-20	26-Mar-20	19-Mar-20	26-Mar-20	5-Mar-20
CT	10-Mar-20	17-Mar-20	16-Mar-20	23-Mar-20	12-Mar-20		8-Mar-20
DC	11-Mar-20	16-Mar-20	16-Mar-20	25-Mar-20	13-Mar-20	1-Apr-20	7-Mar-20
DE	13-Mar-20	16-Mar-20	16-Mar-20	24-Mar-20	16-Mar-20	24-Mar-20	11-Mar-20
FL	9-Mar-20	16-Mar-20	17-Mar-20	30-Mar-20	3-Apr-20	3-Apr-20	2-Mar-20
GA	14-Mar-20	18-Mar-20	24-Mar-20		24-Mar-20	24-Mar-20	2-Mar-20
HI	4-Mar-20	23-Mar-20	17-Mar-20	25-Mar-20	16-Mar-20	25-Mar-20	6-Mar-20
IA	9-Mar-20	3-Apr-20	17-Mar-20		17-Mar-20		8-Mar-20
ID	13-Mar-20	23-Mar-20	25-Mar-20	25-Mar-20	25-Mar-20	19-Mar-20	13-Mar-20
IL	9-Mar-20	17-Mar-20	16-Mar-20	21-Mar-20	13-Mar-20	21-Mar-20	24-Jan-20
IN	6-Mar-20	19-Mar-20	16-Mar-20	24-Mar-20	12-Mar-20	25-Mar-20	6-Mar-20
KS	12-Mar-20	18-Mar-20			17-Mar-20	30-Mar-20	7-Mar-20
KY	6-Mar-20	16-Mar-20	16-Mar-20	26-Mar-20	19-Mar-20		6-Mar-20
LA	11-Mar-20	16-Mar-20	17-Mar-20	23-Mar-20	13-Mar-20	23-Mar-20	9-Mar-20
MA	10-Mar-20	17-Mar-20	17-Mar-20	24-Mar-20	13-Mar-20		1-Feb-20
MD	5-Mar-20	16-Mar-20	16-Mar-20	23-Mar-20	16-Mar-20	30-Mar-20	5-Mar-20
ME	15-Mar-20	16-Mar-20	18-Mar-20	25-Mar-20	18-Mar-20	25-Mar-20	12-Mar-20
MI	10-Mar-20	16-Mar-20	16-Mar-20	23-Mar-20	13-Mar-20	24-Mar-20	10-Mar-20
MN	13-Mar-20	18-Mar-20	17-Mar-20			28-Mar-20	6-Mar-20
MO	13-Mar-20	23-Mar-20	17-Mar-20		23-Mar-20	6-Apr-20	8-Mar-20
MS	14-Mar-20	20-Mar-20	24-Mar-20	31-Mar-20	24-Mar-20	31-Mar-20	11-Mar-20
MT	12-Mar-20	16-Mar-20	20-Mar-20	28-Mar-20	24-Mar-20	28-Mar-20	13-Mar-20
NC	10-Mar-20	16-Mar-20	17-Mar-20	30-Mar-20	14-Mar-20	30-Mar-20	3-Mar-20
ND	13-Mar-20	16-Mar-20	20-Mar-20				11-Mar-20
NE	13-Mar-20	3-Apr-20	19-Mar-20		16-Mar-20		6-Mar-20
NH	13-Mar-20	16-Mar-20	16-Mar-20	28-Mar-20	16-Mar-20	28-Mar-20	2-Mar-20
NJ	9-Mar-20	18-Mar-20	16-Mar-20	21-Mar-20	16-Mar-20	21-Mar-20	4-Mar-20
NM	11-Mar-20	16-Mar-20	16-Mar-20	24-Mar-20	16-Mar-20		11-Mar-20
NV	12-Mar-20	16-Mar-20	17-Mar-20	21-Mar-20	19-Mar-20	31-Mar-20	5-Mar-20
NY	7-Mar-20	18-Mar-20	16-Mar-20	20-Mar-20	13-Mar-20	22-Mar-20	1-Mar-20
OH	9-Mar-20	17-Mar-20	15-Mar-20	24-Mar-20	12-Mar-20	24-Mar-20	9-Mar-20
OK	15-Mar-20	17-Mar-20	25-Mar-20	26-Mar-20	24-Mar-20	24-Mar-20	6-Mar-20
OR	8-Mar-20	16-Mar-20	17-Mar-20		16-Mar-20	23-Mar-20	28-Feb-20
PA	6-Mar-20	16-Mar-20	17-Mar-20	23-Mar-20	16-Mar-20	23-Mar-20	6-Mar-20
RI	9-Mar-20	16-Mar-20	16-Mar-20		17-Mar-20	28-Mar-20	1-Mar-20
SC	13-Mar-20	16-Mar-20	18-Mar-20		18-Mar-20	26-Mar-20	7-Mar-20
SD	13-Mar-20	16-Mar-20			6-Apr-20		10-Mar-20
TN	12-Mar-20	20-Mar-20	23-Mar-20	1-Apr-20	23-Mar-20	2-Apr-20	5-Mar-20
TX	13-Mar-20	23-Mar-20	20-Mar-20		20-Mar-20	2-Apr-20	13-Feb-20
UT	6-Mar-20	16-Mar-20	18-Mar-20		16-Mar-20	27-Mar-20	6-Mar-20
VA	12-Mar-20	16-Mar-20	17-Mar-20		15-Mar-20	30-Mar-20	7-Mar-20
VT	13-Mar-20	18-Mar-20	17-Mar-20	25-Mar-20	13-Mar-20	24-Mar-20	7-Mar-20
WA	29-Feb-20	17-Mar-20	16-Mar-20	25-Mar-20	11-Mar-20	23-Mar-20	21-Jan-20
WI	12-Mar-20	18-Mar-20	17-Mar-20	25-Mar-20	17-Mar-20	25-Mar-20	5-Feb-20
WV	16-Mar-20	16-Mar-20	17-Mar-20	24-Mar-20		24-Mar-20	17-Mar-20
WY	13-Mar-20	16-Mar-20	19-Mar-20		20-Mar-20		11-Mar-20

