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Business Restrictions and COVID-19 Fatalities

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We collect a time-series database of business and related restrictions for every county in the United States from March through December 2020. We find strong evidence consistent with the idea that employee mask policies, mask mandates for the general population, restaurant and bar closures, gym closures, and high-risk business closures reduce future fatality growth. Other business restrictions, such as second-round closures of low- to medium-risk businesses and personal care/spa services, did not generate consistent evidence of lowered fatality growth and may have been counterproductive. (*JEL* I18, H70, G38)

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Worldwide, the COVID-19 pandemic has taken nearly four million lives as of the publication of this paper. In an attempt to slow this loss of life, policy makers around the globe have introduced a wide range of interventions. But there remains widespread disagreement about which policies are effective. Given concerns about the economic costs of widespread business and social restrictions, it is crucial that policy makers make informed trade-offs. This paper aims to shed light on this empirical issue. We construct a time-series database of business closures and related restrictions for every county in the United States

* We emphasize that we are not epidemiologists and are not aiming to explain how the virus spreads or why people might alter their behavior in ways that make certain interventions more or less effective at reducing COVID-19-related deaths. Rather, we view our work as a start toward a cost-benefit analysis of the various economic and social policies that locales have adopted in response to the pandemic. We thank Itay Goldstein (the editor) and Ed Kaplan for helpful comments. We are grateful to Timothy Akintayo, William Babalola, William Cook, Josephine Cureton, Nora Draper, Golden Gao, Patrick Hayes, Kevin Hong, Nina Huang, Aykhan Huseynov, Ryan Jennings, Alex Liang, Christine Liaw, Gen Li, Emily Lin, Natalie Lord, Paul Nash, Vanika Mahesh, Stephen Martinez-Hernandez, David Mason, Paul Nash, Daniel Nguyen, Joojo Ocran, Sean-Michael Pigeon, Elissa Prieto, Preston Smith, Mingjun Sun, Crystal Wang, Joanna Wrobel, and Charlotte Zimmer for excellent research assistance. We also thank the Tobin Center for Economic Policy and the International Center for Finance for their generous financial support. Two earlier versions of this manuscript (with the same title) analyzed policy and fatality data through September 1, 2020, and December 1, 2020, respectively. Data licenses are available. See https://som.yale.edu/covid-restrictions for details. Send correspondence to Matthew Spiegel, matthew.spiegel@yale.edu.

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from March 1 through December 31, 2020. Using these data, we then relate policies to future deaths due to COVID-19. County-level restrictions are the unit of interest because a county is the finest level of detail for which daily death counts are available. To our knowledge, ours is the most comprehensive database of U.S. COVID-19 business restrictions that has been assembled to date.

State and county governments in the United States have introduced a variety of policies to reduce virus transmission and deaths. These include stay-at-home orders; general business closures; specific closures targeting bars, restaurants, gyms, and personal care services (which we define as "spas"); no visitation policies at nursing homes; mandatory mask orders; park and beach closures; and limits on the size of gatherings. We collect start and end dates and policy restarts, where applicable, for each of these restrictions and we use them to relate current policy interventions to future growth in fatalities. The variety of tools available to regulators, heterogeneous adoption and staggered timing that we observe in the data helps us understand the role of policy in the pandemic.

Recent papers report somewhat conflicting results on how effective various policies have been. For example, Courtemanche et al. (2020) find evidence that some government-imposed restrictions have aided in the control of COVID-19, while Atkeson et al. (2020) suggest that they may not. Another strand of literature seeks indirect evidence of how policies affect health by looking at changes in mobility (e.g., Dave et al. [2021] and Nguyen et al. [2020] both report evidence that restrictions decrease mobility). Many of these recent papers focus on policies introduced at the state level (e.g., Abouk and Heydari 2021; Friedson et al. 2020; Dave et al. 2020) or they rely on cross-country evidence (e.g., Askitas, Tatsiramos, and Verheyden 2020; Flaxman et al. 2020), where social norms, health care infrastructure, and demographics are likely to vary widely.

We analyze counties rather than states (or countries) in order to exploit the granularity of the available fatality data as well as county location and relative size within a state. The paper focuses on fatalities rather than cases because of substantial variation in testing capacity over time and region. We examine a number of specifications that are designed to deal with the twin issues of potential false positives and false negatives. Given the progression of the disease (i.e., days from exposure to infection to hospitalization and death), we focus most of the discussion on policy effectiveness at the 4- to 6-week-ahead horizons because these are more likely to capture the potential effects of current policies.

Overall, we find strong and consistent evidence across specifications that employee mask policies, mask mandates for the general population, restaurant and bar closures, gym closures, and the types of businesses allowed to open in phase 3 of a state's plan (risk level 3 businesses, which often include movie theaters and bowling alleys) predict lower 4- to 6-week-ahead fatality growth across specifications. These relationships are significant, both statistically and in magnitude. For example, baseline estimates imply that a county with a mandatory mask policy in place today will experience 4- and 6-week-ahead increases in new deaths per 10,000 residents that are each 0.044 and 0.060 lower than a county without one. These reductions represent approximately 11.2% and 15.3% (respectively) of the sample mean of weekly new fatalities per 10,000 population. Costlier policies, such as gym closures and high-risk business closures, are also associated with reductions in fatalities, but they are similar or even smaller in magnitude. The evidence suggests that restaurant closures are likely to be particularly effective. Baseline estimates imply that counties implementing restaurant closures today reduces new deaths per capita of .143 in 6 weeks, or 36.4% of the fatality growth rate's sample mean.

We fail to find consistent evidence in support of the hypothesis that closing spas, parks and beaches, and general low- to medium-risk businesses reduce fatality growth.[1] In fact, spa closures and second-time closures of relatively low-risk businesses (those allowed to reopen in typical phase 1 and phase 2 reopenings) appear to have been counterproductive. The same is true for gathering limits at 100 or more. These findings may indicate substitution by the public into other types of activities that increase transmission or endogenous reductions in the exercise of caution when safety rules are in place.[2]

Any study that tries to link policy interventions and outcomes has to distinguish between correlation and causation. Policies implemented near the natural peak of an outbreak will be followed by mechanical declines in death rates and can lead to false positives. Conversely, policies that only partially mitigate death rates and are implemented in an environment where fatalities are rising may yield false negatives. We try to address the false positive and negative issue through a variety of methods. As a starting point, all of our regressions control for the current level of deaths per capita, lagged fatality growth rates, and a number of demographic and weather-related variables. Thus, our regressions predict differences in the future growth in fatalities in two counties that today have the same current level of deaths per capita, the same recent trajectory in deaths and similar demographics and climate. They differ in that their governments have introduced different policy interventions. In addition, we take three approaches to help address potential endogeneity concerns.

First, we investigate whether the estimates vary with how long a given policy is in place. If policy introductions simply reflect expected trends, then we would

1 These businesses were allowed to open in what many states call “Phase 1” openings. Businesses in this category vary according to the counties’ definitions of risk, but they often include retail outlets, offices, childcare services, and manufacturing facilities. Phase 2 typically expands the list to personal care services. Since restaurants, bars, gyms, and spas are tracked separately, a particular phase is listed as starting or ending only when it includes a business line outside one of these four groups.

2 Evidence consistent with this view has been found in other settings. For example, Risa (2001) finds that mandatory seatbelt laws increase the rate at which other road users (e.g., pedestrians and bicyclists) are injured in urban areas. Jones and Tomcheck (2000) find that pedestrian crosswalks in Los Angles increase the rate at which pedestrians are involved in accidents. In athletics, a meta-analysis by Schneider et al. (2017) finds that protective headgear and face shields are ineffective at reducing concussion risk.

not expect the relationship between policies and future fatalities to vary with how long a policy is in force. We find evidence that policy duration matters for many of the policies that appear to help, as well as some of the counterproductive ones. The results of the dynamic analysis also shed some light on how long a policy maker should wait before lifting restrictions.

Second, we use the fact that many of the county regulations that we observe are imposed at the state-level through Governors' executive orders. Even if we assume optimal policy setting by the states, it is likely that what is optimal for some counties within a given state is not optimal for others. Moreover, a state's most populous areas are likely to be an important focus of elected officials. Following this intuition, we remove the top 5 most populous counties in each state from the sample and repeat the analysis. The idea is that smaller counties often inherit state-level regulations that are intended to reduce transmission and deaths in the state's more populous regions. To date, we have seen legal challenges to state regulations by rural counties in Pennsylvania, reports of defiance of state mandates in some California and Texas counties, and requests by some North Carolina and Maryland counties for looser state restrictions. All of this suggests less populous areas are not driving policy. This lets us examine potentially "out-of-equilibrium" policies in the more rural areas to help with identification.[3]

Finally, we conduct matched-sample tests in which we focus on policy variation near state borders. However, unlike typical designs that exploit discontinuities by focusing on differences between counties that lie on state boundaries, we focus on the subset of counties that lie near, but not on, a state border. For expositional purposes, we refer to these counties as "near-border" counties. We examine these near-border counties (as opposed to on-the-border counties) to reduce spillover effects. These spillovers come in two forms. First, if a neighbor's policy reduces disease transmission in its jurisdiction, it will also lower the transmission level across the border and thereby reduce fatalities in the county of interest. Second, a restrictive policy in one county (such as bar closures) and a less restrictive one in a neighboring county may induce residents of the county with the tighter restriction to travel to and engage in otherwise prohibited activities in the less restrictive one. These spillovers can cause direct comparisons between the counties that share a border to generate false negatives and false positives. We try to mitigate this problem by putting at least a one-county buffer between any near-border county in our sample and its neighboring state.

In the near-border county analysis, potential matches must be in another state and have a population centroid within 100 miles of the target county's population centroid. Among this group, the county that is closest to the county of interest across several population and weather characteristics (based on a

[3] See, for example, *County of Butler et al. v. Thomas W. Wolf et al.* (Civil Action No. 2:20-cv-677), Koseff (2020), Weinberg (2020), Walter (2020), and Lewis (2020).

Euclidean measure) is then selected. We perform a variety of checks to validate the matches. We then add the near-border neighboring county's policies as control variables in the predictive regressions. The underlying assumption is that differences in policies across state borders are likely due to differences in opinion (which introduces exogenous error). Under these assumptions, we find that the results are largely in line with what we find using the other approaches. In addition to current deaths per capita and lagged growth, we include a number of additional controls variables to sharpen the overall interpretation.

Although all of these tests and controls should mitigate concerns about potential endogeneity, we acknowledge that we still lack a clean experimental setting that would allow us to make unambiguous causal statements. All of our findings should be interpreted with the understanding that there are limits to the design, but our hope is that the analysis provides new insights into policies and future fatalities.

As noted previously, the tests in this paper control for population demographics, weather and lagged growth in COVID-19 fatalities. Some of these control variables are of independent interest. For example, the findings presented here parallel those in the literature on demographic disparities in COVID-19 (e.g., Millitt et al. 2020; Moore et al. 2020). Counties with a greater proportion of Black and Hispanic individuals in the population, those with lower per-capita income, and in those counties with high rates of some comorbidities (such as diabetes) all experience higher fatality growth rates. We also find that extreme weather provides additional explanatory power.

Ultimately, society needs a way to trade off the costs and benefits of policy interventions. Initially, papers like Barro et al. (2020) and Correia et al. (2020) used data from the 1918 Spanish flu outbreak to link economic restrictions to lives saved. Absent any other data, this seems like a reasonable first pass. However, the considerable advances in technology since 1918 limit the degree to which these historical data might apply now or going forward. Theorists have also sought to help us better understand the trade-offs between economic restrictions and lives saved. Both Eichenbaum, Rebelo, and Trabandt (2021) and Jones and Tomcheck (2000) produce models in which policy makers can broadly slow the economy in exchange for slowing the spread of the virus. Given how little we know about the effectiveness of various restrictions, it is difficult to introduce a more nuanced and better-calibrated set of restrictions into the mathematical settings. Papers like this one can help refine that discussion. Once we know how people react to various interventions over time, models like these can help us reduce the economic damage per life saved.

Our work also opens up a way to refine some of the early research on the impact of COVID-19-related restrictions on the microeconomy. The asset pricing literature now includes papers on growth expectations (Gormsen and Koijen 2020), sector-by-sector stock returns (Bretscher et al. 2020), commercial real estate values (Ling, Wang, and Zhou 2020) and household spending (Baker et al. 2020b). On the corporate side, papers tackle borrowing patterns

(Acharya and Steffen 2020), bank lending (Li, Strahan, and Zhang 2020b), and capital market access (Halling, Yu, and Zechner 2020). These studies help quantify the economic impact from some of the policies issued by higher level government authorities. Going forward, however, the question of what impact effective versus ineffective policies had on the economy remains. To answer such questions, papers like this one can provide a set of variables that can be used to see how the economy reacted to various restrictions the interplay between them and the pandemic's spread.

