Measuring voluntary and policy-induced social distancing behavior during the COVID-19 pandemic

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Staying home and avoiding unnecessary contact is an important part of the effort to contain COVID-19 and limit deaths. Every state in the United States enacted policies to encourage distancing and some mandated staying home. Understanding how these policies interact with individuals' voluntary responses to the COVID-19 epidemic is a critical initial step in understanding the role of these nonpharmaceutical interventions in transmission dynamics and assessing policy impacts. We use variation in policy responses along with smart device data that measures the amount of time Americans stayed home to disentangle the extent that observed shifts in staying home behavior are induced by policy. We find evidence that stay-at-home orders and voluntary response to locally reported COVID-19 cases and deaths led to behavioral change. For the median county, which implemented a stay-at-home order with about two cases, we find that the response to stay-at-home orders increased time at home as if the county had experienced 29 additional local cases. However, the relative effect of stay-at-home orders was much greater in select counties. On the one hand, the mandate can be viewed as displacing a voluntary response to this rise in cases. On the other hand, policy accelerated the response, which likely helped reduce spread in the early phase of the pandemic. It is important to be able to attribute the relative role of self-interested behavior or policy mandates to understand the limits and opportunities for relying on voluntary behavior as opposed to imposing stay-at-home orders.

COVID-19 | stay-at-home order | avoidance behavior | nonpharmaceutical interventions | social distancing

Worldwide, people stayed home to reduce transmission of severe acute respiratory syndrome coronavirus 2 (SARS-CoV-2), the virus causing the COVID-19 pandemic. This behavioral shift helped prevent the COVID-19 pandemic from being worse. How much of the staying home response was driven by individuals acting in their own interests in response to health risks, and how much was the result of policy mandates or orders? The need for evidence to resolve this question is characterized by two statements from US governors' offices. Governor Burgum of North Dakota stated (1), "We believe in the power of individual responsibility. And we need individual responsibility now more than ever to slow the spread of COVID-19," whereas, following new stay-at-home orders, the office of Governor Brown of Oregon said (2), "If people aren't going to take this virus seriously, we are prepared to offer consequences…[and] hold people accountable in making smart choices that can save another's life." Mandates can accelerate or strengthen the public response. This is a necessary, but not sufficient, condition for mandated nonpharmaceutical interventions to reduce SARS-CoV-2 transmission. For such transmission reductions to reduce deaths, the mandates must not compromise health services (3). However, mandates are also politically costly, difficult to sustain, and difficult to enforce. There have been over 1,000 lawsuits filed in the United States over COVID-19 public measures (4), and public health resources may need to be diverted to defend against these lawsuits. COVID-19 vaccines do not nullify the importance of relying on voluntary behavior or mandates. Vaccines could still take a substantial amount of time to distribute, and there are a large number of pathogens similar to SARS-CoV-2 that could cause another pandemic (5).

Theory suggests that people alter behavior voluntarily to avoid becoming sick, including staying home (6–10). Evidence from the 2009 H1N1 swine flu pandemic (11–13), Lyme disease (14), the 2003 SARS epidemic (15, 16), and HIV (17, 18) support the theory. The emerging evidence from COVID-19 (19–21) also supports the theory that people alter behavior in response to infectious disease risk. Localized shutdowns in Mexico City during the H1N1 pandemic encouraged people to stay in, but the response was short lived (22).

The 2020 COVID-19 pandemic is one of the first opportunities to investigate how public health mandates in the form of nonpharmaceutical interventions interact with voluntary behavioral shifts during epidemics. All 50 US states and the District of Columbia issued emergency orders. A total of 39 states and the District of Columbia issued stay-at-home or shelter-in-place orders, which are two names for the same thing (hereafter stay-at-home). However, there is substantial heterogeneity in the timing of local cases and stay-at-home orders (Fig. 1). Most counties were under emergency orders (99.4%) and closed schools (98%) prior to experiencing a single COVID-19 case (*SI Appendix*, Fig. S1). Recent studies (23–25) have attempted to evaluate the effectiveness of

Significance

Early in the US COVID-19 epidemic, Americans spent substantially more time at home to reduce cases. Disentangling voluntary from policy-induced behavioral changes is critical for governments grappling with relaxing or renewing restrictions. We estimate the number of additional reported cases that would have been needed to elicit a voluntary behavioral response equivalent to the behavioral response to policy. A substantial share of the observed behavioral response was voluntary. Stay-at-home orders increased the time people spent at home by replacing voluntary actions that likely would have emerged as cases rose. Our analysis is an initial step in answering the critical policy question as to whether fast forwarding the response provides sufficient public health benefits to justify the mandates.

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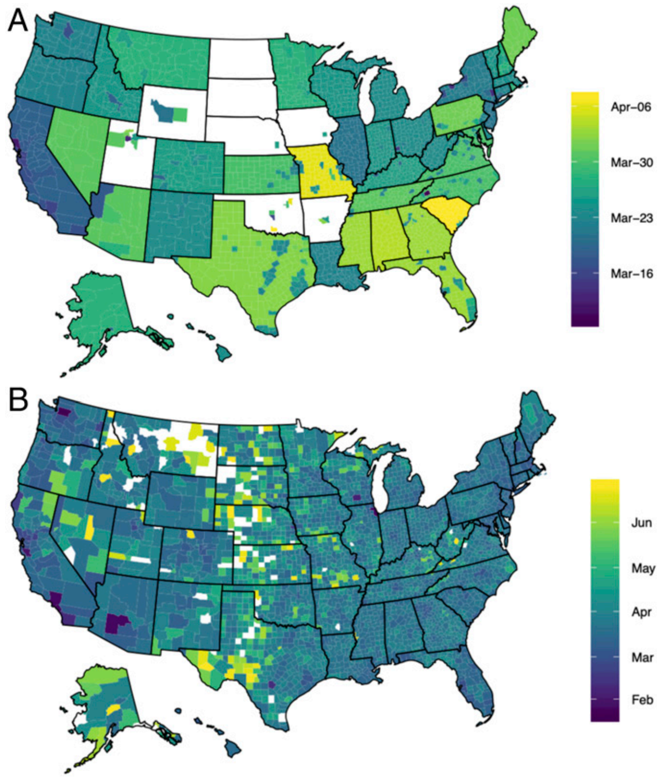

Fig. 1. Date that counties enacted stay-at-home order (*A*) and date of the first case reported within a county (*B*).

restrictions that ordered people to stay in on slowing the spread of SARS-CoV-2. Still, these studies assume no voluntary change in behavior as the alternative scenario to restrictions. Yet voluntary behavior is a fundamental part of the transmission process. If the true data generating process for an epidemic involves the theoretically predicted voluntary behavioral avoidance response, then an empirical model that assumes away that behavioral response can fit the epidemiological data as well as a model that specifies the true data generating process (6). The problem is that the misspecified model introduces many opportunities for confounding processes in epidemiology (26), and a misspecified model cannot provide information about behavioral response, whether voluntary or because of public health mandates. Therefore, we focus on the first step in a potential causal chain.

