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Economic and Epidemiological Effects of Mandated and Spontaneous Social Distancing

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Economic and Epidemiological Effects of Mandated and Spontaneous Social Distancing*

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Abstract

Based on a standard epidemiological model, we derive and apply empirical tests of the hypothesis that contacts, as proxied by mobility data, have an effect on the spread of the coronavirus epidemic, as summarized by the reproduction rates, and on economic activity, as captured by subsequent initial claims to unemployment benefits. We show that changes in mobility through the first quarters of 2020, be it spontaneous or mandated, had significant effects on both the spread of the coronavirus and the economy. Strikingly, we find that spontaneous social distancing was no less costly than mandated social distancing. Our results suggest that the rebound in economic activity when stay-at-home orders were lifted was primarily driven by the improvement in epidemiological parameters. In other words, without the reduction in the reproduction rate of the coronavirus, we could have expected a doubling down on spontaneous social distancing.

JEL classifications: E1, E3, I1.
Keywords: infectious disease, epidemic, recession, COVID-19.

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1 Introduction

By the end of March 2020, nearly four months after the first detection of significant coronavirus infections in China, most advanced economies adopted measures restricting people's movements and activity on their territory, introduced tough controls at their borders, and mandated norms implementing social distancing. If only with some delay, governments converged on the idea that some restrictions were required to reduce the human cost of the disease—strongly influenced by early scenario analyses in which an uncontrolled and rapid spread of the disease would have overwhelmed national health systems and caused a sharp rise in mortality rates.[1] At the same time, mobility fell precipitously (although not uniformly across locations) as individuals took precautions. During the subsequent months, contagion and death rates, while high, turned out to be much lower than indicated by these early scenario analysis, as social distancing, whether mandated or spontaneous, became widespread practice.

A key question in academic and policy debates concerns the extent to which social distancing is effective in reducing contagion and mortality, and, most crucially, whether, for given epidemiological effects, the economic costs of social distancing can be expected to be lower when driven by individual decisions, as opposed to policy measures. These questions are obviously complex, as the evolution of the disease over time responds to a number of factors, including environmental factors (e.g., extreme hot or cold weather may bring people to to spend more time indoors), mutation in the virus (at the end of 2020, a new surge associated with more infectious variants of the virus motivated once again the widespread adoption of strict lockdown policies) as well as the adoption and efficacy of precautions (such as wearing masks or washing hands) in social contacts. With these considerations in mind, we exploit cross-sectional epidemiological, institutional and mobility data for the U.S. states, to derive a test of the epidemiological and economic effects of social distancing—distinguishing the latter depending on its spontaneous or mandated origin.

We show that changes in mobility through the first quarters of 2020 slowed down both the spread of the coronavirus and economic activity, regardless of whether these changes stemmed

[1] Among the leading papers that have formalized this view ealry on, see Eichenbaum, Rebelo, and Trabandt (2020), Jones, Philippon, and Venkateswaran (2020) and Alvarez, Argente, and Lippi (2020). The literature on the economic effects of the COVID-19 pandemic has grown very fast, see Atkeson (2020), Alfaro, Chari, Greenland, and Schott (2020),Baker, Bloom, Davis, Kost, Sammon, and Viratyosin (2020), Guerrieri, Lorenzoni, Straub, and Werning (2020), and Koren and Petó (2020) among many others.

from individual or policy decisions. Strikingly, our evidence suggests that spontaneous social distancing was no less costly than stay-at-home orders.

We derive our empirical framework from the standard epidemiological model. We test the null hypotheses is that contacts, as proxied by mobility data, have an effect on the spread of the epidemic, as summarized by the reproduction rates, and economic activity, as captured by initial claims to unemployment benefits. Our sample consists of state-level data for the United States from March through September 2020. We proxy contacts using Google mobility data, and instrument mobility with either the stay-at-home orders issued by individual U.S. states, or political leanings as captured by the share of the vote for the Republican presidential candidate in the 2016 elections by state. Specifically, in a first test, we run a panel regression model instrumenting changes in mobility with stay-at-home orders, taking advantage of the different timing of these orders across states. In a second, cross-sectional, test, we investigate the evolution of contagion in the two-week period in March 2020 that preceded any mandatory measures at the state level. This sample choice implies that all observed variation in mobility stems from spontaneous decisions. Google data suggest that, while much of the reduction in mobility had already occurred by the time the first stay-at-home order was imposed, this initial mobility reduction was far from homogeneous across states. We instrument mobility with political leanings, drawing on the results by Gollwitzer et al. (2020), who document a correlation between these leanings and the spread of COVID-19. Given our focus on the first part of March, before the introduction of mandatory measures, we collapse the time dimension of our initial panel regression and rely only on the cross-sectional variation at the state level. As epidemiological outcome, we use, alternatively, the reproduction rates estimated by Fernández-Villaverde and Jones (2020) and the rates estimated by Systrom, Vladek, and Krieger (2020).