We leave policy makers with the unenviable task of balancing public health concerns with the costs and benefits associated with the various restrictions that have been considered. Our primary goal is to provide data that can inform the calculation. Still, we also emphasize that in every specification, mask mandates are associated with lower future fatality rates and have by relatively low economic and social costs. The evidence that masks are beneficial is growing rapidly (Howard et al. (2020) provide a review of the literature[4]), but our paper is the first (to our knowledge) to compare it with such a wide range of alternative interventions. Other policies come at higher cost. In those cases, we hope our results can help policy makers better assess the necessary trade-offs.

1. Nonpharmaceutical Interventions and COVID-19: What We Know So Far

The literature on nonpharmaceutical interventions (NPIs), which are intended to help reduce transmission and disease without medications or vaccines, is growing fast. Many papers exploit cross-country differences in policy approaches to the pandemic and they introduce epidemiologic models and simulation methods that use contact probabilities to estimate the effect of NPIs on the spread of COVID-19 (see, e.g., Flaxman et al. 2020; Haug et al. 2020). Other studies take the approach of using mobility data to estimate the effects (e.g., Askitas, Tatsiramos, and Verheyden 2020, 2021). Studies that examine the effectiveness of masks often rely on laboratory evidence on filtering efficacy of various types of masks and on evidence from randomized controlled trials (RCTs) that study transmission of other viruses, such as influenza (Howard et al. [2020] provide a review).

The literature has some conflicting views about the effectiveness of strong versus weak social and business regulations imposed at the national level. Bendavid et al. (2021) compare daily growth in infections across counties with strong (England, France, Germany, Iran, Italy, the Netherlands, Spain, and the United States) and weak (Sweden and South Korea) limits. They conclude that stronger business restrictions did little to slow the spread of COVID-19 during March and April of 2020. Their direct cross-country comparison bears some resemblance to our nearest neighbor analysis. While some of our results support

[4] See also Abaluck et al. (2020) and Lyu and Wehby (2020).

their conclusions, we also find that many interventions help. This difference could be due to our focus on U.S. counties. It also could be because our data extend many months into the pandemic.

Recent work by Li et al. (2020a) also examines a cross-country setting, but the analysis is dynamic: the authors quantify the transmission impact of introducing and lifting restrictions over time on the London School of Hygiene & Tropical Medicine (London, UK) estimated COVID-19 reproduction number (R). They find that school closures, workplace closures, public events and gathering bans, stay-at-home and mobility restrictions are all associated with lower transmission of COVID-19 with a lag of 1–3 weeks. Our approach is consistent with the horizon required to achieve lower case numbers in Li et al. (2020a) in that we focus on 4- and 6-week-ahead fatality growth (and, in an extended analysis, we examine the dynamic relationship). In addition, we find support for some of their conclusions in that some, but not all, workplace closures reduce COVID-19 fatality growth. Unlike Li et al., we do not find evidence that gathering bans help. The differences may be due to the granularity of the data. Li et al. (2020a) use broad aggregated restriction measures, while our study uses categories that are more specific.

Our study contributes to the question of how of various restrictions to stem the spread of COVID-19 relate to future fatalities. By running a "horserace," papers like ours can help identify those interventions with the highest impact and lowest cost. Unlike most existing studies, we use data at the U.S. county level, and we add to the analysis-specific business restrictions that have been common in the United States, such as restaurant, bar, gym, and spa closures.

2. Data

The main goal of this paper is to relate weekly growth in fatalities to policies restricting businesses and related activities. The data come from a variety of sources, which we describe below.

2.1 Growth in new deaths

The fatalities data are from USAFacts.org, which disseminates daily U.S. deaths and confirmed cases at the county level. For each week t, we examine the relationship between policies in place and the future growth in new deaths due to COVID-19. The dependent variable of interest is the t-week-ahead growth in fatalities, calculated as the weekly change in cumulative fatalities per 10,000 population. Because of potential irregular reporting following weekends, we focus the analysis on Wednesday-to-Wednesday fatality data. We require at least one fatality in the county as of week t for inclusion in the analysis. This requirement helps ensure at least some virus is circulating in the community as of the date of interest. We control for 6 weeks of lagged weekly fatality growth in all regressions. We end the forecast period on December 31, 2020, which means that we predict fatality growth through February 10, 2021 (in the 6-week-ahead regressions).

2.2 Policies

We collect the county-level restrictions through internet searches for county and state orders (usually available on their websites) as well as news publications. When the state document that imposes an order is found but does not clarify the date on which a restriction becomes effective or ends, we conduct a search of news articles to determine the start or end date. Because news reports can provide inconsistent information, we try to find at least two articles to confirm the date.[5]

Table 1 fully lists all policies that we track. In many cases, the date that a particular order goes into effect is collected from Governors' executive orders and affects all counties within a given state. In some cases, county commissioners issue their own orders. In a few others, state courts overturned some or all of the regulations. When gathering the data, unless a state order applies to every county and negates all of the individual county orders, the date of the state's order is entered only into the counties to which it applies. A county is recorded as having an order in place on a particular date if either the county or the state imposes that order on or before the date in question and neither the county nor the state is recorded as having ended the order. A court order ending a restriction is entered into the counties to which it applies on the date that the court order goes into effect.

In addition to county and state government websites, we sent emails to all counties (usually public health divisions, where such contact information is listed on county websites) to confirm the restrictions and dates that are in our data. In most cases, when we heard back, respondents provided confirmation that the information was either correct or that our start or end date was off by less than one week.[6] Whenever we receive corrections, we replace our data with the data from the email response.

Although ours is the most comprehensive data set of county-level policies of which we are aware, it is also important to note that school closures are not included in the analysis. This is because school reopenings and temporary closures have been occurring at the district or even subdistrict level. Even when open, some schools follow hybrid models, while others remain fully open. This introduces substantial within-county and between-county variation that

[5] To further improve the data's accuracy, most of the entries have been verified two or three times by different individuals.

[6] At the time of this writing, we received replies from 240 counties. In 61.3% (147) of the responses, changes were suggested. When we received changes, they were additions to the list of orders (18.3 percent of responses); date corrections within one week of the original week (4.2%); and date corrections exceeding 7 days of the original week (2.9%). The remainder (13.3% of the responses) contained other information, such as links (without further clarification) to orders we had already parsed.

Table 1
County-level business restrictions due to COVID-19

Policy intervention	Description
Stay-at-home orders	"Stay-at-home order" issued by state or county government
State of emergency	"State of emergency" issued by state or county government
Nursing home must accept positive	Nursing homes required to accept COVID-19-positive residents
No nursing home visitation	Nursing home visitors prohibited
Employee masks	Mandatory or recommended face coverings for employees
Masks recommended in public	Recommended face coverings in public
Mandatory masks in public	Mandatory face coverings anywhere. This includes policies that mandate face coverings in all public places, as well as those that require masks in a subset of public places
Beaches or parks closed	Beaches or parks completely closed to the public. Closures must be total; no pedestrian traffic
No elective procedures	Any elective medical procedures (medical procedures including dental and eye) prohibited
Restaurants and bars closed	Both restaurants and bars closed with the possible exception of takeout services
Bars closed/restaurants open	Additional restrictions on bars, with bars and nightclubs closed (with the possible exception of takeout services), while restaurants are open
Gyms closed	Fitness facilities and gyms closed to all indoor activities
Spas closed	Personal care services, such as barbershops, salons, and related services closed to all indoor activities
Gatherings limited to 10	Gathering ban, where gatherings are limited to 10 people
No gatherings over 100	Gathering ban, where the limit is less than or equal to 100 people, and greater than 10.
No gatherings, limit>100	Gathering ban, where the limit exceeds 100 people
Risk level 1 closed	General business closure policy in effect. Business risk levels are defined in accordance with the reopening phases set by counties. When a county adopts more than four phases, we group additional phases according to their proximity to one another in time. If all businesses are open, risk level 1, risk level 2, risk level 3, risk level 4 dummies all equal zero. When a general business closure policy is in effect, risk level 1, risk level 2, risk level 3, risk level 4 dummies all equal one
Risk level 2 closed	Phase 1 reopening policy in effect, where all but low and medium-risk businesses remained closed. When a county is in phase 1, the risk level 1 dummy equals zero and the dummies for risk levels 2, 3, and 4 all equal one
Risk level 3 closed	Phase 2 reopening policy in effect, where higher- and highest-risk businesses remained closed. When a county is in phase 2, the risk level 1 and 2 dummies equals zero and the dummies for risk levels 3 and 4 equal one
Risk level 4 closed	Phase 3 reopening policy in effect, all but the highest-risk businesses remain closed. When a county is in phase 3, the risk levels 1, 2, and 3 dummies equal zero and the dummy for risk level 4 equals one
Business reopenings reversed	Phased business reopening reversed

This table describes each of the policy interventions. Policy variables are dummies equal to one if a given policy is effective during week t and zero otherwise.

we have not gathered data on and for which we cannot control in the county-level regressions. Reassuringly, despite several papers on the question, only weak evidence suggests that school settings are important contributors to the spread of COVID-19 (for a review, see, e.g., Viner et al. 2020).

Table 1 shows that, unlike other policies, this study pairs bars and restaurant closures. Although in theory jurisdictions can close restaurants, while leaving bars open, none ever has. The regressions and related tests therefore treat policies that close both restaurants and bars as one variable and those that close bars while opening restaurants as another.

2.3 Other control variables

We control for several demographic variables known to be associated with COVID-19 fatalities. These include the fraction of the population that are Black, Hispanic, Asian, Native American, and other races (*Black, Hispanic, Asian, Native American*, and *other*, respectively); fraction of the population that is over the age of 65 (*Age65plus*) and over the age of 85 (*Age85plus*); the fraction of the population living in nursing home facilities *(Nursing Home Pop.)*; per capita income; the fraction of the population with diabetes, who are obese, or who smoke (*Diabetes, Obesity*, and *Smoker*, respectively); density of the population, defined as total population divided by the land square miles of the county; and housing density. The demographic controls that come from the U.S. Census are based on the most recent year for which data are available. Per capita income is from the Bureau of Economic Analysis. Finally, county health data on diabetes, obesity and smoking comes from County Health Rankings & Roadmaps (see https://www.countyhealthrankings.org/ for additional details).

We also control for weather conditions, given the evidence that indoor transmission is more likely than outdoor spread and that climate has the potential to play a role (e.g., Baker et al. 2020a; Qian et al. 2021; Carlson et al. 2020). We introduce five weather variables to capture the propensity of people to find outdoor air uncomfortable and to seek temperature-controlled indoor environments: *HotHumidWeekdays*, equals one of the average weekday temperature is above 80°F and the average weekday dew point is above 60°F; *HotHumidWeekends*, the percentage of weekend days in which the average weekend temperature is above 80°F and the average weekend dew point is above 60°F; *ColdWeekdays*, the percentage of weekdays (Monday through Friday) in which the average temperature is below 60°F; *ColdWeekends*, the percentage of weekend days in which the average temperature is below 60°F; and *AverageTemperature*, the absolute value of the difference between the average daily temperature for the week and 70°F. The weather data are at the station level and are obtained from the National Climatic Data Center. We take the daily average of each temperature and dew point variable from the three weather stations that are closest to the coordinates of the county's population centroid.

Finally, we consider the number of days since the county's first reported case of COVID-19 and the number of days since March 1, 2020. The latter is included to control for potential improvements in the treatment and management of the disease over time. Table 2 summarizes all of the fatality, policy, and control variables that we use in the analysis.

