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The Impact of Government Interventions on COVID-19 Spread and Consumer Spending

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Abstract. We examine the impact of government interventions on the spread of COVID-19 and consumer spending. We do this by first estimating models of COVID-19 spread, consumer spending, and social distancing in the United States during the early stages of the COVID-19 pandemic. Social distancing has a large effect on reducing COVID-19 spread and is responsive to national and local case numbers. Nonmask government interventions reduce COVID-19 spread, whereas the effectiveness of mask mandates is much smaller and statistically insignificant. Mask mandates tend to increase social distancing, as do nonmask governmental restrictions as a whole. Social distancing hurts spending in the absence of a mask mandate but has a negligible effect on spending if there is a mask mandate. Mask mandates have a direct effect of increasing spending in counties with high levels of social distancing while reducing spending in counties with low levels of social distancing. We use these three estimated models to calculate the effect of mask mandates and other governmental interventions on COVID-19 cases, deaths, and consumer spending. Implemented mask mandates decreased COVID-19 cases by a statistically insignificant 774,000 cases, saving 28,000 lives, over a four-month period, but led to $76B–$155B of additional consumer spending. Other nonmask governmental interventions that were implemented reduced the number of COVID-19 cases by 34M, saving 1,230,000 lives, while reducing consumer spending by approximately $470B–$703B over our 4-month period of the study. Thus, these restrictions were cost effective as long as one values each saved life at $387,000–$608,000 or more.

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1. Introduction

COVID-19 has been a disruptive force throughout the world. As of February 24, 2022, there have been 429 million (M) confirmed cases worldwide and almost 78M confirmed cases in the United States; almost 6M people have died, including more than 930,000 deaths in the United States.[1] Furthermore, the pandemic has devastated the worldwide economy (International Monetary Fund 2020) and pressed the U.S. economy into a recession (National Bureau of Economics Research 2020). Whereas the impact of COVID-19 has been significant, there is uncertainty about how much masking policies and government nonpharmaceutical interventions (closing public venues, closing nonessential venues, closing schools, imposing shelter-in-place restrictions, limiting the sizes of gatherings, and limiting religious gatherings; henceforth, collectively referred to as NPIs) have affected the spread of COVID-19, social distancing, and the level of consumer spending.

We address these questions by first measuring the impact of social distancing, mask mandates, and NPIs on the spread of COVID-19. We show that social distancing reduces the spread of COVID-19, whereas mask mandates only have a statistically insignificant effect on reducing the spread of COVID-19. We also show that some NPI policies slow the spread of COVID-19.

We then examine the effects of mask mandates and NPIs on social distancing levels. Consistent with Seres et al. (2020) and Marchiori (2020), we find that mask mandates increase the level of social distancing, as do nonmask governmental NPIs as a whole. Furthermore, social distancing increases as COVID-19 cases and growth rates increase nationally, but the impact of local cases is smaller.

We also evaluate the impact of mask mandates and NPIs on spending. We find that mask mandates may have a small positive effect on spending in some situations, whereas nonmask NPIs decrease consumer spending.

Finally, we compare the amount of COVID-19 spread and spending that would have occurred if (1) none of the counties had a mask mandate instead of the mask mandates that were actually implemented, and (2) none of the counties introduced NPIs instead of the NPIs that were actually imposed. We find that the mask mandates that were implemented saved a statistically insignificant 28,000 lives and increased consumer spending by $76 billion (B)–$155B over the four-month time period we study. Thus, mask mandates may be both prohealth and probusiness, although some statistical uncertainly exists behind this conclusion. In the case of government NPIs, we see a tradeoff between lives saved and consumer spending. Over the four-month time period of our study, the implemented NPIs saved 1,230,000 lives but reduced consumer spending by approximately $470B–$703B. The cost of each life saved was around $387,000–$608,000, which was a worthwhile cost according to most estimates of values for lives.

The paper is organized as follows. Section 2 discusses the data we use for the analysis. Section 3 presents the model and estimation for the spread of COVID-19. Section 4 examines shifters of social distancing. Section 5 presents the model and estimation for consumer spending. Section 6 presents the counterfactual analysis of how contagion and spending are affected by the different interventions. Finally, Section 7 concludes.

2. Data

Our analysis covers a four-month period from April 1, 2020 to July 31, 2020. We begin our analysis on April 1 because by then most of the country was affected by COVID-19 and a large fraction of the county had already begun social distancing. Although one may want to contrast shopping or distancing behaviors before versus after COVID-19 began, there was likely an unobservable structural break between the way people shopped and socially distanced before COVID-19 compared with what they did during the COVID-19 pandemic; we are unlikely to be able to capture this structural break within our model. We chose the end date for our analysis because our data on government NPIs end at this time.

Our data come from a number of sources. Our data on the number of daily confirmed cases for 3055 U.S. counties or country-equivalents come from the New York Times. In this data set, the numbers are diagnosed cases on a given day. COVID-19 has an average incubation period of five days (Lauer et al. 2020, Li et al. 2020). We are also informed by local health officials that there was, on average, a five-day gap between the onset of a patient's symptoms and the final diagnosis during the time frame we study. Accordingly, we assume the infection date of a case occurs 10 days before it is reported by the New York Times. Thus, we assume that the cases that were reported on April 11, 2020, actually occurred on April 1, 2020.

Our demographic data come from the Census Bureau's 2014–2018 American Community Survey. Our weather data come from the National Oceanic and Atmospheric Administration. These variables, a full description of each variable, and the computer codes we use in this paper can be found at the following website: https://tinyurl.com/CovidDataShare.

We supplement these public data with a few other data sources. Our social distancing data come from SafeGraph, a data company that aggregates anonymized location data from numerous applications to provide insights about physical places, via the SafeGraph Community. To enhance privacy, SafeGraph excludes census block group information if fewer than two devices visited an establishment in a month from a given census block group. Although the data are proprietary, they are available free of charge to academics studying COVID-19 (https://www.safegraph.com/covid-19-data-consortium). We create a social distancing index using a principal component analysis (PCA) of four metrics: the percentage of residents staying home, the percentage of residents working full-time at their workplace, the percentage of residents working part-time at their workplace, and the median duration that residents stay home. The resulting first principal component of the PCA is negatively correlated with the percentage of people staying home and the duration that people stay home and positively correlated with the two work metrics. To make sure the social distancing index is more numerically intuitive, we define the negative of this first principal component as the social distancing index so that a higher index corresponds to a greater level of social distancing.

Ultimately, the fitted social distancing index is $SocialDistIndex = 0.53 FractStayHome - 0.51 FullTimeWork - 0.61 PartTimeWork + 0.31 StayHomeDuration$, where the four right-hand variables have been demeaned, and the stay-home duration is defined in terms of minutes.[2] These four variables are significantly correlated. In particular, the correlation between the percentage of residents staying home full-time and the stay-home duration is 0.39. The correlations between the percentage of residents staying home full-time and percentages of residents working full-time or part-time are −0.56 and −0.68, respectively. Intuitively, the index says that social distancing increases as more people stay at home, and people spend a greater percentage of their time at their homes, whereas social distancing decreases as people spend more time at work.[3]

The SafeGraph data are supplied at the daily level for residents of each Census Block Group. We aggregate this index to the county level by taking the weighted median, where the weights are the number of cellphones in the data at each Census Block Group. We run some of our analysis at a weekly level because our spending data are smoothed over seven-day periods. In such cases, we average our measure across the corresponding seven days from Tuesday to Monday.

Our spending data are provided by https://tracktherecovery.org/. These data are made publicly available by Opportunity Insights and have been collected from a number of sources. Chetty et al. (2020) provide a detailed summary of the variables in the data set. We use the consumer spending data that come from consumer credit card and debit card purchases originally supplied by Affinity Solutions. The spending data are at the county-daily level for 1,685 counties. These counties account for 87% of the population of the 3,055 counties in our COVID-19 case data. This data set is smoothed over seven-day periods, and we use the Tuesday iteration of this measure to track aggregate weekly spending. Each observation measures the seasonally adjusted change relative to the January 2020 index period,[4] which we refer to as the consumer spending recovery index.

The facial mask mandate data come from three sources. The first source is Wright et al. (2020), who collect county-level facial mask mandate information. We compile a second data set from online sources for state-level facial mask mandates.[5] Third, we use data on employee mask mandates for businesses, which are collected by Lyu and Wehby (2020). We define the mask mandate to be one on any date where either the county or the state has a mask mandate (regardless of whether it is for the public or only for employees of businesses).

