Poverty and Economic Dislocation Reduce Compliance with COVID-19 Shelter-in-Place Protocols

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Abstract
Shelter-in-place policies reduce social contact and mitigate the spread of COVID-19. Inconsistent compliance with social distancing creates local and regional interpersonal transmission risks. Using county-day measures on population movement derived from cellphone location data, we investigate whether compliance with local shelter-in-place ordinances varies across US counties with different economic endowments. Our theoretical model implies economic endowments will influence compliance with social distancing. We find evidence that low income areas do comply less than counties with stronger economic endowments. Findings suggest targeted economic relief could improve future compliance with public health interventions.

JEL Classification: H12, I18.

Keywords: COVID-19, shelter-in-place, compliance.

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Introduction

Shelter-in-place policies reduce social contact and risks of interpersonal COVID-19 transmission (Hsiang et al., 2020; Viner et al., 2020; Anderson et al., 2020; Bai et al., 2020; Matrajt and Leung, N.d.). Though the economic consequences of these policies are substantial (Stapleton, 2020; Baker et al., 2020; Gormsen and Koijen, 2020), local non-compliance creates public health risks and may cause regional spread (Lewnard and Lo, 2020; Chen et al., 2020). Clarifying how economic endowments influence compliance provides actionable insights for policy makers and public health officials responding to the COVID-19 pandemic.

We study compliance with shelter-in-place policies using novel data on population movement, local policy changes, and variation in economic endowments. Our research design flexibly accounts for a number of potential confounding factors, including government restrictions on social and business activities, local COVID severity, exposure to recent trade disputes, political partisanship, exposure to slanted media, unemployment, as well as population density.

We find compelling evidence that shelter-in-place policies reduce population movement and these reductions increase with income. Event study designs suggest much of the nation-wide reduction in population movement is driven by counties above the median level of income. These core findings survive a number of robustness checks that account for county-specific time-varying effects of related economic and political mechanisms.

The results are consistent with the theoretical model of compliance, which is presented in the Appendix. Our basic model considers an environment, in which the “shelter-in-place” ordinance is issued, and agents decide whether to comply with these orders or not. Agents have heterogeneous economic endowments (income), which capture the assets available to individuals as well as their exposure to economic shocks, e.g., from trade conflict. This maps directly to
the empirical research design (see Section 2). Then, we extend the setup to cover an environment in which agents receive additional information about the probability to get infected if they do not comply provided by a media that might have a bias over the importance of compliance.

In equilibrium, there is a positive relationship between person’s wealth (income) and compliance: wealthier agents are more likely to comply with shelter-in-place protocols. This is intuitive: for a low-income person, the same gain due to non-compliance results in a higher marginal utility. Aggregating at the county level, we prove that richer counties feature higher compliance, which is fully supported by our empirical analysis. Empirically, we find a significant decline in population movement after the local shelter-in-place policies were enacted. As predicted by the theoretical model, counties with above median income comply with shelter-in-place policies by reducing movement by an additional 72% relative the baseline policy impact.

Though our primary focus is the economic determinants of compliance, we check robustness by accounting for political factors such as partisanship and slanted media exposure that may confound this relationship. Consistent with contemporary empirical work (Gadarian, Goodman and Pepinsky, 2020; Painter and Qiu, 2020; Barrios and Hochberg, 2020; Brzezinski et al., 2020), our model suggests that partisan media viewership will reduce compliance with shelter-in-place policies. President Trump’s supporters are more skeptical of mainstream media and federal government policies in general (Iyengar et al., 2019) and early statements by administration officials and some media dismissed or deflected the COVID-19 threat (Painter and Qiu, 2020; Bursztyn et al., 2020). Our empirical results show that partisanship and exposure to slanted media may play a complementary yet distinct role in shaping public compliance with shelter-in-place orders.

\[^1\] In the empirical design, assets available immediately prior to the onset of COVID-19 are unknown. Instead, we rely on a baseline measure of income by county (2016). We account for various other factors that might offset these initial endowments (e.g., negative economic shocks due to the US trade war).
This study advances a long standing literature on the relationship between income and strategic decision-making, including household financial choices as well as social compliance. Economic endowments, income uncertainty, and liquidity constraints impact risk preferences and self control (Jalan and Ravallion, 2001; Banerjee and Mullainathan, 2008; Guiso and Paiella, 2008; Tanaka, Camerer and Nguyen, 2010; Dupas and Robinson, 2013; Bernheim, Ray and Yeltekin, 2015; Carvalho, Meier and Wang, 2016). These factors may also influence willingness to abide by government policies (Gaventa, 1982; Scott, 1985; Tyler, 1990; Levi, 1997). The evidence we present suggests that economic endowments influence substantive behavioral choices which pose a first-order challenge to public health interventions and broader government measures to stop the interpersonal transmission of COVID-19.