3. Empirical Analysis

3.1 Forecasting *t*-week-ahead fatality growth

We begin with a baseline specification, in which we forecast 1-, 2-, 4-, and 6-week-ahead fatality growth (change in deaths per 10,000 population) as a

Table 2
Summary of fatality, policy, demographic, and weather variables

A. Fatalities

Variables	N	Mean	25th pctl	Median	75th pctl	SD
Deaths per capita (10,000)	84,193	5.416	1.208	3.316	7.385	6.180
$Growth_t$	84,193	0.393	0.000	0.051	0.444	0.946

B. Policies

		Policy dummy	Weeks in force, conditional on $Policy_t = 1$				
Variables	N	Mean	Mean	25th pctl	Median	75th pctl	SD
Stay at home	84,193	0.061	10.904	6.000	8.000	11.000	9.408
State of emergency	84,193	0.954	26.011	18.000	27.000	35.000	9.79
Nursing home accept pos.	84,193	0.147	21.122	13.000	21.000	30.000	10.027
No nursing home visit	84,193	0.463	19.303	11.000	18.000	26.000	9.562
Employee masks	84,193	0.812	19.035	11.000	19.000	27.000	9.725
Masks recommended	84,193	0.946	21.492	14.000	22.000	30.000	9.794
Mandatory masks	84,193	0.609	15.649	8.000	15.000	22.000	9.023
Beaches or parks closed	84,193	0.028	13.73	6.000	9.000	21.000	9.887
No elective procedures	84,193	0.042	4.983	2.000	5.000	7.000	3.357
Restaurants and bars closed	84,193	0.057	7.014	6.000	7.000	9.000	2.876
Bars closed/rest. open	84,193	0.194	9.086	3.000	7.000	14.000	7.01
Gyms closed	84,193	0.117	9.792	7.000	9.000	12.000	5.433
Spas closed	84,193	0.086	10.26	7.000	9.000	12.000	5.658
Gatherings limited to 10	84,193	0.280	12.187	6.000	10.000	17.000	8.666
No gatherings over 100	84,193	0.375	12.053	4.000	11.000	19.000	8.678
Gathering limit over 100	84,193	0.210	11.531	5.000	11.000	18.000	7.632
Risk level 1 closed	84,193	0.030	7.765	6.000	7.000	8.000	3.557
Risk level 2 closed	84,193	0.094	12.954	7.000	9.000	13.000	9.229
Risk level 3 closed	84,193	0.309	20.632	10.000	19.000	31.000	11.164
Risk level 4 closed	84,193	0.530	22.889	13.000	23.000	32.000	10.554
Bus. openings reversed	84,193	0.090	12.515	5.000	12.000	20.000	8.177

function of current deaths per 10,000 population; a vector of lagged weekly death growth; time since the first positive COVID-19 case in the county; and the time since March 1, 2020. We also include the controls for current weather conditions and county demographics. The results are in Table 3.

From column 1 of Table 3, we observe that weekly fatality growth is positively autocorrelated at up to approximately five lags and is decreasing with the current level of fatalities. We also find that the growth in deaths is generally higher in climates where temperatures are uncomfortable (i.e., they deviate more from 70°F), but this relationship is dampened on hot and humid and cold weekends, perhaps because it is easier to stay at home when weather is uncomfortable. We find that counties with greater Black, Hispanic, and Native American populations experience greater fatality growth than other counties. Counties with larger nursing home populations,[7] higher population densities and more residents who smoke or have diabetes also see greater future growth in fatalities. High per capita income predicts lower future fatalities.

[7] In predicting future fatalities, the fraction of the population residing in nursing home residents is more important than the fraction of the elderly population.

Table 2
Continued

C. Demographic and other controls

Variables	N	Mean	25th pctl	Median	75th pctl	SD
Black	84,193	10.835	0.900	3.500	14.000	15.548
Hispanic	84,193	9.675	2.300	4.400	10.100	13.936
Asian	84,193	1.534	0.400	0.700	1.500	2.835
Native American	84,193	1.574	0.200	0.300	0.700	6.296
Other	84,193	2.195	0.400	1.000	2.300	3.827
Age65plus	84,193	17.769	15.000	17.400	19.900	4.307
Age85plus	84,193	2.149	1.600	2.000	2.500	0.816
Nursing home pop.	84,193	0.675	0.373	0.596	0.875	0.460
Per capita income	82,619	44.100	36.561	41.765	48.592	13.017
Diabetes	84,193	12.282	9.400	11.800	14.700	4.022
Obesity	84,193	33.029	29.600	33.300	36.700	5.475
Smoker	84,193	17.564	15.118	17.266	19.809	3.471
Population density	84,193	333.989	26.199	60.552	163.960	2022.760
Housing density	84,193	143.691	12.904	28.753	72.003	961.096
Average temperature	84,193	14.054	5.149	10.031	20.583	11.864
Hot humid Weekdays	84,193	0.077	0.000	0.000	0.000	0.266
Hot humid Weekends	84,193	0.101	0.000	0.000	0.000	0.302
Cold weekday	84,193	0.410	0.000	0.200	1.000	0.438
Cold weekend	84,193	0.406	0.000	0.000	1.000	0.474
Time since first case	84,193	5.053	4.771	5.170	5.438	0.478

Each observation is a county-week between March 1, 2020, and December 31, 2020. Fatality variables are in panel A. *Deaths per capita* is defined as total deaths per 10,000 population as of week t. Weekly fatality growth ($Growth_t$) is the number of new deaths per 10,000 population from week $t-1$ to week t. Only counties with at least one death as of week t are included in the sample. Panel B shows the policy variables. For the dummy column, a policy dummy equals one if it is in effect during week t. Policies are defined in Table 1. The other columns enumerate the number of consecutive weeks each policy variable has been in force as of week t. These values are calculated using data for those observations where the policy dummy equals one. Demographics and other controls are in panel C. *Black, Hispanic, Asian, Native American*, and *Other*, are the fraction of the county's population that are White, Black, Hispanic, Asian, Native American, and other races/ethnicities (respectively). *Age65plus* and *Age85plus* are the fractions of the population that are over the age of 65 and over age 85. *Nursing Home population* is the fraction of the population living in in skilled nursing facilities. *Per capital income* is the average per capita income in the county. *Diabetes, Obesity*, and *Smoker* are the fractions of the population with diabetes, who are obese, or who smoke, respectively. *Population density* is defined as total population divided by the land square miles of the county. *Housing Density* is defined as the total number of homes in the county divided by residential land area. *AverageTemperature*, the absolute deviation of the average daily temperature for the week from 70°F. *HotHumidWeekdays* and *HotHumidWeekends* are the percentage of weekdays and weekend days (respectively) in which the average temperature is above 80°F and the dew point is above 60°F. *ColdWeekdays* and *ColdWeekends* are percentage of weekdays and weekend days in which the average high temperature is below 60°F. *Time since first case* is the natural log of the number of days since the first reported case of COVID-19 in a county.

From this baseline specification, we add policy variables so that we can examine the main economic question of interest. The goal is to hold the constant the current level and recent trajectory of new deaths so that we can compare the future growth in fatalities in counties with and without various restrictions in place at time t.

3.2 Baseline analysis: Policy interventions and t-week-ahead weekly fatality growth

The main regression specification, in which we forecast the t-week-ahead weekly growth in fatalities (*Growth(t+x)*) is as follows.

Table 3
Baseline specification.

	(1)		(2)		(3)		(4)	
Variables	$Growth_{t+1}$	SE	$Growth_{t+2}$	SE	$Growth_{t+4}$	SE	$Growth_{t+6}$	SE
Deaths per capita capita	−0.004***	0.001	−0.004***	0.001	−0.006***	0.001	−0.005***	0.001
$Growth_{t-1}$	0.205***	0.014	0.183***	0.012	0.090***	0.009	0.042***	0.007
$Growth_{t-2}$	0.149***	0.014	0.098***	0.011	0.059***	0.009	0.029***	0.007
$Growth_{t-3}$	0.063***	0.012	0.036***	0.012	0.015*	0.008	0.018*	0.011
$Growth_{t-4}$	0.022*	0.013	0.027***	0.009	0.007	0.008	0.006	0.008
$Growth_{t-5}$	0.023**	0.010	0.004	0.009	0.016	0.013	−0.001	0.011
$Growth_{t-6}$	−0.005	0.010	0.004	0.008	0.005	0.009	−0.024***	0.007
Days since 1st case	0.031	0.043	−0.027	0.055	−0.060	0.052	−0.025	0.058
Days since March 1	0.069	0.051	0.203***	0.065	0.337***	0.062	0.361***	0.070
Avg temperature	0.010***	0.001	0.006***	0.001	0.001	0.001	−0.004***	0.001
HotHumid weekday	0.026**	0.011	0.076***	0.013	0.091***	0.013	0.072***	0.014
HotHumid weekend	−0.031***	0.009	0.000	0.011	0.029***	0.011	0.040***	0.011
Cold weekdays	0.045***	0.015	0.160***	0.017	0.337***	0.018	0.417***	0.018
Cold weekend	−0.044***	0.010	−0.019*	0.011	−0.014	0.013	0.018	0.013
Age 65+	0.001	0.001	0.001	0.002	0.004**	0.002	0.005***	0.002
Age 85+	0.015	0.010	0.023**	0.012	0.019	0.013	0.013	0.014
Asian	−0.002*	0.001	−0.003**	0.001	−0.005***	0.002	−0.006***	0.002
Black	0.002***	0.000	0.003***	0.000	0.003***	0.000	0.003***	0.000
Hispanic	0.004***	0.000	0.005***	0.000	0.007***	0.001	0.008***	0.001
Native American	0.003***	0.001	0.004***	0.001	0.006***	0.001	0.005***	0.001
Other	−0.003***	0.001	−0.004***	0.002	−0.004**	0.002	−0.005***	0.002
Per capita income	−0.000**	0.000	−0.000**	0.000	−0.000**	0.000	−0.000**	0.000
Population density	−0.000	0.000	−0.000	0.000	−0.000	0.000	−0.000	0.000
Diabetes	0.004***	0.001	0.006***	0.001	0.007***	0.002	0.009***	0.002
Obesity	−0.000	0.001	−0.001	0.001	−0.001	0.001	−0.001	0.001
Smoker	0.005***	0.001	0.007***	0.002	0.008***	0.002	0.011***	0.002
Housing density	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Nursing home pop.	0.153***	0.017	0.176***	0.020	0.201***	0.021	0.223***	0.023
Constant	−0.756***	0.069	−1.166***	0.080	−1.720***	0.087	−2.020***	0.097
Observations	82,619		82,619		82,619		82,619	
Adjusted R-squared	.1956		.1576		.1199		.1070	

This table shows results of regressions in which we regress x-week-ahead change in deaths per 10,000 population (*Growth(t+x)*) on current cumulative deaths per 10,000 population in the county (*Deaths per capita*); lagged changes in deaths per capita, time controls, weather information, and demographic data. *Growth(t-x)* denotes the x-week lagged weekly change in deaths per 10,000 population in deaths. *Time since 1st case* is the number of days since the first reported case of COVID-19 in a county and t is the number of days since March 1, 2020. The demographic variables are defined in Table 2. Each observation is a county-week. All standard errors (in parentheses) are clustered at the county level. $*p<.1$; $**p<.05$; $***p<.01$.

$$\Delta Deaths\,Per\,Capita_{i,t+X} = \alpha + \beta_1 Policies_{i,t} + \beta_2 Deaths\,Per\,Capita_{i,t} + \sum_{\tau=1}^{6} \beta_{3,\tau} \Delta Deaths\,Per\,Capita_{i,t-\tau} + \beta_4 * Days\,First + \beta_5 * t + \beta_6 * Controls + \varepsilon_{i,t} \tag{1}$$

In the calculated weekly growth, X is set to 1, 2, 4 or 6 depending on the specification. *Policies* is the vector of policies in place in county i during week

t, as defined in Table 1; *DaysFirst* is the number of days since the first case is reported in the county; t is the number of days since the beginning of the sample period (March 1, 2020), and *Controls* are a vector of population demographics and other county-level control variables, as shown in Table 2. All standard errors are clustered at the county level.

The specification in Equation (1) uses information available through week t to forecast the X week-ahead weekly growth in fatalities. We focus on the relationship between fatalities and policies in place as of week t, after controlling for the current level of deaths per capita, the recent trajectory of growth, and a number of demographic and other controls. According to the CDC, the median incubation period from exposure to symptom onset is 4-5 days. Among people with severe disease, the median time to ICU admission from the onset of illness or symptoms ranges from 10 to 12 days.[8] For patients admitted to the hospital and who do not survive, Lewnard et al. (2020) report a median duration of hospital stay of 12.7 days (ranging from 1.6 to 37.7). Given the progression of the disease, we focus the discussion on the 4- to 6-week-ahead horizons because these are more likely to capture the potential effects of current policies than the 1- and 2-week ones. Links between policies and short-term fatality growth could reflect autocorrelation in policies. That is, we could observe a link between a given policy and week-ahead deaths because that policy has already been in place in a county for some time (Table 2, panel B, shows that most of the policies that we study are in force for several weeks at a time). Links between policies and short-horizon growth might also indicate existing trends or show when a policy is perhaps coincidental or even reactive. The longer-horizon forecasts are less likely to reflect trends or reactive policies. In later analyses, we examine trends at the time of policy introduction in order to shed light on this potential issue. We also estimate the relationship between the number of weeks a policy has been in force and fatality growth.

Results from estimating Equation (1) are in Table 4. Columns 3 and 4, that predict 4- and 6-week-ahead weekly fatality growth, are of greatest interest. Policies that have negative and significant coefficient estimates (i.e., predict lower future fatality growth) at both 4 and 6 weeks are employee mask recommendations, mandatory mask use for the general population, restaurant and bar closures, and gym closures. High-risk business closure (risk level 3) has a negative and significant coefficient at the 6-week, but not the 4-week, horizon.