Finally, we obtain other COVID-19 NPI policy data from the company Keystone Strategy, which contain exact dates of each NPI restriction in each county when the restriction was in effect.[6] We focus on six common restrictions: shelter-in-place orders, closing of public schools, closing of public venues, closing nonessential businesses, limiting large gatherings, and limiting religious gatherings.

We provide a summary of all variables used in our analyses in Table 1.

3. Spread of COVID-19

We begin our analysis by estimating a model of COVID-19 spread as a function of social distancing, mask mandates, and other NPIs. Our estimation is based on a standard susceptible-infected-recovered (SIR) model. The SIR model is widely used in predicting the contagion of infectious diseases (Adda 2016), including COVID-19 (Chinazzi et al. 2020, Kissler et al. 2020, Liu et al. 2020).

Mathematically, we consider that new infections, $y_{i,t}$, in a given county i on date t follow the following process:

$$y_{i,t} = R_{i,t}\, S_{i,t}(Y_{i,t-2} - Y_{i,t-8}), \tag{1}$$

where $R_{i,t}$ is the rate of infection and $S_{i,t}$ is the percentage of population in county i who have not contracted the disease. $Y_{i,t}$ represents the cumulative cases in county i by date t and, accordingly, the term of $Y_{i,t-2} - Y_{i,t-8}$ accounts for individuals who were infected between seven and two days before date t. Our assumption of a six-day infectious period, during which the infected individuals can further spread the disease, follows the literature (Nishiuram et al. 2020). As a result, $Y_{i,t-2} - Y_{i,t-8}$ represents the infectious population who may directly cause infections on date t. The assumption of the length of the infectious period has little impact on the estimation results; Liu et al. (2020) shows that using a 14-day infectious period (i.e., $Y_{i,t-2} - Y_{i,t-16}$) versus a 6-day infectious period yield extremely similar simulated forecasts.

The rate of spread of COVID-19 might change over locations and time. Thus, we model $R_{i,t}$ to vary with

Table 1. Summary Statistics

	Mean	Standard deviation	Minimum	Maximum
Temperature (°F)	59.942	3.411	−3.847	97.396
Humidity (%)	67.619	15.494	0.409	100.000
Precipitation (inch)	0.100	0.157	0.000	1.010
Social distancing	0.630	1.010	−5.756	5.128
Mask mandates	0.500	0.500	0.000	1.000
Closing of public venues	0.571	0.495	0.000	1.000
Closing of nonessential businesses	0.524	0.499	0.000	1.000
Closing of schools	0.855	0.352	0.000	1.000
Shelter in place	0.443	0.497	0.000	1.000
Gathering size limits	0.754	0.431	0.000	1.000
Religious gathering limits	0.370	0.483	0.000	1.000
Local weekoverweek growth rate in cases	0.200	3.979	1.000	906.000
National weekoverweek growth rate in cases	0.126	0.275	−0.139	1.395
Local cases in the past 7 days per 1,000 people	0.523	1.214	0.000	115.385
National cases in the past 7 days per 1,000 people	0.727	0.327	0.413	1.414
Consumer spending recovery index: total spending	−0.112	0.165	1.370	0.724
Log(population density)	3.884	1.692	−1.313	11.183
Fraction of Black	0.093	0.146	0.000	0.874
Trump 2020 vote share	0.647	0.160	0.054	0.962

multiple factors:

$$R_{i,t} = exp\ (\alpha_i + \beta_t + \mu' X_{i,t} + e_{i,t}), \qquad (2)$$

where α_i and β_t are county fixed effects and date fixed effects, respectively; $X_{i,t}$ includes average temperature, humidity, the social distancing index, an indicator variable denoting the presence of a mask mandate, and a set of indicators for each NPI policy. Furthermore, we include interactions between social distancing and the mask mandate, as well as allowing social distancing, mask mandates, and the NPIs to have heterogeneous effects based on the fraction of the population that is Black, the log of the population density, and the fraction of the population that voted for Trump in 2020.[7] The Black population has been disproportionately hit harder by COVID-19 than other racial groups (Chowkwanyun and Reed 2020). Population density is related to COVID-19 spread because the number of people one is exposed to varies across urban vs. rural areas. Similarly, population density could affect the impact of government interventions, both because the extent to which these interventions reduce contact is affected by baseline interaction rates, and because people in high population-density areas may self-distance more even in the absence of government orders because they perceive that they are getting more exposure to COVID-19. Finally, President Trump repeatedly mocked mask mandates and other governmental NPIs, perhaps in an attempt to keep the economy running. It is feasible then that supporters of Trump may respond differently to mask mandates or other governmental interventions based on their perception about the importance of these mandates. These different perceptions may also be shaped by the different media Trump supporters and Trump nonsupporters watch (Simonov et al. 2020).

Finally, we assume that the true number of cases is five times the number of diagnosed cases. We choose this scaling factor according to Phipps et al. (2020), which shows that the detection rate of COVID-19 was about 20% in the United States by the end of August 2020. This assumption only affects $S_{i,t}$, the fraction of people in the county that have not yet had COVID-19 and are assumed to remain susceptible, and the scaling of the fixed-effects parameters from the SIR regression (which are five times larger than they would be if we used only reported cases).[8] We use reported cases everywhere else in the paper: for the social distancing and spending models. Also, we divide the number of cases obtained from the model by five before reporting the case numbers and before feeding these case numbers into the social distancing and spending models during the simulations in Section 6. Thus, the numbers in Section 6 are comparable to the reported numbers of cases and deaths.

We estimate the case model by taking the logarithm of both sides of Equation (1) and rearranging. Occasionally, $y_{i,t}$ are zero for some counties on certain dates. To assure $ln(y_{i,t})$ is well defined, we add one to each observation of daily county cases and to the number of infectious individuals. After rearranging, we have

$$[ln(y_{i,t} + 1) - ln(S_{i,t}) - ln\ (Y_{i,t-2} - Y_{i,t-8} + 1)] \\ = \alpha_i + \beta_t + \mu' X_{i,t} + e_{i,t}. \qquad (3)$$

We call the left-hand side of this equation the log of the reproduction ratio.

Social distancing, mask mandates, and NPIs may be endogenous because they can be affected by the severity of the pandemic. To address such endogeneity, we use a two-stage least squares approach, where we instrument for the social distancing, mask mandates and other non-mask government NPIs with the interactions of week dummies and dummies indicating the party composition of the state government, which we define by four variables indicating the party of the state's governor and whether both houses of the legislature are also controlled by the same party.[9] These partisan outcomes were determined before the presence of COVID-19 and likely affect the policies that the government implemented. However, because we also include the county-level vote share for Trump in 2020 (which has a 98% correlation with the Trump vote share in 2016), the state-level partisan composition should not predict the local behavioral responses to the government policies conditional on the level of the local vote shares. As a second set of instrumental variables, we also use week dummies interacting with the vote share that Trump received in 2016 for the designated market area (DMA) in which a given county sits, which should influence the slant of the media that all counties in that DMA receive but is orthogonal to each county's severity of the pandemic. In that sense, the vote share in a given DMA can be interpreted as a preference-externality-style instrumental variable (Waldfogel 2003, Thomas 2020, Li et al. 2023). We use the 2016 vote share for Trump to ensure that this instrument is not influenced by COVID or the government's response to COVID. However, the vote share for Trump in 2016 should be correlated with the media slant that people in that market receive. There can be quite a lot of variation in Trump's vote shares across counties within each DMA, so the impact of political preferences on behavior is still identified.[10] We also include instruments consisting of the interactions between county demographics (percentage Black, Trump 2020 vote share, and the log(population density)) and both the dummies about which party controls the state government and the DMA Trump vote shares.[11]

Table 2 presents the estimation results.[12] We demeaned each of the demographic variables (percent of Black residents, log population density, and Trump's vote share) to make the main effects on social distancing, mask mandates, and NPIs easier to interpret. We observe that social distancing lowers the transmission rate substantially. It is