Our main results are as follows. Concerning mandatory social distancing, based on our panel analysis, we find that, at the first stage of our regression model, stay-at-home orders push up the residential mobility index 1.85 percent (capturing an increase in time spent at home). At the second stage, a 1 percent increase in the instrumented residential mobility reduces the running reproduction rate about 3.5 percent, all else equal. Putting these two estimates together, on average, the stay-at-home orders led to a decline in the reproduction rate of about $1.85 \times 3.5 \approx 6.5$ percent. In other words, starting from a basic reproduction rate of 2, the stay-a-home order would reduce it to about 1.9. Correspondingly, our regression results point to an increase in the unemployment rate of roughly 0.3 percentage point for every week that the stay-at-home orders were in force. With a median duration of 6 weeks and the orders applying to much of the country, this could account for about a 2 percentage points rise in unemployment.

Our estimates imply that most of the fall in mobility was linked to spontaneous social distancing—a point stressed early on by Goolsbee and Syverson (2021). To study the effect of spontaneous distancing, we take advantage of the fact that no mandatory measures were enacted in the 14-day period through March 17 2020, two days before the first stay-at-home order went into effect in California. Remarkably, for the initial claims to unemployment benefits, the elasticity estimated in this exercise for spontaneous social distancing is close to the corresponding elasticity estimated from mandated distancing (our point estimates are, respectively, 0.15 and 0.17). At the margin, social distancing, whether spontaneous or not, has analogous economic effects. However, the elasticity of the reproduction rate to spontaneous mobility reductions is lower (our point estimates for spontaneous and mandated social distancing are, respectively, 2.3 and 3.5). In other words, for the same economic impact, a decline in spontaneous mobility leads to a smaller decline in the reproduction rate. Or, to put it in another way, the economic costs of containing the reproduction rate are no lower for spontaneous than for mandated reductions in mobility.

These findings suggest that, while economic activity rebounded as stay-at-home orders were lifted, this rebound was possible in large part because of the improvement in the epidemiological parameters—that is, without the observed reduction in the reproduction rate of the coronavirus, we could have expected a doubling down on spontaneous social distancing. Our analysis cannot rule out nonlinearities such that the marginal costs of reducing the spread of the disease rises progressively with the reduction in mobility. However, one may note that, since spontaneous social distancing preceded the imposition of stay-at-home orders, such non-linearities would not undermine our main conclusions.

Several other papers have sized empirically the economic effects of mandated social distancing, including Allcott et al. (2020) and Coibion, Gorodnichenko, and Weber (2020). Our approach is closest to Gupta et al. (2020), who also use a difference in difference approach to size the effects on the labor market. Our framework helps us distinguish between the direct effects of the structured policies through reduction in mobility and outcomes related to spontaneous social distances predating the policies. Goolsbee and Syverson (2021) also rely on a difference in difference estimation method but use more capillary data at the local level. Nonetheless, their results on the economic effects of mandated social distancing are broadly in line with ours. Alternative approaches to estimating the effects of mandated social distancing measures are offered by Chernozhukov, Kasahara, and Schrimpf (2021) and Huang (2020). They focus on epidemiological effects, whereas we are also interested in a comparison of the epidemiological benefits and of the economic consequences of mandated and spontaneous social distancing.

The rest of the paper is structured as follows. Section 2 sets the stage for our analysis by providing and discussing evidence on the dynamic of the COVID-19 pandemic in the United States in the first three quarters of 2020, and the effects of social distancing on the spread of the disease and unemployment across U.S. states. Throughout our analysis, we will make extensive use of mobility data to approximate social distancing and trace its effect on the economy. Section 2 describes a one-group SIRD model—capturing how a disease spreads by direct person-to-person contact in a population. Section 3 reviews stylized facts on the diffusion of the disease over time and across states in the United States, including data on mobility and health measures adopted at state level. Drawing on the SIRD model, Section 4 specifies a simple econometric framework and provides evidence on the effects of social distancing on the dynamic of the pandemic and employment.

2 A Baseline One-Group SIRD Model

The one-group SIRD model in this section follows Fernández-Villaverde and Jones (2020) — broader introductions to epidemiological modeling are given in Hethcote (1989), Allen (1994), and Brauer, Driessche, and Wu (2008). Time is discrete and measured in days. At every instant in time, the total population N is divided into the classes of:

1. susceptible S_t consisting of individuals who can incur the disease but are not yet infected;
2. infective I_t consisting of individuals who are infected and can transmit the disease;
3. resolving R_t consisting of sick individuals who are no longer infective;
4. recovered (or, equivalently, cured) C_t consisting of individuals who have recovered from the disease;
5. dead D_t consisting of individuals who died from the disease.