Among the policies with statistically significant and negative coefficients, it is possible that some of the coefficients reflect existing trends that are not captured in the lagged fatality controls. We compare the findings for short- and longer- horizon fatality growth to help with the overall interpretation. If the week 1 and week 2 coefficient values and significance levels are different

[8] https://www.cdc.gov/coronavirus/2019-ncov/hcp/clinical-guidance-management-patients.html

Table 4
Policy interventions and *t*-week-ahead weekly new fatalities

	(1)		(2)		(3)		(4)	
Variables	$Growth_{t+1}$	SE	$Growth_{t+2}$	SE	$Growth_{t+4}$	SE	$Growth_{t+6}$	SE
Stay at home	0.013	0.015	0.012	0.017	0.040**	0.019	0.004	0.019
State of emergency	0.023*	0.012	0.042***	0.015	0.050***	0.019	0.056***	0.021
Nursing accept pos.	0.004	0.007	0.002	0.008	0.008	0.010	0.009	0.011
No nursing visits	0.033***	0.007	0.029***	0.009	0.025**	0.011	0.042***	0.012
Employees masks	−0.027**	0.012	−0.040***	0.014	−0.052***	0.016	−0.049***	0.017
Masks recommended	0.054***	0.010	0.083***	0.012	0.106***	0.015	0.147***	0.017
Mandatory masks	−0.014*	0.008	−0.027***	0.010	−0.044***	0.012	−0.060***	0.013
Beaches or parks closed	0.028**	0.014	0.036**	0.016	0.029*	0.017	0.011	0.016
No elective procedures	0.126***	0.023	0.154***	0.026	0.104***	0.026	0.100***	0.026
Restaurants and bars closed	−0.029	0.019	−0.034*	0.020	−0.108***	0.022	−0.143***	0.023
Bars closed/rest. open	0.014*	0.008	0.003	0.009	−0.027**	0.011	−0.039***	0.011
Gyms closed	−0.007	0.009	−0.009	0.011	−0.034***	0.012	−0.062***	0.012
Spas closed	0.045***	0.010	0.042***	0.011	0.055***	0.012	0.069***	0.013
Gatherings limited to 10	−0.008	0.016	0.030	0.018	0.033	0.021	0.017	0.023
No gatherings over 100	0.010	0.013	0.044***	0.014	0.080***	0.017	0.065***	0.018
No gatherings limit>100	−0.050***	0.013	−0.023	0.015	0.005	0.018	0.077***	0.019
Risk level 1 closed	0.030**	0.015	0.008	0.017	−0.007	0.016	0.026	0.016
Risk level 2 closed	0.015	0.010	0.033***	0.011	0.016	0.013	−0.003	0.014
Risk level 3 closed	−0.021***	0.008	−0.015	0.009	−0.017	0.011	−0.036***	0.013
Risk level 4 closed	0.020***	0.008	0.024***	0.009	0.017	0.011	0.020	0.012
Reopenings reversed	0.052***	0.015	0.075***	0.018	0.169***	0.022	0.172***	0.022
Constant	−1.126***	0.095	−1.689***	0.113	−1.946***	0.127	−2.042***	0.138
Observations	82,619		82,619		82,619		82,619	
Adjusted R-squared	.198		.161		.125		.114	
Controls	Yes		Yes		Yes		Yes	

This table shows results of regressions in which we regress x-week-ahead change in deaths per 10,000 population (*Growth(t+x)*) on policy dummies and county demographic variables. All of the variables are defined in Tables 1 and 2. Like in Table 3, we also control for current and cumulative deaths per 10,000 population in the county; 6 weeks of lagged 1-week fatality growth; time controls; weather controls; and demographics controls. These controls are estimated, but not reported in the table. Each observation is a county-week. All standard errors (in parentheses) are clustered at the county level. $*p<.1$; $**p<.05$; $***p<.01$.

from what we see in weeks 4 and 6, then it is less likely that the findings at the horizons of interest reflect a trend. In Table 4, the estimated coefficients for employee masks, mandatory masks, restaurant and bar closures, and risk level 3 closures are all more negative as we lengthen the horizon from 1 and 2 weeks to 4 and 6 weeks. This strengthens the conclusion that these policies are likely to reduce new fatalities. The bar closures coefficients even switch signs as we vary the horizon. In the next section, we examine this issue further by estimating model residuals at the time of policy introductions. Doing so helps uncover any county-level trends that may be occurring at the time of implementation.

Because the policy variables are dummies, the estimated magnitudes of the coefficients in Table 4 are directly comparable. For example, the column 4 estimated coefficients of -0.049 and -0.060 on mandatory masks and employee masks, respectively, are significant, both statistically and in magnitude (they imply reductions in new deaths that are approximately 12.5% and 15.3% of the sample mean weekly new deaths of .393 per 10,000 population). These are similar or greater than the magnitudes of the estimated coefficients for other policies with negative estimated coefficients, such as gym closures (−0.062) and risk level 3 business closures (-0.036). This type of comparison is useful because the costs of these policies are likely to differ substantially.

In addition to the policies that appear to help curb fatality growth, Table 4 also shows that several policies are associated with higher future fatality growth. State of emergency declarations, no nursing home visits, mask recommendations, elective procedure limits, spa closures, gathering limits and reopening reversals show significant positive coefficient estimates in both the 4- and 6-week columns. With the exception of elective procedures, these coefficients all become more positive and significant as the forecast horizon is lengthened. The state of emergency findings are consistent with what one might expect since these declarations indicate that a policy maker expects to need resources to manage a future crisis, but how might the other policies contribute to fatalities? The finding in Table 4 that banning family nursing home visits did not lead to a strong reduction in future fatalities (to the contrary, it is associated with greater fatality growth) seems inconsistent with what we know about the number of nursing home deaths relative to the rest of the population.[9] However, all of the regressions control for the number of nursing home residents relative to total population (coefficients for this variable are positively and significant at all horizons, as shown in Table 3). It may be that most of the nursing home cases resulted from unregulated factors rather than family visits. As documented in Chen, Chevalier, and Long (2020) staff and service people frequently travel between nursing homes and they may have been the primary spreaders of infections. For other policies, it is possible that regulations caused substitution by the public into other types of activities that increase transmission. They might also cause endogenous reductions in the exercise of caution when safety rules are in place, as researchers have found in other safety settings (see e.g., Risa [2001] and Jones and Tomcheck [2000] for road safety and Schneider et al. [2017] for athletic safety equipment). For example, a rule that gatherings are restricted to 100 people could encourage weddings with 99 guests.

Below, we summarize the main findings from Table 4. The table lists policies with significant estimated coefficients at the 4- and 6-week horizons. We consider a particular result "significant" if we observe a statistically significant coefficient at one or both of the horizons (i.e., 4 weeks and/or 6 weeks). The policies in bold indicate a change sign or change in significance when we vary the horizon from short (1 to 2 weeks) to longer.

Recent work by Chen et al. (2020) uses cell phone data to link point of interest traffic to the spread of COVID-19. In line with some of the findings in Table 4, the authors report that closing restaurants, fitness centers, cafes, and snack bars would substantially reduce the transmission. Unlike our paper, the authors do not focus on government orders (which could cause variation in mobility). Nor do they examine the potential role for potentially

[9] As of March 31, 2021, the *New York Times* reported that approximately 33% of U.S. coronavirus deaths have been linked to nursing homes, while nursing homes account for only 4% of cases.

Summary of Findings in Table 4. Relationships between policy variables and future new fatalities.

	Significant at 4- or 6-week horizon	Significant at both 4- and 6-week horizons
Negative, significant	Risk level 3 closed	Employee Masks, Mandatory Masks, Restaurants and Bars Closed, **Bars Closed/Restaurants Open, Gyms closed**
Positive, significant	**Stay-at-Home**, Beaches or Parks Closed	State of Emergency, No Nursing Home Visits, Masks Recommended, No elective procedures, Spas Closed, Gatherings Limited to 100, **Gatherings limited** >**100**, Reopenings Reversed

Bold indicates a change sign or change in significance when we vary the horizon from short to longer.

important interventions unrelated to specific points of interest, such as mask mandates.

3.3 Short-horizon trends near policy implementation

The general finding in Table 4 that many policies are significant over 1- and 2-week horizons merits further discussion. To examine this, we focus the analysis on policy introductions. We then estimate the model so that we can characterize the trajectory of fatalities when restrictions are imposed. For each policy, we calculate the residuals from the regression of week-ahead change in deaths per 10,000 population. We include all of the variables from Table 3, as well as any other restrictions that are already in place at the time of the specific policy introduction (excluding policy i, which is just being introduced during introduction week I). The basic idea is that these residuals capture the trend that policy makers are reacting to at the time of policy introduction. Table 5 shows the mean week $t+1$ and $t+2$ average residual at the time of each policy introduction. From the table, with the exception of recommended masks, very little evidence indicates that policy makers are introducing policies as fatalities are declining. If anything, fatalities are rising more than the model (which controls for current and past fatalities) predicts. There are two observations. First, many of the significant coefficients that we observe over short horizons may indeed reflect policy makers responding to recent upticks in fatalities. Analysis of the relationship between policy duration and fatality growth should help disentangle policy effectiveness from the alternative explanation that policy introductions reflect these trends. Second, Table 3 shows that deaths from COVID-19 are positively autocorrelated up to 5 weeks. Given the positive residuals in Table 5, this implies that any negative coefficients that we observe over the 4- to 6-week horizons in Table 4 are strong evidence of reversal, rather than continuation, of trends. Even some of the positive coefficients, to the extent that they are smaller in magnitude to what we would observe in the absence of the policy, are consistent with some effectiveness (e.g., elective procedures are implemented when the county is already experiencing excess fatalities per 10,000 population of .113 at $t+1$ and .161 at $t+2$, which are both greater than the estimated coefficient for elective procedures of .100 at the 6-week horizon from Table 4).

Table 5
Residual fatality growth near policy introductions

	1 week ahead		2 weeks ahead	
Variables	$Mean_{t+1}$	SE	$Mean_{t+2}$	SE
Stay at home	0.091***	0.027	0.068***	0.025
State of emergency	0.015	0.042	0.105**	0.043
Nursing home accept pos.	0.288***	0.072	0.286***	0.057
No nursing home visit	0.075	0.05	0.036	0.026
Employee mask	0.018	0.016	0.004	0.016
Masks recommended	−0.048***	0.017	−0.059***	0.017
Mandatory masks	0.043**	0.022	0.031	0.022
Beaches or parks closed	0.021	0.029	−0.038	0.024
No elective procedures	0.113***	0.04	0.161***	0.039
Restaurants and bars closed	0.023	0.035	0.042	0.035
Bars closed/restaurants open	0.056***	0.015	0.054***	0.015
Gyms closed	0.096***	0.025	0.093***	0.025
Spas closed	0.100***	0.029	0.077***	0.027
Gatherings limited to 10	0.004	0.018	0.030*	0.017
No gatherings over 100	0.009	0.014	0.014	0.012
Gathering limit over 100	−0.033**	0.015	0.021	0.017
Risk levels 1 to 4 closed	0.049*	0.026	0.033	0.025
Bus openings reversed	0.099***	0.036	0.187***	0.046

This table calculates residuals from a regression of week-ahead change in deaths (*Growth(t+1)*) during the week immediately following the introduction of policy i. The control variables from Table 3 (current cumulative deaths in the county lagged changes in deaths per capita, time controls, weather information, and demographic data) are included in the regression. We also include all policies as of period t from Table 4, but we exclude newly implemented policy i, where policy i is the policy listed in the first column. $Mean_{t+1}$ denotes the week $t+1$ average change in deaths per 10,000. $Mean_{t+2}$ denotes the week $t+2$ average change in deaths per 10,000. $*p<.1$; $**p<.05$; $***p<.01$.

3.4 Dynamics

The estimated coefficients for the policy dummies in the main specification are interpreted as the relationship between having a policy in place during week t and future new fatalities during week $t+x$. While the dummy variable specification provides clear and easy to interpret results, it is also useful to understand the dynamics that might result from having a policy in place for varying time horizons. Doing so can provide further insights into the policy effectiveness interpretation. From Table 2, panel B, average duration varies by policy, with a range of 5 weeks for restrictions on elective procedures to 26 weeks for state of emergency orders. If policy introductions simply reflect expected trends, then the relationship between policies and future fatalities should not vary with how long a given policy is in force.