Notes: Author compilations based on Fullman (2020), the public use map/tracker of K-12 school closures (Education Week), and our own compilations; we collected data on the timing of the first COVID-19 case announcements from media reports in each state. Data current as of April 10th 2020

*Table A2: Event Study Coefficients: Corresponding to Figure 5a (Other models available **at link**)*

Travel outside the State, State Policy and Information Events

	(1) First Confirmed Case	(2) Emergency Declarations	(3) School Closure	(4) Stay at Home
15 days prior to event	0.00387	0.000853	-0.0116	0.0103*
	(0.004)	(0.003)	(0.008)	(0.005)
14 days prior to event	0.00384	0.00224	-0.0114	0.0116
	(0.006)	(0.004)	(0.008)	(0.009)
13 days prior to event	0.00524	0.000839	-0.00772	0.0117
	(0.006)	(0.004)	(0.008)	(0.009)
12 days prior to event	0.00162	-0.00337	-0.00293	0.0121
	(0.006)	(0.005)	(0.008)	(0.010)
11 days prior to event	0.00278	0.00451	-0.00113	0.0144
	(0.007)	(0.005)	(0.009)	(0.011)
10 days prior to event	0.00430	0.00766	-0.00292	0.0119
	(0.006)	(0.005)	(0.010)	(0.012)
9 days prior to event	0.00875	0.00842	-0.00725	0.0154
	(0.006)	(0.005)	(0.011)	(0.013)
8 days prior to event	0.00978	0.00695	-0.0128	0.0180
	(0.006)	(0.005)	(0.012)	(0.014)
7 days prior to event	0.00625	0.00705	-0.00626	0.0163
	(0.006)	(0.005)	(0.013)	(0.014)
6 days prior to event	0.00324	0.00643	-0.00821	0.0168
	(0.005)	(0.006)	(0.013)	(0.013)
5 days prior to event	0.000283	0.00175	-0.00201	0.0163
	(0.005)	(0.005)	(0.011)	(0.011)
4 days prior to event	-0.00116	0.00389	0.000116	0.0106
	(0.004)	(0.004)	(0.010)	(0.009)
3 days prior to event	-0.00193	0.00386	0.000366	0.00596
	(0.003)	(0.003)	(0.007)	(0.007)
2 days prior to event	0.000579	0.00537*	-0.00188	0.00427
	(0.002)	(0.002)	(0.003)	(0.004)
Day of event	-0.00320*	-0.00329	0.00716	-0.00861
	(0.002)	(0.002)	(0.005)	(0.006)
1 day after event	-0.00763	-0.00731	0.00858	-0.0175