This model differs from the standard SIRD model by distinguishing between the infective and the resolving class. Fernández-Villaverde and Jones (2020) found this distinction necessary to obtain a good model fit in their empirical application to U.S. data.

An important assumption of standard SIRD models is that "law of mass action" applies: The rate at which infective and susceptible individuals meet is proportional to their spatial density $S_t I_t$. The effective contact rate per period β_t is the average number of adequate contacts per infective period. An adequate contact of an infective individual is an interaction that results in infection of the other individual if that person is susceptible. Thus, β_t can be expressed as the product of the average of all contacts q_t and the probability of infection (transmission risk) given contact between an infective and a susceptible individual, μ_t.

It is important to note that the effective contact rate is not constant but can vary over time for a number of reasons. First, an individual's number of contacts, q_t, can drop in a pandemic because of mandated social restrictions (e.g., school closures, closures of shops and restaurants, stay-at-home orders) or voluntary adjustments of behavior (e.g., online shopping instead of in-person shopping, refraining from attending larger gatherings). As both mandated and spontaneous contact restrictions may take place simultaneously, it may be challenging to disentangle their effects on β_t. We may note, however, that from the perspective of our study restrictions have an impact on the economy regardless of whether they are mandated or spontaneous in nature. Second, the probability of infection given contact between an infectious and a susceptible individual μ_t can vary over time. In the case of COVID-19, this probability is influenced both by human behavior (e.g., masks, keeping sufficient physical distance) and by the characteristics of the virus (e.g., transmission in closed versus open spaces, sensitivity to temperature and seasonality, aggressiveness of the virus strains).

In detail, we write the discrete time SIRD model as:

$$S_{t+1} = S_t - \beta_t S_t I_t / N, \tag{1}$$

$$I_{t+1} = I_t + \beta_t S_t I_t / N - \gamma I_t, \tag{2}$$

$$R_{t+1} = R_t + \gamma I_t - \vartheta R_t, \tag{3}$$

$$C_{t+1} = C_t + (1 - \varpi)\,\vartheta R_t, \tag{4}$$

$$D_{t+1} = D_t + \varpi \vartheta R_t, \tag{5}$$

$$N = S_t + I_t + R_t + C_t + D_t, \quad (6)$$

with the initial conditions $S_0 > 0$ and $I_0 > 0$. In addition, $S_t \geq 0$, $I_t \geq 0$, and $S_t + I_t \leq 1$. Total new infections at time t are given by $\beta_t S_t I_t / N$. Infectiousness resolves at the Poisson rate γ. A person in the resolving class (R_t) either recovers (C_t) with probability $1 - \varpi$ or dies (D_t) with probability ϖ. The recovery rate is denoted by ϑ. In principle, the recovery rate and the death rate could also be time-varying to reflect advancements in medical treatment as the pandemic progresses.

The basic reproduction number $R_{0,t} = \frac{\beta_t}{\gamma}$ determines whether the infectious disease becomes an pandemic, i.e., the disease goes through the population in a relatively short period of time. This is the case for $\frac{\beta_t}{\gamma} > 1$; otherwise, the number of infective individuals decreases to zero as time passes. If $R_{0,t} \leq 1$, there is no pandemic, and the number of infective individuals converges monotonically to zero.

3 The Dynamic of the COVID-19 Spread in the United States

Conditional on keeping the effective contact rate β, with an empirically relevant reproduction rate equal to 2, almost the entire population is infected in a matter of months. According to leading scenarios debated in March 2020, for instance, it could not be ruled out that between 15 and 20 percent of the U.S. population could have simultaneously developed symptoms, and that, over a short time frame, 20 percent of these symptomatic individuals would have required hospitalization.[2] These developments would have put devastating pressure on the health care system.