The rest of the paper is organized as follows. Section 2 describes our data and introduces the research design. Section 3 reports the results, and Section 4 concludes. The Appendix contains the theoretical model, data overview, and some additional results and robustness checks for the empirical analysis.

2 Data and Research Design

We study changes in population movement (“social distancing”) after the onset of local shelter-in-place ordinances in the United States. Cellphone location data enables us to quantify population movement within and across origin counties by day. Patterns of cellphone use vary across counties. To standardize measures of population movement, all data is compared to a local day-of-week base rate calculated using data prior to the onset of COVID-19 in the United States (March 8). The outcome of interest is county-day variation in standardized population movement. This is illustrated in Figure 1 Panel A. We provide a detailed overview of the data used in our analysis in the Appendix. Where individuals comply with local shelter-in-place laws, we observe a reduction in intersecting sets (less distance traveled on average). Although
population movement has declined by approximately 25% (national average) between March 8 and April 1, there is high variance in movement (Figure 1 Panel B). This suggests variation in compliance with government ordinances. The secondary quantity of interest is how compliance varies across counties with differing economic endowments.

2.1 Research Design

We employ a difference-in-difference design to estimate the impact of the shelter-in-place ordinances on population movement (Bertrand, Duflo and Mullainathan, 2004). Introduction of social distancing policies was staggered across states in time (Figure 2 Panel A). More than 125 counties adopted policies prior to state-wide mandates. We leverage this data to examine how localized population movement changes after the introduction of a county-level policy (either via county- or state-wide mandates). Local ordinances are staggered across states and, in a number of cases, counties within states that have not yet enacted state-wide mandates. To account for this staggered variation in the introduction of the natural experiment (local policy change), we include county and day fixed effects (Goodman-Bacon, 2018). This partials out any variation in population movement (dependent variable) that is correlated with factors that remain fixed regarding counties during the sample period (February 23 to April 1) or changes in mobility due to nation-wide shifts in policies or protocols (‘common shocks’). These factors include, for example, population density, industrial sector shares, and federal mandates. Our benchmark specification also incorporates state × week fixed effects, which account for differential trends in population movement and policy interventions by state across time. We produce Figure 2 Panel C by studying equation (1):

\[ y_{c,d} = \alpha + \beta_1 \text{active}_c \times \text{active}_d + \beta_2 \text{active}_c \times \text{income}_c \times \text{above}_c \times \text{median}_c \times \text{2016} \]
\[ + \omega X_{c,d} + \phi \theta_d \times X_c + \lambda_c \times \theta_d + \kappa \theta_d \times \text{income}_c \times \text{above}_c \times \text{median}_c \times \text{2016} \]
\[ + \zeta_c \times \text{state} \times \text{week}_d + \epsilon_{c,d} \]
where $y_{c,d}$ is the measure of population movement derived from cellphone location data, standardized using location-specific day-of-the-week trends prior to March 8, 2020. $active\_policy_{c,d}$ indicates whether a county-specific or state-wide shelter-in-place mandate is active on a given day. We estimate the marginal effects using a threshold indicator for whether a given county is above the median level of income in 2016 ($\times income\_above\_median_{c}^{2016}$). $\lambda_{c}$ indicates county-level fixed effects, $\theta_{d}$ indicates day-level fixed effects, and $\zeta_{state \times week}$ denotes state-by-week specific fixed effects. $\theta_{d} \times income\_above\_median_{c}^{2016}$ is the day-specific effect of the median income measure. This accounts for any linear and non-linear trends in movement across counties with varying levels of income by day. This enables us to interpret the main effects as a triple difference (difference-in-difference-in-differences) design since the baseline shift is otherwise collinear with parameters in the main model (due to staggered policy introduction across counties and states). $\theta_{d} \times X_{c}$ and $X_{c,d}$ are a vector of control variables described in the main text (see (a) through (h) in main text). The unit-specific time-invariant characteristics are interacted with the day-level fixed effects to allow these effects to vary flexibly across the sample period. Since the benchmark specification includes state $\times$ week specific fixed effects, we flexibly account for any state-wide trends over time. Heteroskedasticity-robust standard errors are clustered by county, day, and state $\times$ week.