Table 2. Standard SIR Model

Independent variable	Estimates/standard error	Independent variable	Estimates/standard error
Temperature (°F)	−0.002 (0.001)	*Closing of public venues × Log(pop. density)*	−0.367*** (0.054)
Humidity (%)	0.004*** (0.001)	*Closing of public venues × Frac. of Black*	0.524 (0.547)
Social distancing	−0.433*** (0.090)	*Closing of public venues × Trump 2020 vote share*	−2.125*** (0.584)
Mask Mandates	0.062 (0.063)	*Closing of nonessential businesses × Log(pop. density)*	0.053 (0.053)
Social distancing × Mask mandates	−0.076 (0.066)	*Closing of nonessential businesses × Frac. of Black*	4.238*** (0.883)
Closing of public venues	0.100 (0.081)	*Closing of nonessential businesses × Trump 2020 vote share*	1.443** (0.688)
Closing of nonessential businesses	0.056 (0.099)	*Closing of schools × Log(pop.density)*	−0.194*** (0.048)
Closing of schools	−0.274*** (0.106)	*Closing of schools × Frac. of Black*	−1.595* (0.962)
Shelter in place	−0.072 (0.081)	*Closing of schools × Trump 2020 vote share*	0.662 (0.569)
Gathering size limits	−0.271*** (0.093)	*Shelter in place × Log(pop.density)*	0.009 (0.039)
Religious gathering limits	−0.333*** (0.088)	*Shelter in place × Frac. of Black*	−0.863** (0.359)
Social distancing × Log(pop. density)	0.072*** (0.015)	*Shelter in place × Trump 2020 vote share*	−0.243 (0.488)
Social distancing × Frac. of Black	0.120 (0.169)	*Gathering size limits × Log(pop. density)*	0.047 (0.052)
Social distancing	0.850*** (0.193)	*Gathering size limits × Frac. of Black*	−4.856*** (1.014)
Mask Mandates × Log(pop. density)	0.017 (0.027)	*Gathering size limits × Trump 2020 vote share*	−1.058 (0.865)
Mask Mandates × Frac. of Black	0.614* (0.314)	*Religious gathering limits × Log(pop. density)*	0.329*** (0.057)
Mask Mandates × Trump 2020 vote share	−0.660** (0.307)	*Religious gathering limits × Frac. of Black*	−3.167*** (0.826)
		Religious gathering limits × Trump 2020 vote share	0.107 (0.593)
Observations	372,710	Overidentification statistic	33.70
R^2	0.16	Underidentification statistic	1,442.731
County fixed effects	Yes	Kleibergen Paap weak instrument statistic	14.459
Date fixed effects	Yes		
Estimation period	4/1-7/31		

Notes. Standard errors are clustered at the county level. Dependent variable is *Log(Reproduction Ratio)*.
$^*p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

harder to interpret the impact of masking because the social distancing variable is not demeaned: if we add the coefficient for the mask mandate with the product of the interaction coefficient and the mean of social distancing (0.63), we find that, on average, masks slightly decrease the transmission rate (i.e., $0.062 - 0.076 \times 0.63 = -0.014$), although this effect is far from statistically significant. We also observe that mask mandates are most effective in areas with a higher level of Trump support perhaps because many people in these areas might not mask except when they are required to do so.

We find that, on the whole, other government interventions (i.e., NPIs) reduce the spread of COVID-19. Although several of the coefficients on individual non-mask NPIs are statistically significant, the lack of significance, or even positive coefficients of the other NPIs may partially be due to the high correlation between these variables.[13] It is hard to observe a consistent pattern with the interaction effects.

4. Determinants of Social Distancing

We next estimate the following model to understand how government interventions affect social distancing:

$$d_{i,t} = \alpha_i^d + \beta_{dow(t)} + \rho_{w(t)} + \delta\, q_{i,t} + \varphi\, p_t + \mu^d\, m_{i,t} + \theta\, c_{i,t} + \lambda'\, X_{i,t} + \zeta_{i,t}, \tag{4}$$

where $d_{i,t}$ is the social distancing index of county i on date t, as defined in Section 2; α_i^d, $\beta_{dow(t)}$, and $\rho_{w(t)}$ are county, day-of-the-week, and week fixed effects, respectively; $q_{i,t}$ and p_t represent the county and national confirmed cases per 1,000 people in the past seven days and week-over-week growth rate in the number of confirmed cases, respectively[14]; and $m_{i,t}$ is the average temperature (in Fahrenheit), $c_{i,t}$ is the average precipitation (in inches), and $X_{i,t}$ consists of a string of binary indicator variables of COVID-19 related public orders: the mask mandates and other NPIs, as well as interactions between these variables and the fraction of the population that is Black, the log of the population density, and the share of the vote Trump received in 2020.

Some readers may wonder why we use day-of-the-week fixed effects and week fixed effects instead of date fixed effects. We do this so that we can measure how national case numbers, which are constant across locations on any date, affect social distancing. We also show that using date fixed effects does not change the other estimates.

Because mask mandates and NPIs may be correlated with the same factors that affect social distancing, we run two-stage least squares using the same state-level party control status of the government and DMA-level voter preference instruments that we used in Section 3. The logic behind these instruments is also equivalent to the logic laid out in Section 3.

The results are in Table 3.[15] Column 1 presents our preferred specification, with day-of-the-week and week

Table 3. Social Distancing Model

Independent variable	(1) Estimates/ standard error	(2) Estimates/ standard error	Independent variable	(1) Estimates/standard error	(2) Estimates/standard error
Local week-over-week growth rate in cases	−0.0001 (−0.0002)	0.0001 (0.0002)	*Closing of public venues × Log(pop. density)*	0.084*** (0.031)	0.085*** (0.031)
National week-over-week growth rate in cases	0.074*** (0.009)		*Closing of public venues × Frac. of Black*	0.655** (0.270)	0.636** (0.269)
Local cases in the past 7 days per 1,000 people	0.022*** (0.005)	0.022*** (0.005)	*Closing of public venues × Trump 2020 vote share*	0.169 (0.317)	0.172 (0.314)
National cases in the past 7 days per 1,000 people	0.105*** (0.024)		*Closing of nonessential businesses × Log(pop.density)*	0.005 (0.029)	0.007 (0.029)
Precipitation (inch)	0.067*** (0.002)	0.068*** (0.002)	*Closing of nonessential businesses × Frac. of Black*	1.062** (0.415)	1.038** (0.416)
Temperature (°F)	−0.003*** (−0.0002)	−0.004*** (0.0005)	*Closing of nonessential businesses × Trump 2020 vote share*	0.346 (0.346)	0.277 (0.348)
Mask mandates	0.089*** (0.019)	0.089*** (0.018)	*Closing of schools × Log(pop. density)*	0.105*** (0.028)	0.109*** (0.028)
Closing of public venues	0.026 (0.042)	0.031 (0.041)	*Closing of schools × Frac. of Black*	0.140 (0.574)	0.188 (0.578)
Closing of nonessential businesses	−0.121** (0.058)	−0.127** (0.058)	*Closing of schools × Trump 2020 vote share*	−1.876*** (0.413)	−1.837*** (0.412)
Closing of schools	0.280*** (0.061)	0.287*** (0.061)	*Shelter in place × Log(pop. density)*	0.072*** (0.024)	0.073*** (0.024)
Shelter in place	0.366*** (0.043)	0.367*** (0.043)	*Shelter in place × Frac. of Black*	−0.593*** (0.178)	−0.538*** (0.177)
Gathering size limits	−0.166*** (0.051)	−0.171*** (0.050)	*Shelter in place × Trump 2020 vote share*	0.124 (0.289)	0.220 (0.289)
Religious gathering limits	0.065 (0.045)	0.068 (0.045)	*Gathering size limits × Log(pop. density)*	−0.047 (0.033)	−0.050 (0.033)
Mask mandates × Log(pop. density)	−0.010 (0.010)	−0.009 (0.010)	*Gathering size limits × Frac. of Black*	−0.746 (0.536)	−0.731 (0.531)
Mask mandates × Frac. of Black	−0.043 (0.123)	−0.038 (0.124)	*Gathering size limits × Trump 2020 vote share*	2.021*** (0.563)	2.014*** (0.556)
Mask mandates × Trump 2020 vote share	−0.211* (0.111)	−0.197* (0.110)	*Religious gathering limits × Log(pop. density)*	0.004 (0.024)	−0.002 (0.024)
			Religious gathering limits × Frac. of Black	−1.862*** (0.380)	−1.857*** (0.380)
			Religious gathering limits × Trump 2020 vote share	−0.174 (0.266)	−0.202 (0.264)
Observations	372,710	372,710	Overidentification statistic	187.95	232.81
R^2	0.79	0.81	Underidentification statistic	1,545.917	1,538.885
County fixed effects	Yes	Yes	Kleibergen Paap weak instrument Statistic	26.916	28.103
Day-of-week fixed effects	Yes	No			
Week fixed effects	Yes	No			
Date fixed effects	No	Yes			
Estimation period	4/1-7/31	4/1-7/31			

Notes. Standard errors are clustered at the county level. Dependent variable is *Social Distancing*.

fixed effects rather than date fixed effects, which allows us to estimate the impact of both national and local COVID-19 cases on social distancing. This is especially important for the counterfactual analysis in Section 6, where we want to account for how social distancing changes with the progression of the pandemic. Column 2 shows the same estimation with date fixed effects but having the national case numbers dropped from the regression. We observe that using the day-of-the-week and week fixed effects instead of date fixed effects does not change any of the estimated parameters in a meaningful way.

