TRACKING PUBLIC AND PRIVATE RESPONSES TO THE COVID-19 EPIDEMIC

Evidence from State and Local Government Actions

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ABSTRACT

This paper examines the determinants of social distancing during the shutdown phase of the COVID-19 epidemic. We classify state and local government actions, and we study multiple proxies for social distancing based on data from smart devices. Mobility fell substantially in all states, even ones that did not adopt major distancing mandates. Most of the fall in mobility occurred prior to the most stringent sanctions against movement, such as stay-at-home laws. However, we find evidence suggesting that state and local policies did have an independent effect on mobility even after the large initial reductions occurred. Event studies show that early and information-focused actions such as first case announcements, emergency declarations, and school closures reduced mobility by 1–5 percent after five days. Between March 1 and April 14, average time spent at home grew from 9.1 hours to 13.9 hours. We find, for example, that without state emergency declarations, hours at home would have been 11.3 hours in April, suggesting that 55 percent of the growth is associated with policy and 45 percent is associated with (non-policy) trends. State and local government actions induced changes in mobility on top of a large and private response across all states to the prevailing knowledge of public health risks.

KEYWORDS: COVID-19, mobility, health economics
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I. Introduction

The COVID-19 epidemic has infected millions of people around the world and caused over 550,000 deaths (*New York Times* 2020). The World Health Organization (WHO)

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estimates that the case fatality rate is around 2 percent, but the overall burden of COVID-19 remains uncertain (WHO 2020). In the United States, state and local governments are central actors in responding to the crisis. Early in the epidemic, state and local governments announced important pieces of information, such as the first COVID-19 case and first death in a locality. Most state governments declared the crisis a state of emergency, which may have helped convey a sense of urgency regarding the situation and enabled further state actions to be taken. After emergency declaration, most state governments took additional policy actions to try to reduce the spread of the virus.

State and local *social distancing policies* were primarily intended to reduce the amount of person-to-person contact in the population. The theory behind these ideas is that reducing the frequency of contact between people means that there will be fewer opportunities for the virus to pass from one person to the next. Evidence from microsimulation models suggests that social distancing will help decrease the size of the epidemic and may redistribute the number of cases over time (Ferguson et al. 2020; Peak et al. 2020; Davies et al. 2020; Bento and Teixeira 2020). Flattening the curve of the epidemic in this way should reduce the risk that local health-care systems will be overwhelmed by surges in demand for health services (Keeling and Rohani 2011).

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). In addition to personal freedom costs, closure mandates may also impose substantial economic costs on society (Montenovo et al. 2020; Kahn, Lange, and Wiczer 2020). As states consider relaxing various social distancing restrictions, it makes sense to identify policies that seem to produce the greatest social distance while creating the smallest economic losses.

In this paper, we study the state and local policy response to the epidemic and assess how specific policies have actually affected measures of social distancing. Our study makes two main contributions. First, we develop a typology to classify heterogeneous policy responses to the epidemic. We examine both state- and county-level policies and estimate the share of the US population subject to different policy and information events each day for the first months of the epidemic. We also consider the order in which governments adopted different policy measures. Typologies of policies 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 serve as proxy measures of mobility. We estimate difference-in-difference and event study regressions to assess how mobility patterns respond to mitigation efforts that include formal closure policies as well as information events related to the public health threats facing a state or county.

States undertook roughly six different types of actions related to COVID-19 that might substantially affect mobility: emergency declarations, school closures, restaurant restrictions, gathering restrictions, nonessential business closures, and stay-at-home (SAH) orders. Although not intended to reduce mobility, local announcements of the first confirmed COVID-19 case and the first confirmed COVID-19 death also represent important informational events. Because of the close timing of some policy changes (Figure 2A) and the degree to which they might independently affect mobility, we study effects of first case and death announcements, emergency declarations, school closures, and SAH orders. Even among these, we caution that the policies took place mere days apart, thus making it difficult to isolate the effects of each individual policy or event in a definitive way. We note that in a robustness analysis, we estimate a specification that includes all policies simultaneously (Section VI, Subsection E.2), and our results remain qualitatively unchanged. Our analysis 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 have continued since then. Measures of travel outside the state, outside the county, and outside the home all show massive declines in mobility occurring during a time of the year when we would normally expect a large rise in mobility. For example, data from the US Department of Transportation for the average number of vehicle miles traveled (VMT)[1] show that VMT in March 2020 was 18.6 percent lower than for March 2019, and VMT in April 2020 was 39.8 percent lower than in April 2019. There is usually an increase in VMT each successive month in the spring. The index of out-of-state travel that we use in our empirical analysis fell by 53.96 percent between March 1 and April 14 for states, on average (Figure 4D).[2] For the five states without any form of SAH orders during this time, the decline was still fairly substantial, suggesting that a large fraction of the decline in mobility could be attributable to the national state of knowledge and precautions rather than specific state policies.

Our finding that a substantial share of the fall in mobility happened early and was probably not induced by strong mandates comes from both the simple descriptive trends by state and from event study results that confirm the story seen in the trends. We note that this refers only to the closure policies that took place, and had the circumstances been different and SAH policies happened first, the results could have been quite different. Our study was the first to present empirical evidence that much of the decline in mobility that occurred appears to be due to private responses to changes in risk and that more coercive social distancing policies might not have been the main driver of the behavioral changes in the early part of the epidemic. This was a somewhat controversial point initially, but the emerging literature supports our early work (Goolsbee and Syverson 2020; Cronin and Evans 2020; Cicala et al. 2020).

Although we find that SAH orders played a modest role, we also find that informational or partial closure policies that occurred early in the epidemic have had an important influence on mobility. Early county actions often had as much impact as state ones. Across multiple measures, our event study regressions show that mobility fell after first confirmed case announcements, emergency declarations, and school closures. In most cases, the initial response to the event is only about 1–5 percent. The effects grow to 7–45 percent after 20 days. These estimates come from event study regressions that trace out the mobility responses for a period of 20 days before and after the event, and they represent the incremental change in

1 Unadjusted VMT, US Department of Transportation, Federal Highways Administration, Traffic Volume Trends, accessed July 16, 2020, http://www.fhwa.dot.gov/policyinformation/travel_monitoring/tvt.cfm.

2 As of April 3, Arkansas, Iowa, Nebraska, North Dakota, and South Dakota did not have SAH laws (Vervosh and Healy 2020).

mobility caused by public policy actions and information shocks. We limit the event studies to a 20-day window because other events take place the longer we draw out the window. In particular, state reopening initiatives began after mid-April, and our study is focused only on the closing policies.

The incremental policy effects we report occur on top of the large reductions in mobility that occur independent of policy changes. There were large declines in mobility across the country, even in states that have not adopted stringent mitigation policies. (See Figure 4 for a series of time trends of our mobility indices.) Although the incremental policy and informational effects are not large relative to the nationwide reduction in mobility, there is some evidence that the cumulative effect of the policies does account for a substantial share of the overall decline in mobility and contact that occurred over the past several weeks. Specifically, we find that across the country our measure of average hours spent at home grew by about 53 percent between the first week of March and the second week in April. Our event study regressions imply that state-level emergency declarations could account for about 55 percent of the growth over this time period, with the remaining 45 percent of the growth attributable to secular trends that we interpret as the private (residual to policy) response to the epidemic. Emergency declarations occurred early in the epidemic, and they did not themselves impose mobility restrictions on the economy. The emergency declarations might be interpreted primarily as an information instrument that conveyed the seriousness of the situation to the population. However, emergency declarations could also be viewed as a reduced-form proxy for the collection of policy responses that followed in quick succession in many states. This is the interpretation we view as most appropriate given the econometric difficulty of separately identifying the effects of multiple individual policy actions. First case reports were purely informational, and school closures were partial societal closures but also happened early enough that they could have been viewed as heavily informational, and (again) these larger effects may also capture the downstream effect of the sequence of other state policies that followed in most states.

Our analysis has some important limitations. First, the timing and location of state and local COVID-19 policies are not randomly assigned, and in many cases governments may have adopted policies in response to their own efforts to measure and anticipate local epidemiological conditions. We use a flexible event study framework throughout our analysis to help mitigate concerns about the common trend and non-anticipation assumptions that are central to our research design (Wing, Simon, and Bello-Gomez 2018). In addition, 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, and whether policies of neighbors influence mobility. We also separately consider sensitivity to different policy coding schemes, including models in which nonmandatory stay-at-home policies are included with mandatory stay-at-home policies. These sensitivity analyses do not fundamentally alter our main conclusions.

The data we use to measure mobility patterns come from new sources and have not been widely used in social science research in the past. These are convenience samples based on smart-device owners, most often reflecting those who use apps where location-sharing options are turned on. The panels are not drawn from well-defined sampling frames; thus, it is reasonable to wonder how well they represent state and county populations. Squire

(2019) shows that the number of devices tracked by SafeGraph in counties with different ranges of median household incomes is similar to census population counts, and the number of devices used equals about 10 percent of the US population. Couture et al. (2020b) describe the representativeness of the PlaceIQ data and note that although smart phone and app use are not randomly assigned, the number of devices by location are in line with population distributions, and although not the same database as the SafeGraph one, the PlaceIQ devices also account for about 10 percent of the US population. These methodological studies are encouraging. But there are many other demographic and socioeconomic dimensions that we are unable to test since de-identified device data do not reveal any details about the user other than the location and time of use. Given the uncertainty about data quality and representativeness, there is value in using mobility data from multiple sources in a study of the effects of social distancing responses to the epidemic. In addition to representativeness of the sample used in the measures, there are questions regarding the measures themselves. The measures we use, such as time spent by a device at home, are imperfect proxies for what might be called “responsible social distancing.” They are coarse proxies that do not measure contact itself, let alone distinguish between high- and low-risk contact. In general, all of the measures we study are best understood as proxies for physical movement or the lack of physical movement.

As policy makers debate the merits of “reopening” the economy by lifting sanctions, or bringing back earlier sanctions, it is important to better understand how the policies that went into place in the first phase of the epidemic have affected behaviors related to mobility. Although the estimates we report in this paper may offer 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. Removing restrictions after significant buildup of demand for social interaction may lead to much larger increases in mobility than just the reverse of our estimates. 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. For example, effects of local policies may also depend on the prevailing national and international discourse regarding transmission mitigation strategies, and whether people believe that the prevailing risk of infection is rising or falling in their community.

II. Related Research

There is some empirical support for mitigation policies from studies of prior epidemics in the United States 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 United States may differ substantially from the effects of an epidemic in earlier historical periods or in recent years elsewhere.

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 subpopulations (Kremer 1996; Mossong et al. 2008; Rohani, Zhong, and King 2010; Bento and Rohani 2016; Prem et al. 2020). Contact survey data have been used to parameterize sophisticated epidemiological models of disease transmission (Mossong et al. 2008; Rohani, Zhong, and King 2010; Bento and Rohani 2016; Prem et al. 2020). However, because they are generally collected with a considerable lag, point-in-time contact surveys are not a useful way of evaluating the causal effects of epidemic mitigation policies, 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 initial objective for policy research related to the epidemic. In this paper, we use a collection of measures based on smart devices as proxies for the level of mobility in different parts of the country. None of these measures is perfect from either a construct validity standpoint or a sample construction standpoint (Buckee et al. 2020). However, we think that the correspondence between the results is informative about the way that the population has responded to the epidemic.

This paper is the first comprehensive assessment of human mobility during the COVID-19 epidemic in the United States. It uses cell-signal-based data from multiple sources to examine the way that measures of mobility and contact responded to the epidemic and to specific state and local policy actions. Prior to our study, there were simulation studies that examined the likely effects of social distancing on the course of the epidemic (Jarvis et al. 2020; Prem et al. 2020). Several other research teams were also examining the effects of social distancing policies on mobility around the same time that our working paper was released (Andersen 2020; Painter and Qiu 2020). Since our initial working paper was released, other studies have extended this line of work and found support for most of our initial conclusions (Cronin and Evans 2020; Goolsbee and Syverson 2020; Cicala et al. 2020; Chen et al. 2020; Alexander and Karger 2020). See Online Appendix Table A2 for a summary of all papers we are aware of that study effects of nationwide state policies on human mobility.