The difference-in-differences design provides a causal estimate of compliance with shelter-in-place policies so long as trends in the outcome of interest in untreated counties represent a valid counterfactual for treated counties (Donald and Lang, 2007). We assess the plausibility of this identifying assumption using an event study design. This approach leverages leads and lags of local shelter-in-place policies to study variation the outcome variable prior to and following the policy change.\(^2\) We study equation 2 using 10 window prior to the leads as the base period:

\[^2\]It also enables us, when we estimate separate event studies across median thresholds, to assess the validity of this assumption specifically for the marginal effect of interest ($\times income\_above\_median_{c}^{2016}$).
\[ y_{c,d} = \alpha + \sum_{i=-9}^{-1} (\beta_{i,active\_policy_{c,t+i}}) + \beta_{0,active\_policy_{c,t=0}} + \sum_{i=1}^{5} (\beta_{i,active\_policy_{c,t+i}}) + \omega X_{c,d} + \lambda_c + \theta_d + \epsilon_{c,d} \]  

Notation follows the difference-in-differences design. One exception is that \(active\_policy_{c,t+i}\) coincides with the date of the shelter-in-place policy change, not the policy status (0 or 1) itself. For consistency, heteroskedasticity-robust standard errors are calculated using a three way clustering approach following the main effects in equation 1.\(^3\) We calculate total effects in the threshold-separated tests in Figure 3 Panel C and Figure A-1 Panels A/C/E via a split-sample approach as suggested by Goodman-Bacon (2018).\(^4\)

### 3 Results

In this section, we present main results as well as a battery of robustness checks. We present supplemental analyses in the final subsection.

#### 3.1 Main Effects

We find that shelter-in-place policies reduce movement and that these changes are influenced by economic endowments. Table 1 presents the benchmark results and first robustness checks. Visualization of these point estimates is presented in Figure 2 Panel C. First, on average, we find a significant decline in population movement after the local shelter-in-place policies were enacted (\(\hat{\beta}_{active\_policy} = -0.032, p = 0.001\)). Second, counties with above median income comply

\(^3\)Results are similar if we calculate standard errors at the state level instead.

\(^4\)We confirm the divergent compliance patterns in the split-sample design correspond to marginal effects in a fully interacted event study design.
with shelter-in-place policies by reducing movement by an additional 72% relative the baseline policy impact ($\hat{\beta}_{\text{income}} = -0.024, p = 0.002$). These results are also presented in Table 1.

We assess the ‘common trends’ assumption in Figure 2 Panel B using the event study design outlined above. Notice that the effect of shelter-in-place policies is statistically indistinguishable from zero prior to the onset of local mandates (green line). Population movement significantly declines after the first full day of shelter-in-place ($\hat{\beta}_{t+1} = -0.052, p = 0.002$) and remains stable in subsequent days. This provides baseline support for a causal interpretation of our statistical estimates.

### 3.2 Robustness Checks

We next consider a number of robustness checks, sequentially adding additional control models to the benchmark specification. The timing of shelter-in-place policies could be (a) correlated with related government interventions, including school closures, bans on social gathering, restrictions on eviction notices, and utility cutoffs. The onset of shelter-in-place might also be driven by (b) the severity of COVID-19 (cumulative cases and deaths). We add these parameters to the benchmark model and results remain consistent (square and diamond symbols respectively).

Next, we account for the correlation between local economic endowments and (c) exposure to the ongoing trade war (Fajgelbaum et al., 2020), (d) political partisanship, and (e) exposure to slanted media coverage of the COVID-19 pandemic. Evidence from a series of event studies presented in FigureA-1 suggests compliance with shelter-in-place varies with these factors. To study (c), we rely on data from (Fajgelbaum et al., 2020) measuring exposure to retaliatory tariffs. In (d), we rely on county-level data on Republican vote share in the 2016 elections. In (e), we gather data on Designated Market Area (DMA) penetration by the Sinclair Broadcast Group and focus on their Fox News affiliates. We then capture trends in population movement
correlated with these factors through the use of interacted day fixed effects; this approach flexibly accounts for potentially non-linear effects during our sample. These factors are sequentially added to the model and results are indicated in Figure 2 Panel C with triangle, plus, and grey cross symbols respectively.

It is also possible that local economic endowments are tied to (f) sector-specific dynamics, specifically agricultural and manufacturing industries. These local labor conditions might also make counties more likely to be exposed to import tariffs, which undermine access to foreign suppliers. We allow these effects to vary by day during our sample and include these parameters in the model, indicated by a solid circle symbol.

Next, we account for (g) the potential correlation between income and unemployment. We do this in two ways. First, we flexibly account for unemployment during the period when our income measure is produced using interacted day-specific effects. Second, we track real time demand for information about how to file for unemployment via geocoded Google search activity (Choi and Varian, 2009). We compile DMA-day specific search intensity and use the benchmarking procedure introduced by Goldsmith-Pinkham and Sojourner (2020). We then calculate the cumulative search intensity for each region and assign this value to each corresponding county-day in our sample. We introduce these measures to the model and the corresponding main effects are denoted by solid grey diamonds.