It is important to understand how self-interest and policy mandates interact. The interactions between voluntarily staying home and policy-induced staying home can be viewed through two lenses. On the one hand, the policy can overlap or displace voluntary behavior that would have happened anyway. If the policy mandate overlaps or displaces voluntary contributions of behavioral change and leads to a similar outcome, then mandates achieve the outcome at a greater cost. Ostrom (27) expands on the costs of displacing or crowding out voluntary behavior, writing, "external interventions crowd out intrinsic motivation if the individuals affected perceive them [the policies] to be controlling." She argues that many policies adopted in modern democracies presume authorities must solve all collective action problems, thereby crowding out citizenship, wasting resources, and undermining democracy. Stay-at-home mandates likely fit this characterization. Empirical evidence suggests that as mandatory involuntary contributions increase, voluntary contributions are increasingly crowded out, even when there is a private benefit to the contribution (28).

Conversely, a mandate could achieve a stronger or faster response, arriving at the "full" response faster. Speed is valuable during an epidemic, even if the final response is similar in magnitude. Moreover, a stronger or faster response may be necessary to avoid exceeding healthcare capacity, which could lead to higher morbidity and mortality rates for the same number of cases. The response induced by a mandate may be faster or stronger because people may act to protect themselves voluntarily but fail to internalize the fact that if they become infected, then they can infect others (29).

The metric that provides a common fact to weigh these two perspectives is the case equivalent response—how many more cases would have been needed to elicit the same behavioral response as the mandate. This simultaneously evaluates whether the mandate displaces or overlaps voluntary behavior and measures the speeding-up effect. If the case equivalent response is of similar size as reported cases up to likely measurement error at the time the stay-at-home mandate begins, then we consider the effect epidemiologically small. Conversely, if the case equivalent response is many orders of magnitude greater than current cases and measurement error, then the stay-at-home mandate likely has a large behavioral effect, which may translate into reduced cases and reduced deaths, all else equal.

To measure the case equivalent response, we focus on the number of minutes per day that people spend at home, measured using smart device location data. For well over a decade, epidemiologists have used surveys of contact behavior to parameterize epidemiological models (30–34). Bayham et al. (13) refined earlier work (35), inferring likely contact patterns from the American Time Use Survey (ATUS). They showed that time-use data, based on the ATUS, produced similar contact patterns to those based on the detailed surveys epidemiologist relied on. Bayham et al. also showed that in the case of H1N1, conditioning cases on time spent at home gave a similar reduction in cases as an epidemiological model using the fully specified contact structure. Hence, we use the smart device data to measure the time spent at home as a measure of avoidance behavior. We confirm the primary results with other measures of time use.

We define voluntary response as an increase in minutes at home as a function of reported cases within the county, after controlling for mandates, which provides a relatively local measure of risk. We also consider reports of national and state cases. We focus on three policy mandates that led to "involuntary" behavioral responses. First, we consider stay-at-home orders, which we combine with shelter-in-place orders, colloquially called "lockdowns." While people were ordered to stay home, exceptions were made for vaguely defined essential activities, and these orders were seldom enforced by police. Therefore, we put "involuntary" in quotes—people could ignore the orders. Nevertheless, the intent of the orders was to keep people home involuntarily. Second, we consider school closures that induced parents to stay home from work to care for children. Third, we consider emergency orders that raised awareness and may have led businesses to close or encourage working from home but did not provide direct public mandates. In *SI Appendix*, we repeat the analysis using reported deaths instead of, and along with, cases. We acknowledge that the voluntary aspect of behavior is hard to define. Closures likely increased the salience of concern for COVID-19, making "voluntary" difficult to define in the COVID-19 upheaval, which is why we focus on the early phase of the epidemic in the United States. However, the salience of other indirect policy impacts may have been achievable through more targeted policies.

Here, we use the variation in policy responses along with smart device data to measure the amount of time Americans stayed home and adjusted other behaviors in response to pathogen risk and stay-at-home orders. We contribute to the body of evidence that finds strong voluntary avoidance behavior. We also contribute to the body of evidence that finds a strong effect of mandates. We fill a gap in the literature by connecting voluntary response and policy-induced responses within a single empirical framework. We then compute the case equivalent response of the stay-at-home mandate. Imposing a stay-at-home mandate fast forwards or displaces the voluntary staying home. We find that most counties imposed stay-at-home orders with few cases; these stay-at-home orders induced a time at home equivalent to tens of additional cases, but that most counties would have achieved a similar amount of time allocated to home if cases rose as they did in areas without the stay-at-home orders. However, for some counties stay-at-home orders altered behavior in a manner equivalent to thousands of cases, suggesting the need for policy to adapt to local conditions.

Results

We use SafeGraph smart device median home dwell time data reported at the Census block group level and averaged to the county level for 3,104 counties in the United States and Washington, DC, between January 31 and April 21, 2020. This covers 2 wk before the earliest emergency declaration orders and 2 wk after the last stay-at-home order during the first phase of the US epidemic (Fig. 2*A*). The daily mean of the median time spent at home on January 31 across all counties was 576.8 min (SD 86.8). Time at home gradually declined from January 31, reaching a minimum of 411.8 min (SD 99.5) on February 25. Time spent at home then increased, peaking on April 12, 923.5 min (SD 158.2) (Fig. 2*A*). There is a large dip in time at home around March 21, after the median date of emergency orders and prior to the median date when stay-at-home orders went into effect (Fig. 2*A*). The SafeGraph retail visitation data suggest that people increased visits to stores like Costco, Walmart, and Target around the weekend that preceded a major wave of state stay-at-home orders, perhaps in anticipation of restrictive policies (36). Fig. 2*A* also shows that counties that received stay-at-home orders (black line) were similar in their staying home behavior to counties that never received stay-at-home orders prior to the date that many counties began issuing stay-at-home orders but diverged after those counties began issuing stay-at-home orders.