To incorporate dynamics, we repeat the regressions shown in Table 4, but add to the policy dummies *wks_policy* and *ln_wks_policy*. *Wks_policy* is defined as the number of weeks a given policy has been in force and *ln_wks_policy*, defined as the natural log of 1+ weeks in force. This very flexible function can produce linear, convex and concave relationships. Results of the duration analysis for each policy are in Figure 1. The plots trace fatality growth as a function of *wks_policy* and *ln_wks_policy*, up to 16 weeks.[10] Note that the

[10] Full regression results from the "weeks-in-force" analysis are shown in Table A.1.

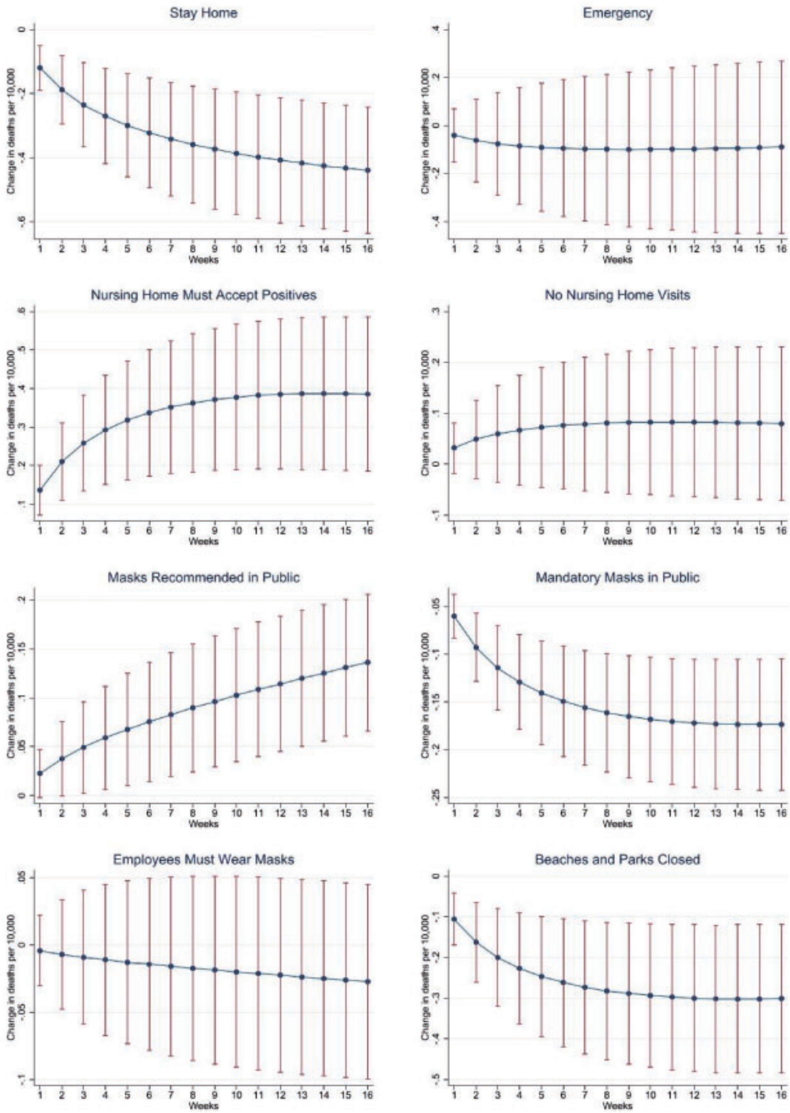

Figure 1
Estimated impact of policy duration on week $t+1$ new fatalities
The panels plot the marginal impact of each policy (defined as the change in deaths per 10,000 population) as a function of the number of weeks that each policy is in force, as of week t. The estimated impact as a function of weeks in force is given by β_1(weeks)+$\beta_2$ln(weeks+1). Estimates for β_1 and β_2 and 95% confidence intervals are based on the regressions shown in the appendix.

plots in Figure 1 do not directly map to the estimated coefficients in Table 4. This is because they come from a nonlinear model that incorporates time, and

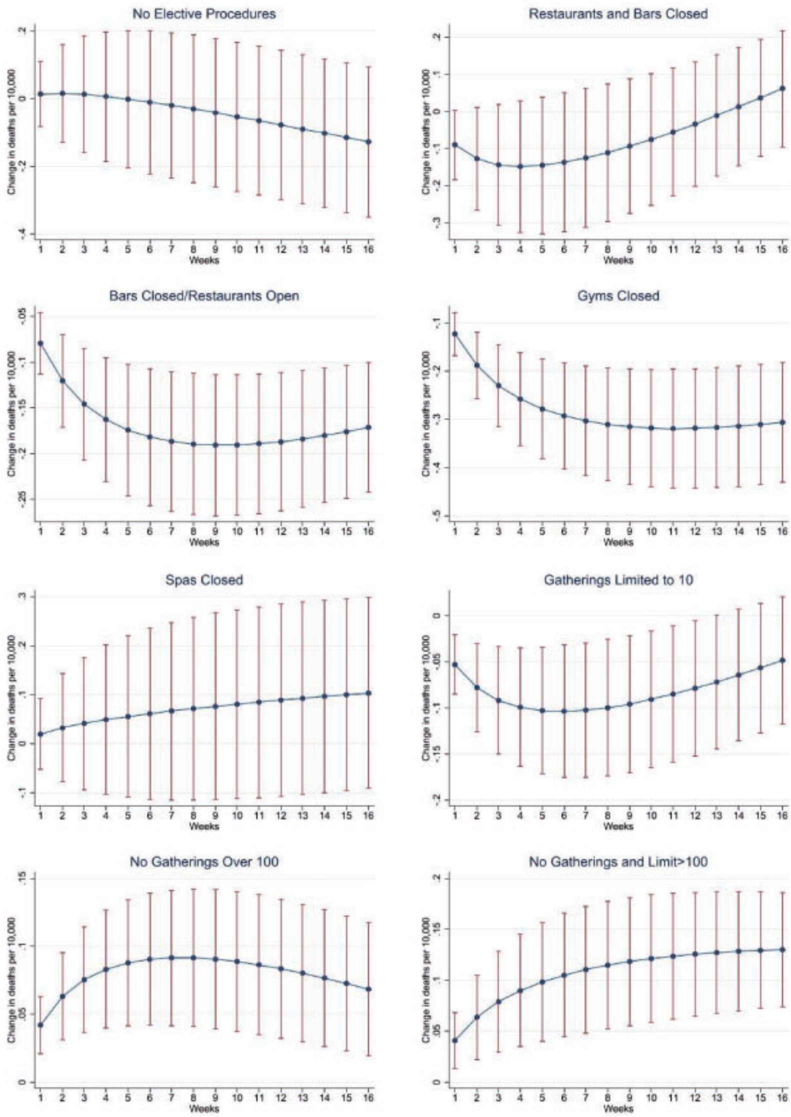

Figure 1
(Continued)

they reflect only the time component, which is of primary interest if we want to understand dynamics.

Figure 1 clearly shows that the relationship between policies and future fatalities varies significantly with the duration that the rules are in place. For example, over the 16-week horizon shown in the figure, fatality growth declines

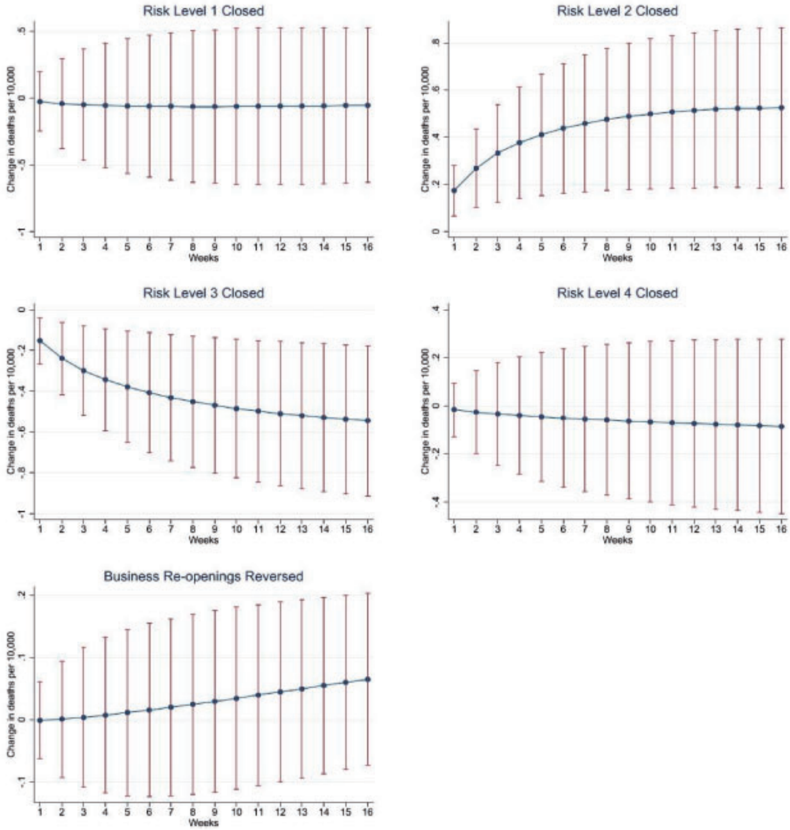

Figure 1
(Continued)

as stay-at-home orders are in place longer and then the relationship becomes more constant. (Note that stay-at-home orders do not appear to play a role in the Table 4 analysis. Perhaps because, as Table 5 shows, jurisdictions introduce them when fatality growth is exceptionally high.) The same is true for mask mandates, beach and park closures, gym closures, and higher-risk (risk level 3) closures. By contrast, long duration policies requiring nursing homes to accept COVID-19-positive patients has the opposite relationship, as do policies that restrict medium- to high-risk (risk level 2) businesses and gathering limits that exceed 100 people. Mask mandates and bar closures become more helpful when they are in place over long horizons, while short closures for restaurants are associated with the greatest declines in fatality growth.[11]

[11] In Table A.2 in the appendix, we use the estimates from the regressions shown in Figure 1 to predict fatality growth when a policy is in place for 4, 8, 12, and 16 weeks.

The findings in Figure 1 shed some light not only on the question of which policies are effective but also on how long to keep them in force. They also help to clarify some of the findings in columns 1 and 2 of Table 4, in which policies observed at week t are related to new fatalities just 1 or 2 weeks later. These relationships stem, in part, from the fact that many policies at week t have been in force for several weeks.

3.5 Removing counties likely to be the focus of regulators (the state's most populous counties)

While suggestive, the evidence in Table 4 does not establish a causal link between policies and future fatality new fatalities. The dynamic analysis of the role of policy duration in Figure 1 and Tables A.1 and A.2 in the appendix should help with the overall interpretation. However, an ideal experiment would take pairs of identical counties, impose regulation R in one county and not the other, and then measure differences in future fatalities across the "treated" counties versus those that are untreated. Because we do not have access to this type of experiment, we use the fact that many county regulations are imposed at the state level (through Governors' orders) to help with the identification. If we assume that state regulators primarily focus on the state's most populous counties, then smaller counties inherit state-level regulations that are intended to reduce transmission and deaths in the more populous regions of each state (allowing us to observe "out-of-equilibrium" policies). Following this intuition, we remove each state's 5 most populous counties from the sample and repeat the analysis in Table 4.

The results using only the less populous counties are in Table 6. Here, employee mask recommendations, mandatory mask use for the general population, restaurant and bar closures, gym closures, and high-risk business closures (risk level 3) all show comparable or stronger negative relationships with future new fatalities than in the full sample. Furthermore, all of the estimated coefficients increase in magnitude and/or significance as we move from short-horizon regression in Column 1 to the longer-horizon predictive regression in Column 4. Assuming the initiation of these policies was driven by infection rates in the more populous counties, these results buttress the idea that all of these policies indeed reduce infection rates and thus ultimately deaths. The only notable differences between Tables 4 and 6 is that we no longer find evidence that closing parks predicts a rise in fatality growth, but we do find some evidence that gathering restrictions at 10 people is counterproductive. We summarize the Table 6 findings below.[12]

[12] Tables A.3 and A.4 in the appendix are analogous to Tables A.1 and A.2 in the appendix, except that we estimate the dynamic model for less populous counties. The main findings are consistent across tables.