The literature on US human mobility during the pandemic documents that there was a large and rather sudden reduction in movement mid-March, which is before any state instituted a stay-at-home (SAH) mandate. All the studies we review in Online Appendix Table A2 use cellular signal data derived from large numbers (i.e., greater than 20 million) of smart devices. These studies typically present descriptive time series as well as quasi-experimental estimates of the effects of state and local policies on mobility patterns. Most studies focus on SAH mandates. Although there are a few outlier results, most of studies find that SAH policies reduced measured mobility by about 5–10 percent within the first week (Abouk and Heydari 2020; Alexander and Karger 2020; Andersen 2020; Chen et al. 2020; Cicala et al. 2020; Cronin and Evans 2020; Dave et al. 2020; Engle, Stromme, and Zhou 2020; Goolsbee and Syverson 2020; Painter and Qiu 2020). Almost all the studies end the post-policy period mid-April, as in our work, so that they do not include the state reopening policy periods. The attention to SAH mandates is understandable since they were considered the most controversial and they seem to be the most restrictive of the mandates. However, some studies also examine other policies, like school closures and specific business closures, which generally happened sooner.

Most of the studies in the literature use SafeGraph data (Goolsbee and Syverson 2020; Cronin and Evans 2020; Painter and Qiu 2020; Andersen 2020). Other studies use data from Unacast (Cicala et al. 2020; Alexander and Karger 2020), Veraset (Chen et al. 2020), or Google Mobility (Abouk and Heydari 2020). Importantly, all of the papers that we are aware of estimate effects with only one data source. Each paper makes important innovations—for example, they consider how responses differ by local political preferences, or by the types of business establishments frequented—and contributes towards a rich understanding of mobility responses during the epidemic.

We focus our study and our literature review on mobility responses because they seem like a logical first step in reducing transmission rates. However, little is known about the overall effect of any of these measures on COVID-19 transmission and mortality rates (Kaashoek and Santillana 2020). 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 United States and the United Kingdom, 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, at least three papers to date examine the partisan angles of US state policy and mobility (Adolph et al. 2020; Andersen 2020; Painter and Qiu 2020). Friedson et al. (2020) make progress towards causal identification in the case of California's SAH laws, using synthetic control. Even if states do not pass policies influenced by prior knowledge of the disease spread in their region, government policies may be 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 mandates.

III. Conceptual Model and Measures

State and local government actions could affect individual mobility behaviors by making it costly or less beneficial to mingle in society, as well as by causing individuals to update their beliefs regarding the threat that COVID-19 poses to their own health and their community's health. For example, when a state government issues an emergency declaration, it is likely conveying a message to the population that the threat from the virus is higher than previously believed. State adoption of stay-at-home (SAH) laws may impose costs (e.g., stigma, fines) on mobility, in addition to also conveying information regarding the seriousness with which officials view the situation facing the state.

Economic research establishes that both the amount of a fine and its salience matter for responses to policy (Chetty, Looney, and Kroft 2009), and suggests that it can be important to consider behavioral nudges in combination with taxes for reducing the welfare costs of tax policy (Farhi and Gabaix 2020). The cognitive salience or affective impact of different informational events could vary across different policies. In the case of important public health threats, governments often use a combination of information, mandates, and fines to try to improve social outcomes. Social distancing policies can be viewed as an effort to mitigate a market failure due to externalities. During an epidemic, each person's reduction in physical interaction generates benefits for other people by lowering downstream viral transmission rates. The benefits of these positive externalities may be particularly large for people with compromised immune systems who are at high risk of infection and mortality, and for essential workers who cannot engage in self-protective behaviors as well as nonessential workers can.

Although it may make sense for state and local governments to pursue social distancing policies on externality grounds, there is also a large private incentive for people to reduce mobility in response to information about public health risks that is disseminated at the local, state, national, and international levels. Epidemiological models integrate evidence of self-adaptive behavior in response to changes in the prevalence of an infectious disease (Fenichel et al. 2011; Fenichel, Kuminoff, and Chowell 2013; Kremer 1996).

Prior evidence on social distancing policies shows evidence of their effectiveness in reducing the spread of illness. For example, Hatchett, Mecher, and Lipsitch (2007) and Bootsma and Ferguson (2007) study the 1918 flu pandemic, which led to 675,000 deaths in the United States and 40 million worldwide (Garrett 2008). But because of obvious data limitations, the literature on the 1918 pandemic did not analyze mobility in ways we can compare with results from the current pandemic. Although there are strong reasons to believe that state actions in early 2020 will create social distance, there may be less responsiveness detected by a direct comparison of states that adopt policies only a few days apart, as personal behavior adapts 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 limitations on travel created by bans. It is more likely that behavior is changed solely through information avenues for policies such as emergency declarations (Riley et al. 2003), whereas for laws like SAH policies, it is likely that both information and direct costs play roles.

This paper focuses primarily on policies that restrict the movement of individuals through suspending activities to which they may travel to supply labor (as workers) or to demand goods and services as customers, through broad-based restrictions such as 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 statewide decision was made), or at different levels of activity (emergency declarations and state school closure laws before a SAH order). We consider six state-level and four county-level policies (state-level policies are emergency declarations, school closures, restrictions on gatherings, travel quarantines, partial and full nonessential 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 early policies may be exogenous with respect to mobility, but the latest set of actions taken by states was after considerable media awareness could have influenced mobility reductions. 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 that we study in this paper are related to whether people remain inside their house, whether they engage in social "mixing" within society (as measured by the average number of devices that come into contact with each other during the day within a community), the fraction of individuals who leave their house within the day, and the extent to which individuals travel outside their state and outside their county. 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 does not claim that reduced movement is the sole goal.

IV. Data

Our study focuses on the first quarter of 2020. The mobility outcomes we examine are available in nearly real time, but they are consistently available only for recent months. We analyze mobility outcomes at the state level and the county level, which are the levels at which COVID-19 policy and other COVID-19 information-related events typically occur.

A. STATE AND COUNTY MITIGATION POLICY DATA

Using state-level policy dates collected by Washington University researchers (Fullman et al. 2020) and Boston University researchers (Raifman et al. 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 unlikely to directly affect mobility in a major way (such as state laws banning utility cancellations for nonpayment 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.[3] 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

3 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.

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. Online Appendix Table A1 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 collected information on the date of first case and death using reported case and death data, and also by searching news outlets. Prior work finds that the first state newspaper report of a case led to substantial online search related to the virus (Bento et al. 2020). For the policy actions, we mainly work with 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.

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 16 (see Figure 1), and the federal government issued its emergency

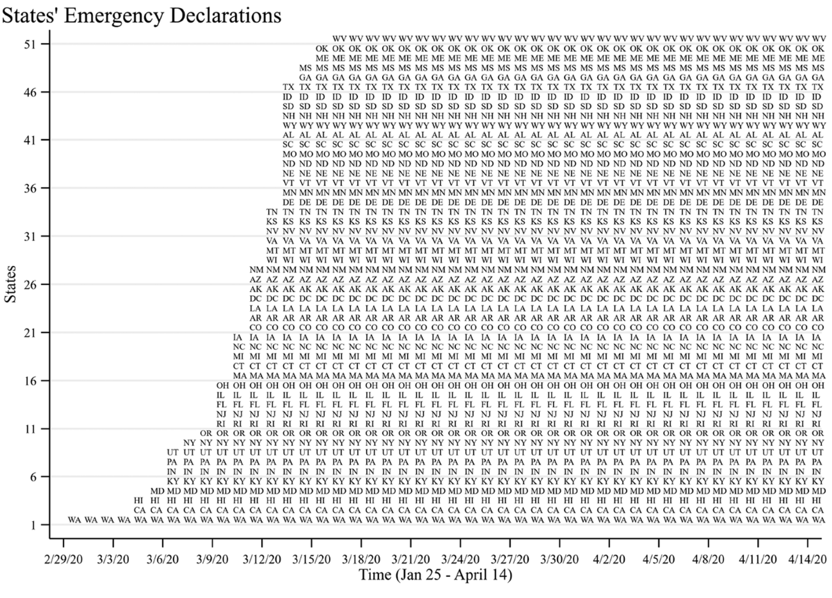

FIGURE 1. State COVID-19 policy enactment and information dates (emergency declarations included here; the remainder appear in the Online Appendix). Sources: Author compilations based on Fullman et al. (2020), the public-use map/tracker of K–12 school closures (Education Week 2020), and author compilations from original sources. For the figure of state first COVID-19 positive case announcements, we collected the timing from local media reports in each state (Bento et al. 2020), cross-checking them with other sources, including https://coronavirus.jhu.edu.

declaration on March 13, we may not expect these actions 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, or 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. Any effects that happen right away are likely due to emergency declarations. But those effects that happen much further down could be due to other policies that the emergency declarations are the first step towards. We might also expect that states spend money in certain ways (such as public health campaigns) that reduce mobility directly due to the emergency declaration, or that states use their emergency declarations to restrict behavior of residents in other ways.

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 mayors 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 rather than act as mandates on people's movements.

2. School closures: Although some school districts closed prior to state-level actions, by April 3, 2020, all 50 states and the District of Columbia 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 designated by Fullman et al. (2020) rather than by Education Week. Thirty-six states had school district closures prior to state mandates. In three states (Nebraska, Idaho, Iowa), school district–level decisions affected over 90 percent of the school population within the state, prior to governors' interventions. The day prior to the state's mandate, on average, 12 percent of the student population was affected by local district mandates. While the local activity varied substantially starting in late February for the first districts, states' decisions where concentrated in the week starting March 16, with that day alone having 24 mandates. States' behavior could have been related to the burgeoning closing activity at the school district level as well as to the emergency declaration issued by the federal government on March 13. While school closure policies would reduce some travel (of children and staff), they could reduce adult mobility as well if parents immediately changed work travel as a result. School closures may also contribute to a sense of precaution in the community. Although many spring break plans were canceled, it is possible we might also capture increased travel due to school closures.
3. Restaurant restrictions (also including other partial nonessential business [NEB] restrictions): These policies were also fairly widespread, with 49 states having such restrictions by April 7, according to Fullman et al. (2020), and would directly reduce movement from the closures.

4. Recommendations or restrictions on gatherings: 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 much action on this front: 44 states enacted gatherings policies. These laws would reduce mobility in a manner similar to restaurant closings. They might have stronger effects given their universal nature, but on the other hand, they may be hard to enforce and rely on cooperation from residents and not be as strong as business or organizational closures.
5. NEB closures (all): These occur when states have already conducted partial closings and now opt to close all nonessential 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 (such as malls and restaurants) and reduce work travel.
6. Stay-at-home (SAH) policies: 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 notable set of states have not issued a SAH in any part of the state (Vervosh and Healy 2020); as of April 14, these included Arkansas, Iowa, Nebraska, North Dakota, South Dakota, Oklahoma, Utah, and Wyoming. However, these states did take several other policy actions. Two states (Oklahoma and Wyoming) enacted curfews (which specify the hours when individuals can leave their homes) instead of complete SAH orders. Connecticut, Kentucky, Massachusetts, and New Mexico adopted SAH recommendations but did not actually impose a mandate (Raifman et al. 2020). In our main specification we do not define recommended/partial curfews as equivalent to SAH policies. We estimated alternative models that treated states with nonmandatory but strong SAH as equivalent to mandatory ones.

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). Local school closures happen at the level of school districts, which do not always correspond directly to a county. Since the mobility outcomes we consider are mainly available at the county level, we linked school districts with counties using a crosswalk from the National Center for Education Statistics (NCES). This allowed us to calculate the percentage of students in a district as well as a county affected by school closures by day. Second, we obtained data on SAH orders, emergency declarations, and business closings at the county-by-day level, from NACo (National Association of Counties 2020).[4] These policy data are among the best available county data thus far, but there are ongoing efforts to improve these data. We also created a variable for the date of the first case and first death in the county as

4 Note that we do not track the city-level closings that are, for example, reported in https://www.nytimes.com/interactive/2020/us/coronavirus-stay-at-home-order.html.

reported by the *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 strongest effect on mobility because they tend to be enforced, rather than being recommendations, but it is possible that people respond to early information such as a 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 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, Figure 2A and Figure 2B. Through the patterns visible in Figure 2A, 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 state policies, as is the first COVID-19 death (Figure 2A). Emergency declarations also appear separate. However, school closures, gatherings restrictions, and restaurant/business closings appear 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 nonessential business closures and SAH policies, although there is more policy activity in SAH laws; we select to follow the latter, as it essentially implies that businesses would close too.