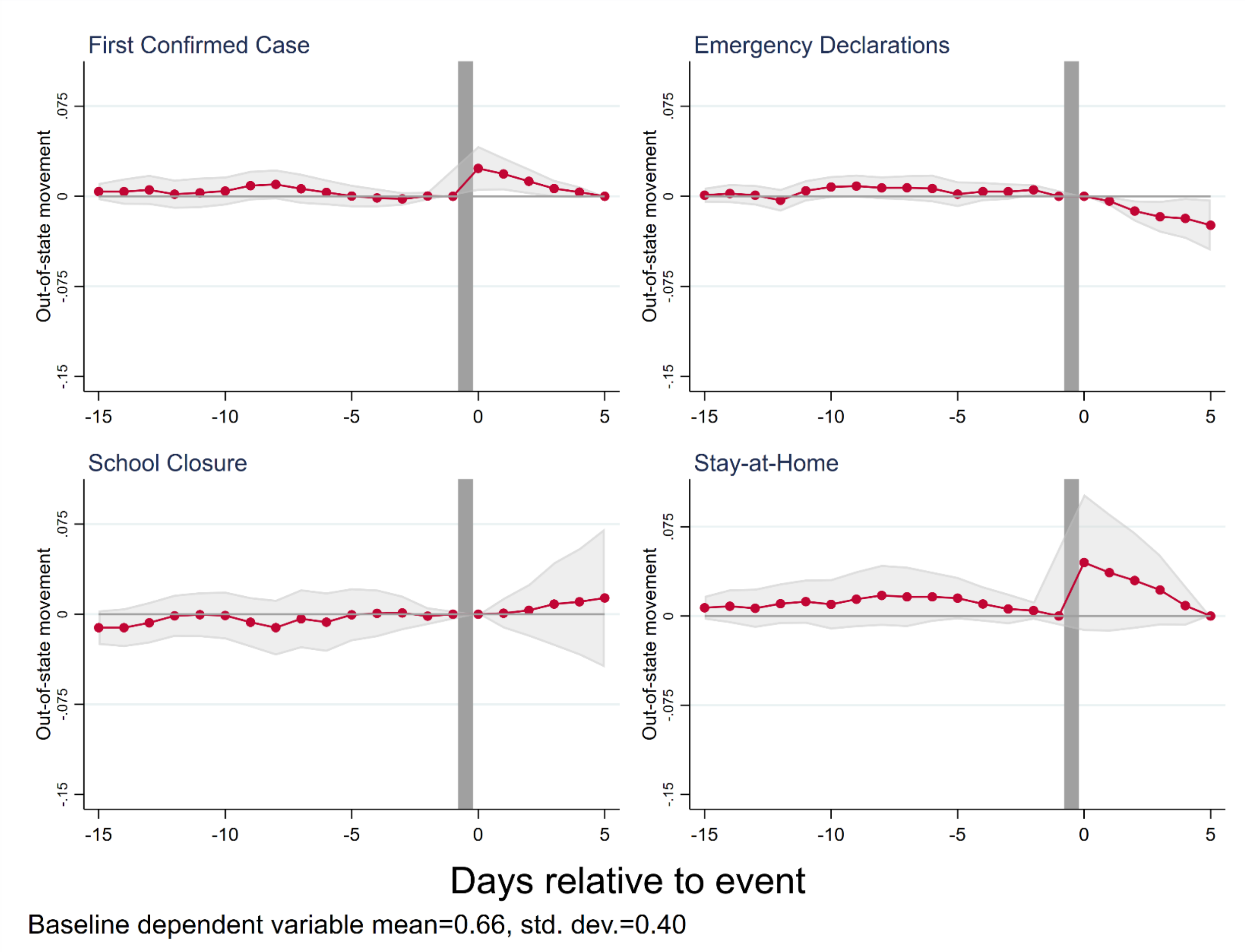
	(0.004)	(0.004)	(0.011)	(0.011)
2 days after event	-0.0141*	-0.0156*	0.0114	-0.0240
	(0.006)	(0.006)	(0.016)	(0.016)
3 days after event	-0.0202*	-0.0207*	0.0172	-0.0324
	(0.008)	(0.008)	(0.022)	(0.021)
4 days after event	-0.0228*	-0.0220*	0.0195	-0.0464
	(0.009)	(0.010)	(0.027)	(0.029)
5 days after event	-0.0266**	-0.0274*	0.0231	-0.0556
	(0.010)	(0.012)	(0.033)	(0.036)
N	999	1117	1078	638