We also (h) incorporate measures of county-specific population density and allow these effects to vary by day. These effects are noted by a solid grey square. In the most saturated model (h), the main effects remain substantively meaningful and statistically precise ($\hat{\beta}_{\text{size}} = -.019, p = 0.004; \hat{\beta}_{\text{income}} = -.016, p = 0.02$).
3.3 Supplemental Analyses

In Figure 3 we conduct several additional analyses. In Panel B, we flexibly estimate the marginal effect of average income in standardized levels (with mean = 0, standard deviation = 1). There is clear evidence of an increasing marginal effect (in absolute terms) until approximately two standard deviations above the mean, where the marginal effect levels off. This suggests that economic endowments reach a saturation effect at this level in the distribution, where an additional dollar of income does not cause an additional marginal decrease population movement. Using a median-based measure absorbs this dynamic by calculating an average across counties above the average threshold value. If, instead we relied on a linear specification of the marginal effect (using average income in standardized levels), the slope might be attenuated by counties with very high income levels. We present these results in Figure A-2 (and Table A-1). Notice that the results are highly stable in the linear specification as well, though the interpretation of the magnitude of the effects changes (to standard deviation shifts in income). In Panel C, we introduce an event study design that leverages the median split of income. Population movement in counties above the median in income decreases sharply the day after policies go into effect ($\hat{\beta}_{t+1}^{\text{above\_income}} = -.084, p = 0.001$), but the population movement below the median remains unaffected ($\hat{\beta}_{t+1}^{\text{below\_income}} = -.002, p = 0.938$).

Taken together, these results suggest economic endowments meaningfully influence compliance with shelter-in-place policies. Although the event study results provide support for the common trends assumption, it is also possible that our dynamic estimates from the fully saturated benchmark model do not fully account for potential sources of bias. Even if the results presented in this study represent descriptive rather than causal evidence, the role economic endowments play in shaping whether and how communities engage in social distancing during the COVID-19 pandemic remains a first order public policy challenge.
4 Conclusion

Shelter-in-place policies and related ‘lockdown’ quarantine orders are among the most economically costly and robust measures governments have taken to combat the COVID-19 pandemic. Our evidence demonstrates that these policies effectively reduce population movement overall and, as evidence in medicine and epidemiology indicate, slow the spread of COVID-19 by reducing interpersonal transmission risks. Compliance with shelter-in-place directives is individually costly and requires behavioral changes across diverse sub-populations. Variation in compliance, however, represents a local and regional risk to public health. Clarifying how income and other economic factors influence compliance provides actionable insights for policy makers and public health officials. We provide robust evidence that economic endowments significantly influence when and by how much communities engage in social distancing.

Our evidence suggests that at-risk, impoverished communities exposed to economic dislocation are the least likely to comply with shelter-in-place policies. Given the prevalence of comorbidity factors and poor health insurance coverage among these populations (Akinyemiju et al., 2016), the risks of continued exposure are substantial. These communities are also more likely to lack sufficient testing and contact tracing capacity to monitor and thwart COVID-19 outbreaks. This means any spread due to non-compliance within these communities will be difficult to detect until a potentially large number of symptomatic infections or deaths occur. Consistent with the findings of this study, targeted economic relief such as direct transfers and increased unemployment benefits may contribute to higher compliance and, as a consequence, limit potential spread of COVID-19 among economically disadvantaged populations.
References


Figure 1: Design for quantifying and studying localized population movement using cellphone data

Panel (A): Cellphone location data is gathered passively and used to measure population movement by origin county and day. Intersecting circles indicate social proximity. Data is gathered on timing of shelter-in-place ordinances. Study assesses change in travel patterns which indicate social distancing (reduced social contact). Panel (B): Variation in cellphone-derived movement data over study period (February 23 to April 1, 2020). Base rate for daily movement after March 8 (onset of COVID-19 in United States) is history of day-of-week movement data collected in prior periods. Local polynomial regression indicates 25% decline (national average) in movement during study period.
Figure 2: Staggered introduction of shelter-in-place ordinances and localized impact on population movement

Panel (A): Map of staggered introduction of shelter-in-place policies across the United States. Darker shades of green indicate earlier policy dates, beginning on March 19 (California). Localized policies were adopted in more than 125 counties. Data on state-level policies corrected for additional policies up until April 7. Data on county-level policies is from Painter and Qiu (2020). Additional details in Appendix.

Panel (B): Event study design using leads and lags of policy change to assess pre-treatment and post-treatment changes in population movement. No evidence of anticipation effects. Substantive and stable declines in movement estimated after the first full day of shelter-in-place. 90% confidence intervals reported.