We find that social distancing increases when cases of COVID-19 are high and increasing. The coefficient is much larger for national cases, likely reflecting the attention COVID-19 receives in the press. That said, there is a lot more variation in local breakouts, and when there is a strong local breakout of cases, this will lead to substantially more social distancing.[16] Mask mandates increase social distancing, and the nonmask government NPIs as a whole also increase distancing. The positive impact of mask mandates on social distancing likely come from the masks serving as a reminder to increase distancing, consistent with Seres et al. (2020) and Marchiori (2020). Trump-supporting areas socially distance less in the presence of mask mandates perhaps as a protest counter-reaction.

5. Determinants of Consumer Spending

In this section, we investigate how social distancing and government interventions affect consumer spending. For this analysis, our data are provided in a format where the dependent variables are smoothed over seven days, as described in Chetty et al. (2020). Given this smoothing, we estimate the model at the weekly level, with weeks defined as Tuesday through Monday:

$$s_{i,\tau} = a + \omega' X_{i,\tau} + \epsilon_{i,\tau}, \tag{5}$$

where $s_{i,\tau}$ is the consumer spending recovery index at county i on week τ, as defined in Section 2; a is a constant term; and $X_{i,\tau}$ consists of social distancing, amounts of precipitation, average temperature, the fraction of the population that is Black, the log of population density, Trump's 2020 vote shares, and indicator variables for mask mandates and the other NPIs, as well as demographic interactions with social distancing, mask mandates and the NPIs, where the demographic variables have been demeaned.

In our first specification, we do not include county or week fixed effects because spending is already expressed as a percentage of the county's pre–COVID-19 benchmark spending, and it is also already seasonally adjusted by comparing the spending to those in the same week one-year prior. However, this logic is somewhat incomplete because there can also be nonseasonal shocks to spending, such as the release of the first series of COVID stimulus checks. Households earning less than \$75,000 per year were given \$1,200 per adult and \$500 per child and were sent out starting in mid-April of 2020. Households earning between \$75,000 and \$100,000 were given a prorated payment. Such large infusions of money could easily have an effect on consumer spending, especially in the early weeks when the stimulus checks were sent out. Thus, we also estimate a version of our spending model that includes week-level fixed effects. One shortcoming of putting in these week-level fixed effects is that the other large source of weekly variation in spending is the national variation in the number of COVID cases. Thus, putting in weekly fixed effects forces that the impact of COVID be measured through local variation in the amounts of COVID cases. However, news stories often presented national numbers more prominently than local numbers for COVID cases. Consequently, by putting in the weekly fixed effects we effectively remove a lot of the important informative variation in the data. Ultimately, the true effect of COVID and the COVID restrictions likely lies in between these two numbers.

Social distancing and government interventions can be correlated with the error of the spending regression. Thus, we instrument for social distancing and these government interventions using the party controlling the state government and DMA Trump vote share, as in the previous sections.

Table 4 presents the estimation results, where column (1) presents the model without weekly fixed effects, whereas column (2) presents the results with weekly fixed effects.[17] Most of the results are similar across the specifications except for the coefficients on social distancing and mask mandates. When we do not put in the weekly fixed effects, we observe social distancing reducing spending: a one-standard-deviation increase in the social distancing measure (1.01; see Table 1) leads to a 9.7% decrease in spending, whereas mask mandates mitigate most of these harmful effects of social distancing on spending. On the other hand, when we include weekly fixed effects, we observe mask mandates as increasing spending but that this benefit is reduced in areas with positive social distancing indexes and is higher in areas with negative social distancing indexes.

Table 4 also shows that, in aggregate, nonmask NPIs depress spending. Limits on closing nonessential businesses and limits on gathering sizes decreased spending the most. Interestingly, closing public venues increases spending. Although some public venues involve spending (such as restaurants and bars), many customers continued to order food but ate it as take out, and perhaps the closure of other venues without as much spending (for example gyms and recreation centers) led to substitution to spending for activities, or renovation, at home.

Table 4. Spending Model

Independent variable	(1) Estimates/ standard error	(2) Estimates/ standard error	Independent variable	(1) Estimates/ standard error	(2) Estimates/ standard error
Precipitation (inch)	−0.003 (0.006)	−0.003 (0.007)	*Closing of public venues × Log(pop. density)*	−0.050*** (0.015)	−0.046*** (0.013)
Temperature (°F)	0.001** (0.0003)	0.0001 (0.0002)	*Closing of public venues × Frac. of Black*	−0.114 (0.129)	−0.104 (0.130)
Log(pop. density)	−0.002 (0.012)	−0.027** (0.012)	*Closing of public venues × Trump 2020 vote share*	−0.306** (0.139)	−0.132 (0.131)
Frac. of Black	0.110 (0.197)	0.235 (0.202)	*Closing of nonessential businesses × Log(pop. density)*	0.008 (0.012)	−0.004 (0.011)
Trump 2020 vote share	−0.481*** (0.138)	−0.256* (0.143)	*Closing of nonessential businesses × Frac. of Black*	0.331** (0.161)	0.256 (0.163)
Social distancing	−0.096*** (0.008)	0.007 (0.019)	*Closing of nonessential businesses × Trump 2020 vote share*	−0.083 (0.155)	−0.291* (0.158)
Mask mandates	0.017 (0.012)	0.059*** (0.015)	*Closing of schools × Log(pop. density)*	−0.030* (0.017)	−0.008 (0.016)
Social distancing × Mask mandates	0.072*** (0.010)	−0.071*** (0.014)	*Closing of schools × Frac. of Black*	−0.075 (0.226)	0.067 (0.233)
Closing of public venues	0.036* (0.020)	0.070*** (0.021)	*Closing of schools × Trump 2020 vote share*	0.375*** (0.137)	0.531*** (0.132)
Closing of nonessential businesses	−0.070*** (0.023)	−0.059** (0.024)	*Shelter in place × Log(pop. density)*	0.001 (0.009)	0.015 (0.009)
Closing of schools	0.004 (0.019)	0.017 (0.025)	*Shelter in place × Frac. of Black*	−0.019 (0.080)	−0.028 (0.077)
Shelter in place	−0.009 (0.014)	−0.011 (0.014)	*Shelter in place × Trump 2020 vote share*	0.198* (0.120)	0.307*** (0.117)
Gathering size limits	−0.070*** (0.020)	−0.060*** (0.020)	*Gathering size limits × Log(pop. density)*	0.048** (0.021)	0.035* (0.019)
Religious gathering limits	0.047** (0.019)	0.022 (0.019)	*Gathering size limits × Frac. of Black*	−0.058 (0.150)	−0.158 (0.144)
Social distancing × Log(pop. density)	0.006 (0.005)	−0.001 (0.005)	*Gathering size limits × Trump 2020 vote share*	0.199 (0.204)	0.014 (0.192)
Social distancing × Frac. of Black	−0.026 (0.053)	0.036 (0.052)	*Religious gathering limits × Log(pop. density)*	0.037*** (0.012)	0.043*** (0.012)
Social distancing × Trump 2020 vote share	0.017 (0.047)	−0.034 (0.047)	*Religious gathering limits × Frac. of Black*	−0.202** (0.091)	−0.240*** (0.089)
Mask mandates × Log(pop. density)	−0.011 (0.007)	0.009 (0.007)	*Religious gathering limits × Trump 2020 vote share*	0.250* (0.142)	0.197 (0.137)
Mask mandates × Frac. of Black	0.069 (0.076)	−0.195*** (0.074)			
Mask mandates × Trump 2020 vote share	0.098 (0.077)	−0.190** (0.083)			
Observations	28,645	28,645	Overidentification statistic	20.24	30.44
R^2	0.08	0.08	Underidentification statistic	977.226	968.809
Week fixed effects	No	Yes	Week fixed effects	No	Yes
Estimation period	4/1-7/31	4/1-7/31	Kleibergen Paap weak instrument statistic	11.745	12.122

Notes. Standard errors are clustered at the county level. Dependent variable: *Total spending*.
$^*p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

6. Effect of Government Interventions on Disease Spread and Spending

We now analyze the impact of (1) mask mandates and (2) all NPIs have on COVID-19 spread, deaths, and spending over the period of April 1–July 31, 2020. Because there is feedback between the case model and the social distance model, we run the simulations for each date by first predicting the social distancing levels for each county using the actual observed values for each variable in X, except for changing either the masking or the other governmental NPIs (and their interaction terms) for the corresponding experiments. We also substitute the actual number of cases and percent changes in cases in the social distancing model with the predicted cases from the previous days. Once we have the date's social distancing levels, we then predict that date's COVID-19 cases, using the observed X variables except for the social distancing level, where we substitute in the predicted social distancing level, and for the relevant mask mandates and other governmental NPIs (and their interaction terms) variables, where we set the relevant policy.[18] Once we complete these calculations for a specific date, we move to simulating the social distancing and cases for the next date. After the whole sequence of cases and social distancing levels are simulated, we then calculate the spending levels using the observed data, except that we substitute the forecasted social distancing levels, the forecasted case levels, and the relevant mask mandates or governmental NPIs, in the place of the corresponding actual values. We conduct this last step twice: once using the model with week fixed effects and once using the model without week fixed effects. As discussed previously, we believe that the true effect of mask mandates and NPIs lies between these two estimates.