Figure 2A could make it appear that states are passing the different policies together, even if different states drive the action on each. Thus, in Figure 2B 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 Figure 2A reflect what is happening at an individual state level, the events we follow henceforth are state first cases and deaths, emergency declarations, school closures, and SAH laws. For the county level, we show in Figure 3A 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 percent of the population at the most active point. School closures are measured on a different axis in Figure 3A, as those decisions are made at the school district level rather than at 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 percent and 90 percent rules and find the results robust in terms of which counties we considered closed (very few fall into 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 towards the end of the period.

In Figure 3B we show separately the county COVID-19 case and death initiation pattern. Although the first case was reported on January 25, 2020, there was a fairly long time lag before there was a substantial increase in other communities, but after March 5 there was a rapid increase. As of March 5, 1 percent of the US population had experienced a first case in their county. By March 15 this number was 50 percent; by March 25 it was

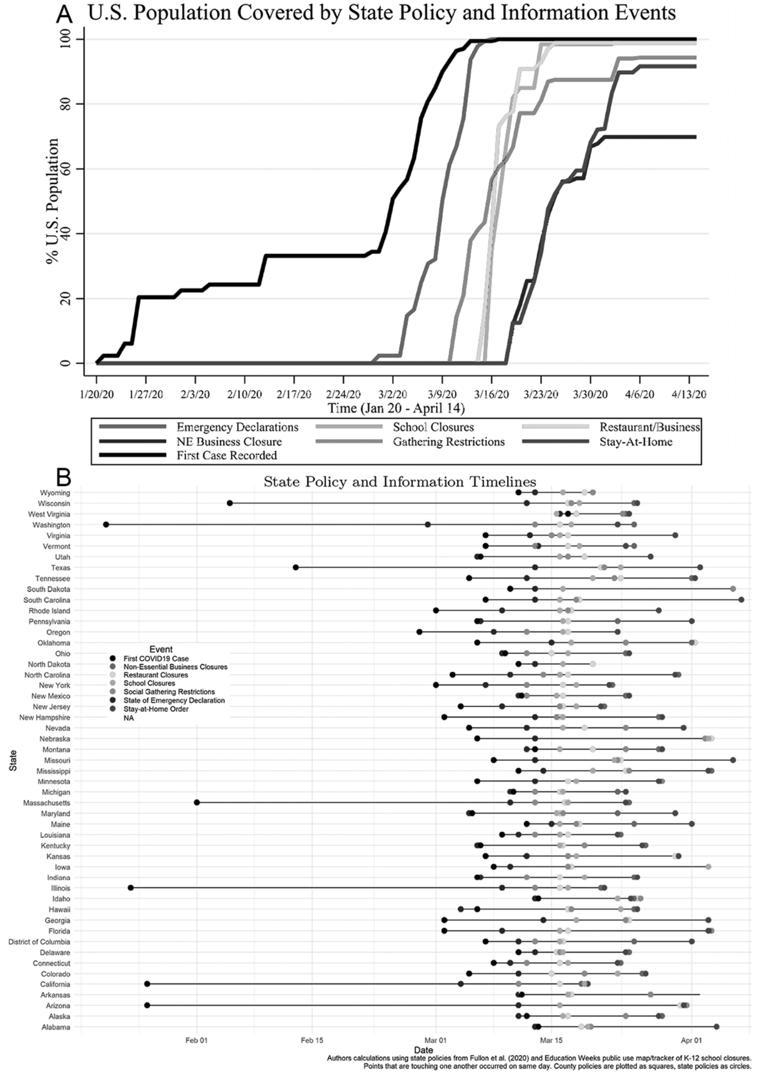

FIGURE 2. State policies and timeline. A: US population covered by state policy and information events. Please see notes to Figure 1. Each line represents the percentage of the US population exposed to the corresponding state policy or information event between January 20, 2020, and April 14, 2020. B: State policy and information timelines. Authors' calculations using state policies from Fullman et al. (2020) and Education Week's public-use map/tracker of K–12 school closures (Education Week 2020. Points that are touching one another occurred on the same day. County policies are plotted as squares, state policies as circles. The 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, 2A, and Online Appendix Table A1. A color version of this figure is available online.

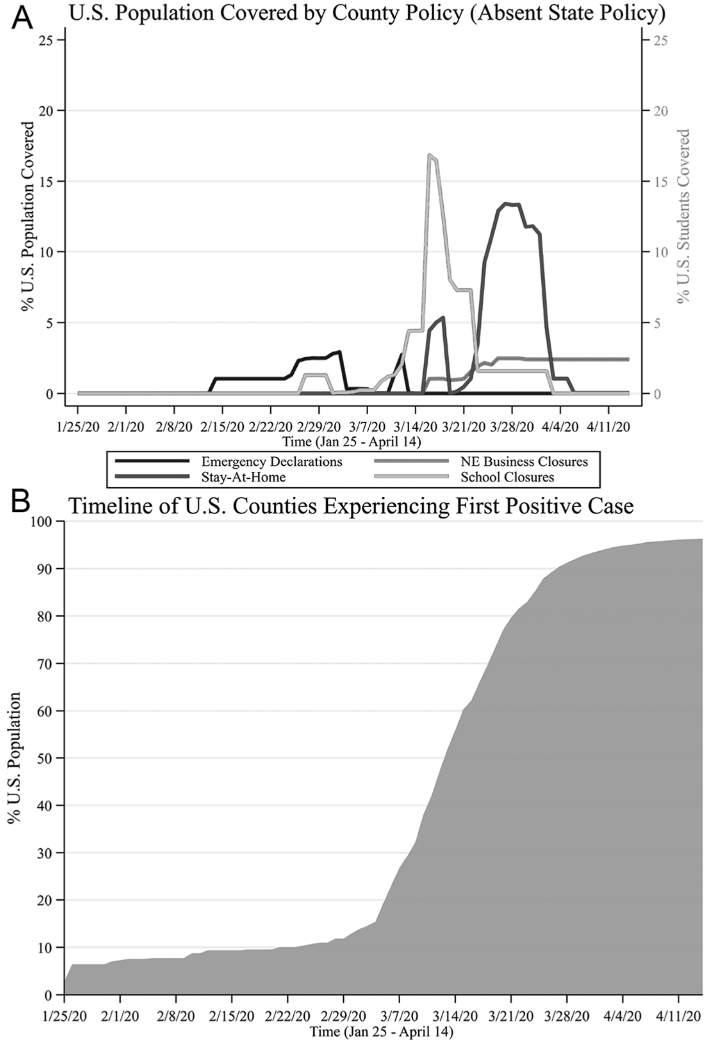

FIGURE 3. County policies and timeline. A: US population covered by county policy (absent state policy). See notes to Figure 1. We use county populations as of 2018 as the weights. Each line represents the percentage of the US population (or K–12 student population) exposed to their resident county's policy, absent a concurrent corresponding state policy. The first county emergency declaration was announced on January 25. Note that the right axis refers to the school closure measure, as we denote it by percentage of students covered in the relevant districts, weighted to the county levels. The left axis measures the percentage of the US population represented by the relevant counties. B: Timeline of US counties experiencing first positive case. Data are from the *New York Times*, based on reports from state and local health agencies (*New York Times* 2020). A color version of this figure is available online.

about 90 percent, and this pattern somewhat flattened after that. For deaths, there is a similar steep increase around March 18, although the first death was reported on February 29, 2020.

As this section demonstrates, there are some principles we use for 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 (nonformally) whether we could plausibly separate the effects of these policies. In future work, researchers should consider further opportunities for investigating the heterogeneity of responses to the policies.

B. SOCIAL DISTANCING AND MOBILITY OUTCOME DATA

We use mobility data from PlaceIQ (publicly provided; Couture et al. 2020a), SafeGraph (provided upon free research agreement), Apple Mobility, and Google Mobility. We use each of these data sources to construct several measures of how much people circulate in society, as proxied by detected movement of smartphones. In general, each smartphone is assigned a "home" geographical location based on the location where the device is primarily during the night. These data are originally collected for commercial purposes. Typically, companies receive data from mobile applications that include opt-in features for geolocation tracking. The companies with the databases have provided researchers with time-limited free access to these resources to assist with efforts related to the current crisis. As these data are not collected primarily for research purposes and could have discrete jumps depending on which apps participate, and because the representativeness of the underlying populations is unknown, we think there is substantial value in confirming results across multiple sources.

We focus on five mobility measures that are available daily at the county and state levels:

1. Mixing index: An index that measures, on average, how many other devices were present at some point during the day at locations visited by the device. We compute state-by-day and county-by-day averages of this measure using underlying data from the PlaceIQ device exposure measure (DEX) for the period January 20, 2020, to April 14, 2020 (Couture et al. 2020b). We interpret the mixing index as a measure of the average exposure of a device in a county or state to other devices on that day.[5] We consider this a measure of how much society "mixes" in that location. All 50 US states and the District of Columbia are represented in the PlaceIQ DEX measure; however, they are available for only 2,018 of the more than 3,000 US counties (counties with at least 1,000 device samples as of late January).
2. Time at home: The average time a device is detected in the home location. This measure is constructed using mobility data provided free for COVID-19 research by SafeGraph. SafeGraph reports that it tracks 35 million unique devices per month,

5 This measure is the average across devices in the county or the state of the average number of other devices that also visited locations that my device visited (Couture et al. 2020b, 2). An example value of this index is 100.

and we use data available from January 1, 2020, through April 14, 2020. These data provide a measure of the median minutes/hours spent at home by devices at the census block group level. We collapse the block group data to the county-by-day and the state-by-day level and interpret that data as the average time spent at home across devices in the county or state.

3. Fraction leaving home: An index of whether devices leave the home location at all during the day. Also provided by SafeGraph, this measure is based on the number of devices that are detected to be entirely at home during the day. The raw data are provided at the census block group level; we sum these to county-by-day or state-by-day levels. We measure the "fraction who left the house" by the ratio of the number of devices that are detected to leave the house divided by the total number of tracked devices.
4. Out-of-state movement: An index of the degree to which devices from a state were detected to be out of state at any point during the past 14-day rolling window. This measure is constructed from the publicly available anonymized, aggregated location exposure indices (LEX) provided by Couture et al. (2020b) and based on PlaceIQ data. Specifically, the LEX is an $N \times N$ matrix that measures, among smart devices that pinged in a given location (one of $N = 51$ US states and the District of Columbia), the approximate percentage of those devices that pinged in each other state at least once during the previous 14 days. Technically, we cannot tell whether the same device traveled to more than one outside state/county. We take the sum of the approximate percentages, so the measure should be considered an index of out-of-state movement rather than literally the percentage of devices that traveled out of state/county.[6]
5. Out-of-county movement: An index of the degree to which devices in a state were detected to be outside their home county at any point during the past 14 days. The out-of-county index is similar to the out-of-state mobility measure, but at the county-by-day level. It is also based on the PlaceIQ LEX measure provided by Couture et al. (2020b). It measures, among smart devices that pinged in a given county location ($N = 2{,}018$ US counties), the approximate percentage that pinged in each other county location at least once during the previous 14 days. We collapse the data to the county-by-day and state-by-day levels.[7]

6 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 $P_{st} = \Sigma_{j \neq s} jst$. The aggregate index is the sum of a collection of proportions, and therefore it can take on values that are greater than 1. 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.

7 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: $P_{ct} = \Sigma_{d \neq c} P_{cdt}$.