Scenarios conditional on a constant β played a crucial role in motivating stark health measures in many countries—for this reason, we will study this type of scenario as a benchmark reference below. Remarkably, however, these grim developments did not come to pass. Figure 1 superimposes data for the spread of COVID-19 in the United States, death rates and confirmed cases, and data on the timing of stay-at-home orders and changes in residential mobility—culled from cellphones, as captured in Google's mobility reports, and reflecting both trips towards

[2] For instance, see Ferguson et al. (2020).

residential addresses and time spent at those addresses.[3]

Tracking the spread of COVID-19 is no easy feat. Even the best available data are subject to important drawbacks. As Figure 1 shows, confirmed new cases surged in March 2020, reached a first peak in early April, a second peak in mid-July and climbed back up through the fall. Using confirmed new cases to measure the intensity of the pandemic is challenging as severe rationing of testing at the beginning of the pandemic kept the data artificially low. Data on death rates do not suffer from that problem and confirm at least three cycles for the spread of the disease, with death rates climbing again through October, albeit with a delay relative to the number of confirmed cases. However, the relationship between the spread of the disease and death rates can also vary as new treatment protocols are developed or the age composition of infected individuals evolve, given that older individuals experience greater mortality rates. The middle panel of the figure shows the reproduction rate for the model in Equations (1)-(6) estimated by Fernández-Villaverde and Jones (2020) based on data on death rates. The solid black line shows the overall estimate for the United States. Two cycles are clearly visible in the estimates of the reproduction rate. The state-level estimates show much greater variation, as indicated by the point-wise maximum and minimum dashed red lines for these estimates.

Figure 1 also shows that stay-at-home orders were put in place at different points in time across states, roughly within a three week window from mid-March to early April.[4] These orders had a median duration of six weeks, but the duration also varied considerably by state. Twelve states did not impose stay-at-home orders. In the states that did, the shortest orders lasted three weeks and the longest, for California, is still standing in parts of the state at the time of writing.

The figure suggests that social distancing contributed significantly to slowing down the spread of the disease. It also shows that mobility capturing time spent at home ramped up even before the imposition of stay-at-home orders at the regional level. We will take advantage of the timing of these events to gain some insight on the relative role of spontaneous vs. mandated social distancing in driving the evolution of the disease.

[3] The data for death rates and confirmed cases are from JHU CSSE (2020), also see Dong, Du, and Gardner (2020). The data on stay-at-home orders are from Raifman et al. (2020). The mobility data are from Google LLC (2020).

[4] The earliest stay-at-home order started in California on March 19.

4 The Effects of Social Distancing

In this section we provide evidence that social distancing, be it spontaneous or mandatory, has comparable epidemiological and economic effects. Specifically, based on the epidemiological model, we derive and apply two empirical tests of the hypothesis that contacts, as proxied by mobility data, have an effect on the reproduction rate and the initial jobless claims. First, we will focus on changes in mobility in response to stay-at-home orders, using a difference-in-difference approach. Then we will investigate the dynamic evolution of contagion in the two-week period in March that preceded any mandatory measure, based on cross-sectional evidence.

For both tests below, we derive our regression framework from the SIRD model described in Section 2. In the SIRD framework, the status of the pandemic is summarized by the reproduction rate

$$R_{0,t} = \frac{1}{\gamma}\beta_t. \tag{7}$$

where the effective contact rate β_t is the product of contacts q_t, normalized to 1, and the probability of transmission, μ_t. We can therefore express the reproduction rate as

$$\ln(R_{0,t}) \quad = \quad -\ln(\gamma) + ln(\mu_t) + \ln(q_t - r_t) \tag{8}$$

where the term r_t represents policy restrictions that can reduce the level of contacts. We will use this equation to derive a panel regression and a cross-sectional test. Atkeson, Kopecky, and Zha (2021) provide framework consistent with ours to decompose the reproduction rate but allow for a feedback mechanism between the reproduction and infection rates.

4.1 Mandated Social Distancing: A Panel Regression Approach

The relationship between the reproduction rate and contacts in Equation 8 can be mapped into the following panel regression equation:

$$\ln(R_{0,s,t}) = FE_m + bm_{s,t} + FE_s + e_{s,t}. \tag{9}$$

where the subscript s denotes the geographical region and the term $R_{0,s,t}$ is the regional counterpart to the aggregate $R_{0,t}$ in Equation 8. The dependent variable in our baseline, consistent with the model in Section 2, is the reproduction rate estimated by Fernández-Villaverde and Jones (2020). We average the daily estimates by these authors to the weekly frequency and use readings for the 48 U.S. states in their dataset and the District of Columbia.[5] We use monthly fixed effects, FE_m, to capture the time-varying probability of transmission μ_t, which might depend on taking precautions such as frequent hand-washing and mask-wearing that have become more prevalent with the spread of the virus.[6] We proxy contacts $q_t - r_t$ at the regional level with the term $m_{s,t}$, the Google index for residential mobility in percent deviation from its value at the beginning of 2020, also averaged to the weekly frequency. The term FE_s denotes regional-level fixed effects, which allow for regional characteristics to influence the relationship between contacts and mobility. Finally, $e_{s,t}$ is a stochastic term in the relationship between contacts and mobility. Our main interest is the regression coefficient b. An important restriction imposed by our regression framework is that this coefficient does not vary across regions.