Table 6
Policy interventions and *t*-week-ahead weekly new fatalities in less populous counties

	(1)		(2)		(3)		(4)	
Variables	$Growth_{t+1}$	SE	$Growth_{t+2}$	SE	$Growth_{t+4}$	SE	$Growth_{t+6}$	SE
Stay at home	0.016	0.017	0.018	0.020	0.047**	0.022	0.013	0.022
State of emergency	0.021	0.014	0.040**	0.017	0.046**	0.021	0.053**	0.023
Nursing accept pos.	0.005	0.008	0.003	0.009	0.010	0.012	0.009	0.013
No nursing visits	0.035***	0.009	0.029***	0.010	0.023*	0.012	0.042***	0.013
Employees masks	−0.028**	0.013	−0.044***	0.015	−0.058***	0.018	−0.055***	0.019
Masks recommended	0.067***	0.012	0.100***	0.014	0.127***	0.019	0.173***	0.020
Mandatory masks	−0.014	0.009	−0.028**	0.011	−0.043***	0.013	−0.059***	0.014
Beaches or parks closed	0.029*	0.016	0.040**	0.018	0.032	0.020	0.013	0.019
No elective procedures	0.137***	0.025	0.168***	0.029	0.112***	0.029	0.103***	0.028
Restaurants and bars closed	−0.039*	0.021	−0.051**	0.023	−0.130***	0.024	−0.163***	0.026
Bars closed/rest. open	0.018**	0.009	0.007	0.010	−0.023*	0.012	−0.034***	0.013
Gyms closed	−0.008	0.010	−0.010	0.012	−0.036***	0.013	−0.068***	0.013
Spas closed	0.046***	0.012	0.044***	0.013	0.056***	0.014	0.068***	0.014
Gatherings limited to 10	−0.003	0.018	0.040*	0.021	0.047*	0.024	0.027	0.025
No gatherings over 100	0.014	0.014	0.052***	0.016	0.092***	0.019	0.075***	0.020
No gatherings limit>100	−0.049***	0.015	−0.018	0.017	0.015	0.019	0.095***	0.021
Risk level 1 closed	0.033*	0.017	0.009	0.019	−0.014	0.019	0.017	0.019
Risk level 2 closed	0.017	0.011	0.037***	0.013	0.020	0.015	−0.002	0.016
Risk level 3 closed	−0.026***	0.009	−0.018*	0.011	−0.017	0.013	−0.035**	0.015
Risk level 4 closed	0.022**	0.009	0.025**	0.011	0.014	0.012	0.014	0.013
Reopenings reversed	0.052***	0.016	0.075***	0.019	0.173***	0.024	0.179***	0.024
Constant	−1.214***	0.108	−1.815***	0.129	−2.054***	0.144	−2.112***	0.157
Observations	74,275		74,275		74,275		74,275	
Adjusted R-squared	.192		.155		.120		.108	
Control	Yes		Yes		Yes		Yes	

This table shows results of regressions in which we regress x-week-ahead change in deaths per 10,000 population (*Growth(t+x)*) on policy dummies. Like in Table 3, we also control for current cumulative deaths per 10,000 population in the county; 6 weeks of lagged 1-week fatality growth; time controls; and demographics controls. These controls are estimated, but not reported in the table. The specification is identical to that in Table 4 except we remove the 5 most populous counties in each state. All standard errors (in parentheses) are clustered at the county level. *$p<.1$; **$p<.05$; ***$p<.01$.

Summary of Findings in Table 6. Relationships between policy variables and future new fatalities, excluding the State's most populous counties

	Significant at 4- or 6-week horizon	**Significant at both 4- and 6-week horizons**
Negative, significant	Risk level 3 closed	Employee Masks, Mandatory Masks, Restaurants and Bars Closed, **Bars Closed/Restaurants Open, Gyms closed**
Positive, significant	**Stay-at-Home**, Gatherings limited to 10	State of Emergency, No Nursing Home Visits, Masks Recommended, No elective procedures, Spas Closed, Gatherings Limited to 100, **Gatherings limited >100**, Reopenings Reversed

Bold indicates a change sign or change in significance when we vary the horizon from short to longer.

3.6 County pair analysis

In this section, we exploit variation in policies across matching counties to help sharpen the interpretation. Standard methodology compares outcomes in two counties that share a border but are in different states, that is, a nearest-neighbor analysis. The COVID-19 setting contends with significant concerns about spillover effects. That is, a policy that reduces infections and ultimately fatalities in one county is likely to be helpful to neighbors as well. These cross-border effects may then yield estimates that imply a policy has no value to the jurisdiction imposing it when, in reality, it is not only effective but also so effective that its neighboring jurisdictions benefit as well.[13]

To help mitigate the policy spillover problem, this paper uses a variant of the nearest neighbor pairing system. We still focus on counties near state borders, but any county with a border *on* the state line is removed from the database. This leaves only counties interior to their state, putting at least one county between them and the impact of a neighboring state's policies. From the list of interior counties, the algorithm starts by calculating the distance between the target's population centroid and those of all other interior counties. For a given target county, any interior county whose population centroid lies within 100 miles of the target and located in a different state is then considered as a possible match. From the set of possible matches, counties are compared on per capita income, fraction of the population over 85, population density, housing density, weekly temperature and rain. These variables are all standardized so that a difference of 1 standard deviation is coded as 1. The distance between the counties in characteristic space is then the equally weighted Euclidean distance based on a list of demographic and weather variables:

$$d_{i,j} = \sqrt{\sum_{k=1}^{n} \left(x_{k.i} - x_{k,j}\right)^2}, \tag{2}$$

where $d_{i,j}$ is the hedonic distance between county i and j. The $x_{k,I}$ and $x_{k,j}$ represent the standardized value of characteristic k for county i and j respectively. A county i is then paired with the county that generates the lowest value of $d_{i,j}$ among the eligible set. By matching on both demographic and weather-related properties, the two counties should have similar propensities with regard to infection rates and ultimately fatalities, while minimizing spillover effects.

Table 7 compares the demographic attributes of the near-border counties with those of their neighbors. Importantly, the differences are small in magnitude relative to the attribute standard deviations. The column labeled Diff./SD displays the ratio of the mean difference across pairs divided by the standard deviation of the demographic variable across the treated counties. If matches are purely random, the difference across pairs should equal $\sqrt{2}$ times the standard deviation. However, none of the ratios exceeds 0.2 in absolute magnitude, and many are considerably smaller.

[13] Spillover effects can also generate false positives. Suppose a county with a large number of infections relative to its neighbor closes a venue (such as bars) and its neighbor does not. Residents of the county with the higher infection rate and tighter restriction may travel across the border to circumvent the regulation. Transmissions will then increase in the neighboring county relative to the county that imposed the rule. It will then appear, on a relative basis, that the rule reduced the fatality rate in counties that imposed it. In reality, the rule just increased fatalities in neighboring county.

Table 7
Near-border analysis: Match quality

	Near-border counties			Near-border neighbors		Diff. test p-value: Null: 0		Diff. test p-value: Null 0.1SD	
	Mean	Median	SD	Mean	Median	Mean	Median	Mean	Diff./SD
Date of first case (days)	160.454	126.000	77.790	164.097	133.000	.188	.106	.865	−0.047
White	82.516	89.900	17.243	82.775	91.300	.589	.163	.998	−0.015
Black	11.857	3.600	16.415	12.006	2.900	.723	.176	1.000	−0.009
Hispanic	4.916	3.100	5.482	4.285	2.800	.001	.055	.343	0.115
Asian	1.131	0.600	1.818	0.937	0.500	.002	.009	.163	0.107
Native American	0.945	0.300	4.090	0.996	0.300	.770	.717	.961	−0.012
Other	1.359	0.700	1.852	1.155	0.600	.003	.054	.216	0.110
Age65plus	17.977	17.800	3.596	18.390	18.200	.001	.097	.330	−0.115
Age85plus	2.188	2.100	0.846	2.267	2.200	.002	.018	.187	−0.093
Nursing home population	0.777	0.672	0.513	0.804	0.682	.165	.601	.789	−0.053
Per capita income	42385	40817	9966	41509	40056	.003	.055	.315	0.088
Diabetes	13.003	12.500	4.016	13.737	13.200	.000	.000	.972	−0.183
Obesity	34.015	33.900	5.050	34.447	34.700	.034	.015	.282	−0.085
Smoke	18.413	18.380	3.284	18.635	18.677	.039	.222	.680	−0.067
Population density	193.855	54.866	617.348	128.217	50.541	.001	.257	.154	0.106
Housing density	83.411	25.159	259.422	55.838	23.778	.001	.176	.154	0.106

This table compares attributes of the near-border counties with those of the near-border neighbors. Neighbors are counties in different states that are located within 100 miles of the near-border counties. All variables are defined in Table 1. The temperature variables reflect average values for the entire sample period. All other county attributes are time 4invariant. Median p-value is based on a sign test. The Null 0 columns list the p-values against the hypothesis that the treated and nearest neighbor counties have identical values. The Null 0.1SD columns lists the p-value against the hypothesis that treated and nearest neighbor counties have values within 0.1 standard deviations of each other. The standard deviation is the standard deviation among the treated counties for the variable in question. The Diff./SD column reports the ratio of the difference in the treated and nearest neighbor means divided by the treated counties' standard deviation. The number of observations equals 1,063.

Table 7 has three columns displaying p-values. The two "Null 0" columns indicate the probability the difference in category's magnitude per matched pair is equal to zero. Given the large number of counties in our data set, it is not surprising that the matching system is not perfect, and that one can routinely reject this hypothesis. The real question is the degree to which the matches differ relative to the standard deviation of the variable in question. In other words, are any differences economically meaningful? To assess this, the column labeled *Null 0.1SD* tests whether the mean difference across pairs, for the demographic, lies within .1 of the value in the standard deviation of that difference. This is a very strict standard, and even under it none of the p-values lies below 15%, and many are well over 50%.

Table 8 repeats the tests in Table 7, but this time examines the parallel trends assumption. We compare the growth in fatalities in the near-border counties with those of their near-neighbor matches in the weeks leading up to each policy introduction for the treated near-border counties. Little evidence suggests that matches differ more than by a small amount in either their fatality growth rates or recent growth in fatality growth rates. Looking at the *Null 0.1SD* column, only one policy introduction (risk level 2 closed) lies below 15% in either panel, and none is significant at the 10% level. Overall, the tests in Tables 7 and 8

Table 8
Near-border analysis: Trends in new fatalities at policy introduction

	Treated near-border counties			Nearest neighbors		Diff. test p-value: Null: 0		Null 0.1SD	
	Mean	Median	SD	Mean	Median	Mean	Median	Mean	Diff./SD
A. Prior COVID-19 Fatality Growth$_{t-1}$									
Stay at home	0.049	0.000	0.239	0.045	0.000	.760	.241	.913	0.015
State of emergency	0.001	0.000	0.016	0.000	0.000	.145	.061	.920	0.045
Nursing accept pos.	0.332	0.000	0.936	0.035	0.000	.000	.014	.994	0.317
No nursing visits	0.047	0.000	0.374	0.007	0.000	.001	.004	.192	0.107
Employees masks	0.231	0.000	0.605	0.164	0.000	.007	.002	.227	0.111
Masks recommended	0.119	0.000	0.396	0.109	0.000	.567	.067	.902	0.026
Mandatory masks	0.252	0.000	0.609	0.244	0.000	.798	.089	.934	0.013
Beaches or parks closed	0.113	0.000	0.548	0.070	0.000	.178	.031	.282	0.079
No elective procedures	0.151	0.000	0.511	0.142	0.000	.670	.479	.986	0.019
Rest. and bars closed	0.085	0.000	0.450	0.106	0.000	.299	.716	.824	−0.046
Bars closed/rest. open	0.151	0.000	0.414	0.098	0.000	.001	.000	.545	0.127
Gyms closed	0.070	0.000	0.332	0.079	0.000	.657	.004	.853	−0.026
Spas closed	0.025	0.000	0.155	0.019	0.000	.520	.000	.716	0.037
Gatherings limited to 10	0.175	0.000	0.524	0.171	0.000	.839	.283	.987	0.008
No gatherings over 100	0.233	0.000	0.577	0.220	0.000	.439	.175	.995	0.023
Gathering limit>100	0.389	0.000	1.119	0.305	0.000	.044	.612	.548	0.075
Risk level 1 closed	0.019	0.000	0.132	0.011	0.000	.126	.014	.742	0.057
Risk level 2 closed	0.207	0.000	0.716	0.157	0.000	.050	.006	.623	0.069
Risk level 3 closed	0.149	0.000	0.427	0.124	0.000	.153	.010	.756	0.056
Risk level 4 closed	0.136	0.000	0.428	0.120	0.000	.367	.213	.842	0.039
Reopenings reversed	0.556	0.265	0.843	0.805	0.194	.168	.577	.647	−0.295
B. Change in COVID-19 Fatality Growth$_{t-1}$ − Growth$_{t-2}$									
Stay at home	0.033	0.000	0.213	0.037	0.000	.754	.194	.856	−0.019
State of emergency	0.001	0.000	0.016	0.000	0.000	.145	.061	.920	0.045
Nursing accept pos.	0.226	0.000	0.841	0.017	0.000	.000	.010	.939	0.249
No nursing visits	0.034	0.000	0.341	0.003	0.000	.001	.003	.229	0.091
Employees masks	0.032	0.000	0.701	−0.017	0.000	.005	.000	.584	0.070
Masks recommended	−0.011	0.000	0.556	0.005	0.000	.564	.028	.935	−0.027
Mandatory masks	0.022	0.000	0.886	0.015	0.000	.780	.176	.903	0.008
Beaches or parks closed	0.084	0.000	0.500	0.048	0.000	.179	.046	.383	0.073
No elective procedures	0.030	0.000	0.426	0.009	0.000	.578	.147	.747	0.049
Rest. and bars closed	0.029	0.000	0.417	0.019	0.000	.242	.549	.857	0.024
Bars closed/rest. open	−0.016	0.000	0.446	−0.026	0.000	.000	.008	.900	0.022
Gyms closed	0.028	0.000	0.294	0.031	0.000	.616	.061	.948	−0.011
Spas closed	0.016	0.000	0.190	0.014	0.000	.522	.001	.919	0.008
Gatherings limited to 10	0.043	0.000	0.444	0.031	0.000	.826	.000	.912	0.026
No gatherings over 100	0.007	0.000	0.599	0.005	0.000	.385	.368	.998	0.004
Gatherings limit>100	0.147	0.000	0.995	0.060	0.000	.022	.128	.255	0.087
Risk level 1 closed	0.011	0.000	0.176	0.006	0.000	.129	.010	.953	0.024
Risk level 2 closed	0.050	0.000	0.557	−0.009	0.000	.045	.030	.112	0.105
Risk level 3 closed	−0.035	0.000	0.634	−0.023	0.000	.132	.049	.982	−0.018
Risk level 4 closed	−0.003	0.000	0.428	−0.033	0.000	.369	.083	.498	0.070
Reopenings reversed	0.101	0.000	0.838	0.315	0.000	.161	.575	.650	−0.255