Panel (C): Difference-in-differences results for estimated impact of shelter-in-place. Baseline effect of policy plot in upper left. Heterogeneous (marginal) effects plotted below. Additional coefficient plots represent sequential regression models with additional control variables and fixed effects.
Figure 3: Event study designs indicate varying compliance

Panel (A): Map of varying income levels by county. Legend in lower right corner. Panel (B): Flexible marginal effects of average income in standardized levels. These effects were estimated using the *interflex* package compiled by Hainmueller, Mummolo and Xu (2019). Distribution trimmed above and below for top percentile (98% of sample remaining). Panel (C): Event study design using leads and lags of policy change in above versus below the median level of income by county. Event study results suggest large reduction population movement in above median counties; no change in below median counties. 90% confidence intervals reported.
Table 1: Heterogeneous impact of shelter-in-place orders via economic endowments mechanism (above median threshold)

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**MODEL PARAMETERS**

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Notes: Outcome of interest is population movement. The quantities of interest are the main effect of shelter-in-place policies and marginal effect of income (above median threshold = 1). Columns 1-9 include county, day, state × week, and income-above-median × day fixed effects. Columns 2-9 correspond to results (a)-(h) in the main text. Additional parameters included in models indicated in table notes (below estimated effects). Heteroskedasticity robust standard errors clustered by county, calendar day, and state × week are reported in parentheses. Stars indicate *** p < 0.01, ** p < 0.05, * p < 0.1.
Appendix

A1 Theoretical Model

Our basic model considers an environment, in which the “shelter-in-place” ordinance is issued, and agents decide whether to comply with these orders or not. Agents have heterogeneous economic endowments (income). The economic endowments in the model capture assets available to individuals as they decide whether or not to comply with shelter-in-place. Endowments (income) in the model capture the sum of baseline endowments and subsequent economic shocks. Agents receive information about the probability to get infected if they do not comply. The information is provided by a media that might have a bias over the importance of compliance. We analyze the decision that agents make as a function of their income and opportunities to get additional income out of non-compliance; also, we analyze the effect of aggregate shocks to the economy.

**Setup** Consider a continuum of agents who have income of $w_i$; for each agent $i \in [0, 1]$, $w_i$ is obtained from distribution $F(\cdot)$ over $[w, \bar{w}]$, $\bar{w} > w > 0$. The only decision that each agent makes is whether or not to comply with the “shelter in place” ordinance, $a_i \in \{c, n\}$, for “comply” and “not comply”, respectively. If agent $i$ complies, she consumes her income $w_i$. If she does not comply, she gets an additional income $r$, in which case her consumption is $w_i + r$, yet risks getting coronavirus. We assume that $r$ incorporates, in addition to benefits of noncompliance, the expected costs, e.g., fines. The utility function of agent $i$ is quasilinear: $u_i = \ln (w_i + r \mathbb{1}_{a_i = n}) - pH$, where $p$ is the probability to get the disease, and $H > 0$ is the expected health damage to the infected person.

Agents’ optimal decisions about compliance depends on their assessment of risk probabilities. There are two possible states of the world, $s \in \{C, N\}$, for “coronavirus threat” and “no threat”; the *ex ante* probabilities are $P(C) = \theta \leq \frac{1}{2}$ and $P(N) = 1 - \theta$. Agent $i$’s utility
depends on both the action and the state of the world as follows.

\[
\begin{align*}
\ln w_i & \text{ if } a_i = c, \\
\ln (w_i + r) - H & \text{ if } a_i = n, \ s = C, \\
\ln (w_i + r) & \text{ if } a_i = n, \ s = N.
\end{align*}
\]

Before making the decision, agents get additional information from a media outlet, which operates as follows. The media outlet receives information about the true state of the world \(s \in \{C, N\}\), and is committed to report a recommendation \(\hat{s} \in \{\hat{c}, \hat{n}\}\) for “comply” and “go”.\(^5\) If \(s = N\), then the report is \(\hat{s} = \hat{n}\). If \(s = C\), then the report is \(\hat{s} = \hat{n}\) with probability \(m\) and \(\hat{s} = \hat{c}\) with probability \(1 - m\). Thus, a media with low \(m\) is putting more emphasis on dangers of the virus, while high \(m\) corresponds to the desire to downplay the danger.

**Analysis** Suppose that citizen \(i\) watched the news with the media slant \(m\). If the media report is \(\hat{s} = \hat{c}\), then she infers that the probability to get the virus is

\[P(s = C|\hat{s} = \hat{c}) = 1.\]

Similarly, if the media report is \(\hat{s} = \hat{n}\),

\[P(s = C|\hat{s} = \hat{n}) = \frac{P(\hat{s} = \hat{n}|s = C)P(C)}{P(\hat{s} = \hat{n}|s = C)P(C) + P(\hat{s} = \hat{n}|s = N)P(N)} = \frac{m\theta}{m\theta + 1 - \theta},\]

which is an increasing function of \(\theta\), the *ex ante* probability of danger, and \(m\), the media slant.