We estimate Equation 9 by two-stage least squares, using a dummy for the stay-at-home orders as an instrument for residential mobility. To lessen endogeneity concerns we lag the dummy for the stay-at-home orders by one week. At the first stage, we also allow for monthly and regional fixed effects. The estimation sample has starting points that vary by region, in line with regional variation in the spread of the disease. The earliest estimates of the reproduction rate are for the state of Washington, starting on March 12, 2020. By contrast estimates of the reproduction rate for Hawaii only start on August 7, 2020. The end point for our sample is September 28, 2020, across all regions. Overall, the sample includes 1204 observations.

Our estimates of Equation 9, first and second stage, are shown in Table 1. In the table, Column 1 indicates that stay-at-home orders push up the mobility index 1.85 percent. Returning to the table, Column 2 shows that a 1 percent increase in residential mobility reduces the reproduction rate by about 3.5 percent, all else equal. Putting the two estimates in columns 1 and 2 together, on average, the stay-at-home orders led to a decline in the reproduction rate of about $1.85 \times 3.5 \approx 6.5$ percent. In other words, starting from a basic reproduction rate of 2, the stay-a-home order would reduce it to about 1.9. One may note that, at its peak, the index of residential mobility increased by about 20 percent (reflecting an increase in time spent at home). Even if all states had enacted stay-at-home orders, our estimates would attribute only

[5] The dataset of Fernández-Villaverde and Jones (2020) excludes Wyoming and Montana.
[6] The framework of Atkeson, Kopecky, and Zha (2021) captures these effects as a time-varying wedge.

1.85 percentage points of this increase to those orders. Accordingly the great majority of the 20 percent increase was linked to spontaneous social distancing.

To gauge the effects of the stay-at-home orders on initial unemployment claims, we use a regression framework analogous to that of Equation 9. We consider

$$U_{0,s,t+1} = FE_m + b_u m_{s,t} + FE_s + e_{s,t+1}, \tag{10}$$

where the term $U_{0,s,t}$ represents initial jobless claims as a share of the working age population in region s at time t. For the sake of comparison, we select an estimation sample with exactly the same span of the sample for the regression of the reproduction rate. We also estimate Equation 10 by two-stage least squares, using a dummy for the stay-at-home orders as an instrument for residential mobility. Once again, using standard Durbin and Wu-Hausman tests, we fail to reject the null hypothesis that the instrument is exogenous. This time, probability values for the tests are of 0.13 and 0.14, respectively. Connecting the estimates in columns (1) and (3) of Table 1, the regression results point to an increase in the unemployment rate of roughly 0.3 ($1.85 \times 0.153 \approx 0.3$) percentage point for every week that the stay-at-home orders were in force. With a median duration of 6 weeks and the orders applying to much of the country, they could have accounted for an increase in the unemployment rate of about 2 percentage points.

4.2 Spontaneous Social Distancing: A Cross-Sectional Approach

To study the effect of spontaneous social distancing, we consider a two-week period before the imposition of any stay-at-home order—the 14-day period through March 17, which is two days before the first stay-at-home order went into effect in California. The evidence reviewed above suggests that much of the reduction in mobility had already occurred by the time mandatory rules started to be imposed. Yet, this initial mobility reduction was far from homogeneous across states.

A useful observation for our purpose is by Gollwitzer et al. (2020), who note that individual political leanings influence social distancing practices, and through these practices also influence health outcomes. We design a second test of our hypothesis building on this observation. Namely, we instrument mobility with political leanings by U.S. state, as captured in the share of the vote for the Republican candidate in the 2016 presidential election. Given our focus on the first part of March, before the introduction of mandatory measures, we collapse the time dimension of our initial panel regression and rely only on the cross-sectional variation at the state level.

Starting from the regression framework in Equation 9, we now difference the specification between two points in time on the same month. Focusing on the regression for reproduction rate, this differencing yields

$$\ln(R_{0,t,s}) - \ln(R_{0,t-h,s}) = b(m_{s,t} - m_{s,t-h}) + e_{s,t} - e_{s,t-h}. \tag{11}$$

We proceed analogously for equation 10, which focuses on initial jobless claims.

We again estimate the elasticity coefficient b by two-stage least squares. In the first stage we use political leanings to instrument the change in mobility between two points in time. In the second stage, we regress our dependent variable—either the reproduction rate or the initial claims—on the fitted change in residential mobility. In this exercise, we cannot use the estimates of the reproduction rate in Fernández-Villaverde and Jones (2020), since these start in the second half of March for most regions. We rely instead on the estimates from Systrom, Vladek, and Krieger (2020), which start earlier and are based on an adaptation of the estimation method of Bettencourt and Ribeiro (2008). The middle and bottom panels of Figure 1 offer a comparison of these alternative estimates of the reproduction rate when aggregated at the national level.