We calculate the changes in consumer spending in actual dollar amounts instead of as an index. We do this by multiplying the spending from the 2020 monthly national personal consumer expenditure (PCE) by the ratio of the weighted average monthly consumer spending recovery index under each hypothetical scenario to the actual recovery index.[19]

Because there is uncertainty in each of the model parameters, we obtain our mean results and confidence intervals by running 200 sets of simulations, where each simulation is based on a draw of coefficients from a multivariate normal distribution with the mean of the point estimates of the coefficients, and the variance-covariance matrix being the clustered variance-covariance matrix estimated empirically from each model.

6.1. Effects of Mask Mandates

We show in Sections 3 and 4 that mask mandates increase the amount of social distancing and statistically insignificantly decrease the rate of COVID-19 spread. In Section 5, we find that mask mandates can have a positive impact on consumer spending in some situations. We put these results together and account for the feedback loop between cases and social distancing through our simulations. To carry these out, we first compare the cases and consumer spending under the original values for all of the X variables to those where we set the mask mandate variables (and the corresponding interaction terms) to zero. In both scenarios, we keep the nonmask government NPIs equal to their actual values. Setting the mask mandate variables to zero represents our forecast of what would have happened if no mask mandates had been imposed.

We find that, over our four-month study period, the mask mandates that were imposed reduced the number of COVID-19 diagnosed cases by 774,000 (95% confidence interval (CI) = −432,000 to 1,746,000), saving 28,000 lives (CI = −16,000 to 64,000).[20] Although the impact of mask mandates on cases is statistically insignificant, the point estimate on the cases reflects an approximately 20% reduction in cases. Interestingly, we estimate that the implemented mask mandates increased spending by \$76B (when we include week fixed effects, CI = −\$19B to \$152B) to \$155B (when we do not include week fixed effects, CI = \$90B to \$229B), which reflects a change of about 1.7%–3.5% of the actual consumer spending.[21]

6.2. Imposition of Governmental Restrictions

We next examine the impact of a suite of nonmask governmental NPIs: closing of public venues, closing of nonessential businesses, closing schools, imposing shelter-in-place orders, and limiting public and religious gatherings. We impose all of these restrictions because the correlation between these restrictions is high, making it hard to accurately tease apart the effect of each specific order. In all of these simulations, the mask mandates are assumed to be at the levels that are observed in the data.

Our model finds that these restrictions were very successful at reducing the spread of COVID-19, much more than masks: Comparing the number of diagnosed cases that would be forecasted when all variables (except cases and social distancing, as described previously) are at their actual levels to the forecasts when these six NPIs were not imposed anywhere shows that the NPIs that were imposed reduced COVID-19 cases by 34M (CI = 27M–40M), corresponding with 1,230,000 lives saved (CI = 1,005,000–1,446,000). To get a sense of how large this effect is, this effect size reflects a 90% decrease in the number of cases that we forecast would have occurred if the NPIs were not implemented. However, these restrictions came at a significant cost; Our model with week fixed effects estimates a loss of consumer spending of \$470B (CI = \$123B–\$859B), whereas our model without week fixed effects estimates a loss of consumer spending of \$734B (CI = \$372B–\$1,101B), reflecting an 11%–16% reduction of spending compared with what we forecast spending would have been in the absence of these restrictions. In total, the impact of the NPIs on lives saved and spending corresponds to a cost of $387,000 (CI = $44K–$788K) to $608,000 per life saved (CI = $221K–$1,003K).[22]

It is helpful to benchmark our cost per life saved against economic estimates of the value of a human life. The government's value of a life is $7.4–11.6M,[23] implying that it was strongly worth imposing these NPIs. Some readers may object that older people are more likely to die from COVID-19, so the average value of lost lives might be lower. Hall et al. (2020) find that each year of a lost life is valued at $100,000–$400,000. Using the ratio of years of deaths from COVID-19 in the United States, as reported in Mitra et al. (2020) (Table 3, assuming a lifespan of 80 years), we see that each COVID-19 death represents a loss of approximately seven years, implying a valuation of $700,000–$2,800,000 per death. Thus, the imposition of these NPIs was cost effective, even if the cost per life saved is at the high end of our confidence interval.

7. Conclusion

Given the contentious views many politicians and citizens had toward mask mandates and other governmental restrictions that were imposed to stem the spread of COVID-19, it is important to understand the extent to which these interventions reduced the spread of COVID-19 and their effects on consumer spending. We show that social distancing and governmental NPIs reduced the spread of COVID-19. Mask mandates may also reduce the spread of COVID-19, and they appear to actually somewhat increase consumer spending. The other governmental restrictions we examine are more effective at stopping the spread of COVID-19 than masks but come with a reduced level of consumer spending. Thus, we evaluate the cost of each life that is saved in terms of lost consumer spending, finding that these NPIs were a very cost-effective way to save lives.

Appendix

A.1. Converting County-Weekly Level Predicted Consumer Spending Recovery Index to Actual Dollars

Given that the predicted response of our spending model is consumer spending recovery index and that we are interested in converting such indices to actual dollar amount in the counterfactual studies, we implement the following steps to achieve the goal.

We first get the iteratively predicted county-level social distancing and case measures for each day and for all counties. We then take the average of the seven daily social distancing indices across the week.

Once we get the predicted county-weekly indices, we then seek to convert them to actual dollars for easier interpretation. Because we only have national-monthly personal consumption expenditures (PCEs) in 2020, and our predicted indices are at the county-weekly level, we further do the following transformation. We first aggregate county-weekly indices to state-weekly indices, weighting by 2019 county-level GDP.[24] We then average the predicted and actual state-weekly indices in each month for each state so that we have a proxy for the predicted and actual state-monthly recovery index. Based on how the recovery index is defined in Chetty et al. (2020), we derive the state-monthly ratio between predicted and actual indices by calculating the following:

$$\textit{County Monthly Ratio} = \frac{\textit{Predicted County Monthly Index} + 1}{\textit{Actual County Monthly Index} + 1}.$$

Finally, we get the national-monthly ratio by weighting the state-monthly ratio obtained previously with 2019 state-level GDP.[25] The idea is that a 1% recovery in a large state (reflected by pre-COVID GDP) has a larger effect on national PCE spending in 2020 than a 1% recover in a small state. After calculating the national-monthly ratio, we get the predicted national-monthly PCE as

$$\begin{aligned}&\textit{Predicted National Monthly PCE}\\&\quad = \textit{Predicted National Monthly Ratio}\\&\qquad \times \textit{Actual National Monthly PCE in 2020},\end{aligned}$$

where *Predicted National Monthly Ratio* is the weighted sum of all state-monthly ratios defined previously, and *Actual National Monthly PCE* is obtained from the Bureau of Economic Analysis.

A.2. Sensitivity of Case Regressions with Different Ratios of Actual Cases to Reported Cases

A.2.1. First-Stage Regression F Statistics We report the first-stage F statistics of each endogenous variable in regressions reported in the paper in Tables A.2–A.4. The IV-induced improvements of R^2 in those first-stage regressions can be accessed at https://tinyurl.com/CovidDataShare.

A.2.2. Robustness Check: Subsample vs. Full Sample for Case and Social Distancing Estimations We report our estimations of the disease and social distancing models using both the subsample of 1,685 counties for which we have the spending data and those that use the full sample of counties in Tables A.5 and A.6. We observe qualitatively similar results.