This table compares the growth in fatalities in the near-border counties with those of their nearest-neighbor matches in the weeks leading up to policy introduction. Panel A shows the change in fatalities during the week prior to when a given policy goes into effect. Panel B shows the change in fatality growth over the 2 weeks prior to when a policy goes into effect. Median p-value is based on a sign test. The Null 0 columns list the p-values against the hypothesis that the treated and nearest neighbor counties have identical values. The Null 0.1SD columns lists the p-value against the hypothesis that treated and nearest neighbor counties have values within 0.1 standard deviations of each other. The standard deviation is the standard deviation among the treated counties for the variable in question. The Diff./SD column reports the ratio of the difference in the treated and nearest neighbor means divided by the treated counties' standard deviation.

show that the county matches are economically close to each other in terms of demographics, fatality growth rates and even the rate of growth in the fatality growth rate when policies are implemented.

As a final check, Figure 2 displays the growth rate in fatalities over a longer horizon from 4 weeks prior to a policy's introduction to the 4 weeks afterward. The main goal is to see whether treated and near-border control counties are on different fatality growth paths during the weeks prior to policy introduction. Consistent with Table 8, with the exception of requiring nursing homes to accept positive cases, we do not observe evidence of differential trends prior to policy introduction week 0. Of course, the graphs are noisy in that they do not condition on control variables (such as other policies that are in place or demographics); however, along with the findings in Table 8, they help validate the near-border analysis. Finally, note that the graphs in Figure 2 do not directly map onto the regressions. The regressions only include counties after they record at least one fatality (to ensure that virus is in the community). The graphs examine the data available to policy makers on the date they make their decisions. The former is appropriate if we want to know whether a policy alters the virus' spread once it appears in an area, and the latter useful if we want to rule in or out reverse causality in the decision process.

The results of the matched county analysis are in Table 9. As in the earlier tables, we find that employee mask requirements and restaurant and bar closures bring significantly fewer new deaths at both the 4- and 6-week horizons. As in the earlier tables, the estimated coefficients for these policies are generally larger as the horizon goes from 1 to 6 weeks. We also find that mask mandates and gym closures are related to lower new deaths at the 6-week horizon. However, unlike the earlier tables, we fail to find evidence in Table 9 that closing bars, while opening restaurants or risk level 3 business closures help curb fatalities.

Like Tables 4 and 6, the evidence in Table 9 again suggests that reversing business reopenings may be counterproductive, as are mask recommendations, spa closures, and rules that limit elective procedures. Like the earlier tables, we also find that gathering limits set at more than 100 people are counterproductive. The gathering limit finding is somewhat surprising. It may be that gathering limits set at 100 or more encourage larger-than-ideal small group events.

Because the matched county analysis is least likely to suffer from endogeneity problems, we place somewhat more weight on the findings in Table 9. To further limit claims that may be due to false positives, we place greatest weight on findings that are consistent across at least two of our empirical approaches. Below, we summarize the findings in Table 9.[14]

[14] Tables A.5 and A.6 in the appendix are analogous to Tables A.1 and A.2 in the appendix, except that we estimate the dynamic model for the near-border subsample, and we add control for *weeks* and *ln_weeks* for the border counties. As in the earlier tables, we do find that evidence to support the idea that longer duration stay-at-home policies are beneficial.

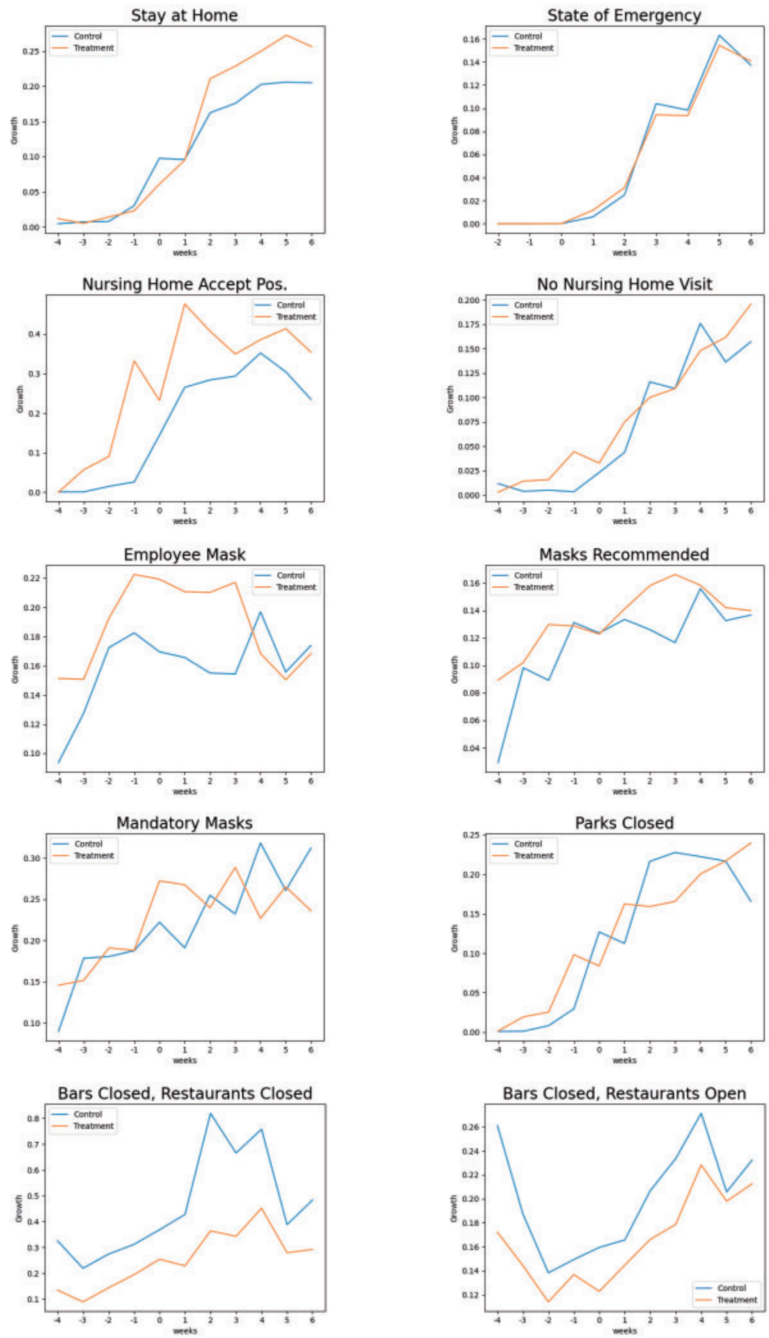

Figure 2
Growth in fatalities per 10,000 people in the 4 weeks prior to the date an order went into effect and the 4 weeks after, averaged across the counties in the nearest neighbor data set
The treatment line represents the average across the treatment counties and the target the average across the nearest neighbor control counties.

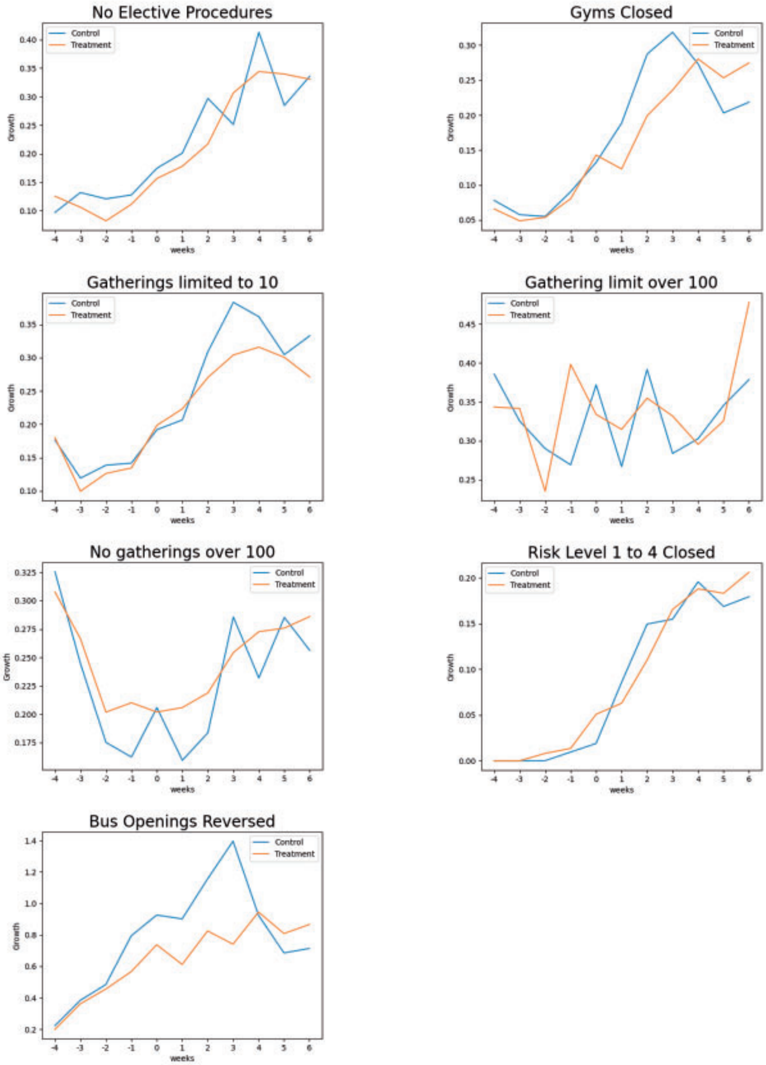

Figure 2
(Continued)

3.7 Summary of the main findings

A condensed summary of all of the estimates in the paper (across Tables 4, 6, and 9) can be found in Table A.7 in the appendix. For those focusing on the restrictions with the most robust results in terms of reducing new fatalities, they are employee mask requirements, mandatory masks, restaurant and bar closures, and gym closures. These policies predict lower 4- to 6-week-ahead

Table 9
Policy variation across the state border: Interventions and t-week-ahead weekly new fatalities (100 miles)