Time spent at home was already rising in the average county when it experienced its first case (Fig. 2*B*) and began rising around the time the state experienced its first case (Fig. 2*C*). Harmonizing to the first county case report, there is a difference in average behavior in counties that received stay-at-home orders (black line) and those that did not (blue line). On average, time spent at home was at its lowest level and began rising prior to emergency declarations (Fig. 2*D*). This adds to the evidence that a portion of the behavioral response was voluntary. Median time at home continued to increase following the emergency declaration. On average, time at home was also rising prior to school closures and stay-at-home orders (Fig. 2 *E* and *F*).

We use 251,992 observations to estimate a regression model of the log of time spent at home on the log of reported local and national cases, the presence of distancing mandates, the interaction between reported county cases and stay-at-home orders, and other covariates (summary statistics in *SI Appendix*, Table S1). The regression results enable us to disentangle voluntary avoidance behavior from the effect of policies (see *Materials and Methods* for details). We test many alternative specifications (see *Materials and Methods*). Almost all coefficients are precisely estimated, and regression tables for alternative specifications are provided in *SI Appendix*.

The average American increased time at home in direct response to COVID-19 risk as measured by case and death reports. From the most basic specification (Table 1, column 1), the

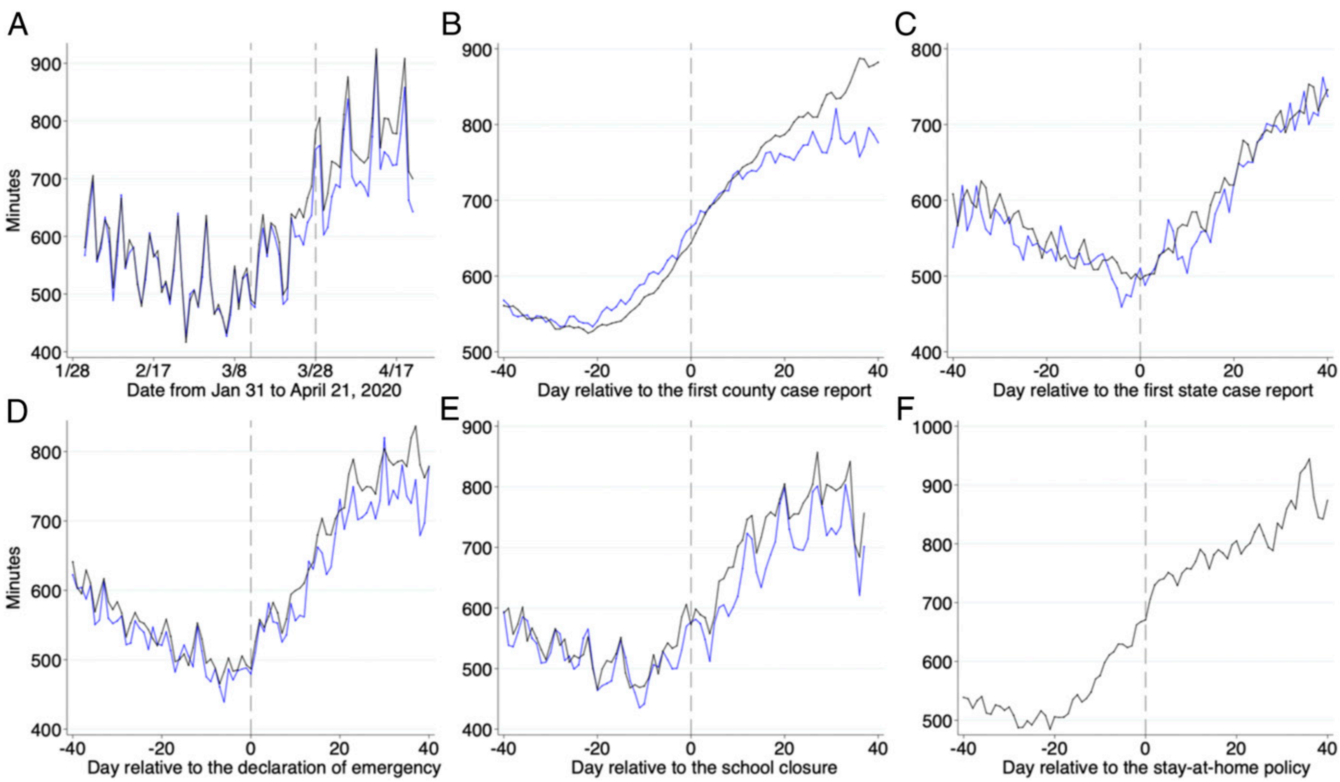

Fig. 2. Trends for the mean time at home in minutes for counties never receiving stay-at-home policies (blue lines) and counties receiving a stay-at-home policy (black lines) by calendar date with vertical lines showing the median date for emergency orders and stay-at-home policies (*A*), aligned to the first case reported at the county level (*B*), aligned to the first case reported at the state level (*C*), aligned to the emergency declarations (*D*), aligned to school closure (*E*), and aligned to the stay-at-home policy (*F*).

coefficient of log county cases shows that a 1% change in cumulative reported cases in a county is associated with approximately a 4% increase in time spent at home. The specification in column 1 may be slightly biased because it omits a measure of the scope for spending time at home or the length of the epidemic. Including industry labor shares helps control for the share of essential workers who could not stay home and the probability of becoming unemployed over the course of the epidemic and epidemic day fixed effects control for the epidemic length (Table 1, column 2). This specification results in a greater response to cases, but this is not statistically different from the base specification.