Standard errors in parentheses
="* p<0.05 ** p<0.01"

Table A3: Model Specifications
(Only first model included here. See full version ***at link****)*

Figure	Data	Geography	Observation Window Of Data	Includes Policy enactments during dates (15-day pre and 5-day post period)	Specification	N	States NOT included
5a	PlaceIQ	State-level	Jan 20-April 2	Feb 3-Mar 28	Regression includes balanced panel of states observed for 15 days pre- and 5 days post event. Regressions include state and day fixed effects, and standard errors are clustered at state level.	First Confirmed Case = 999	AZ, CA, IL, MA, WA
						Emergency Declaration= 1,117	
						School Closure= 1,078	IA, NE
						Stay-At-Home= 638	AR, AZ, CT, DC, FL, IA, KS, KY, MA, MD, MO, MS, NC, ND, NE, NM, NV, SD, TN, TX, VA, WY

Table A4: Specification Check Including Controls for Other Policies
(Results for Figure 5a here, please see this ***link*** *for other specifications.*



Data Quality Appendix for Kinsa

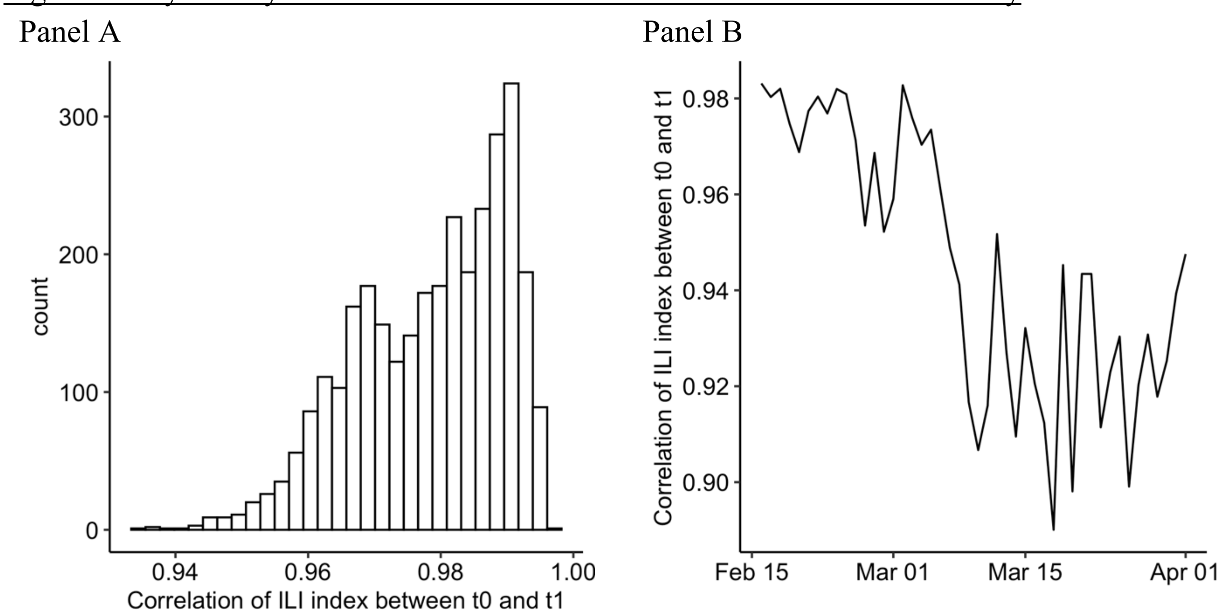
Data Appendix: Validation Exercises for KINSA Smartphone-App Thermometer Data