Table A.1. Sensitivity Check: Different Ratios of Actual Cases to Reported Cases

	$\frac{\text{Actual}}{\text{Report}} = 5$	$\frac{\text{Actual}}{\text{Report}} = 10$	$\frac{\text{Actual}}{\text{Report}} = 1$		$\frac{\text{Actual}}{\text{Report}} = 5$	$\frac{\text{Actual}}{\text{Report}} = 10$	$\frac{\text{Actual}}{\text{Report}} = 1$
Independent variable	Estimates/ standard error	Estimates/ standard error	Estimates/ standard error	Independent variable	Estimates/ standard error	Estimates/ standard error	Estimates/ standard error
Temperature (°F)	−0.002 (0.001)	−0.003* (0.001)	0.000 (0.001)	*Closing of public venues × Log(pop. density)*	−0.367*** (0.054)	−0.383*** (0.064)	−0.298*** (0.038)
Humidity (%)	0.004*** (0.001)	0.004*** (0.001)	0.004*** (0.000)	*Closing of public venues × Frac. of Black*	0.524 (0.547)	0.647 (0.638)	0.298 (0.389)
Social distancing	−0.433*** (0.090)	−0.386*** (0.106)	−0.480*** (0.061)	*Closing of public venues × Trump 2020 vote share*	−2.125*** (0.584)	−2.443*** (0.677)	−1.377*** (0.440)
Mask Mandates	0.062 (0.063)	0.024 (0.073)	0.141*** (0.045)	*Closing of nonessential businesses × Log(pop. density)*	0.053 (0.053)	0.021 (0.062)	0.110*** (0.039)
Social distancing × Mask mandates	−0.076 (0.066)	−0.073 (0.078)	−0.078* (0.046)	*Closing of nonessential businesses × Frac. of Black*	4.238*** (0.883)	5.646*** (1.055)	1.492** (0.612)
Closing of public venues	0.100 (0.081)	0.135 (0.094)	0.006 (0.059)	*Closing of nonessential businesses × Trump 2020 vote share*	1.443** (0.688)	2.323*** (0.816)	−0.334 (0.478)
Closing of nonessential businesses	0.056 (0.099)	0.159 (0.117)	0.126* (0.070)	*Closing of schools × Log(pop. density)*	−0.194*** (0.048)	−0.306*** (0.057)	0.075** (0.036)
Closing of schools	−0.274*** (0.106)	−0.378*** (0.126)	0.038 (0.076)	*Closing of schools × Frac. of Black*	−1.595* (0.962)	−2.015* (1.125)	−0.272 (0.691)
Shelter in place	−0.072 (0.081)	−0.147 (0.097)	0.096* (0.055)	*Closing of schools × Trump 2020 vote share*	0.662 (0.569)	0.482 (0.648)	1.177** (0.460)
Gathering size limits	−0.271*** (0.093)	−0.388*** (0.109)	−0.050 (0.067)	*Shelter in place × Log(pop. density)*	0.009 (0.039)	0.018 (0.046)	0.015 (0.028)
Religious gathering limits	−0.333*** (0.088)	−0.469*** (0.103)	−0.069 (0.062)	*Shelter in place × Frac. of Black*	−0.863** (0.359)	−1.072** (0.427)	0.539** (0.258)
Social distancing × Log(pop. density)	0.072*** (0.015)	0.088*** (0.018)	0.034*** (0.010)	*Shelter in place × Trump 2020 vote share*	−0.243 (0.488)	−0.440 (0.576)	0.148 (0.353)
Social distancing × Frac. of Black	0.120 (0.169)	0.075 (0.199)	0.141 (0.118)	*Gathering size limits × Log(pop. density)*	0.047 (0.052)	0.067 (0.061)	0.020 (0.038)
Social distancing × Trump 2020 vote share	0.850*** (0.193)	0.892*** (0.225)	0.631*** (0.140)	*Gathering size limits × Frac. of Black*	−4.856*** (1.014)	−6.316*** (1.218)	−2.244*** (0.679)
Mask Mandates × Log(pop. density)	0.017 (0.027)	0.039 (0.031)	−0.024 (0.018)	*Gathering size limits × Trump 2020 vote share*	−1.058 (0.865)	−1.283 (0.995)	−0.703 (0.673)
Mask Mandates × Frac. of Black	0.614* (0.314)	0.917** (0.369)	−0.018 (0.221)	*Religious gathering limits × Log(pop. density)*	0.329*** (0.057)	0.369*** (0.067)	0.218*** (0.040)
Mask Mandates × Trump 2020 vote share	−0.660** (0.307)	−0.536 (0.355)	−0.897*** (0.224)	*Religious gathering limits × Frac. of Black*	−3.167*** (0.826)	−4.236*** (0.979)	−1.086* (0.559)
				Religious gathering limits × Trump 2020 vote share	0.107 (0.593)	−0.457 (0.705)	1.196*** (0.408)
Observations	372,710	372,710	372,710	Overidentification statistic	33.70	35.43	20.15
R^2	0.16	0.10	0.31	Underidentification statistic	1,442.731	7,701.519	7,700.639
County fixed effects	Yes	Yes	Yes	Kleibergen Paap	14.459	24.326	24.323
Date fixed effects	Yes	Yes	Yes	Weak instrument statistic			
Estimation period	4/17/31	4/17/31	4/17/31				

Note. Standard errors are clustered at the county level.
$^*p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A.2. Case Model First Stage FStats

Endogenous variable	First-stage F statistics
Social distancing	109.06
Mask mandates	275.413
Social distancing × *Mask mandates*	126.425
Closing of public venues	256.197
Closing of nonessential businesses	262.73
Closing of schools	205.442
Shelter in place	373.826
Gathering size limits	270.058
Religious gathering limits	221.855
Social distancing × *Log(pop. density)*	581.37
Social distancing × *Frac. of Black*	398.932
Social distancing × *Trump 2020 vote share*	643.111
Mask mandates × *Log(pop. density)*	1,533.44
Mask mandates × *Frac. of Black*	3,109.757
Mask mandates × *Trump 2020 vote share*	1,708.102
Closing of public venues × *Log(pop. density)*	1,454.952
Closing of public venues × *Frac. of Black*	1,449.005
Closing of public venues × *Trump 2020 vote share*	1,685.226
Closing of nonessential businesses × *Log(pop. density)*	1,714.373
Closing of nonessential businesses × *Frac. of Black*	1,127.801
Closing of nonessential businesses × *Trump 2020 vote share*	1,742.812
Closing of schools × *Log(pop. density)*	750.381
Closing of schools × *Frac. of Black*	320.165
Closing of schools × *Trump 2020 vote share*	629.136
Shelter in place × *Log(pop. density)*	1,632.565
Shelter in place × *Frac. of Black*	2,859.996
Shelter in place × *Trump 2020 vote share*	1,732.073
Gathering size limits × *Log(pop. density)*	493.765
Gathering size limits × *Frac. of Black*	773.89
Gathering size limits × *Trump 2020 vote share*	546.98
Religious gathering limits × *Log(pop. density)*	830.352
Religious gathering limits × *Frac. of Black*	783.21
Religious gathering limits × *Trump 2020 vote share*	868.485

Table A.3. Social Distancing Model First-Stage F Statistics

Endogenous variable	First-stage F statistics
Mask mandates	271.756
Closing of public venues	255.091
Closing of nonessential businesses	261.130
Closing of schools	205.529
Shelter in place	367.430
Gathering size limits	268.670
Religious gathering limits	221.897
Mask mandates × *Log(pop. density)*	1,536.259
Mask mandates × *Frac. of Black*	3,108.395
Mask mandates × *Trump 2020 vote share*	1,705.222
Closing of public venues × *Log(pop. density)*	1,459.751
Closing of public venues × *Frac. of Black*	1,446.534
Closing of public venues × *Trump 2020 vote share*	1,686.548
Closing of nonessential businesses × *Log(pop. density)*	1,721.644
Closing of nonessential businesses × *Frac. of Black*	1,128.846
Closing of nonessential businesses × *Trump 2020 vote share*	1,741.174
Closing of schools × *Log(pop. density)*	752.001
Closing of schools × *Frac. of Black*	319.264
Closing of schools × *Trump 2020 vote share*	630.084
Shelter in place × *Log(pop. density)*	1,628.465
Shelter in place × *Frac. of Black*	2,863.858
Shelter in place × *Trump 2020 vote share*	1,725.579