The response to risk is stable across specifications considered. The time at home response to county cases ranges between 3.6 and 5% (*SI Appendix*, Table S2). Directly accounting for interactions between the response to cases and the stay-at-home order is important (*SI Appendix*, Table S3). We find that a 1% increase in state-level cases, instead of county cases, is associated with approximately a 3.3 to 4.7% increase in time at home (*SI Appendix*, Table S4). Including county and state cases suggests a slightly greater aggregate voluntary response (*SI Appendix*, Table S4). Focusing on new cases, we find that a 1% increase in new county or state cases yields a 2 to 3% increase in time at home (*SI Appendix*, Table S5). We find that a 1% increase in reported county or state deaths yields a 5 to 7% increase in time at home (*SI Appendix*, Table S6).

The average American also responded to stay-at-home orders. There were two responses to stay-at-home orders. First, anticipating these orders, Americans spent substantially less (>25%) time at home the weekend of March 21 (Table 1), consistent with Fig. 2*A*. Second, the stay-at-home orders kept people home. The direct effect of the stay-at-home order for the preferred specification was a 12.9% increase in time at home (Table 1, column 2), which is statistically indistinguishable from the base model and other specifications (Fig. 3). The effect of the stay-at-home order net of the effects on the response to cases was a 10% increase in time at home.

School closures influenced time at home. Our primary specification (Table 1) suggests that school closures increase time at home by ∼18%. Some alternative specifications (Fig. 3) reduce the estimated response to school closures to 12%, but irrespective of specification, most estimates are statistically indistinguishable from our primary estimate.

The inference is robust to many alternative specifications, including replacing the natural log of time at home with time not at home (*SI Appendix*, Table S7), the level of time at home (*SI Appendix*, Table S8), or accounting for cases in the bordering states (*SI Appendix*, Table S9). Including the rate of change in cases; combining cumulative cases, new cases, and deaths; and considering cases per capita (*SI Appendix*, Tables S10 and S11) do not qualitatively affect inference.

The signs of coefficients are robust to the model specification, and magnitudes tend to be statistically indistinguishable among the models in Table 1 and associated models in *SI Appendix*. Nevertheless, it is important to avoid misattribution that shifts the effect of cases from/to the effect of the stay-at-home order. There are two possible mechanisms that an omitted time-varying variable may lead to the misattribution of causality between the direct "voluntary" responses to case reports and the "involuntary" response to policy mandates. First, counties where the population is more likely to voluntarily respond to the pathogen may be more likely to implement stay-at-home orders earlier. This would lead our model to overestimate the effect of the policy order. This hypothesis garners some support from the fact that including the labor shares in the model increase the measure of the voluntary response. Second, county or state officials

Table 1. Time at home associated with county case reports under the base model, with epidemic day (based on the first case report) fixed effects and with labor share for counties before the stay-at-home orders, counties with no order, and all counties in the United States (January 31 to April 21, 2020)

	Full sample		Full sample prior to stay-at-home orders		Counties receiving mandates before the order	
ln (home dwell time)	1	2	3	4	5	6
ln (county case+1)	0.0397***	0.0421***	0.0237***	0.0278***	0.0343***	0.0516***
	(0.00450)	(0.00798)	(0.00339)	(0.00709)	(0.00393)	(0.00553)
ln (national case+1)	−0.00826*	−0.0181***	−0.0127**	−0.0199***	−0.0255***	−0.0254***
	(0.00361)	(0.00481)	(0.00436)	(0.00466)	(0.00262)	(0.00449)
ln (national case +1) * first county report	0.00319***	0.00249***	0.00812***	0.00601***	0.00635***	0.00393***
	(0.000654)	(0.000546)	(0.000686)	(0.000703)	(0.000477)	(0.000621)
Emergency	0.0283	0.0614***	0.0456**	0.0702***	0.0834***	0.0964***
	(0.0149)	(0.0133)	(0.0170)	(0.0155)	(0.0139)	(0.0172)
Stay-at-home	0.154***	0.129***				
	(0.0200)	(0.0208)				
School closure	0.187***	0.183***	0.187***	0.184***	0.181***	0.171***
	(0.00975)	(0.0188)	(0.00871)	(0.0184)	(0.0117)	(0.0180)
ln (county case+1) * stay-at-home	−0.0213***	−0.0290***				
	(0.00445)	(0.00648)				
Anticipatory weekend	−0.283***	−0.266***	−0.277***	−0.263***	−0.245***	−0.240***
	(0.0121)	(0.0161)	(0.0129)	(0.0162)	(0.0147)	(0.0148)
County FE	X	X	X	X	X	X
Weekday FE	X	X	X	X	X	X
Epidemic day FE		X		X		X
Labor share		X		X		X
R-sq	0.795	0.807	0.745	0.756	0.761	0.769
N	251,992		187,686		147,696	

Standard errors, clustered at the state level, are shown in parentheses. *$P < 0.05$, **$P < 0.01$, ***$P < 0.001$. "X" in the table indicates that the corresponding fixed effects (FE) have been included in the regression model.