The estimates of the reproduction rate from Systrom, Vladek, and Krieger (2020) cover all 50 U.S. states and the District of Columbia. The starting date for these estimates varies by state, in line with the differential spread of the disease. The earliest estimates are for February 19, 2020 for the state of Washington, whereas, at the other end of the spectrum, estimates for Alaska, Idaho, and West Virginia only start on March 8, 2020.[7]

The message from our new exercise is loud and clear. As shown in Table 2, Column 1, there is a strong correlation between political leanings and the change in mobility. In columns 2 and 3, the null hypothesis that the coefficient on the instrumented mobility is 0 can be rejected at standard significance levels, despite the fact that we only have 51 observations. The elasticity of initial jobless claims with respect to mobility in column 3 of this table, at about 0.17, is remarkably close to the analogous elasticity in column 3 of Table 1, which is approximately 0.15. This finding indicates that the economic costs of changes in mobility are comparable, regardless

[7] Given the later start of estimates for the reproduction rate, for Alaska, Idaho, and West Virginia we use a shorter window of nine days when computing the changes in Equation 11.

of whether the changes are driven by mandated or spontaneous measures. However, it could still be the case that for comparable costs, the spontaneous measures could have induced a bigger decline in the reproduction rate. Moving back to Table 1 for the panel regression instrumented with stay-at-home orders, Column 2 shows an elasticity of the reproduction rate with respect to mobility of about -3.5. By contrast the analogous estimate in Column 2 of Table 2 is about -2.3, which implies a lower effectiveness of spontaneous measures in reducing the reproduction rate relative to mandated measures.[8] In other words, for the same economic impact, a decline in spontaneous mobility leads to a smaller decline in the reproduction rate.

5 Conclusions

We investigated empirically the epidemiological benefits and economic costs of social distancing at the onset of the pandemic. We derived our empirical framework from the standard model, proxying contacts using Google mobility data, and instrumenting mobility with either the stay-at-home orders issued by individual U.S. states, or political leanings by state. Our results suggest that, at the margin, changes in mobility through the first quarters of 2020 in the United States had significant effects on both reproduction rates and initial jobless claims. Strikingly, the magnitude of the economic effects is comparable whether social distancing is spontaneous or mandated—the epidemiological effects are however stronger when social distancing is mandated.

In light of these results, it is plausible that when economic activity rebounded as stay-at-home orders were lifted, this rebound was made possible by the observed improvement in the epidemiological conditions. Counterfactually, if the reproduction rate of the coronavirus had remained high or had matched the initially pessimistic scenario, the lifting of the health measured could have been offset by a new hike in spontaneous social distancing.

[8] For our comparison we used estimates based on different datasets for the mandated and spontaneous measures, the datasets of Fernández-Villaverde and Jones (2020) and of Systrom, Vladek, and Krieger (2020), respectively. We can also estimate the elasticity of the reproduction rate with respect to mobility for mandated measures using the dataset of Systrom, Vladek, and Krieger (2020) and find an even more sizable elasticity of about -5.1.

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Table 1: The Effects of Stay-at-Home Orders

	(1) Res. Mobility 2sls 1^{st} step	(2a) Reproduction Rate 2sls 2^{nd} step J.-F.V. dataset	(2b) Reproduction Rate 2sls 2^{nd} step Rt.Live dataset	(3) Init. Unemp Claims 2sls 2^{nd} step
Stay-at-home orders	1.850**			
	(0.000)			
Residential mobility index		-3.502*	-5.059**	0.153**
		(0.010)	(0.000)	(0.000)
r2	0.918	0.153	0.402	0.610
N	1204	1204	1204	1204

p-values in parentheses
$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

All the regressions are run with data at the weekly frequency and include state and month fixed effects. A state-by-state dummy that takes a value of 1 if a stay-at-home order is in force and zero otherwise is the instrument for the Google residential mobility index in the 2-stage-least-squares regressions in columns (2a), (2b), and (3). The results in column (2a) are based on the reproduction rate from the dataset of Fernández-Villaverde and Jones (2020). The results in column (2b) are based on the reproduction rate from the Rt.Live dataset of Systrom, Vladek, and Krieger (2020).