Now, when agent \(i\) chooses \(c\) over \(n\) when the media signal is \(\hat{s} = C\), the choice is trivial. The condition is

\[E u_i (c|\hat{s} = \hat{c}) \geq E u_i (n|\hat{s} = \hat{c}).\]

The utility of complaince with orders when the signal is $\hat{s} = \hat{c}$:

$$Eu_i(c|\hat{s} = \hat{c}) = P(s = C|\hat{s} = \hat{c}) \ln w_i + P(s = N|\hat{s} = \hat{c}) \ln w_i = \ln w_i.$$  

The expected utility of non-compliance is

$$Eu_i(n|\hat{s} = \hat{c})$$

$$= P(s = C|\hat{s} = \hat{c}) (\ln(w_i + r) - H) + P(s = N|\hat{s} = \hat{c}) \ln(w_i + r)$$

$$= \ln(w_i + r) - H.$$  

Now let $w^* (\hat{c})$ be a solution of the following equation.

$$\ln w = \ln (w + r) - H.$$  

Since $\ln (w + r) - \ln w$ is a decreasing function of $w$, there exists at most one such solution; the appropriate choice of $w$ guarantees the existence of a solution. $w^* (\hat{c})$ is the critical threshold: agents with less income than $w^* (\hat{c})$ do not comply, while those with the income exceeding $w^* (\hat{c})$ shelter in place.

The comparative statics is straightforward. When the expected damage to the infected person, $H$, goes up, the threshold $w^* (\hat{c})$ goes down, i.e., more people comply with the ordinances.

When the additional income $r$ goes up (or, equivalently, fines for non-compliance go down), the threshold $w^* (\hat{c})$ goes up.

What happens when the media report is $\hat{s} = \hat{n}$? When $Eu_i(n|\hat{s} = \hat{n}) \geq Eu_i(c|\hat{s} = \hat{n})$?

$$Eu_i(n|\hat{s} = \hat{n}) = P(s = N|\hat{s} = \hat{n}) \ln(w_i + r) + (1 - P(s = N|\hat{s} = \hat{n})) (\ln(w_i + r) - H)$$

$$= \ln(w_i + r) - \frac{m\theta}{m\theta + 1 - \theta} H.$$  

$$Eu_i(c|\hat{s} = \hat{n}) = \ln w_i.$$  

Now, there is a new threshold $w^* (\hat{n})$ that separates compliers and non-compliers when the media report is $\hat{n}$. Again, agents with income below $w^* (\hat{n})$ do not shelter in place, and those with
income above the threshold comply. The comparative statics with respect to $H$, the cost of getting the disease, and $r$, the additional income, is similar to the previous case. Also, an increase in the *ex ante* probability of danger, $\theta$, leads to a lower threshold $w^* (\widehat{n})$. With the media slant $m$ increasing, the threshold goes down as well: with a media slanted against emphasizing the danger of the coronavirus, the signal $\widehat{s} = \widehat{n}$ is less persuasive. It is a straightforward exercise to show that $w^* (\widehat{n}) > w^* (\widehat{c})$, i.e., more people shelter in place when the report recommends to shelter ($\widehat{c}$), then when the report recommends not to ($\widehat{n}$).

In addition to results about individual decisions as a function of income and income, one can do comparative statics with respect to the aggregate income distribution. For example, suppose that county 1 is wealthier than county 2; e.g., county 2 is more affected by a trade war. Mathematically, this corresponds to the distribution of income with c.d.f. $F_1 (\cdot)$ first-order stochastically dominating the distribution with c.d.f. $F_2 (\cdot)$: for any $w$, $F_1 (w) \leq F_2 (w)$ (Mas-Colell, Whinston and Green, 1995). As the threshold $w^* (\widehat{n})$ and $w^* (\widehat{c})$ are independent of the income distribution, there is a larger share of agents sheltering in place in the more well-to-do county 1.

Finally, how does the media bias, $m$, affect the people behavior?

Agents receive report $\widehat{s} = \widehat{c}$ with probability $\theta (1 - m)$ and report $\widehat{s} = \widehat{n}$ with probability $m\theta + 1 - \theta$. If the media report is $\widehat{s} = \widehat{c}$, then $\int_{w^* (\widehat{c})}^{w^* (\widehat{n})} dF$ people shelter in place; if $\widehat{s} = \widehat{n}$, $\int_{w^* (\widehat{n})}^{w^* (\widehat{n})} dF$ people do. To make the calculus tractable, let us assume that $F$ is a uniform distribution over $[w, \overline{w}]$. Then the expected number of non-compliers is as follows.

$$En(m) = \theta (1 - m) \cdot \frac{1}{w - w} (w^* (\widehat{c}) - w) + (m\theta + 1 - \theta) \cdot \frac{1}{w - w} (w^* (\widehat{n}) - w).$$

There are two effects of media slant on the expected number of non-compliers $En(m)$. First, when $m$ is higher, the probability that the report is $\widehat{s} = \widehat{c}$ is lower, and $w^* (\widehat{c})$ does not change with $m$. As $w^* (\widehat{n}) > w^* (\widehat{c})$ for any $m$, with a higher probability of report $\widehat{s} = \widehat{n}$, the number of
non-compliers increases with slant $m$. At the same time, conditional on report $\hat{s} = \hat{n}$, a higher level of slant $m$ leads to more sheltering in place, as citizens take the slant into account. In general, the outcome of an increase in $m$ is ambiguous. However, as $En(m)$ is a differentiable function, and $\frac{\partial En}{\partial m} > 0$ in the neighborhood of $m = 0$, there is a range of parameters for which more biased reports result in a higher noncompliance.