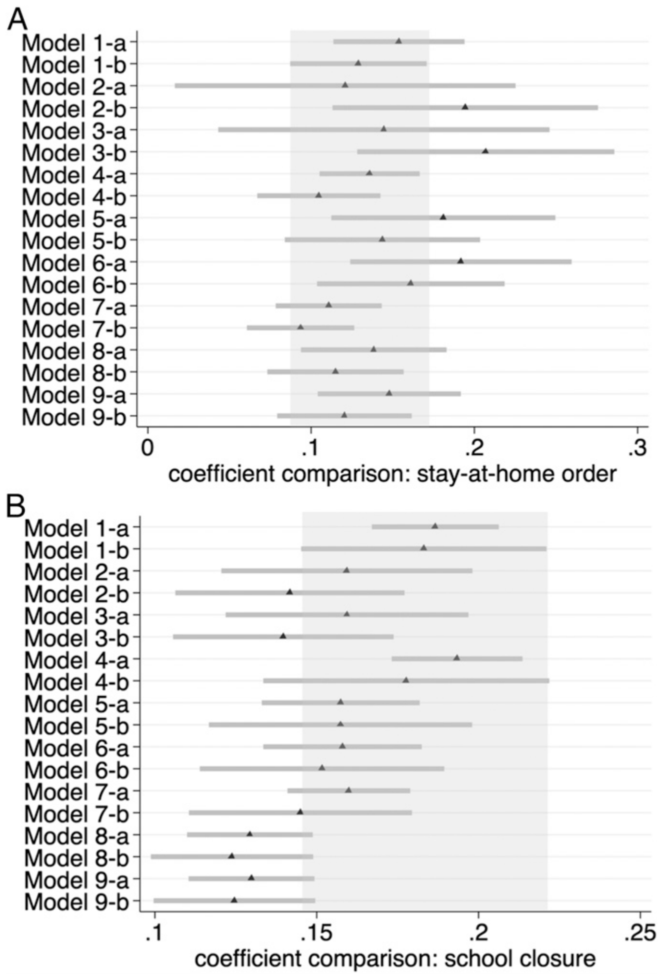

Fig. 3. Summary of regression results on policy effects. The specifications are: 1) models with cumulative county cases, 2) models with cumulative state rather than county cases, 3) models with cumulative state and county cases, 4) models with new county cases rather than cumulative cases, 5) models with new state cases rather than cumulative cases, 6) models with new county and state cases rather than cumulative cases, 7) models with cumulative county deaths, 8) models with cumulative state rather than county deaths, and 9) models with cumulative state and county deaths. Model *A* is the basic county fixed effects model without time-varying fixed effects, and *B* includes interaction of labor share with county fixed effects, and fixed effects relative to the first case.

observe the voluntary response of their populations and delay orders selectively in places with a strong voluntary response. We are unaware of any public record that documents this mechanism. In some places, counties imposed mandates independently. However, most counties were subject to state-wide policy mandates. To address this general concern, we estimate the models on data prior to the time the stay-at-home orders are implemented, investigate a split sample, and implement an augmented synthetic control (37).

First, we estimate our primary set of models using only data generated prior to the imposition of stay-at-home orders with and without counties that never receive stay-at-home orders (Table 1). We find the effect of cases is stable and statistically indistinguishable, implying that there is sufficient variation in the data during this period to identify the effect of county cases. This suggested limited scope for an omitted variable to confound the primary effect of county cases.

Next, we examine the pretrend response to cases for counties receiving stay-at-home orders and the full sample with counties never receiving the order. The pretrend response to county cases and school closures are similar to our main result. A 1% increase in county cases is associated with a 2.1 to 3.6% increase in time at home (Table 1, columns 3 to 6 and *SI Appendix*, Table S2). This suggests counties without stay-at-home orders did not show a stronger voluntary response.

A related question is what impact does the stay-at-home mandate have on counties that receive the mandate relative to those that did not? Augmented synthetic control (37) is useful for investigating this question but does not provide an estimate of the voluntary response. An advantage of the augmented synthetic control approach is that it provides a straightforward approach to verify that the counties used as comparators are indeed good "controls" and have parallel trends prior to the stay-at-home mandate. Implementing the augmented synthetic control (Fig. 4), we find an immediate direct effect of the stay-at-home order of ∼4%, which is lower than the 10% effect from our preferred specification. This suggests that, if anything, our primary specification underestimates the voluntary response and overestimates the measure of equivalent cases. However, the augmented synthetic control results are statistically indistinguishable to the net effect of stay-at-home orders in a version of the model with stay-at-home day fixed effects (*SI Appendix*, Table S2, column 5), but that model also implies a marginal decrease in time at home with increase cases following the stay-at-home order because that specification and the augmented synthetic controls do not allow for differentiated responses to cases following the stay-at-home order, whereas the primary specification does.

The estimated parameters from model 2 in Table 1 imply that there may be an additional number of COVID-19 cases in the county that would elicit the same amount of time spent at home, the equivalent cases for the stay-at-home response (Fig. 5) (details in *Materials and Methods*). Most counties that imposed stay-at-home orders did so with few cases in the county (Fig. 6*A*). An equivalent number of cases was calculated for counties that imposed stay-at-home orders (Fig. 6*B*) and scaled to the county's population (Fig. 6*C*). For the median county, 29.3 (parametric bootstrap 95% CI [11, 47,607]) additional cases would have led to behavioral changes associated with the stay-at-home order. The upper end of the confidence interval is driven by a long thin right tail (*SI Appendix*, Fig. S2). Counties with larger urban populations generally stand out as requiring more cases to elicit the same amount of time at home as the stay-at-home orders, but there are a number of smaller and rural counties that also require a large number of cases (Fig. 6*B*). However, Fig. 6*C* shows

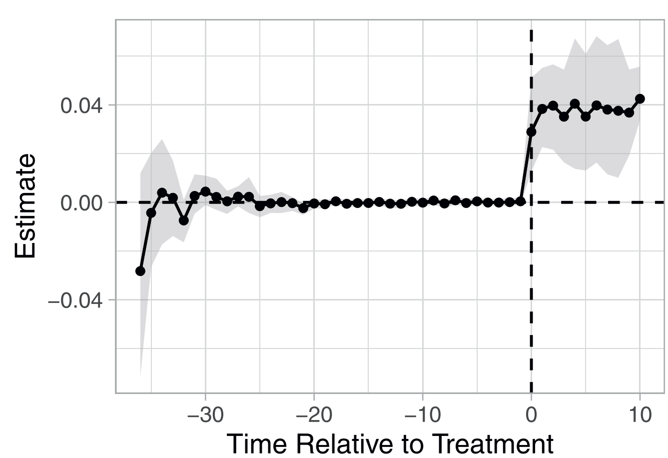

Fig. 4. Augmented synthetic control estimate of the effect of the stay-at-home order (time 0) and m-out-of-n bootstrap 95% confidence intervals. The minimal difference between the treated and control counties prior to treatment is evidence of parallel pretrends.