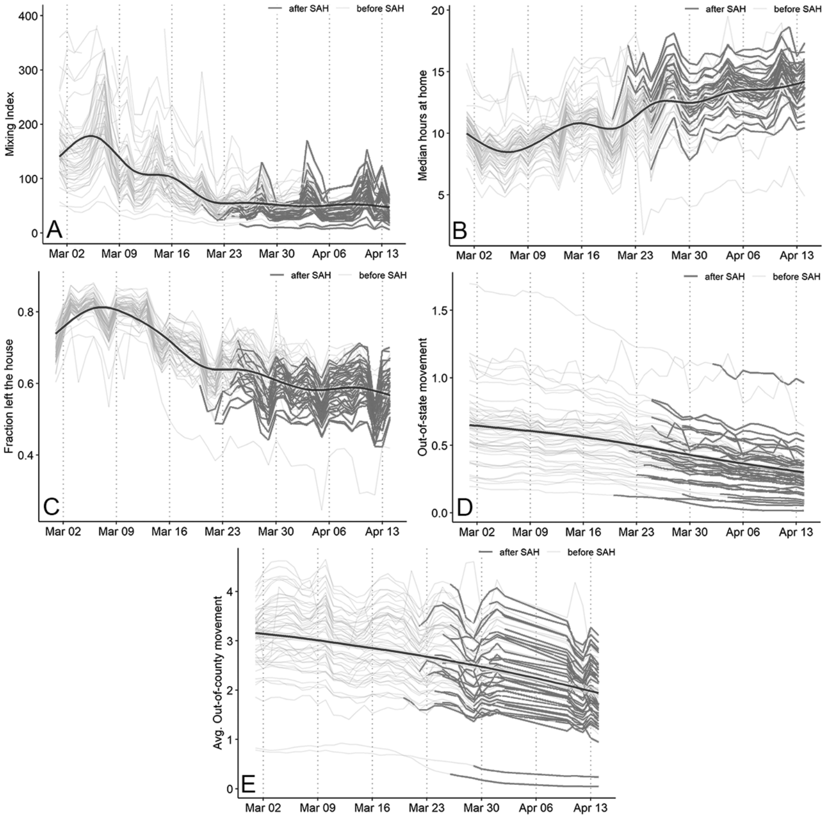

FIGURE 4. National and state time trends in outcomes. A: Mixing index, by state by day (March 1–April 14, 2020). Each light gray line represents a state and shows the value of an index for the amount of mixing of device owners that happens in a state on that day. Dark gray lines represent states with SAH laws, for the period after the law is in effect. The thick dark gray line represents a "smoothed" national local average (a generalized additive model, GAM) of the states; there is a drop of 70.5 percent from March 1 (141.43) to April 14 (41.67). B: Average of median hours at the house, by day by state (March 1–April 14, 2020). Each light gray line represents a state and shows the mean number of hours a device spent in total in the house during the day. Dark gray lines represent states with SAH laws, for the period after the law is in effect. The thick dark gray line represents a "smoothed" national local average (a generalized additive model, GAM) of the states; there is a rise of 42.02 percent from March 1 (9.98 hours) to April 14 (14.18 hours). C: Fraction leaving the house, by day by state (March 1–April 14, 2020). Each light gray line represents a state and shows the fraction of devices detected out of the house at some point during the day (as opposed to those spending the entire day within the house). Dark gray lines represent states with SAH laws, for the period after the law is in effect. The thick dark gray line represents a "smoothed" national local average (a generalized additive model, GAM) of the states; there is a drop of 23.18 percent from March 1

We end our study period on April 14, 2020, for two reasons. One is that by the middle of April, states were announcing reopening plans, with South Carolina reopening on April 20. By stopping the study, we avoid confounding effects from reopenings. Second, the longer we look out, the more that other policies occurred and it is hard to separate out effects of separate policies.

The group of plots in Figure 4 shows the national and state-by-state raw trends for each of the five outcome measures. The lines each indicate a state, shown in light gray before the state adopts and SAH mandate, and shown in dark gray once a state has a SAH mandate in place. The dark gray line indicates the "smoothed" (generalized additive model) average of the states' values.

Figure 4A shows how the mixing index (measure 1) evolved over time in each state. Weekend patterns and other seasonal effects are visible, where all lines move together. There is a substantial drop in the amount of social interaction in society over time, indicating a 70.5 percent drop in values from 141.43 to 41.67, March 1 to April 14. March 1 was a Sunday and April 14 was a Tuesday, so some change is due to week day and seasonality, but these effects will be captured in the regression date fixed effects, and the smoothed average of the states clearly shows a decline. Furthermore, the relevant mental concept is that spring is usually a time of increased mobility, so any decline is abnormal. When considering the overall reductions in mobility that we observe nationally during March 2020, this places the statistics in the Figure 4 series in stark contrast.

Another noteworthy feature of all the Figure 4 series is that states without much policy change appear to experience large declines regardless of the SAH policies. States with no SAH policies at all (light gray throughout) see declines in movement almost as dramatic as in other states, and states with SAH policies see reductions before policies go into effect. A simple average of the five states with no policies shows that mobility declined a large amount, relative to the national average (by tracking the lines that remain light gray to the end of the

(0.738) to April 14 (0.567). D: Index for leaving the state (in last 14 days as of this day), by day by state (March 1–April 14, 2020). Each light gray line represents a state and shows the sum of the percentage of cell phones detected out of state in the last 14 days. Dark gray lines represent states with SAH laws, for the portion after the laws are in effect. The thick dark gray line represents a "smoothed" national local average (a generalized additive model, GAM) of the states; there is a drop of 53.96 percent from March 1 (0.650) to April 14 (0.299). E: Index for leaving the county (in last 14 days), by state by day (March 1–April 14, 2020). Each light gray line represents a state and shows the sum of the percentage of cell phones detected out of the home county, in the last 14 days, county population weighted average at the state level. Thus, this is the state's average of people's movement out of their own county. Dark gray lines represent states with SAH laws, for the period after the law is in effect. The thick dark gray line represents a "smoothed" national local average (a generalized additive model, GAM) of the states; there is a drop of 37.72 percent from March 1 (3.16) to April 14 (1.97). A color version of this figure is available online.

time period), and that the lines that turn color shows that the trends do not look substantially different after the policy.

Figure 4B shows trends in measure 2, the intensity of remaining at home—time spent (measured in hours), taking a state average of medians reported at the census block groups. There is a 42 percent increase in this measure between March 1 and April 14. Figure 4C shows the time trend in devices having left the house (measure 3), again indicating a 23 percent decline from March 1 to April 14. In unreported figures, when we excluded work travel, we see that this measure decreased by a larger amount, as expected. This measure is fairly generous in the meaning of leaving the house, as even a short walk outside the house would count, thus we also consider it one that may not show large adjustments, compared with our intensive measure of time at home (measure 2).

Figures 4D and 4E show the "out of vicinity" travel measures that are available only as a 14-day moving average. In Figure 4E, the measure is the county population weighted average at the state level, from county-level observations. These measures 4 and 5 show a 53.96 percent decrease in the out-of-state travel index between March 1 and April 14, and a 37.72 percent decline in the average movement outside of counties, on average across states.

Several other measures of mobility data are now available for COVID-19-related monitoring and research. For example, Apple (https://www.apple.com/covid19/mobility) released an index of request intensity for driving, walking, or transit directions from Apple Maps starting from January 13, 2020. Although not our main focus, we use these for sensitivity analyses and to verify similar patterns across location and time. In Online Appendix Figure A6, we document the changes that occurred for several major cities and nationally in requests for directions. Between March 1 and April 15, 2020, there was a 37.6 percent reduction in requests for driving directions (shown), a 71.6 percent reduction in transit directions, and a 50.7 percent reduction in walking directions (not shown). Online Appendix Figure A4 shows data for Google Mobility (https://www.google.com/covid19/mobility/), an index released recently that shows from March 1 to April 14 large declines in visits by devices to various nonhome locations, and increased detection in the home location. These indices are derived from Google location services, for grocery and pharmacy (shown), parks/beaches, transit stations, retail and recreation, workplaces, and residential (not shown). For example, their index for retail and recreation decreases from a value of 13 on March 1 to a value of −45 on April 11, a reduction of over 400 percent. Online Appendix Figure A4 shows an increase that happened in stocking up prior to the large national declines.

Facebook (https://dataforgood.fb.com/) also offers maps of population movement, and 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 25. They (consistent with other sources) pinpoint the decline to starting between Friday, March 13, and Monday, March 16, 2020, such that by Monday, March 23, 2020, they find that "most major metropolitan areas in the United States experienced on average a 50 percent reduction in typical commutes to/from work." Thus, although each national index of mobility shows a decline over the month of March, the magnitudes tend to vary somewhat, but are mostly in the 40–70 percent range.

V. Methods

A. COUNTY CROSS-SECTIONAL REGRESSIONS

To shed light on the overall patterns in social distancing over the early part of the epidemic, we start with a descriptive analysis of the total county-level change over the month of March in time spent at home and cell phone social mixing. Specifically, let ΔY_{cs} be the difference between March 1 and March 31 in either time spent at home or cell phone mixing in county c from state s. Focusing on these one-month differences removes stable differences in the *level* of mobility across counties, and focuses the analysis on the recent change in mobility that happened over the month of March. Some counties experienced much larger declines in mobility in March than others. We link these one-month-difference measures with a vector of county-level covariates related to the urbanicity, population size, demographic composition, socioeconomic status, and health of the county from the Area Health Resources Files (AHRF) and the County Health Rankings (CHR) databases (HRSA 2020; County Health Rankings 2020), and we summarize the relationship between one-month-change scores and county covariates using the following cross-sectional regression model:

$$\Delta Y_{ct} = Urban_{cs}\beta_1 + Demography_{cs}\beta_2 + SES_{cs}\beta_3 + Political_{cs}\beta_4 + \varepsilon_{cs}.$$

In the model, $Urban_{cs}$ is a vector of covariates describing county population, population density, and urbanicity; $Demography_{cs}$ is a vector of covariates describing the detailed age, gender, and racial population shares in the county; SES_{cs} is a vector of covariates describing median household income, poverty rate, health uninsurance rates, and whether the county is a major recreation destination or retirement destination; and $Political_{cs}$ records the Republican vote share in the 2016 presidential election.

B. STATE-LEVEL EVENT STUDY

We use event study regression models to examine how state-level measures of social distancing 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, and we fit event study regression models with the following structure:

$$y_{st} = \sum_{a=-21}^{-2} \alpha_a 1(TSE_{st} = -a) + \sum_{b=0}^{21} \beta_b 1(TSE_{st} = b) + \theta_s + \gamma_t + \varepsilon_{st}.$$

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; and 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 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. The reference period in all event studies is the period before adoption, when $TSE_{st} = -1$. We estimate standard errors using a cluster robust variance matrix that allows for heteroskedasticity and for clustering at the state level.

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. In principle, the length of the event time "window" could be very long. However, the coefficients that are far from the onset of the event would be identified by only a few states that adopted the policy very early or very late. To avoid bias from composition change from one event study coefficient to the next, we set the length of the focal event time window to run from 20 days before the event and 20 days after the event, which keeps compositional variation low across all samples. In practice, this means we set $TSE_{st} = 21$ if $t - E_s \geq 21$ and $TSE_{st} = -21$ if $t - E_s \leq -21$ to "dummy out" the event study coefficients outside the focal range. The event study graphs only show the coefficients in the focal range 20 days before and after. Figure 5 shows an example plot of the number of states that would contribute to the identification of each event study coefficient over a period of 60 days before and after states announced emergency declarations. The graph makes it clear that very few states made emergency declarations early or late enough to help identify event study coefficients more than 50 days out. But the composition of states is very stable inside narrower windows. We used graphs like this to guide our decision to use a symmetric 20-day window in all of our event study regressions.

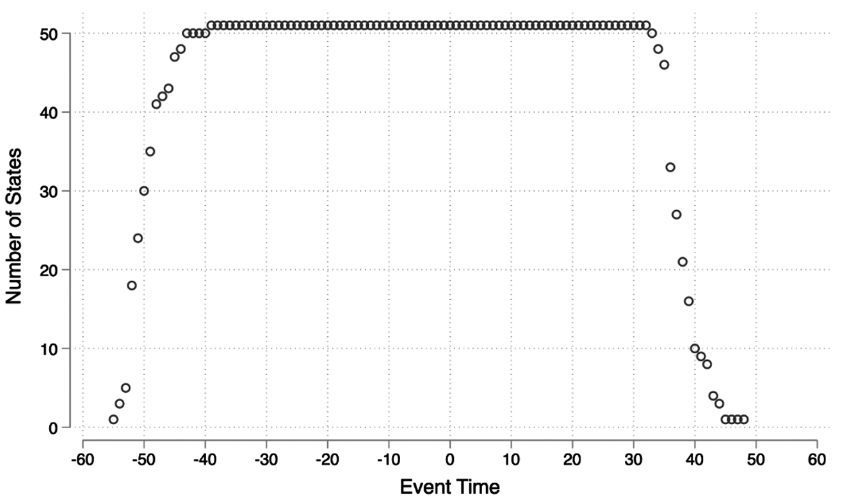

FIGURE 5. Composition of sample identifying event time effects for a policy. Figure shows composition of event study emergency declarations. For the policy that is the first in the set we follow, we produce this figure to understand the number of states that contribute towards identifying event study coefficients.