Table A.3. (Continued)

Endogenous variable	First-stage F statistics
Gathering size limits × Log(pop. density)	494.543
Gathering size limits × Frac. of Black	770.143
Gathering size limits × Trump 2020 vote share	546.280
Religious gathering limits × Log(pop. density)	830.704
Religious gathering limits × Frac. of Black	778.998
Religious gathering limits × Trump 2020 vote share	869.542

Table A.4. Spending Model First-Stage F Statistics

Endogenous variable	First-stage F statistics
Social distancing	77.411
Mask mandates	55.859
Social distancing × Mask mandates	37.953
Closing of public venues	34.484
Closing of nonessential businesses	32.700
Closing of schools	16.934
Shelter in place	46.272
Gathering size limits	19.943
Religious gathering limits	33.091
Social distancing × Log(pop. density)	147.642
Social distancing × Frac. of Black	96.083
Social distancing × Trump 2020 vote share	160.634
Mask mandates × Log(pop. density)	80.063
Mask mandates × Frac. of Black	99.505
Mask mandates × Trump 2020 vote share	85.137
Closing of public venues × Log(pop. density)	46.505
Closing of public venues × Frac. of Black	49.234
Closing of nonessential businesses × Trump 2020 vote share	61.078
Closing of nonessential businesses × Log(pop. density)	49.713
Closing of nonessential businesses × Frac. of Black	41.499
Closing of nonessential businesses × Trump 2020 vote share	60.028
Closing of schools × Log(pop. density)	21.856
Closing of schools × Frac. of Black	16.396
Closing of schools × Trump 2020 vote share	26.451
Shelter in place × Log(pop. density)	59.241
Shelter in place × Frac. of Black	115.630
Shelter in place × Trump 2020 vote share	68.177
Gathering size limits × Log(pop. density)	16.164
Gathering size limits × Frac. of Black	38.323
Gathering size limits × Trump 2020 vote share	18.392
Religious gathering limits × Log(pop. density)	33.119
Religious gathering limits × Frac. of Black	69.558
Religious gathering limits × Trump 2020 vote share	42.820

Table A.5. Standard SIR Model Subsample vs. Full Sample

Independent variable	(1) Estimates/standard error	(2) Estimates/standard error	Independent variable	(1) Estimates/standard error	(2) Estimates/standard error
Temperature (°F)	−0.002 (0.002)	−0.002 (0.001)	*Closing of public venues × Log(pop. density)*	−0.342*** (0.081)	−0.367*** (0.054)
Humidity (%)	0.004*** (0.001)	0.004*** (0.001)	*Closing of public venues × Frac. of Black*	0.874 (0.684)	0.524 (0.547)
Social distancing	−0.205* (0.110)	−0.433*** (0.090)	*Closing of public venues × Trump 2020 vote share*	−1.539* (0.821)	−2.125*** (0.584)
Mask Mandates	0.134 (0.082)	0.062 (0.063)	*Closing of nonessential businesses × Log(pop. density)*	0.002 (0.076)	0.053 (0.053)
Social distancing × Mask mandates	−0.168** (0.081)	−0.076 (0.066)	*Closing of nonessential businesses × Frac. of Black*	1.096 (0.951)	4.238*** (0.883)
Closing of public venues	0.049 (0.119)	0.100 (0.081)	*Closing of nonessential businesses × Trump 2020 vote share*	0.394 (0.946)	1.443** (0.688)
Closing of nonessential businesses	−0.161 (0.146)	0.056 (0.099)	*Closing of schools × Log(pop. density)*	−0.323*** (0.098)	−0.194*** (0.048)
Closing of schools	−0.214 (0.162)	−0.274*** (0.106)	*Closing of schools × Frac. of Black*	−1.564 (1.513)	−1.595* (0.962)
Shelter in place	0.227** (0.107)	−0.072 (0.081)	*Closing of schools × Trump 2020 vote share*	2.329** (0.974)	0.662 (0.569)
Gathering size limits	−0.057 (0.146)	−0.271*** (0.093)	*Shelter in place × Log(pop. density)*	−0.030 (0.054)	0.009 (0.039)
Religious gathering limits	−0.337*** (0.128)	−0.333*** (0.088)	*Shelter in place × Frac. of Black*	−0.674 (0.508)	−0.863** (0.359)
Social distancing × Log(pop. density)	0.052** (0.023)	0.072*** (0.015)	*Shelter in place × Trump 2020 vote share*	0.590 (0.626)	−0.243 (0.488)
Social distancing × Frac. of Black	0.395* (0.236)	0.120 (0.169)	*Gathering size limits × Log(pop. density)*	0.047 (0.114)	0.047 (0.052)
Social distancing × Trump 2020 vote share	0.284 (0.245)	0.850*** (0.193)	*Gathering size limits × Frac. of Black*	−3.486*** (1.061)	−4.856*** (1.014)
Mask Mandates × Log(pop. density)	0.019 (0.041)	0.017 (0.027)	*Gathering size limits × Trump 2020 vote share*	−2.108 (1.465)	−1.058 (0.865)
Mask Mandates × Frac. of Black	0.670 (0.438)	0.614* (0.314)	*Religious gathering limits × Log(pop. density)*	0.350*** (0.085)	0.329*** (0.057)
Mask Mandates × Trump 2020 vote share	−0.831** (0.373)	−0.660** (0.307)	*Religious gathering limits × Frac. of Black*	−1.387* (0.819)	−3.167*** (0.826)
			Religious gathering limits × Trump 2020 vote share	−0.225 (0.844)	0.107 (0.593)
Observations	205,570	372,710	Overidentification statistic	29.73	33.70
R^2	0.10	0.16	Underidentification statistic	543.698	1442.731
County fixed effects	Yes	Yes	Kleibergen Paap weak instrument statistic	2.838	14.459
Date fixed effects	Yes	Yes			
Estimation period	4/17/31	4/17/31			

Notes. Standard errors are clustered at the county level. Dependent variable is *Log(Reproduction Ratio)*.
$^*p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A.6. Social Distancing Model Subsample vs. Full Sample

Independent variable	(1) Estimates/ standard error	(2) Estimates/ standard error	Independent variable	(1) Estimates/ standard error	(2) Estimates/ standard error
Local weekoverweek growth rate in cases	−0.0004** (−0.0002)	−0.0001 (−0.0002)	*Closing of public venues × Log(pop. density)*	0.083** (0.039)	0.084*** (0.031)
National weekoverweek growth rate in cases	0.089*** (0.009)	0.074*** (0.009)	*Closing of public venues × Frac. of Black*	0.525 (0.382)	0.655** (0.270)
Local cases in the past 7 days per 1,000 people	0.039*** (0.005)	0.022*** (0.005)	*Closing of public venues × Trump 2020 vote share*	0.145 (0.387)	0.169 (0.317)
National cases in the past 7 days per 1,000 people	0.175*** (0.028)	0.105*** (0.024)	*Closing of nonessential businesses × Log(pop. density)*	0.029 (0.036)	0.005 (0.029)
Precipitation (inch)	0.075*** (0.003)	0.067*** (0.002)	*Closing of nonessential businesses × Frac. of Black*	0.989** (0.454)	1.062** (0.415)
Temperature (°F)	−0.002*** (0.001)	0.003*** (0.0002)	*Closing of nonessential businesses × Trump 2020 vote share*	0.693 (0.465)	0.346 (0.346)
Mask mandates	0.097*** (0.026)	0.089*** (0.019)	*Closing of schools × Log(pop. density)*	0.125** (0.050)	0.105*** (0.028)
Closing of public venues	0.001 (0.058)	0.026 (0.042)	*Closing of schools × Frac. of Black*	0.762 (0.971)	0.140 (0.574)
Closing of nonessential businesses	−0.103 (0.069)	−0.121** (0.058)	*Closing of schools × Trump 2020 vote share*	−2.562*** (0.571)	−1.876*** (0.413)
Closing of schools	0.163* (0.090)	0.280*** (0.061)	*Shelter in place × Log(pop. density)*	0.052* (0.031)	0.072*** (0.024)
Shelter in place	0.314*** (0.054)	0.366*** (0.043)	*Shelter in place × Frac. of Black*	−0.777*** (0.222)	−0.593*** (0.178)
Gathering size limits	0.139 (0.086)	0.166*** (0.051)	*Shelter in place × Trump 2020 vote share*	−0.185 (0.325)	0.124 (0.289)
Religious gathering limits	−0.114 (0.073)	0.065 (0.045)	*Gathering size limits × Log(pop. density)*	−0.078 (0.059)	−0.047 (0.033)
Mask mandates × Log(pop. density)	−0.007 (0.013)	−0.010 (0.010)	*Gathering size limits × Frac. of Black*	−0.725 (0.619)	−0.746 (0.536)
Mask mandates × Frac. of Black	0.056 (0.192)	−0.043 (0.123)	*Gathering size limits × Trump 2020 vote share*	2.410** (0.942)	2.021*** (0.563)
Mask mandates × Trump 2020 vote share	−0.241* (0.139)	−0.211* (0.111)	*Religious gathering limits × Log(pop. density)*	0.024 (0.041)	0.004 (0.024)
			Religious gathering limits × Frac. of Black	−1.661*** (0.452)	−1.862*** (0.380)
			Religious gathering limits × Trump 2020 vote share	−0.155 (0.346)	−0.174 (0.266)
Observations	205,570	372,710	Overidentification statistic	180.24	187.95
R^2	0.84	0.79	Underidentification statistic	925.24	1,545.917
County fixed effects	Yes	Yes	Kleibergen Paap weak instrument statistic	7.736	26.916
Day of week fixed effects	Yes	Yes			
Week fixed effects	Yes	Yes			
Estimation period	4/17/31	4/17/31			