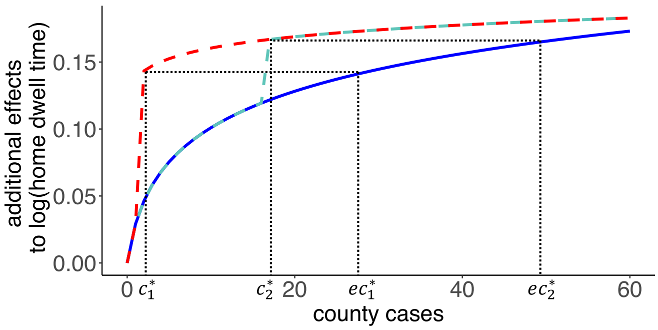

Fig. 5. Illustration of the case equivalent response to the stay-at-home order concept. The symbol c^* is the level of cases when the stay-at-home order went into effect, and $ec^* - c^*$ is the cases needed to induce the equivalent additional time at home. The horizontal difference is the case equivalent response to the stay-at-home order. The blue curve is the voluntary response for the no stay-at-home order counterfactual. The red (cyan) curve is the response with the stay-at-home order when the mandate is imposed on the median (mean excluding New York City's 9,065 cases) number case when a stay-at-home order went into effect. We note that the median county reflects the experience of most Americans better than the mean county.

that county population does not explain the variation, suggesting other local factors such as workforce mix and perhaps local conditions also influence the response (Fig. 6*C*). There is no equivalent voluntary response to cases that achieves the response of the stay-at-home order for Dougherty and Richmond Counties, GA; Christian County, KY; Tunica County, MS; Yuma County, AZ; Montour County, PA; and Anderson County, TN.

Discussion

There is little uncertainty among public health experts that staying home and greatly reducing contacts slowed the spread of the SARS-CoV-2 virus, and Americans increased time at home during the COVID-19 pandemic. Recent assessments have put all the weight on policy orders by assumption (23–25), while other analysts focused on showing that there was a voluntary response (21). Our results support both mechanisms and show the relationship between them.

Americans showed nontrivial voluntary behavioral changes in response to COVID-19 risk. The magnitude of voluntary response likely would have increased with increasing cases. Stay-at-home orders overlapped and replaced the voluntary response to cases. However, mandates kept more people home earlier in the epidemic, and epidemiological processes are sensitive to the magnitude and timing of behavioral changes. The additional time at home may be necessary since voluntary behavior is unlikely to fully internalize the costs of infecting others (38) or congesting hospitals. In some counties, it is unlikely that voluntary shifts in behavior would have had qualitatively similar effects on the epidemic as stay-at-home orders. There are 96 counties with an estimated equivalent case response greater than 10,000 cases. On the other hand, there are some counties where stay-at-home orders may have had little effect on behavior, since for 50% of counties the case equivalent response is 29 cases or less, which may be within the margin of reporting error. There are two complicating factors immediately apparent from our model. First, stay-at-home orders appear to have led to preemptive gatherings related to preparation for the mandate. Second, stay-at-home orders may increase in importance if schools remain open, which is likely because the emerging evidence suggests that schools do not pose a large transmission risk, and schooling is a high-value activity (39). However, school closures likely keep many adults home to provide childcare (40). Public policies may have encouraged employers to allow greater flexibility to work from home and diminished cultural pressure to be present in the workplace. We did not test if policy mandates increase the salience of risk information, and it would be difficult to separate a salience effect from a voluntary effect.

The next step is understanding how layering a stay-at-home order on top of a voluntary response alters the epidemic's trajectory. Quaas et al. (41) attempted such an analysis and found that voluntary behavior was likely sufficient to contain COVID-19 but not suppress it. Such analysis needs to account for the fact that had Americans spent less time at home, then cases and death counts would have risen faster, which also could have accelerated the voluntary behavioral response. Nevertheless, given testing constraints, it is unlikely more cases would have been reported during this period, and a voluntary behavioral response often depends on information about infection rather than the true number of infections (12). Experience matters, and people may respond to a case level on the decline or a second wave differently than the first rise in cases. To ultimately

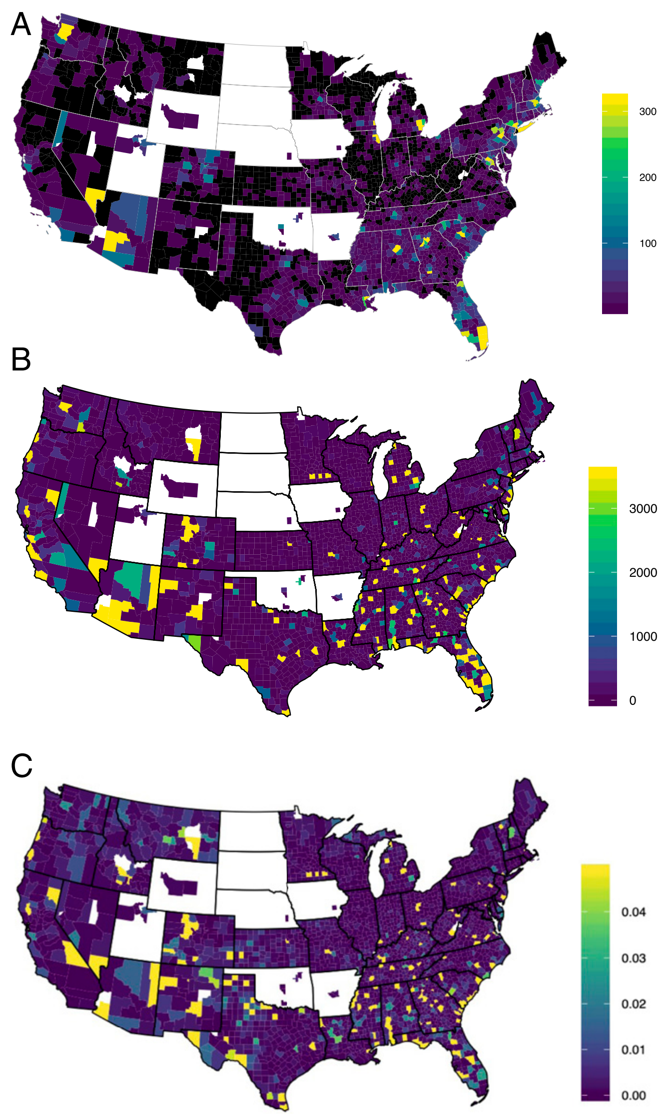

Fig. 6. (*A*) Cases in each county when the stay-at-home order was in effect (censored at 99% = 318 cases). Counties imposing stay-at-home orders with zero cases are shown in black. Missing data or those never imposing stay-at-home orders are white. (*B*) Equivalent cases to the response to the stay-at-home order (values about 95% = 3,553.7 cases are colored yellow) for each county using coefficients of column 2 in Table 1. (*C*) Equivalent cases per population by county.

evaluate the role of stay-at-home orders in preventing cases or saving lives, analyses must account for the fact that stay-at-home orders can simultaneously influence transmission and other behavioral responses that influence transmission such as distancing fatigue and mask wearing (42), along with the arrival and distribution of pharmaceutical treatments and vaccines. Therefore, it is important to first separate the behavioral response to the stay-at-home order from voluntary behavioral responses to cases, only then can we start investigating the effect of the stay-at-home order on avoiding cases and deaths.