For this paper, we use Kinsa's US Health Weather Public API to collect county-level daily influenza-like illness incidence from February 16 2020 to April 2 2020. Miller et al (2018) shows that Kinsa's anomalous influenza-like illness incidence (ILI) index, developed from real-time geospatial thermometer data, provided highly accurate 12-week illness forecasts in 2015-2017. Here, conduct similar validation exercises for the 2020 data which we use in our analysis. In sum, we conclude that the Kinsa ILI index appears to shows high reliability in terms of distributional characteristics, and is fairly correlated with other measures related to COVID-19 and the seasonal flu.

Reliability

There are no missing values during this period across any counties, but we are unable to directly understand the percent of the population represented across geography since we do not have information about the number of smartphone users who activated Kinsa's application in each county. Although we cannot estimate the uncertainty of ILI index (i.e., standard error for daily ILI patients in each county), we instead examine the reliability of the observed ILI index within county by estimating the correlation between two time periods (t0 and t1). Low correlation would indicate large day-to-day irregularities. Figure 1, Panel A confirms that the day-to-day correlation within county is very high across all counties (r > 0.94),. Further, Panel B shows that this high correlation is maintained throughout the study period (r > 0.90).

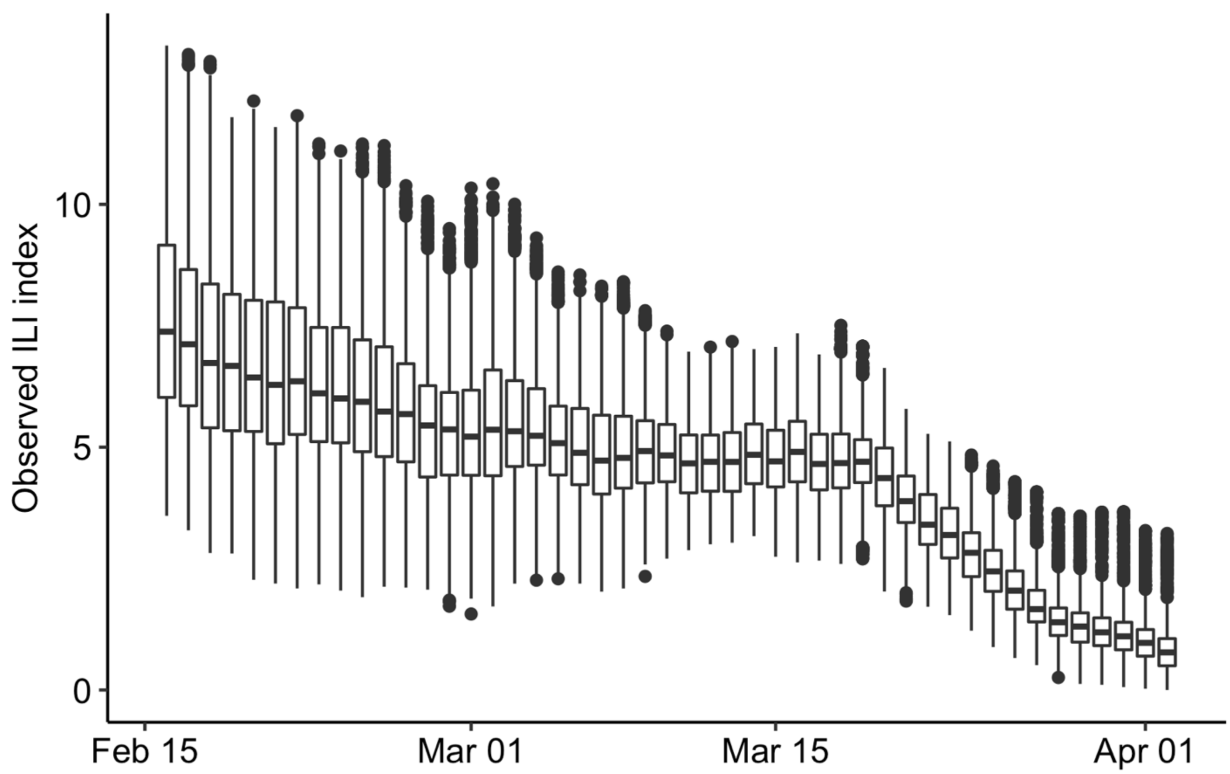
Figure 1. Day-to-Day Correlations As Measure of Kinsa's ILI Index Reliability