C. 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 the following:

$$y_{cst} = \sum_{a=-21}^{-2} \alpha_a 1(TSE_{ct} = -a) + \sum_{b=0}^{21} \beta_b 1(TSE_{ct} = b) + \theta_c + \gamma_t + \sigma_{st} + \varepsilon_{st}.$$

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; and σ_{st} is a *state* × *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 from the state and county models to understand the response of individuals to state versus 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. We estimate standard errors using a cluster robust variance matrix that allows for heteroskedasticity and for clustering at the county level.

Following the state-level analysis, our county-level event study regressions are based on a balanced panel of counties and a symmetric 20-day event study window.

Online Appendix Table A3 records the details of the state- and county-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, and the sample size.

VI. Results

A. CHANGES IN SOCIAL DISTANCING AT THE COUNTY LEVEL DURING MARCH 2020

Table 1 shows regression coefficients from models of the county-level change from March 1 to March 31 in our measures of time spent at home and social mixing.[8] Across the 3,106 counties with complete data on the time spent at home measure, the average change in time spent at home was 0.80 hours, but this varied substantially across counties. Counties at the 90th percentile increased time at home by 3.2 hours, and counties at the 10th percentile actually reduced time at home by about 1.2 hours (not in table). As seen on rows 3–5 of Table 1, time spent at home tended to increase a larger amount in more urban counties, consistent with the idea that business activities in a rural community are systematically more "essential" in nature (Brown and Hanson 2020). Compared with a reference group of counties that are rural and

8 We examined this period to keep this analysis to the convenient concept of a month during which large changes occurred, although March 1 and March 31 represent different days of the week (a Sunday and a Tuesday), which will be captured in the intercept.

TABLE 1. County-level correlates of change in time at home and mixing

	Change in time at home			Change in mixing		
	B	SE	p	B	SE	p
Population/1,000	0.00	0.00	0.007	−0.02	0.01	0.058
Pop. density	0.00	0.00	0.447	0.00	0.00	0.348
Metro area > 1 million	0.40	0.09	0.000	−29.44	5.41	0.000
Metro area 250k to < 1 million	0.24	0.06	0.000	−11.28	3.45	0.001
Metro area LT 250k	0.15	0.07	0.034	−0.81	2.96	0.784
Republican vote share 2016	−1.31	0.32	0.000	2.65	17.36	0.879
Percentage white	0.00	0.00	0.411	0.75	0.44	0.090
Percentage black	−0.02	0.00	0.000	0.69	0.37	0.061
Median HH income	0.03	0.01	0.000	−0.79	0.41	0.052
Poverty	0.02	0.01	0.042	0.85	0.59	0.150
Uninsured	−5.63	0.83	0.000	−349.79	44.89	0.000
Recreation county	0.26	0.11	0.014	−9.88	5.28	0.062
Retirement destination	−0.05	0.09	0.598	−7.04	3.75	0.060
Age and gender composition						
Percentage male 20–24	−0.07	0.07	0.301	−0.83	3.37	0.804
Percentage male 25–29	0.08	0.13	0.562	−24.09	7.98	0.003
Percentage male 30–34	−0.10	0.17	0.569	12.08	10.14	0.234
Percentage male 35–44	0.30	0.10	0.003	−14.39	6.04	0.017
Percentage male 45–54	−0.21	0.11	0.052	6.79	4.90	0.165
Percentage male 55–59	−0.48	0.25	0.056	−9.79	7.90	0.216
Percentage male 60–64	−0.02	0.23	0.929	11.43	9.08	0.208
Percentage male 65–74	−0.01	0.18	0.944	−12.65	6.96	0.069
Percentage male 75–84	−0.38	0.25	0.134	−5.23	9.49	0.581
Percentage male > 84	−0.34	0.46	0.450	−15.89	14.66	0.279
Percentage female 20–24	−0.36	0.07	0.000	−16.68	3.04	0.000
Percentage female 25–29	0.24	0.18	0.187	0.52	8.82	0.953
Percentage female 30–34	−0.06	0.21	0.784	−17.81	8.31	0.032
Percentage female 35–44	0.27	0.13	0.034	−29.32	6.22	0.000
Percentage female 45–54	0.22	0.13	0.078	−11.35	5.20	0.029
Percentage female 55–59	0.26	0.26	0.327	2.90	8.07	0.720
Percent female 60–64	0.13	0.22	0.568	−0.91	10.00	0.928
Percentage female 65–74	0.13	0.19	0.488	1.88	6.76	0.781
Percentage female 75–84	0.37	0.18	0.037	−6.54	7.22	0.366

TABLE 1. *Continued*

	Change in time at home			Change in mixing		
	B	SE	*p*	B	SE	*p*
Percentage female > 84	−0.21	0.18	0.229	−4.63	6.68	0.488
Constant	−1.52	1.44	0.289	439.58	91.86	0.000
Mean long difference		0.80			−92.11	
SD long difference		1.71			65.34	
R^2		0.4025			0.4486	
N		3,106			2,008	

Sources of county characteristics: Area Health Resources Files (HRSA 2020) and County Health Rankings (2020); we use the latest year available in each original source.
Note: Specification: Simple ordinary least squares using cross-sectional data at county level. Change measures as between March 1 and March 31. Each column represents results from a separate regression, where the dependent variable is the outcome listed.

nonmetro areas, the coefficients from the regression imply that time spent at home went up by about 0.40 hours more in metro areas with more than 1 million people, 0.24 hours more in metro areas with 250,000 to 1 million people, and by 0.15 hours more in small metro areas with fewer than 250,000 people.

Time spent at home also rose more in counties that are recreation and tourist destinations. Reduced mobility also tended to be higher in counties with higher median household income, higher poverty, and higher uninsurance rates, suggesting a complicated relationship between income, inequality, and social distancing. Counties with a higher Republican vote share in the 2016 election tended to have lower increases in time spent at home. The model implies that a 15 percentage point increase in the Republican vote share reduces the time spent at home by about 0.2 hours. Finally, time spent at home did vary with the age-gender mix of the county population. Specifically, reduced mobility was higher in places with a higher population share of men and women aged 35–44 and women aged 75–84. Increases in time spent at home was lower in places with a higher population share of men aged 45–54 and 55–59 and women aged 20–24.

The change in the social mixing index also varied across the 2,008 counties where those data were available. The cross-county average change in the mixing index was −90.1 (not shown). The 10th percentile change was −160.3 and the 90th percentile change was −38.0. The reduction in mixing was larger in counties with larger and more urban populations. The mixing index fell by about 30 points more in metro areas with more than 1 million people (row 3) than in the reference group of nonmetro and rural counties. Expressed relative to the average change, this is about a 32 percent differential. Likewise, the index fell by about 11 points more in metro areas with 250,000 to 1 million people than in the reference group. The decline in social mixing was also associated with the age and gender mix of the county. Mixing fell more in counties with a larger share of men aged 25–29 and 35–44 years and in counties with more women aged 20–24, 30–34, 35–44, and 45–54 years.

The results in these descriptive cross-sectional regressions suggest that compliance with social distancing likely varies across communities and across people. It is unclear whether the observed variation in social distancing across various parts of the country was due to different levels of actual versus perceived risk, different compliance costs, or differences in public policy. Examining these patterns in more detail is left to future work.

B. STATE-LEVEL EVENT STUDIES

In the Figure 6 series, we present event study coefficients from state policy and information event models, examining the impact on our five measures of mobility. In Online Appendix Table A3, we present the detailed event study regression results.

Figure 6A examines the event study effects on measure 1 (amount of social mixing). The results suggest that the concentration of devices in particular locations does not trend differentially in the period leading up to any policy or informational event. However, we do not find statistically significant evidence that the policy or information events have induced substantial changes in mixing at the state level except for a large effect of emergency declarations. The event study coefficients imply that emergency declarations reduced the state-level mixing index by about 39 percent after 20 days, relative to the value of the index on March 1, which is the baseline reference period for all effect sizes (in percentages) reported here. First death announcements also carry a large coefficient, but this event is not statistically significant; school closures and stay-at-home (SAH) laws have statistically insignificant and wrong-signed coefficients.

In Figure 6B, we examine the responsiveness of time spent at home to state events. The hours at home measure is one of the only measures that combines both intensive and extensive margin mobility responses, as the mixing index measures activity only conditional on a cell device having interacted in venues outside the house. Since this outcome measures time at home rather than time out of the home, we expect it to rise, rather than fall, in response to mitigation policies and information events. There is weak evidence of differential pre-trends in these event studies only for the first case event. We find here too evidence that emergency declarations seem to have induced a substantial increase in time spent at home. The coefficients trend upward across the post-event period and imply that the emergency declarations increased time spent at home by 22 percent after 20 days. Time spent at home also appears to independently rise by about 8 percent 20 days after the announcement of the first death in the state. The other events have smaller correctly signed coefficients but are noisily estimated.

In Figure 6C, we examine the percentage of devices that leave the home. This measures a fairly extreme extensive measure, as few people may change whether they step outside the home at all. There is little evidence of differential pre-trends in these models. However, the event study estimates from these measures suggest that there are significant decreases in mobility after emergency declarations, SAH policies, and first deaths. The leaving home index falls by 11 percent 20 days after emergency declarations and by 7 percent 20 days after the first death. SAH policy effects are not statistically significant by 20 days after the policy but have detectable effects of about 4 percent for much of the post-policy period. The next two outcomes are indices of travel outside the state and county, which are key issues for understanding transmission of the disease. These measures are averages over the past two weeks,

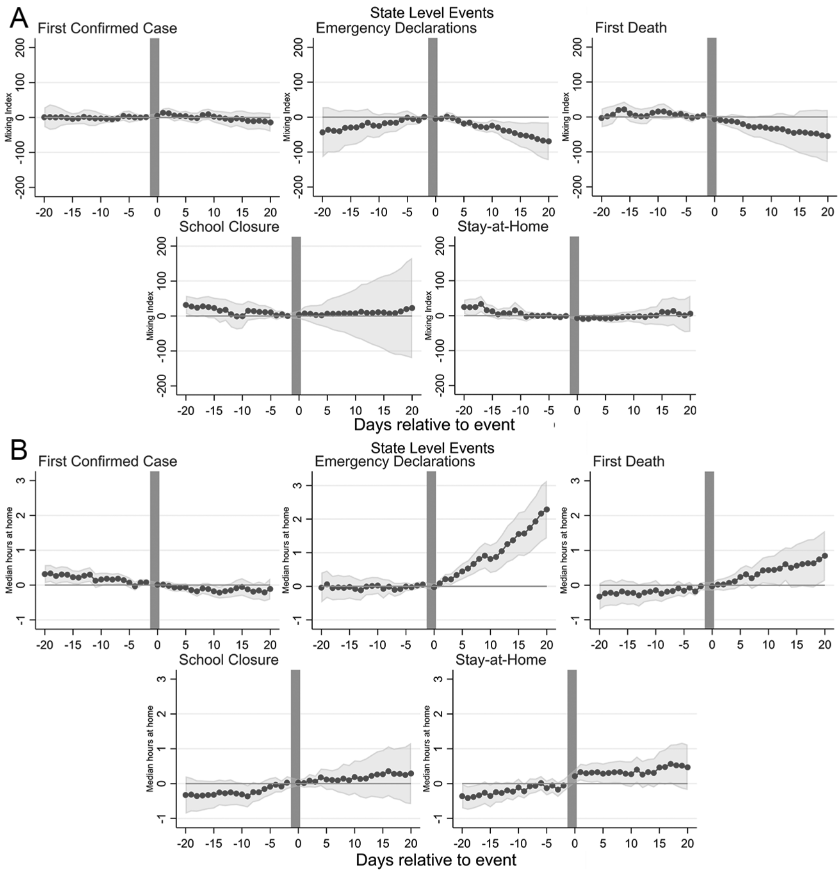

FIGURE 6. Effects of mitigation policies and information events. A: Effects on mixing index. Regression results (coefficients and 95 percent confidence intervals). Baseline dependent variable mean = 178.64, SD = 97.59. The dependent variable shows the state's index for mixing (average amount of mixing within its census block groups). Standard errors are clustered at the state level. Full event study estimates are available in Online Appendix Table A4, and effect sizes are available in Online Appendix Table A5. Regression estimates of date fixed effects are graphically presented in Online Appendix Figure A1. Corresponding raw state mixing index time plots relative to state emergency declarations and stay-at-home policies are available in Online Appendix Figure A2. Source: PlaceIQ geolocation data. B: Effects on median hours at home. Regression results (coefficients and 95 percent confidence intervals). Baseline dependent variable mean = 10.34, SD = 0.96. The dependent variable shows the average of (census block group) median times at home, in a state. Standard errors are clustered at the state level. Full event study tables are available on request, and effect sizes are summarized in Online Appendix Table A5. Source: SafeGraph aggregated mobility metrics. A color version of this figure is available online.