Notes. Standard errors are clustered at the county level. Dependent variable is *Social Distancing*.
$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Endnotes

[1] World Health Organization COVID-19 Dashboard, https://covid19.who.int. Accessed on February 24, 2022.

[2] See https://tinyurl.com/CovidDataShare for more details.

[3] This measure of social distancing is imperfect for at least two reasons. First, consumers regularly click into and out of the apps that are collecting this location data. The hope is that by using aggregated information that we obtain a measure that averages out the individual variability of who is online, at least to a factor of proportionality. Second, it is possible that people who are at home are not socially distancing, because they could be hosting a gathering. Similarly, people who are not home may be isolated in their activity away from their house.

[4] $\frac{\left(\frac{Spending(Date\ 2020)}{Spending(January\ 2020)}\right)}{\left(\frac{Spending(Date\ 2019)}{Spending(January\ 2019)}\right)} - 1$. See Chetty et al. (2020) for more details.

[5] See https://www.littler.com/publication-press/publication/facing-your-face-mask-duties-list-statewide-orders and https://www.cnn.com/2020/06/19/us/states-face-mask-coronavirus-trnd/index.html. Accessed on October 28, 2020.

[6] See https://www.keystonestrategy.com/coronavirus-covid19-intervention-dataset-model/, accessed on May 15, 2021.

[7] Acemoglu et al. (2020) and Gomes et al. (2020) show the importance of including heterogeneity in SIR models. Elder people are also disproportionally affected by COVID-19. However, we are unable to incorporate them in the analysis because there is a high correlation between the proportion of elder people and Trump vote share (Pew Research Center 2018).

[8] We consider a robustness check by setting the scaling factor between actual and reported cases as 10 or 1. These alternative assumptions have little impact on the magnitudes of other variables than the fixed effects. Please see Table A.1.

[9] The four dummy variables are as follows: Democrat governor with Democrat legislature; Democrat governor with at least one legislative branch controlled by the GOP; GOP governor with at least one legislative branch controlled by the Democrats; GOP governor with GOP legislature. We thank an anonymous reviewer for this suggestion.

[10] The logic of our instruments is based on the assumption that the extent to which a person's responsiveness to the mask mandates, NPIs, and even local social distancing patterns, is driven by politics that is dependent on their views and the media they watch, but not directly on the politics of people in different counties. The politics of the state or DMA as a whole can affect the policies that they will face or the media slant that they are exposed to, but we assume that people residing in different counties do not affect the responsiveness of individuals in different counties except through these policies or media messages. Thus, the instruments are measured at larger geographic levels (state and DMA), which should affect the regulations and political slant of the media, while the responsiveness to the endogenous variables (NPIs, masking and social distancing) operates only at a more-local (county) level.

[11] The F statistics of first-stage regressions appear in the appendix: see Table A.2 for the SIR model, Table A.3 for the social distancing model, and Table A.4 for the spending model. The corresponding IV-induced incremental R^2 of the first-stage regressions are reported at https://tinyurl.com/CovidDataShare.

[12] We assess goodness of instruments by reporting overidentification, underidentification, and Kleibergen Paap weak instrument statistics in each of the tables. In Table 2, the overidentification statistics has a p value of nearly one, which implies the IVs are jointly uncorrelated with the errors. The underidentification statistic shows the IVs are significantly correlated with the endogenous variables ($p < 0.01$). The Kleibergen–Paap statistic can be used to test weak IV and is robust to heteroskedasticity (Kleibergen and Paap 2006, Baum et al. 2007). Although researchers have not found the cutoffs for hypothesis inference of this statistic, Baum et al. (2007) suggest using 10 as a "rule-of-thumb" cutoff value. Accordingly, the Kleibergen–Paap statistic value of 14.459 implies the IVs are unlikely to be weak IVs.

[13] The pairwise correlations between the six NPI policies range from 0.18 to 0.75, with a median correlation of 0.43.

[14] We define local or national week-over-week growth rate in the confirmed cases as (total confirmed cases in the past 1–7 days)/(total confirmed cases in the past 8–14 days + 1) − 1.

[15] In Table 3, the overidentification statistics of columns (1) and (2) have p values of 0.999 and 0.996, respectively. They imply, for both specifications, the IVs are jointly uncorrelated with the errors. The underidentification statistics of both columns show the IVs are significantly correlated with the endogenous variables ($p < 0.01$ in both cases). The Kleibergen–Paap statistics are greater than 10 in both columns, implying the IVs are unlikely to be weak IVs.

[16] Although we believe that the estimates reflect the real tradeoff of local versus national cases, it is also the case that there is more measurement error (in percentage terms) in local cases. Thus, we cannot rule out that some of this difference in the estimates is due to attenuation bias.

[17] In Table 4, the overidentification statistics of both columns (1) and (2) have p values of nearly one, which implies the IVs are jointly uncorrelated with the errors. The underidentification statistics of both columns show the IVs are significantly correlated with the endogenous variables ($p < 0.01$ for both columns). The Kleibergen–Paap statistics are greater than 10, implying the IVs are unlikely to be weak IVs.

[18] Extracting the cases from the fitted log of the reproduction ratio (Equation (3)) also involves accounting for the past cases. For this, we use the predicted cases from the previous days.

[19] The National Personal Consumer Expenditure (PCE) is published monthly by the Federal Reserve Bank of St. Louis, see https://fred.stlouisfed.org/series/PCE (accessed March 22, 2021). We report more details on converting index to dollars of spendingin the appendix.

[20] We assume that 3.657% of confirmed cases lead to death. This is calculated by taking the cumulative number of confirmed COVID-19 cases on July 31, 2020, and comparing that to the total number of COVID-19 deaths on August 13, 2020. The 13-day delay between diagnosis to death is based on this article: https://wwwnc.cdc.gov/eid/article/26/6/20-0320_article, accessed March 16, 2021.

[21] If mask mandates had been imposed on the rest of the country, this would have saved a statistically insignificant 37,000 additional lives (CI = −11,000 to 99,000). The spending change prediction depends on whether one includes week fixed effects (a decrease of $50B in spending, CI = $4B increase to $114B decrease) or does not include week fixed effects (an increase of $187B, CI = $157B − $224B).

[22] These ratios are calculated for each set of parameter draws and then we take the average. They are not ratios of the averages. Also, we replicate our simulations with case and distancing estimations that use only the observations for the 1,685 counties for which we have the spending data. The estimates of these models are reported in Tables A.5 and A.6. Our subsample estimates yield a cost of $476K per life saved (CI = $118K–$971K) without fixed effects, or $374K per life saved (CI = $22K–$786K), which is statistically indistinguishable from the numbers using the full-sample estimates.

[23] The Environmental Protection Agency uses $7.4M (https://www.epa.gov/environmental-economics/mortality-risk-valuation#whatvalue, accessed June 3, 2021). The Department of Transportation uses $11.6M

(https://www.transportation.gov/office-policy/transportation-policy/revised-departmental-guidance-on-valuation-of-a-statistical-life-in-economic-analysis, accessed June 3, 2021).

[24] We choose to use 2019 county-level GDP as opposed to 2019 county-level PCE for weighting because county-level PCE is not publicly available.

[25] We find a 99% correlation between state-level PCE and state-level GDP, which adds support to our choice of county-level GDP for weighting.

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