Understanding the relative role of policy and individual preferences in people's changes in behavior is critical. The scientific and policy question is how much credit stay-at-home orders can take for the behavioral shifts and the associated reduction in contacts in the resulting pathogen spread or, conversely, how much blame these orders deserve for economic disruption. These connected questions are important for three reasons. First, public health resources that could be used to detect and communicate local risks need to be allocated to the enforcement and defense of policy mandates when they are issued. Second, nonpharmaceutical interventions are at times framed as lives versus the economy, but our results suggest that this framing is tenuous on its face for most counties because stay-at-home mandates overlapped with behavior that would have happened as cases rose. Third, the measure of the equivalent cases requires a publicly knowable risk (e.g., COVID-19 cases reported in the county). This implies that any appeal to our results for forgoing stay-at-home mandates must also advocate for high levels of testing and accurate and timely communication.

Public policies led to greater levels of time spent at home while displacing voluntary efforts. On one hand, the more rapid response likely reduced cases during the initial wave. The reduction likely came through timing rather than the ultimate magnitude of the response, which was seldom greater than would have been achieved with a reliance on voluntary behavior. This implies that the direct economic harms of stay-at-home orders may be overestimated, though they are not zero. Public health strategies that rely on voluntary behavior require accurate information that allows people to make informed decisions. In the case of COVID-19, that meant accurate and timely local testing and clear, trusted information (43), two conditions that were rarely met in the first wave of the epidemic.

Materials and Methods

Data on county-level emergency declarations and stay-at-home orders are cross-referenced from five sources: Raifman et al. (44), National Association of Counties (45), the New York Times (46), the COVID-19 policy trackers website (47), and the Crowdsourced COVID-19 Intervention Data (48). Reported case and death data came from the New York Times (49). These data are a measure of what Americans knew about the state of the epidemic and risks, though testing was evolving and incomplete during this period. All policies considered were put in place between February 14 and April 7, 2020. District level school closure data comes from MCH Strategic Data (https://www.mchdata.com/covid19/schoolclosings). We exclude Alaska, Hawaii, five US territories, and four independent cities in Virginia because of missing data.

Data on time spent at home is based on anonymized and aggregated mobile device location data from SafeGraph (50). Benzell et al. (51) used these data to develop a merit order for business closing, and the use of these data are increasingly common (e.g., ref. 52). We used three SafeGraph products to quantify behavior during the epidemic. First, we used median home dwell time reported at the Census block group on each day. Dwell time is the time that a device is present at its common evening location, which is assumed to be home. Common evening location is where the device most often rests overnight over the preceding 6 wk. We construct a county average of median home dwell time (Census block group) by weighting the estimate in each Census block group by the number of devices reported on that day. The normalization is required to compare estimates over time because the panel of devices changes over time, and individual Census block groups can have reporting artifacts. We dropped the lowest 1% of county dwell times to reduce the misreporting of zero or low home dwell times. Squire (53) discusses the potential biases in the SafeGraph data and provides evidence that these biases are unlikely to be large for behavior aggregated at the county level.

We construct a set of daily county-level weather control variables by aggregating 4 km gridded estimates of maximum and minimum temperature, maximum and minimum relative humidity, precipitation amount, surface solar radiation, and wind speed. The data are processed using https://github.com/jbayham/gridMETr and based on http://www.climatologylab.org/gridmet.html (54). Summary statistics for all variables are presented in *SI Appendix*, Table S1.

We focus on the period between January 31 and April 21, 2020. As of January 31, only three travel-related cases of COVID-19 were reported in the United States. On February 14, California issued the first emergency declaration. By April 21, all stay-at-home orders that were issued during the initial period had been in effect for at least 2 wk.

Our base specification is

$$\begin{aligned}\ln(Y_{it}) = \alpha + \beta_0 \ln(C_{it}+1) + \beta_1 \ln(N_t+1) + \beta_2 \kappa_{it} \ln(N_t+1) + \gamma_0 E_{it} + \gamma_1 H_{it} \\ + \gamma_2 S_{it} + \eta \ln(C_{it}+1) H_{it} + \rho X_{it} + w_t + q_t + a_i + z_{it} + \epsilon_{it},\end{aligned} \quad [1]$$

where $\ln(Y_{it})$ is the natural log of minutes spent at home in county i on day t. The case variables are C_{it}, which represent the cases in county i and N_t, which is reported cases from the United States. κ_{it} is an indicator for whether the first case in the county had occurred, which likely influences the saliency of national cases. We calculate the natural log of cases because of the exponential growth in the early phase of the epidemic. We add one to address the zeros in the data. The policy main effects are E_{it}, a dummy variable indicating whether an emergency order has been issued for the county, H_{it}, a dummy variable indicating whether a stay-at-home (or equivalent shelter-in-place) order has been issued for a county, and S_{it}, a dummy variable for school closures in the county. We consider a county under a mandate the first day that a county has a local or state mandate. The expression $\ln(C_{it}+1)H_{it}$ is the interaction between the natural log of county cases plus one and the stay-at-home order. The interaction is important to allow the response to cases to vary with and without a stay-at-home order. We condition on county-level weather X_{it} and day of week w_t with fixed effects. We also condition for the weekend of March 21. Device counts vary by county and date. Therefore, we separate device counts into 50 even-size bins and include a device count fixed effect, z_{it}, that varies by day and county for all models. Changing the number of device count bins between 25 and 100 does not meaningfully affect the results. We cluster SEs at the state level to account for state-level serial correlation and heteroskedasticity caused by the phase in of mandates. We choose to cluster at the state level rather than the county level because most policies are state wide.