Validity through Investigating Data Volatility

Here, we first plot the distribution of the ILI index over time using box plots in Figure 2. We find that ILI index shows higher mean and volatility in the earlier period, but was declining, especially after March 20. This makes sense given the natural decrease in seasonal flu by March (CITE). Given that many policy measures such as school closures and stay-at-home orders are implemented following the national emergency declaration, this general downward pattern may simply show the efficacy of these policy measures in reducing all types of infectious diseases above and beyond seasonal declines. Alternatively, it may reflect sample selection bias, for example, smartphone users who had COVID-19 infections potentially dropped out of their smartphone sample due to hospitalization, although such effects could be bounded and unlikely to be a large effect here.

Figure 2. Time Trend in Box-Blot Distribution of ILI index



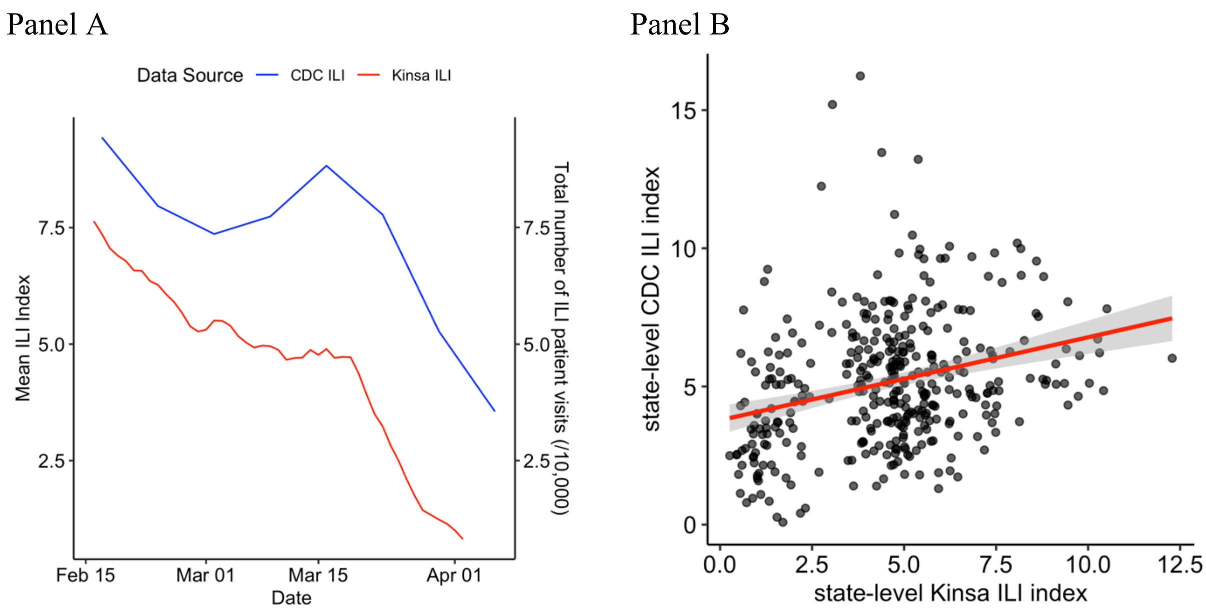
Note: Box plots show the minimum, first quartile, median, third quartile, and maximum of the data, by day.