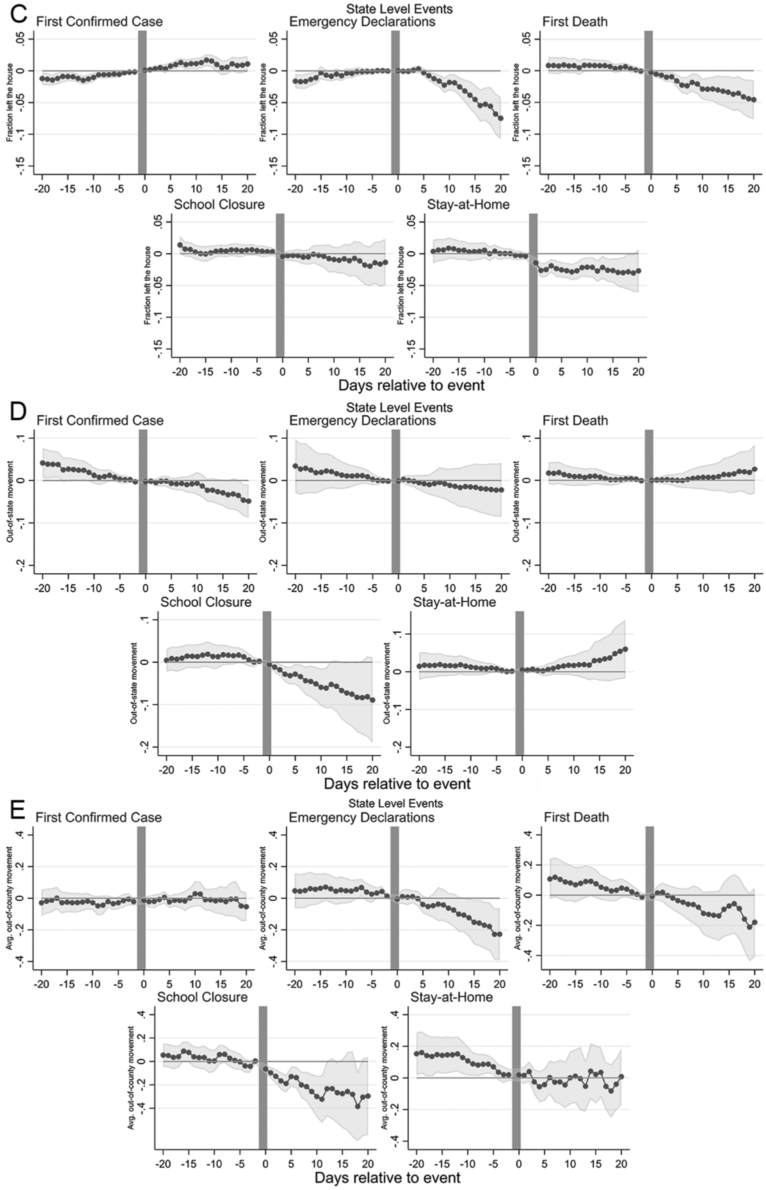

FIGURE 6. *Continued*. C: Effects on fraction leaving the house. Regression results (coefficients and 95 percent confidence intervals). Baseline dependent variable mean = 0.69, SD = 0.03. The dependent variable shows the fraction of cell phones detected out of the home at some point during the day, as a share of all devices that day. Full event study tables are available on request, and effect sizes are summarized in Online Appendix Table A5. Source: SafeGraph aggregated mobility metrics. D: Effects on out-of-state movement. Regression results (coefficients

thus they are not expected to reflect changes immediately. However, by 20 days post-policy, we should be able to detect substantial impacts. The five panels in Figure 6D 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, showing a 7 percent decrease 20 days post-policy. Emergency declarations have a similar-sized coefficient but are not statistically significant. School closures show an effect that is statistically significant at the 10 percent level 20 days post-policy.

Figure 6E shows our last cell-signal-based measure of mobility in an index showing the extent to which people in a state traveled out of their home county during the previous 14 days.[9] The event study results do not suggest much evidence that mobility patterns were trending differentially in the lead-up to state policy changes. The results 20 days post-policy are statistically significant for emergency declarations and school closures (at the 10 percent level), which suggest that out-of-county travel declined 8–10 percent. Although the other policies do not show statistically significant effects, the coefficients are consistent with effect sizes in the 0–11 percent range, with the smallest being for SAH policies.

Although PlaceIQ and SafeGraph data represent our main mobility sources, we also investigated the effect of state policy and events on mobility indices from Apple and Google, described in the data section. These measures do not contain technical appendices from which we know the number of devices that contribute the data, or exactly how the indices are calculated, although we expect them to be high quality. In Online Appendix Figures A3 and A5, we find evidence reinforcing the results seen above. For example, in the Apple Mobility indices (which are relevant only for large cities, and capture travel directions requests of all types—for driving, walking, and transit), we see pronounced declines, but only from state emergency declarations (Online Appendix Figure A5). In the Google Mobility data (Online Appendix Figures A3 and A4), we see some new information not apparent in our

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and 95 percent confidence intervals). Baseline dependent variable mean = 0.66, SD = 0.28. The dependent variable shows sum of the percentage of cell phones detected out of state in the last 14 days, which is thus an index for out-of-state travel. Standard errors are clustered at the state level. Full event study tables are available on request, and effect sizes are summarized in Online Appendix Table A5. Source: PlaceIQ geolocation data. E: Effects on average out-of-county movement. Regression results (coefficients and 95 percent confidence intervals). Baseline dependent variable mean = 3.03, SD = 0.70. The dependent variable shows state-level average of the sum of the fraction of cell phones detected out of the home county in the last 14 days (thus, an index for out-of-county travel), population-weighted averaged from counties to the state level. Standard errors are clustered at the state level. Full event study tables are available on request, and effect sizes are summarized in Online Appendix Table A5. Source: PlaceIQ geolocation data. A color version of this figure is available online.

9 We compute state-level averages of the county-level mobility rates, weighting by county population. The county mobility rate is an index of how much people in that county have traveled outside the county.

other measures of mobility: we see evidence of sharp declines (after an increase the day before it) in groceries and pharmacy mobility (Online Appendix Figure A3), and also for retail and recreation (not shown), following SAH orders. No other policy has statistically significant effects and parallel trends in those two figures. There are no causal effects for parks and beaches or for transit stations, but changes in workplace mobility also show some declines after SAH and somewhat also after first deaths; these two effects are also reflected in increased presence at home. At the county level (which is available only in Google Mobility), we see evidence of decreases in mobility for SAH and for first cases (also not shown). Parallel trends violations apply to all other outcomes at the county level.

C. HOW MUCH DOES POLICY MATTER?

The state-level event study analysis suggests that emergency declarations led to substantial increases in time spent at home, reductions in the mixing index, reductions in measures of leaving home, and small reductions in out-of-state and out-of-county travel. The incremental effects of the emergency declarations were typically small initially, but event study coefficients are consistent with these effects growing substantially over time.

Emergency declarations do not directly mandate changes in social distancing, but they likely influence it through two channels. First, they are an information policy instrument that state governments may use to signal the seriousness of the situation to the population. Second, the emergency declarations were typically an opening salvo in a sequence of state policies that played out similarly across states, as indicated in the timeline that we present in Figure 2A on the typical sequence of policy actions.

Our event study analysis provides estimates of the period-by-period incremental effect of each policy measure, but it does not provide a clear assessment of how much state policies have altered the trajectory of social distancing across the country. Here, we use the estimated coefficients from the event study regressions to construct counterfactual predictions of the time trends that would have prevailed if states had not issued emergency declarations.

To understand the counterfactual exercise we conduct, consider first the basic event study regression model:

$$y_{st} = \sum_{a=-21}^{2} \alpha_a 1(TSE_{st} = -a) + \sum_{b=0}^{21} \beta_b 1(TSE_{st} = b) + \theta_s + \gamma_t + \varepsilon_{st}.$$

Let $\hat{y}_{st}$ be the fitted value for state s on date t from the estimated event study regression. These fitted values are a model-based estimate of what actually happened in the state. That is, the fitted value includes the event time-specific impact of the emergency declaration policy in state s if state s had adopted such a policy as of date t. Next, let $y^*_{st} = \widehat{y_{st}} - \Sigma_{b=0}^{21} \hat{\beta}_b 1(TSE_{st} = b)$ be the estimated counterfactual outcome in state s on date t. The counterfactual outcome is simply the realized fitted value net of the state's policy effects.

We collapsed the state-by-day fitted values and counterfactual estimates by day to form a time series of cross-state national averages. Online Appendix Figure A7 plots these two time series. The solid orange line in the graph shows the realized time trend, which is inclusive of policy effects as they occur across states and over time. The dashed blue line shows the counterfactual line in which the effects of the emergency declarations have been removed. The two lines are identical until early March, when states being making emergency declarations. The lines rise in lockstep during the early part of March, suggesting that this initial change is driven primarily by secular trends that would have happened in the absence of state policy announcements. But the lines diverge in the later part of the March, and the counterfactual line suggests that hours spent at home would have been substantially lower by early April if states had not declared emergencies and begun to take action.

To quantify the relative importance of secular trends versus emergency declaration policies, we computed the average of fitted values and counterfactuals for two one-week periods: a starting week in early March (Wednesday, February 26, 2020, to Tuesday, March 3, 2020) and an ending week in the second week of April (Wednesday, April 8, 2020, to Tuesday, April 14, 2020). These weeks and the averages of the time at home outcomes are indicated in Online Appendix Figure A7. In the first week of March, the cross-state national average time spent at home was about 9.1 hours in both the realized and the counterfactual time series.

By the second week of April, realized time at home had grown to 13.9 hours, which is a 53 percent increase over the baseline. However, the counterfactual estimates imply that without the boost in social distancing induced by the emergency declarations, time at home would only have grown to 11.3 hours. The residual-from-policy secular trends in time at home explain about $(11.3 - 9.1)/(13.9 - 9.1) \times 100 = 45$ percent of the total realized growth in time at home. Thus, the event studies imply that emergency declarations explain about 55 percent of the total growth in time at home that occurred across the state over the month of March.

D. COUNTY-LEVEL EVENT STUDIES

Next, we consider responses to county-level policies and information effects, where we examine variation only from county policies that went beyond their state's policies. We ensure this by including state-by-day fixed effects in addition to event study specifications of the county policy. Since there were so few emergency declarations that were only at a substate level (Figure 3A), we do not examine that policy in the county context, even though that is the most consistently with statistically significant effects at the state level. We examine four measures of mobility; we do not examine whether county policy affects interstate travel, the first outcome of the earlier set results. Figure 7A examines the effects of county policy and information events on the index for society-wide "mixing," finding that there are very substantial effects. These are the largest effect sizes found in our analysis. The announcement of the first case in a county is linked with a 39 percent decline, 20 days out. School closures reduce mixing by 40 percent after 20 days. There are statistically significant coefficients from SAH laws but parallel trends violations prevent assigning a causal interpretation to these results. There are also parallel trend violations in the first death outcome. These results suggest that county-level policies have been highly effective in reducing social mixing, and further research should explore the possible reasons that local governments have bigger effects on behavior than similar policies adopted at the state level.