It is always possible for omitted variables to bias the estimate of marginal effects. To remove the potential correlation between any non–time-varying omitted variable and the regressors of interest, we include county fixed effects, a_i. These fixed effects control for omitted variables such as county population and political preferences, which do not vary meaningfully over the short study period. We address the potential for time-varying omitted variables in multiple ways described in the following paragraphs.

After estimating the base specification, we consider several alternative specifications. The purpose of these alternative specifications is to test if the parameter estimates are stable and robust across specifications. The alternative specifications include replacing $\ln(Y_{it})$ with Y_{it} and including cases from bordering states. We replace county cases with state cases and estimate multiple specifications with both. We replace cumulative reported cases with newly reported cases at the county, state, and national levels, and we estimate multiple models that have cumulative and new cases at all three levels. We replace county cases with reported deaths, and we estimate models with reported cases and deaths. We replace the time at home metric with a separate metric from SafeGraph, which is time not at home. Because of the way smart device data are recorded, these two metrics do not sum to 1,440 min per day.

One concern is that time spent away from home may lead to cases leading to reverse causality. This has been a concern in research in response to other infectious agents when data were only available monthly or weekly (14). However, the time between infection, testing, and reporting is ∼8 to 14 d (https://www.cdc.gov/coronavirus/2019-ncov/hcp/planning-scenarios.html#table-2). This lag breaks the potential for simultaneity within the daily data.

There remains the potential for a time-varying omitted variable to bias the results, or that county cases are correlated with county responses in a time-varying manner. If the correlation is not time varying, then county fixed effects remove the confound. However, if the average individual in a county is more or less likely to spend time at home in response to cases and counties

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are more or less likely to adopt policies earlier or later, then the estimated impact of the policy could be biased. One mechanism for this is that counties vary in a systematic way in the amount of time the average person can spend at home, which could be driven by the share of essential workers or the share of the population unemployed. Unemployment data in this period is not reliable at the spatial and temporal scale of our analysis.

We employ multiple approaches to address potential omitted confounders that could lead us qualitatively astray. These test the robustness of our core estimates. First, we consider models with day relative to the first case in the county fixed effects and day relative to stay-at-home order fixed effects. These help address the correlation caused by either the epidemic process or a social process associated with the stay-at-home order.

Next, we include the deviation from the mean county's labor share in 21 sectors by reported county cases by stay-at-home order interaction, with the deviation from the mean employment shares in those 21 sectors using data from the US Bureau of Labor Statistics (https://www.bls.gov/cew/downloadable-data-files.htm). This controls for variations in maximum time the average person in a county can stay home and for the share of essential workers. Furthermore, the county employment shares are likely good predictors of the unemployment effects counties experienced because these effects were largely sector specific. Including labor share yields

$$\begin{aligned}\ln(Y_{it}) = {} & \alpha + \beta_0 \ln(C_{it}+1) + \sum_{j=1}^{21} \beta_{0j}\{\ln(C_{it}+1)\cdot ds_{ij}\} + \beta_1 \ln(N_t+1) \\ & + \beta_2 \kappa_{it} \ln(N_t+1) + \gamma_0 E_{it} + \gamma_1 H_{it} + \sum_{j=1}^{21} \gamma_{1j}\{H_{it}\cdot ds_{ij}\} + \gamma_2 S_{it} \\ & + \eta \ln(C_{it}+1) H_{it} + \sum_{j=1}^{21} \eta_j \{\ln(C_{it}+1) H_{it} \cdot ds_{ij}\} + \rho X_{it} + w_t + q_t + a_i + z_{it} \\ & + \epsilon_{it},\end{aligned} \quad [2]$$

where $ds_{ij} = s_{ij} - mean(s_j)$ and s_{ij} is the share of 2020s first-quarter average employment in county i for industry j, where s_{ij} is the number of workers in industry j divided by the employed civilian population in county i.

Third, we examine the pretrend sample with counties before the stay-at-home orders and 498 counties that never experience a stay-at-home order. While the counties that never experience a stay-at-home order are not ideal controls for the other counties, the more credible omitted variables story suggests that these counties should show a more muted voluntary response than counties that experienced orders, and we expect that under the confounding trend hypothesis they provide a lower bound on voluntary behavior.

Fourth, we directly address concerns about divergent pretrends in the comparators using the augmented synthetic control method (37). This method forces us to change the question to what is the average effect of a stay-at-home order on those counties that received stay-at-home orders? It does not allow investigation of the voluntary response to cases. We implement this approach using the R package associated with ref. 37 (https://github.com/ebenmichael/augsynth). The approach is challenged by the large number of treated units (55) and is computationally inefficient on a large data set. We first estimated the model on the largest sample that was computationally feasible (all 498 untreated counties and about one-third [869] of the treated counties). However, jackknife SEs are not computationally feasible on the large sample, so we employ an m-out-of-n bootstrap (56). We draw 1,000 samples of 200 treated and 200 control counties and estimate the model. The m-out-of-n bootstrap overestimates the SEs and the spread of the confidence interval.

A complement to the augmented synthetic control approach is to estimate the direct response to cases on a data set trimmed to only include data prior to stay-at-home orders. Therefore, we reestimate our primary specification on data over the period when counties have not received stay-at-home orders.

We use the primary model estimates to compute the number of cases that yields an equivalent behavioral response of the stay-at-home order. This is defined as the number of additional cases needed at the point when the stay-at-home order goes into effect to elicit the same response voluntarily: $Z(ec, H=0) = Z(c^*, H=1)$, where $Z(case,\ H)$ equals to $\ln(Y_{it})$ as a function of county case and whether or not a stay-at-home order was in effect. This holds other factors constant. Equivalent cases are calculated by county, where county variation comes from interactions with labor shares information and the local state of the epidemic when the stay-at-home order was issued:

$$equivalent\ cases = ec - c^* = \exp\left(\frac{(\beta_0+\eta)\ln(c^*+1)+\gamma_1}{\beta_0}\right) - 1 - c^*. \quad [3]$$

Confidence intervals are generated by parametric bootstrap for the median county.

Data Availability. Replication code is available on GitHub (https://github.com/youpeiyan/covid_endogenous_policy_response). The data to run the code are in OPENICPSR: https://www.openicpsr.org/openicpsr/project/135121/version/V1/view. All other study data are included in the article and/or *SI Appendix*.

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