Proposition 1 summarizes the above discussion.

**Proposition 1** (i) There exist two thresholds, $w^*(\hat{c})$ and $w^*(\hat{n})$, such that any agent $i$ with $w_i > w^*(x)$ shelters in place, $a_i = c$, upon receiving signal $x \in \{\hat{c}, \hat{n}\}$ while every agent $i$ with $w_i < w^*(x)$ does not comply ($a_i = n$). More people comply when the media report is $\hat{c}$, i.e., $w^*(\hat{c}) < w^*(\hat{n})$.

(ii) If county 1 is wealthier than county 2, i.e., distribution of income $F_1(\cdot)$ first-order stochastically dominates distribution $F_2(\cdot)$, then there is a larger share of sheltering in place in county 1.

(iii) The lower is the agent’s ex ante belief that there is no danger (the lower is $\theta$), the higher are both thresholds $w^*(x), x \in \{\hat{c}, \hat{n}\}$, i.e., less people shelter in place. The higher is the cost of having the disease, $H$, or the lower is additional income that an agent can get by non-complying, $r$, the lower are both thresholds $w^*(x), x \in \{\hat{c}, \hat{n}\}$, i.e., more agents comply.

(iv) Suppose, additionally, that the distribution $F(\cdot)$ is uniform. There exists a threshold $\overline{m}$, $0 < \overline{m} \leq 1$, such that the expected number of non-compliers increases in media bias towards minimizing the threat, $m$, over $[0, \overline{m}]$.

**Remarks** Although our model is concerned with the distribution of income, rather than wealth, the qualitative results would be the same. Also, it is straightforward to incorporate people’s ability to borrow against their endowment.
Our model does not differentiate agents by their ability to generate income by non-complying. This might look unrealistic, as there is a high correlation between wealth and earning capacity. However, what matters for the decision to be non-complaint is the relative marginal income due to non-compliance which need not to be correlated with wealth. Also, our model abstracts away from many non-economic reasons for non-compliance.

In our basic model, the media is not strategic. However, it is straightforward to extend the model to the case in which the bias \( m \) is determined endogenously (e.g., Gehlbach and Sonin, 2014). The same model can be used to incorporate strategic censorship by the government. Also, we assume that there is no correlation between the income of a county and the media bias; however, if we assume such a correlation, this would have only strengthened our main findings.

Perhaps the most significant drawback of our very basic model is that the \textit{ex ante} probability of the environment being dangerous to an individual, \( \theta \), is independent of the share of people who decide, in equilibrium, to maintain social distancing and shelter-in-place. However, at the cost of more algebra, this can be incorporated in the model. That would require a new definition of equilibrium that will include the share of people who do not comply, then probability \( \theta \) being a function of the expected share of non-compliers. By the Brouwer’s fixed point theorem, there will be such \( \theta \) that will be both a function of the equilibrium share of non-compliers and result in every agent satisfying her incentive compatibility constraints.
Data Overview and Links to Materials

We outline the sources of each component of our data below.

- **Population movement** We rely on a measure of population movement derived from cell-phone location data. Location pings via Global Positioning System (GPS) capabilities of smartphones enable data processing firms to trace population movement across space from an origin site (‘home’). Data used in our analysis was shared by UNACAST and is accessible for academic research upon request (https://bit.ly/2RoEN4w). To standardize the scale of movement across counties, the data provider deseasonalizes variation after the onset of COVID-19 in the United States (March 8) using county-specific day-of-week trends from the period before (March 7 and before). Reductions in movement correspond to social distancing and, on average, reduced interpersonal mechanisms for viral transmission.

- **County-specific and state-wide policies** We study state-wide data compiled by Julia Raifman and collaborators. The data are available for download here: https://bit.ly/34p4Duk. The source files are available for review here: https://bit.ly/2Rs1vsg. This data includes information about the onset of shelter-in-place policies, bans on social interaction in group settings (restaurants, movies, gymnasiums), and school closures. We make several updates to the data, noting the dates of policy changes in Connecticut, Kentucky, South Carolina, Tennessee, and Texas. County-specific policies were compiled by Marcus O. Painter and Tian Qiu. For additional details, see Painter and Qiu (2020). This effort builds on data and local sources compiled by the New York Times here: https://nyti.ms/2UZHMCG. We make several adjustments to account for policies introduced after 1200 local time, shifting back the onset of the policy one calendar day. We thank them for generously sharing the pre-release version of this data.
- **Economic and political measures** Economic and political measures are drawn from Fajgelbaum et al. (2020) and available for download here: https://bit.ly/3aTo1SF. Tariff exposure is calculated by Fajgelbaum et al. (2020) by weighting retaliatory tariff changes variety-level 2013–17 trade shares as well as county-level sector data. Source data has multiple sources detailed in the original text.