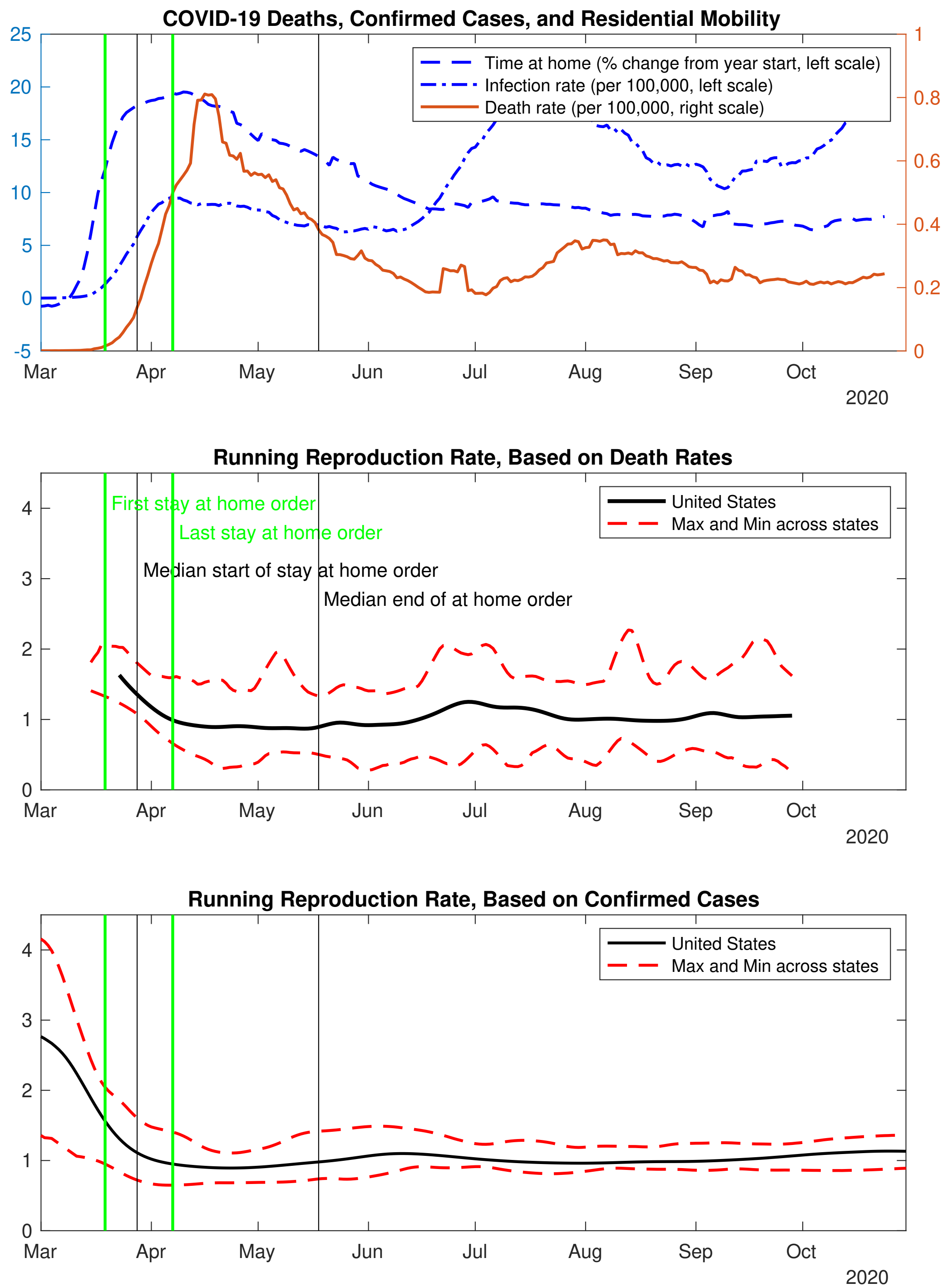
Table 2: The Effects of Spontaneous Social Distancing

	(1) % Change Res. Mobility 2sls 1^{st} step	(2) % Change R 2sls 2^{nd} step Rt.Live dataset	(3) PPt. Change Init. Claims 2sls 2^{nd} step
% Republican Votes in 2016	-0.189**		
	(0.000)		
PPt. Change Res. Mobility		-2.268^{+}	0.168**
		(0.099)	(0.003)
r2	0.607	0.0439	0.140
N	51	51	51