In Figure 7B we find that effects of county policies are much smaller in effect size for time spent at home. There are three statistically significant effects on time spent at home,

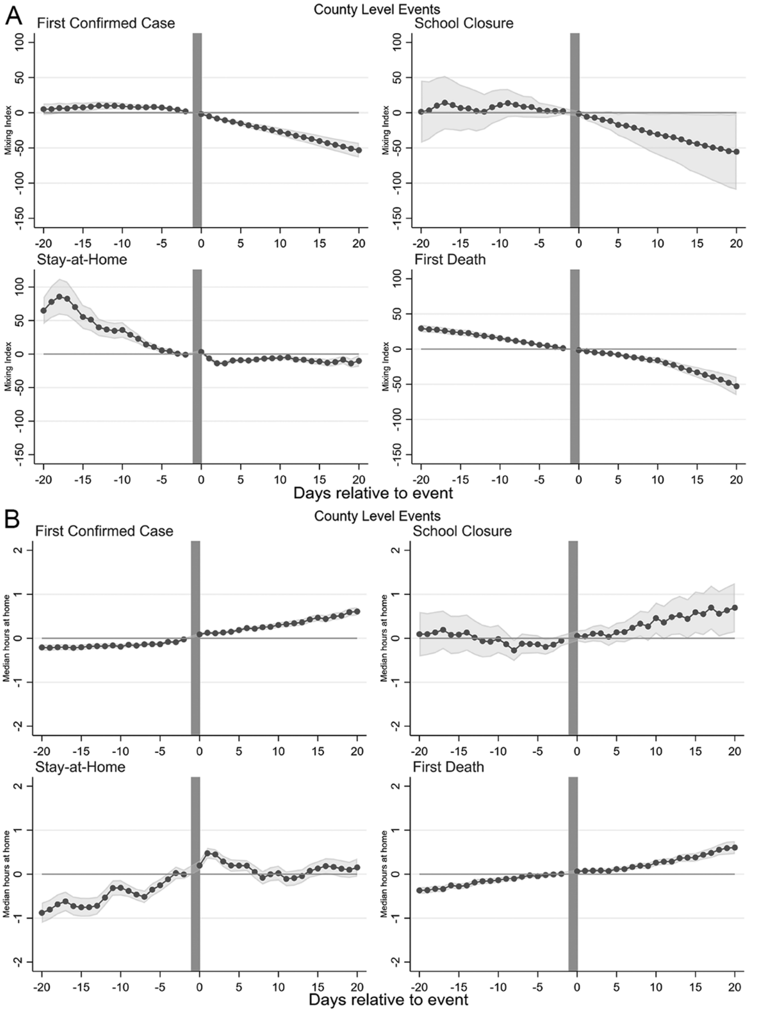

FIGURE 7. Effects of mitigation policies and information events. A: Effects on mixing index. Regression results (coefficients and 95 percent confidence intervals). Baseline dependent variable mean = 137.67, SD = 80.14. The dependent variable shows the county's index for mixing (average amount of mixing within its census block groups). Standard errors are clustered at the county level. Full event study tables are available on request, and effect sizes are summarized in Online Appendix Table A5. Source: PlaceIQ geolocation data. B: Effects on median hours at home. Regression results (coefficients and 95 percent confidence intervals). Baseline dependent variable mean = 10.10, SD = 1.74. The dependent variable shows the average of mean time at home, in that county. Standard errors are clustered at the county level. Full event study tables are available on request, and effect sizes are summarized in Online Appendix Table A5. Source: SafeGraph aggregated mobility metrics. A color version of this figure is available online.

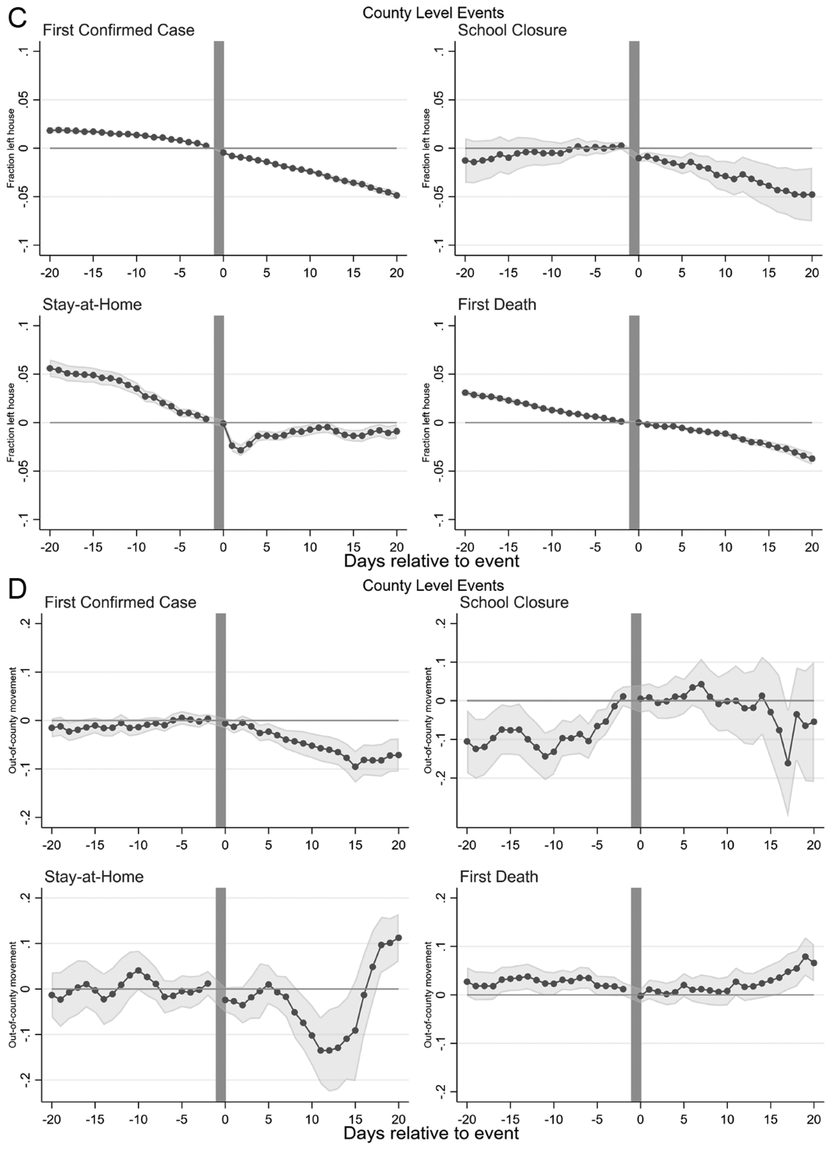

FIGURE 7. *Continued*. C: Effects on fraction leaving house. Regression results (coefficients and 95 percent confidence intervals). Baseline dependent variable mean = 0.70, SD = 0.06. The dependent variable shows the fraction of cell phones detected out of the home at some point during the day, as a share of devices that day, in that county. Standard errors are clustered at the county level. Full event study tables are available on request, and effect sizes are summarized in Online Appendix Table A5. Source: SafeGraph aggregated mobility metrics. D: Effects on out-of-state movement. Regression results (coefficients and 95 percent

but they are small: 6 to 7 percent 20 days post-policy, for first cases, school closures, and first deaths; SAH policies have a statistically insignificant 2 percent coefficient. Figure 7C shows event study estimates of the effects of the county policy and information events on measures of travel outside of the home. There is more evidence of differential pre-trends in these data, suggesting that people were already staying home more even before the key county-level information and policy events. There is only one statistically significant result that does not violate parallel trends assumptions: school closures reduced the fraction leaving home by about 7 percent.

Finally, Figure 7D 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 (2 percent 20 days after the event). There is evidence of pre-trends in out-of-county movement for school closures, suggesting that people had started responding to other signals (such as confirmed cases in the state) that safety was an issue, and that school closures may also have been in response to these concerns.

E. SUMMARY OF RESULTS AND SENSITIVITY ANALYSIS

E.1. SUMMARY OF EVENT STUDIES. Online Appendix Table A5 gives a more digestible summary of the results of the event study regressions for each outcome and policy/information event. The table has a row for each state and county outcome variable, and a column for each policy/information event. The top panel shows the effect size five days after the event, expressed as a percentage of the average value of the outcome variable on March 1, 2020. The bottom panel shows the effect size after 20 days, also expressed as a percentage of the average outcome on March 1. We bold and indicate with ** the effects that are statistically significant at the 5 percent level or better and where parallel trends hold, and * and bold for significant ones at the 10 percent level. The cells that are shaded in gray have possible violations of the differential pre-trends assumption and should be largely overlooked (we do not indicate statistical significance for them).

E.2. CONFOUNDING FROM OTHER POLICIES. Our main event study specifications examine the effects of individual policy/informational events on social distancing measures in a one-at-a-time fashion. We selected the policies we follow by examining timing plots such as Figures 2 and 3, indicating that our policies looked fairly spaced within state. But, as noted in Figure 2B, in most states the first COVID-19 case and/or emergency declarations were quickly succeeded by restrictions on social gatherings, school and restaurant closures, and finally stay-at-home (SAH) orders. One view is that the early policy events — such as emergency declarations—can be viewed as a reduced-form summary of the

confidence intervals). Baseline dependent variable mean = 3.36, SD = 1.02. The dependent variable shows sum of the percentage of cell phones detected in a different county in the last 14 days, which is an index for out-of-county travel. Standard errors are clustered at the county level. Full event study tables are available on request, and effect sizes are summarized in Online Appendix Table A5. Source: PlaceIQ geolocation data. A color version of this figure is available online.

entire collection of state policies that appears over time. This is an explanation for why the emergency declaration effects grow so much in the 20 days after they are announced. The later event time coefficients may be picking up the effects of subsequent policies. There is also a case that we should control for other state-level regulatory/informational events in order to better isolate the effects of specific public actions. We examine whether the estimated impact of the individual events on social distancing is sensitive to controlling for other policies in two ways: event studies with binary controls for other policies, and models that include linearized event studies for all policies in a single model.

First, we reestimate each of our event study specifications with additional controls for other social-distance-inducing policy/information events in effect surrounding the focal event. We examined the event study coefficients on the focal policy for each policy/information event after inclusion of controls for other social-distancing-related events. The key results hold up reasonably well. For example, we found that state emergency declarations led to pronounced declines in the mixing index even after controlling for other ongoing events, with statistically significant (≈14 percent) declines five to seven days after the event (Online Appendix Table E1). Similarly, emergency declarations significantly increased the median hours at home by 6 percent seven days after and by 21 percent 20 days after adoption (Online Appendix Table E2). In this case, concurrent SAH orders also appear to be associated with more time at home. In unreported estimates (available upon request), first confirmed COVID-19 case, emergency declarations, SAH laws, and reports of first death led to statistically significant declines of ≈3 percent in the fraction of individuals leaving their homes five days after. Again, treatment effects increased over time; we observed a 7–9 percent decline in the fraction of the devices leaving the home 20 days posttreatment, and estimates were statistically significant only for emergency declarations and first deaths. Finally, state school closures significantly reduced out-of-state (7 percent reduction after five days and 15 percent reduction after 20 days) and out-of-county travel (6–9 percent reduction), even with other policies in the model. We also estimated county event study models with controls for all the separate policy/informational events. We find these estimates to be generally noisy with significant pre-policy trends. We also find evidence of county school closures reducing the fraction of devices leaving home, and information of first confirmed case in county reducing out-of-county movement.

Ideally, we would fit event study regressions that include event study indicators for each of the policies and events of interest at once. The models are too imprecisely estimated to make this approach feasible. To make progress, we fit linearized versions of the event studies. Specifically, let TSE_{st}^{j} be the event time variable for policy/information event j for $\in \{FC, ED, SC, SAH, FD\}$. $Post_{st}^{j} = 1(TSE_{st}^{j} \geq 0)$ is a dummy variable set to 1 if the jth policy/information event has actually occurred in the state.