- **COVID-19 cases and deaths** County-day level information about COVID-19 cases and related deaths are tracked and compiled by the New York Times from various government sources. The tracker is available via their Github page here: https://bit.ly/2wrK0RB.

- **Sinclair exposure** We classify exposure to Fox News stations operated by the Sinclair Broadcast Group using a map published by Sinclair and archived by Reproducible Journalism (https://bit.ly/39U1iVn). The .json file was converted to a .csv and linked with a Designated Market Areas (DMA) shapefile available here: https://bit.ly/3eab8FI. The SBG station data is available for review here: https://bit.ly/3c6w52v (download link). Counties were assigned to the DMA with a larger share of coverage.

- **County Shapefiles** We visualize county-level variation in policy changes and economic and political measures using the 2016 TIGER/Line shapefile made available by the US Census Bureau (Department of Commerce) via the DATA.GOV initiative. Data is available for download here: https://bit.ly/2JU4ZQe.
Figure A-1: Event study designs indicate varying compliance by trade exposure, presidential vote share, and slanted media exposure

Panel (A): Event study design using leads and lags of policy change in above versus below the median level of exposure to retaliatory tariffs from US trade war. Event study results suggest large reduction in population movement in below median counties (less trade exposure); no statistically significant changes in above median counties (more economic dislocation). 90% confidence intervals reported. Panel (B): Map of retaliatory tariff exposure by county. Legend in lower right corner. Panel (C): Event study design using leads and lags of policy change in above versus below the median GOP vote share in 2016 presidential election (Trump). Event study results suggest large reduction in population movement in below median counties (fewer votes cast for Trump); no statistically significant changes in above median counties (more Trump votes). Panel (D): Map of Trump vote share by county. Legend in lower right corner. Panel (E): Event study design using leads and lags of policy change in counties with and without exposure to a Sinclair Broadcast Group Fox station. Event study results suggest large reduction population movement in counties without access; no change in counties with access. Panel (F): Map of SBG Fox station exposure by county.
Figure A-2: Estimated marginal effects of average income in standardized levels on population movement

Difference-in-differences results for estimated impact of shelter-in-place. Baseline effect of policy plot in upper left. Heterogeneous (marginal) effects plotted below. Quantity of interest is average income in standardized levels (mean =0; standard deviation = 1). Additional coefficient plots represent sequential regression models with additional control variables and fixed effects. 90% confidence intervals reported.
Table A-1: Heterogeneous impact of shelter-in-place orders via economic endowments mechanism (standardized levels)

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<td>-0.0108***</td>
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**MODEL PARAMETERS**

- County Fixed Effects: Yes, Yes, Yes, Yes, Yes, Yes, Yes, Yes, Yes
- Day Fixed Effects: Yes, Yes, Yes, Yes, Yes, Yes, Yes, Yes, Yes
- State × Week Fixed Effects: Yes, Yes, Yes, Yes, Yes, Yes, Yes, Yes, Yes
- Income × Day Fixed Effects: Yes, Yes, Yes, Yes, Yes, Yes, Yes, Yes, Yes
- Govt Interventions: No, Yes, Yes, Yes, Yes, Yes, Yes, Yes, Yes
- COVID severity: No, No, No, Yes, Yes, Yes, Yes, Yes, Yes
- Retaliatory Tariffs × Day FE: No, No, No, No, Yes, Yes, Yes, Yes, Yes
- Trump Vote Share × Day FE: No, No, No, No, No, Yes, Yes, Yes, Yes
- Slanted Media Exposure × Day FE: No, No, No, No, No, No, Yes, Yes, Yes
- Sector Shares + Imports × Day FE: No, No, No, No, No, No, No, Yes, Yes
- Unemployment × Day FE: No, No, No, No, No, No, No, No, Yes
- Pop. Density × Day FE: No, No, No, No, No, No, No, No, Yes

**MODEL STATISTICS**

- No. of Observations: 115082, 115082, 115082, 115082, 115082, 115082, 115082, 115082, 115082
- R²: 0.731, 0.731, 0.731, 0.733, 0.736, 0.736, 0.738, 0.739, 0.740

Notes: Outcome of interest is population movement. The quantities of interest are the main effect of shelter-in-place policies and marginal effect of income (standardized levels). Columns 1-9 include county, day, state × week, and income × day fixed effects. Columns 2-9 correspond to results (a)-(h) in the main text. Additional parameters included in models indicated in table notes (below estimated effects). Heteroskedasticity robust standard errors clustered by county, calendar day, and state × week are reported in parentheses. Stars indicate *** p < 0.01, ** p < 0.05, * p < 0.1.