To account for the Kinsa user population being potential a select sample that differs from the relevant full US sample with flu like symptoms, we also examine the correlation between the smartphone-based ILI index and the CDC's official report on healtth care visits for ILI[7] Figure 3, Panel A, shows that the ILI indices from both data sources follow the same downward trends,

[7] The data was downloaded from the CDC official website (last accessed April 12): https://gis.cdc.gov/grasp/fluview/fluportaldashboard.html

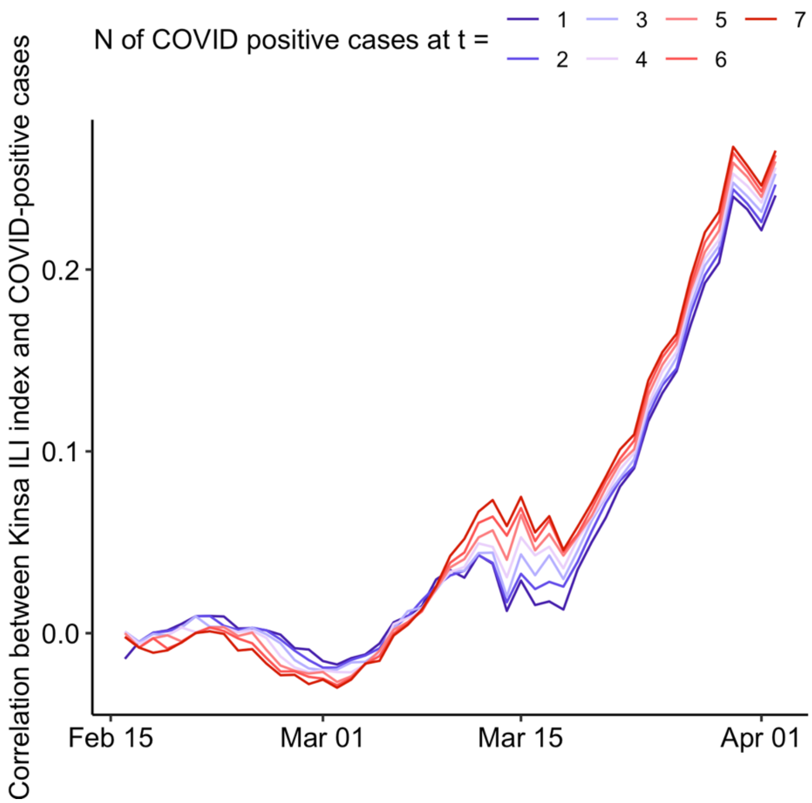
and the temporal patterns are very similar. We further examine the correlation between two indices across 50 states, and find in Panel B that they are positively correlated. While the degree of correlation is not high (r=0.29), we cannot ascertain the source of the discrepancy. For example, it is possible that the some in the Kinsa data might not be able to be diagnosed in the CDC data because of health care access toCOVID-19. But to further examine the predictability of Kinsa ILI index data for COVID-19 positive cases, we calculate the county-level correlation between the number of daily COVID-19 cases and the Kinsa Index over time. Figure 4 shows that the correlation between two is almost zero before March 15, but it had been sharply increased in more recent periods (the correlation is 0.27 on April 2, 2020). The increasing correlation could also be indicative to higher testing rates for COVID-19 over time, which is reassuring regarding the ability of smart-thermometers to highlight areas with symptoms.