The linearized event study model is the following:

$$Y_{st} = \sum_{j} \left[\alpha_j TSE_{st}^{j} + \beta_j Post_{st}^{j} + \delta_j \left(TSE_{st}^{j} \times Post_{st}^{j} \right) \right] + \theta_s + \gamma_t + \varepsilon_{st}.$$

In the model, the α_j capture linear differential pre-trends associated with each policy. The β_j represent the immediate effects of each policy, and the δ_j measure the evolution of the policy effect over time. For example, the effect of the emergency declaration policy after 20 periods would be $\beta_{ED} + \delta_{ED} \times 20$. The county-level specification is similar, but it includes a full set of state-by-time fixed effects. Estimates from linearized event study specifications are presented in Online Appendix Tables A6 (state-level events) and A7 (county-level events).

The linearized models allow us to examine the event studies for multiple policies in a single model at the expense of a more restrictive functional form. The qualitative results hold up well in these specifications. Online Appendix Table A6 shows that the estimated coefficients on the linearized event time main effect is almost always statistically insignificant for all events, reiterating the absence of significant pre-trends in our state-level social distancing outcomes. The linearized models imply that there is an immediate 2 percent increase in the median hours spent at home and 3 percent decline in the fraction of devices that left home following state SAH laws. Following the immediate increase in social distancing following SAH laws, there is no further increase in social distancing due to these policies over time. In contrast, we find a significant 1 percent increase in the fraction of devices leaving home following state emergency declarations. However, this increase is not sustained over time. The event time by post interaction term implies that the emergency declarations are associated with growing median hours spent at home and declines in the fraction of devices leaving the home. Finally, the county-level estimates are again noisy with significant pre-trends in outcomes. We note significant declines following the county-level events with effects growing over time.

E.3. OTHER SENSITIVITY CHECKS. We estimated alternative models that treated states with nonmandatory but strong SAH orders as equivalent to mandatory ones, but did not find our main results changed. We also consider whether there would be a different response to the policy issue date as opposed to the enactment date (our base specification). We reestimated 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 SAH policies, half of the states announced and implemented the policy on the same day. The other half had a gap of between one and three days.

Another sensitivity check estimates event studies using the sample of treated states only. These analyses rely on variation in timing of treatment only, and not on whether the state implemented treatment. Event study plots presented in Online Appendix B and the top panel of Online Appendix Table A8 for our five mobility measures show that our results are again robust to this alternative specification. This is again expected since there are few "nontreated" states. The largest incidence of nontreatment is for SAH orders, which were not implemented by seven states (Arkansas, Iowa, North Dakota, Nebraska, Oklahoma, South Dakota, and Wyoming; refer to Online Appendix Table A1). Although this sensitivity check confirms that our results are not driven by differences between treated and nontreated states but identified from the specific timing of treatment, recent literature on event study and difference-in-difference research design cautions against removing never-adopting groups and relying exclusively on differential timing of adoption of treatment among adopters

(Goodman-Bacon 2018). For this reason, the results in the main text remain our preferred specification.

Our analysis considers the effects of the informational events and policies on mobility up until 20 days after. Ideally, we would like to consider longer-term effects as well. However, longer post-periods are problematic for the late policies like school closures and SAH orders. We observe at maximum 27 days after state SAH orders and 29 days after state school closures. In additional tests we reestimate event studies with up to 30 days of pre-period and post-period (event study coefficient plots presented in Online Appendix C) and from −40 days pre-period and up to 30 days post-period (event study coefficient plots presented in Online Appendix F). Implied effect sizes 26 days after from the former are presented in the second panel of Online Appendix Table A8, and support our finding that pre-trends are generally stable and effect sizes of policy and informational events continue to grow over time. However, our main specification with a 20-day post-period is preferred over the slightly longer post-period since the later event times are observed only for the early adopting states and these coefficients may suffer from compositional issues.

In a final sensitivity check we reestimate the county analysis, without state-by-date fixed effects to focus on the identifying variation. The results of this sensitivity test are presented in Online Appendix D and summarized in the last panel of Online Appendix Table A8. There are more parallel trend violations without state-by-date fixed effects. However, magnitude effect sizes and significance are comparable (though not causally interpretable once pre-trends present).

VII. Discussion

By early April 2020, the United States experienced more confirmed COVID-19 cases and deaths than any other nation. Public and private actors have taken drastic steps to limit the spread of the virus through social distancing. This paper examines the effects of public policy, information events, and voluntary measures on proxies for social distancing during the initial stage of the epidemic in the United States. We classify state and local government actions and document their order and timing. We use event study regressions to assess their effects on multiple near-real-time measures of mobility from commercial smart-device data bases. 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, but the private responses of individuals will likely be insufficient to account for externalities and are unlikely to contain the epidemic. Thus, government policies to increase social distancing play an important role in theory. The optimal way for governments to encourage additional social distancing is not well understood. Economists often favor Pigovian taxes and subsidies as a way to help the market internalize negative and positive externalities, but legalistic approaches like bans, quotas, and mandates often play a role in practice. During the early months of 2020, state and local governments have embraced this role of social distancing supporter to varying degrees and have adopted a set of policies that they hope will increase the amount of mitigating behavior beyond the levels that would arise from private responses alone. Most of the policies that state and local governments have pursued so far emphasize non-Pigovian solutions, such as issuing guidance and safety information, closing various businesses and schools, banning group events, and issuing stay-at-home (SAH) orders.

The federal government has also made important attempts in promoting social distancing. So far, it has used instruments that could be viewed as "more Pigovian." The federal government has moved, for example, to subsidize social distancing by offering enhanced unemployment benefits and cash transfers that should make it easier for people to remain away from workplaces and unemployed during the crisis. These policies can partly be viewed as consumption smoothing and poverty mitigation, but they may also subsidize the positive externality people generate by staying home and compensate people for the sacrifice that staying home currently represents. Although federal efforts may become important over time, our focus in this paper is on state and local policy and news events. We used smart-device cell signal data as proxy measures of social distancing behavior, and we used event study regressions to identify the incremental change in mobility that is attributable to specific government actions. The estimates that 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.

We find large declines in mobility in all states since the start of the epidemic, even ones without major mitigation mandates. This indicates that a substantial share of the fall in mobility was induced prior to strong mandates, such as SAH orders, and event study regressions also support these findings. All policies likely carry informational content, while some are solely information and do not impose costs of moving around in society. Informational or partial closure policies that occurred early in the epidemic appear to have had an important influence on mobility. Early county actions often had as much impact as state ones. Across multiple measures, the event studies show that mobility fell after first confirmed case announcements, emergency declarations, and school closures. In most cases, the initial response to the event is only about 1–5 percent, but the effects grow to 7–39 percent after 20 days.

These early events and policies may have conveyed information about the seriousness of the epidemic and act as a summary of the downstream sequence of government policy. The early effects add up over time. For example, across states, average time spent at home was about 9.1 hours during the first week of March, but grew to 13.9 hours by the second week of April. In the absence state emergency declarations, event study results imply that hours at home have grown only to about 11.3 hours. This suggests non-policy-induced trends explain about 45 percent of the growth in time at home during the month of March, while the policy explains 55 percent of the growth. Overall, our results suggest that state and local government policy and information events induced changes in mobility on top of what appears to be a large response across all states to the prevailing knowledge and events at both national and international levels.

The results of the paper can be considered in the context of several subsequent studies that have now examined the connection between state social distancing policies and measures of human movement. Ours is the first comprehensive assessment to have found that a large amount of the mobility reduction occurred prior to specific state policy actions. It is also the first to report estimates of the quantitative magnitude of the effects of multiple social distancing policies on diverse measures of mobility, from multiple data sources. Since our initial working paper was released, other studies have extended our work and have reached broadly similar conclusions. They find that large declines in mobility occurred before states adopted more stringent policies (Cronin and Evans 2020; Goolsbee and Syverson 2020; Cicala et al. 2020; Chen et al. 2020; Alexander and Karger 2020). Even in studies that point out that policy effects appear large in absolute terms, the effects are relatively small relative to the overall scale of the decline in mobility that occurred in the early months of the epidemic. Most of the studies use SafeGraph as their sole source of data (Goolsbee and Syverson 2020; Cronin and Evans 2020; Painter and Qiu 2020; Andersen 2020). Other studies use Unacast (Cicala et al. 2020; Alexander and Karger 2020), Veraset (Chen et al. 2020), or Google Mobility (Abouk and Heydari 2020). Because each cell signal aggregator company has a potentially different set of apps from which they draw their device data, we continue to think that it is important to look at results across multiple data systems.

We should bear these results on the role of information events and seemingly voluntary responses 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 SAH orders and reopening schools may have differential effects on overall social activity depending on the corresponding change in national or global actions and prevailing attitudes (Cornwall 2020). In other words, it is possible that the effects of government mitigation policies will have asymmetric effects. When they commence, the policies may have a relatively small impact that largely reinforces private actions. Lifting a policy, on the other hand, if perceived as a signal that the level of danger has fallen, may cause different results. In this case, lifting a ban could have large impacts even if applying the ban had minor impacts. These questions are important, but the analysis in this paper does not provide clear answers about the likely consequences of reopening, thus it is important to continue monitoring real-time mobility data.

While we show that several policy changes are relatively exogenous to the outcomes that 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; we do not focus on this angle in our paper. 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 with the analysis of earlier epidemics? Our work contributes to existing research on the effectiveness of government policies on mobility during epidemics, although much of the existing work involves the 1918 epidemic, which differed in many ways from the current crisis. For one, the 1918 epidemic affected the young more than the current epidemic does. If the young are more mobile and consider health threats to be less severe, policies may face more resistance in attempts to reduce mobility. Indeed, we find that the age distribution in a county is correlated with mobility reduction during March 2020.

What should society be aiming for as the optimal amount of social distance during an epidemic, balancing costs and benefits? In this paper, social distancing policies are judged only on the extent to which they reduced mobility. We do not examine their normative implications. Even during an epidemic, the optimal amount of physical contact between people is not likely to be zero. Some mobility needs are necessary, and our measures of mobility do not distinguish between "justifiable" and "unjustifiable" mobility. It is also true that people can take steps to minimize the harm of their mobility and interaction, such as by keeping distance between people, wearing a mask, and interacting outdoors. Our data do not capture these kinds of mitigation strategies. Our mobility measures likely capture mobility from essential workers, emergency events, and mobility aided by masks. In future work we will attempt to control for the different occupational and industry distributions across geographical areas to try to understand more about the patterns in the data.

There is also an economic trade-off implicitly made between lives saved and economic decline, which Friedson et al. (2020) discuss with information on mortality versus jobs, and which is built into unemployment benefits and other payments being directed at people whose jobs are lost in an attempt to increase social distance. Barro and Weng (2020) use data from the 1918–20 flu deaths to predict that GDP and consumption could decline 6 and 8 percent, respectively, from the current COVID-19 crisis. Most of that fall in output is attributable to the health shock of the epidemic and is probably not driven by the incremental costs of the policies used to curtail the epidemic. Nevertheless, it surely makes sense to consider the most efficient ways to increase social distance while causing the least economic harm.

Several caveats should be kept in mind when considering our analysis. First, the device signal data that we used are as yet new to the literature. We carried out some data validation checks and did not find major problems, but it is possible that as more data continue to be released, this literature will discern pros and cons to different sources of mobility data. 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 to collect information on the timing and location of different policy events. We focus our attention on creating a typology for grouping the key policies and on estimating their impacts on mobility. Our estimates are best interpreted as an average effect across different states/counties and time periods. The average may mask substantial heterogeneity across states and counties. We hope to study that heterogeneity in future work.

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, the data sets remain "convenience samples" that are not derived from a well-defined sampling. Fourth, our analysis of population health metrics related to the epidemic should be treated with caution. In particular, the number of confirmed cases may be a poor measure of the spread of the virus because the case counts are partly a function of the testing environment. Data on deaths may provide a better—albeit lagging—measure of the severity of local epidemics. But even these data are likely underreported. In terms of event studies and quasi-experiments, it could be that states adopt distancing policies and alter their testing effort and capacity at the same time. If that is the case, the observed reductions in positive cases may be understated.

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 research.

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