p-values in parentheses
$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

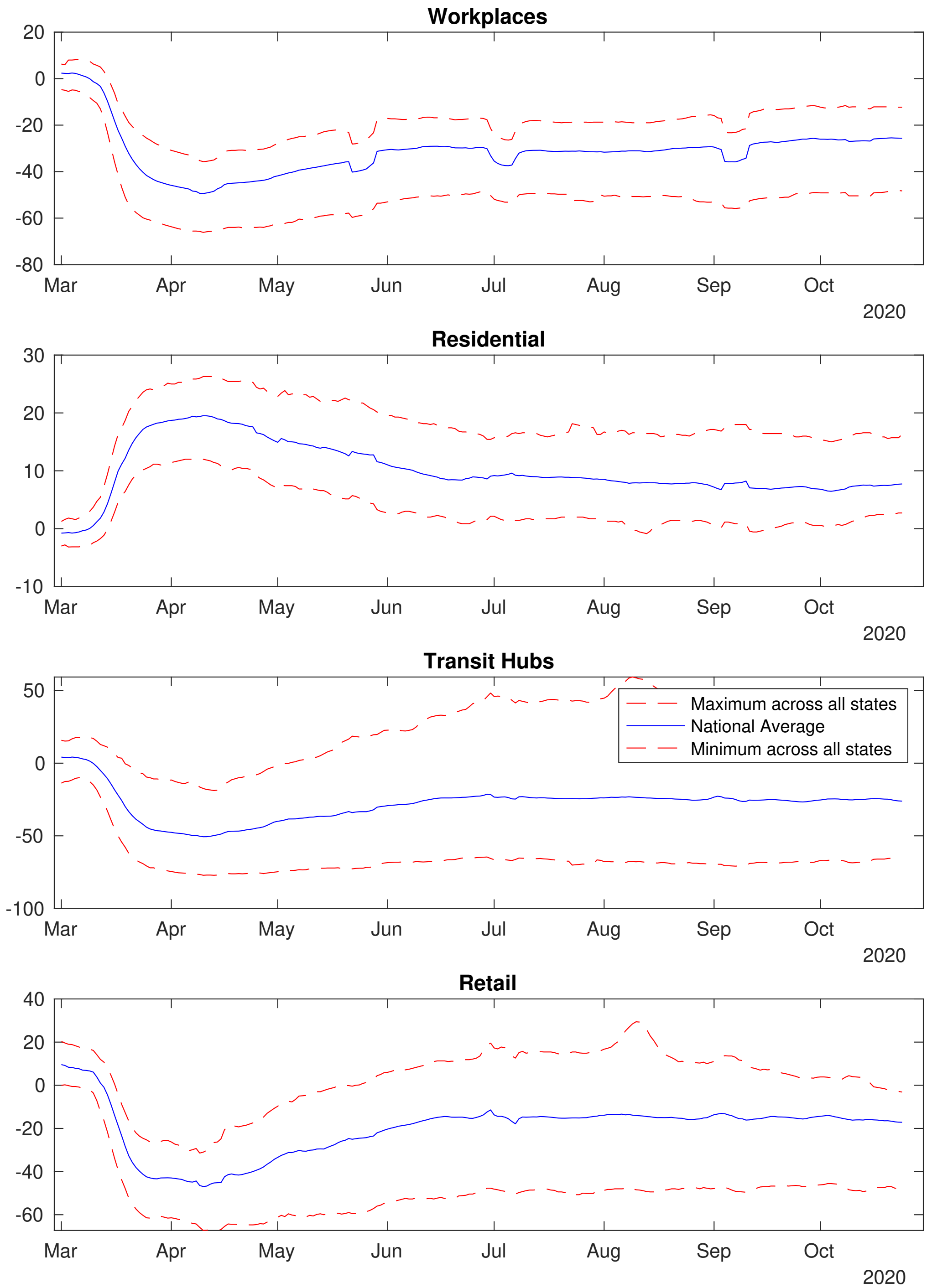
Political leanings, as measured by the share of votes for the Republican presidential candidate in the 2016 election are the instrument for the Google residential mobility index in the 2-stage-least-squares regressions in columns (2) and (3). The results in column (2) are based are based on the reproduction rate from the Rt.Live dataset of Systrom, Vladek, and Krieger (2020).

Figure 1: Stay-at-Home Orders, Mobility, COVID19 Death and Infection Rates — 7-Day Moving Average



Note: The vertical lines denoting key dates are repeated in each panel. Sources: The data for death rates and confirmed cases are from JHU CSSE (2020). The data on stay-at-home orders are from Raifman et al. (2020). The residential mobility data are from Google LLC (2020). The estimates of the running reproduction rate based on deaths are from Fernández-Villaverde and Jones (2020). The estimates of the running reproduction rate based on confirmed cases are from Systrom, Vladek, and Krieger (2020).

Figure 2: Workplace and Residential Mobility—7-Day Moving Average



Note: The dips in workplace mobility at the end of May, beginning of July and end of September correspond to national holidays. Their effects are prolonged by the moving average.
Source: Google LLC (2020).