Figure 3. The relationship between CDC's official ILI index and Kinsa's ILI index

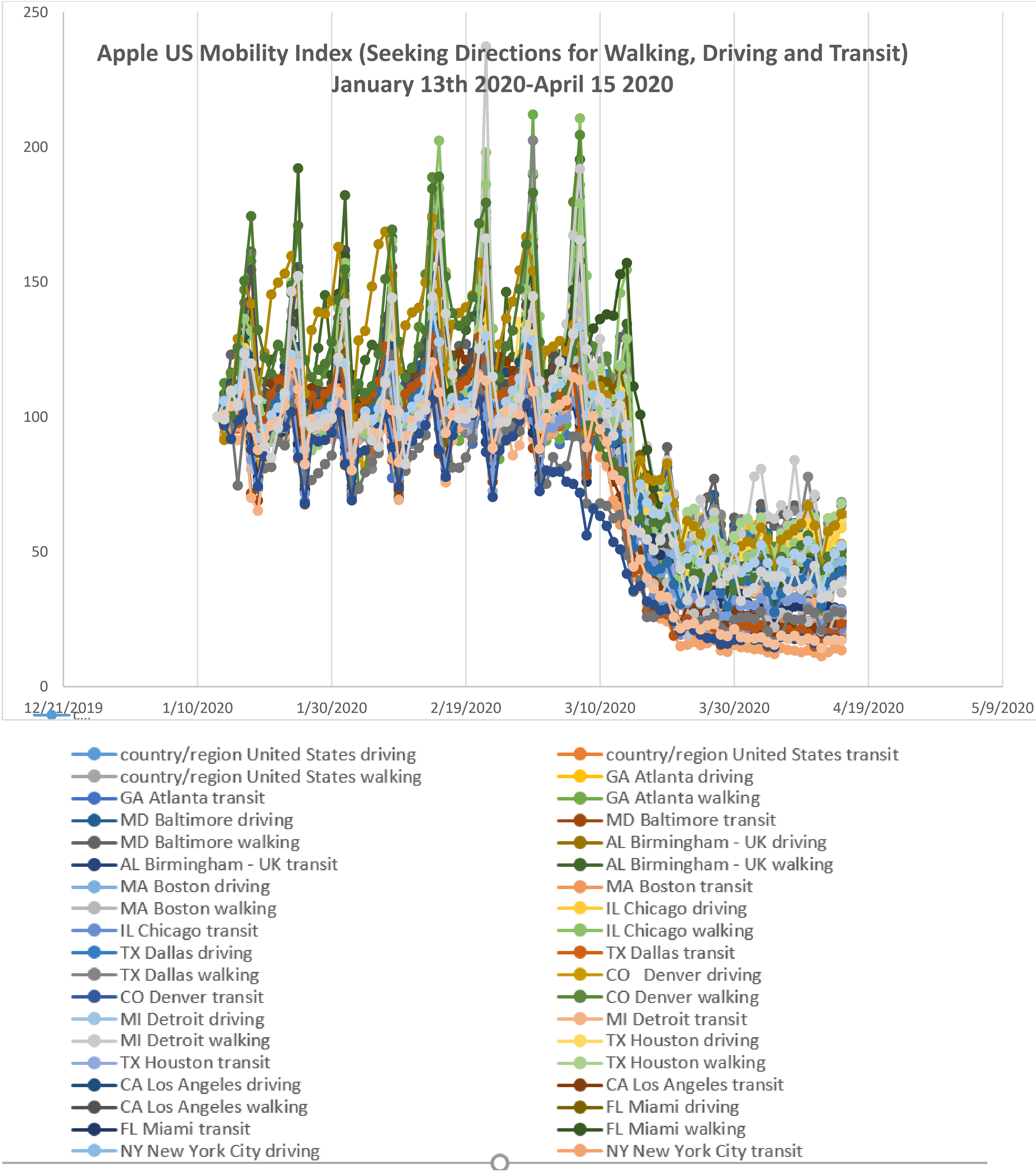


Note. On Panel A, Kinsa ILI index is plotted against the left y-axis, and CDC ILI index is plotted against the right y-axis.

Figure 4. The county-level relationship between Kinsa ILI index and the number of COVID-19 positive patients over time. Source of COVID19 cases is the NYT site (https://www.nytimes.com/interactive/2020/us/coronavirus-us-cases.html. Last accessed April 11 2020).



Please see ***link*** *for data appendix for PlaceIQ*



Source: Produced from raw data downloaded from https://www.apple.com/covid19/mobility