Variables	(1) $Growth_{t+1}$	SE	(2) $Growth_{t+2}$	SE	(3) $Growth_{t+4}$	SE	(4) $Growth_{t+6}$	SE
Stay at home	0.068***	0.020	0.070***	0.023	0.111***	0.029	0.094***	0.031
State of emergency	−0.025	0.032	−0.016	0.041	−0.016	0.050	−0.010	0.053
Nursing accept pos.	0.006	0.012	−0.002	0.014	0.001	0.018	−0.004	0.019
No nursing visits	0.016	0.012	0.004	0.015	−0.007	0.017	0.011	0.019
Employees masks	−0.029	0.019	−0.048**	0.023	−0.067**	0.027	−0.065**	0.028
Masks recommended	0.090***	0.020	0.130***	0.024	0.180***	0.028	0.220***	0.031
Mandatory masks	0.003	0.014	−0.015	0.016	−0.017	0.018	−0.036*	0.020
Beaches or parks closed	0.015	0.021	0.021	0.023	0.013	0.025	−0.015	0.025
No elective procedures	0.055	0.038	0.106**	0.047	0.042	0.056	0.110*	0.058
Restaurants and bars closed	−0.040	0.038	−0.070*	0.038	−0.189***	0.041	−0.241***	0.044
Bars closed/rest. open	0.035**	0.014	0.018	0.017	−0.000	0.021	−0.002	0.021
Gyms closed	−0.040**	0.016	−0.041**	0.019	−0.033	0.021	−0.043**	0.021
Spas closed	0.032	0.020	0.009	0.021	0.027	0.021	0.056**	0.024
Gatherings limited to 10	0.005	0.028	0.070**	0.032	0.050	0.035	0.013	0.039
No gatherings over 100	0.005	0.022	0.060**	0.024	0.103***	0.028	0.080***	0.030
No gatherings limit>100	−0.050**	0.021	−0.007	0.024	0.025	0.029	0.105***	0.032
Risk level 1 closed	−0.004	0.029	−0.033	0.030	−0.034	0.030	−0.006	0.027
Risk level 2 closed	−0.012	0.018	0.009	0.021	0.017	0.022	−0.004	0.021
Risk level 3 closed	−0.013	0.014	0.013	0.017	−0.000	0.021	−0.023	0.023
Risk level 4 closed	0.025*	0.013	0.024	0.016	0.037**	0.018	0.053***	0.019
Reopenings reversed	0.022	0.034	0.069	0.043	0.249***	0.059	0.211***	0.056
Constant	−1.088***	0.243	−1.384***	0.301	−1.448***	0.360	−1.626***	0.393
Observations	29,835		29,835		29,835		29,835	
Adjusted R-squared	.219		.177		.141		.135	
Control	Yes		Yes		Yes		Yes	
Border county policies	Yes		Yes		Yes		Yes	

This table shows results of regressions in which we regress x-week-ahead change in deaths per 10,000 population (*Growth(t+x)*) on policy dummies and county demographic variables. We include in the sample only those counties that are within 100 miles of another county and with which they do not share a border ("nearby county"). The specification extends that in Tables 4 and 6 to include we the nearby county policies to the specification. Nearby county policies are estimated, but not reported in the table. All standard errors (in parentheses) are clustered at the county level. *$p<.1$; **$p<.05$; ***$p<.01$.

Summary of Findings in Table 9. Relationships between policy variables and future new fatalities near state borders

	Significant at 4- or 6-week horizon	Significant at both 4- and 6-week horizons
Negative, significant	**Mandatory Masks**, Gyms Closed	Employee Masks, Restaurants and Bars Closed
Positive, significant	No elective procedures**,** **Spas Closed,** **Gatherings limited >100**	Stay at Home, Masks Recommended, Gatherings Limited to 100, **Reopenings Reversed**, Risk level 3 closed, Risk level 4 closed

Bold indicates a change sign or change in significance when we vary the horizon from short to longer.

new fatalities in all three empirical approaches. If one expands the set of potentially useful restrictions to those that show lower future fatalities in two of the three empirical specifications the several other policies look to be helpful. High-risk (Level 3) business closures and bar closures, even when restaurants are open, all fit into this category.

There is also consistent evidence that some policies are counterproductive. Notable among these are spa closures, gathering limitations at 100 or more, mask recommendations, and second round closures of low- to medium-risk businesses.

4. Discussion and Comparison with the Extant Literature

Overall, our findings lie somewhere in the middle of the existing results on how NPIs influenced the spread of COVID-19. Haug et al. (2020), Hsiang et al. (2020), Flaxman et al. (2020), Li et al. (2020a) and Bendavid et al. (2021) examine national level data on a small set of broadly classified restrictions through early April.[15] The former collects data on fatalities from 11 European countries and examines the degree to which lockdowns, cancellation of public events, school closures, self-isolation orders and the encouragement of social distancing affected the reproduction rate (R) of COVID-19. In the end, they only find evidence that lockdowns helped reduce the R-value. The Hsiang et al. (2020) study merges policies into an overall degree of restriction so it is not clear which particular policies helped. However, using their policy aggregate, they do find that tighter restriction levels reduced the growth in reported infection rates. Haug et al. (2020) compare a wide range of policies across countries and their impact on the R of COVID-19 from March through April. They also group policies and find that gathering limits, school closings, increased availability of personal protective equipment and national lockdowns helped.

In the U.S. setting, Friedson et al. (2020) and Siedner et al. (2020a,b) look at state-level policies and COVID-19 case data. The former examines the effectiveness of shelter-in-place orders at reducing cases and the latter on what they label social distancing regulations.[16] Both papers find that the policies they examine help reduce the pandemic's spread. In the case of Siedner et al. (2020b), the authors interpret their findings to imply that, once a state starts issuing social distancing regulations, reported cases decline by approximately 8.4% after 14 days they decline by 14.8% after 21 days. The latter is a reduction by more than half the total of 27.95 without social distancing. A direct comparison of these papers with ours is difficult, since their policy focus is quite a bit narrower. However, using the broader policy definition of Siedner et al. (2020a) we find that some of the factors they include reduce the rate at which COVID-19 fatalities increase, but others do not. For example, closing the kinds of

[15] Li et al. (2020) use data on international travel limits, internal movement limits, stay-at-home orders, public transportation closure, bans on gathering over 10 people, public event bans, workplace closures, and school closures. The data in Bendavid et al. (2020) vary by country and include as few as 2 policies (Iran) and up to 10 policies (the United States).

[16] Siedner et al. (2020a,b) define social distancing regulations as ones that close schools or workplaces, cancel public events, restrict within state movement or close a state's borders. They then define social distancing measures as being in place as of the date of the first measure on their list.

businesses that states allowed to reopen in their third such round does help, while closing less-risky businesses and spas does not.

Some studies have also tried to disaggregate the impact of various restrictions on the spread of COVID-19. One is Li et al. (2020b), who consider eight policies and conclude that five of them (school closure, workplace closure, public event bans, requirements to stay at home and internal movement limits) reduce the *R* rate of COVID-19. In contrast, Bendavid et al.'s (2021) study finds no evidence that stay-at-home orders or business closures help. Our results lie somewhere in the middle. Our data also includes business closures across various types and state government-designated degrees of risk. This allows us to examine closings of particular business types as opposed to general groupings. Because of that, we bridge some of the prior findings. We find that evidence consistent with the hypothesis that some workplace closures (higher-risk types like movie theaters and gyms) did reduce the spread of COVID-19, which is in line with Li et al. (2020b). However, others (like small box retailers and spas) did not appear to help, and those results are closer to what Bendavid et al. (2020) conclude. Some of the conflicts across these papers may be arising from the particular sectors that happen to dominate their relatively broad categories.

In terms of future work, a wide range of COVID-19-related social and business restrictions are included in our data. Those working on the economic fallout of the COVID-19 pandemic can use results like those we present to parse the economic consequences of effective, ineffective, and counterproductive policies. In current macro-level models, like those in Eichenbaum, Rebelo, and Trabandt (2021) and Jones, Philippon, and Venkateswaran (2021), policy makers can broadly restrict economic activity as a way to reduce a disease's spread. As a first pass on how to manage a pandemic, while mitigating costs, these papers make reasonable assumptions. However, as this paper shows, not all policies are equally effective, and there is some time variation in the public's response to each one. Ultimately, we need to know the cost per life saved from the whole panoply of restrictions governments might consider. Once we do, economic and epidemiological model pairings will yield even better risk management.

Beyond the broad macroeconomic effects from general business closures, targeted policies also have important economic consequences. For example, closing gyms and small establishments like spas must initially hurt the targeted sector. At the same time, it could help other sectors as people change their spending habits. The evidence in this paper suggest that closing gyms helped reduce COVID-19 fatalities and closing spas may have increased it. Markets may therefore react in fundamentally different ways to each of these regulations. Bretscher et al.'s (2020) analysis of equity returns asks whether the market reacted differently to specific types of business closures given how each ultimately played out with the pandemic. The same question arises with Ling, Wang, and Zhou's (2020) examination of commercial real estate values. Both spas and gyms typically lease their building spaces. However, our results indicate that the fatality growth paths following closures of these two types of businesses were very different. Perhaps, their impact on commercial property owners varied as well. Ineffective or counterproductive policies may have extended the public's hesitancy to patronize any local business, hurting both the target sector (spas in this example) and other open venues. Conversely, if closing a sector proves beneficial to public health (gyms in this example) that might also ultimately harm the property owners directly involved. But it may help others as the waning pandemic leads people to venture back out to those businesses that can open safely. To answer these questions, we need to know what policies ultimately fall into which category.

5. Conclusion

U.S. policy makers have the unenviable job of trading off costs and benefits in a situation where human lives are at stake. This paper aims to aid in this decision-making by providing evidence that relates a variety of policies to future growth in fatalities due to COVID-19. We find that employee mask policies, mask mandates for the general population, restaurant and bar closures, gym closures, and high-risk business closures (risk level 3) predict lower 4- to 6-week-ahead new fatalities. These relationships are significant, both statistically and in magnitude.

At the same time, some policies are associated with higher future new fatalities in at least two of three specifications. These are spa closures, restricting gatherings to 100 or more, and second-time closings of low- to medium-risk businesses, rules that limit elective procedures, mask recommendations, and stay-at-home orders. State of Emergency orders also fall into this category. However, unlike the other policies examined in this paper, emergency declarations do not in and of themselves impose any restrictions on the population. In this case we may be seeing evidence that policy makers are foreseeing the troubles that lie ahead. For the other policies, our policy duration analysis sheds additional light on whether restrictions hurt or help. We find that long-lasting stay-at-home orders appear the help curb fatalities, while the other policies that predict higher future fatalities do, indeed, appear to be counterproductive.

The regressions produce estimates relating policies to future fatalities, which can be weighed against each policy's cost. Lawmakers place their own weights in their policy objective functions when balancing various trade-offs. However, lower-cost regulations, such as mask mandates, appear to be obvious choices as the world waits for advances in science. This is consistent with recommendations in Abaluck et al. (2020) and Lyu and Wehby (2020).

We note some important limits to the overall interpretation of this paper's findings. For example, while we conduct a range of tests aimed at reducing concerns about potential endogeneity, we still lack a clean experimental setting that would allow us to make unambiguous causal statements. There is also likely to be unobserved variation in enforcement and adherence to policies some populations may voluntarily limit their activities. Still, the results in this paper strongly suggest that a small number of targeted interventions are likely to curb the loss of life, while other potentially costly measures are less effective.

This paper does not address other outcomes that are of considerable interest, such as hospitalizations (relevant to younger segments of the population than fatalities) and positivity rates.[17] At the time of this writing, historical county-level data on these variables are still limited; thus, we leave these analyses to future research.

[17] Huber and Langen (2020) report that earlier lockdown restrictions led to lower hospitalizations and death rates in Germany.

Appendix

Table A.1 shows full results from the regression used to generate the figures. Table A.2 shows the impact of policy duration on week-ahead fatality growth. Tables A.3 and A.5 repeat the analysis in Table A.1 for the samples of less populous and near-border counties, respectively. Tables A.4 and A.6 show the impact of policy duration on week-ahead fatality growth for the subsamples of less populous and near-border counties. Table A.7 summarizes the findings in Tables 4, 6, and 9 of the main text.

Table A.1
Weeks in force: Policy interventions and week-ahead weekly new fatalities

Variables	(1) *Policy*	SE	(2) *Wks_policy*	SE	(3) *ln_wks_policy*	SE
Stay at home	0.352***	0.092	0.004	0.005	−0.179***	0.057
State of emergency	0.104	0.177	0.007	0.004	−0.07	0.087
Nursing accept pos.	−0.351***	0.096	−0.014***	0.003	0.218***	0.052
No nursing visits	−0.032	0.074	−0.004*	0.003	0.052	0.04
Employees masks	0.015	0.031	−0.001	0.002	−0.005	0.022
Masks recommended	−0.108***	0.033	0.004**	0.002	0.028	0.02
Mandatory masks	0.149***	0.032	0.006***	0.002	−0.095***	0.019
Beaches or parks closed	0.282***	0.086	0.011***	0.004	−0.168***	0.052
No elective procedures	0.143	0.095	−0.016	0.012	0.043	0.086
Restaurants and bars closed	0.113	0.085	0.036***	0.014	−0.181**	0.088
Bars closed/restaurants open	0.168***	0.034	0.013***	0.004	−0.133***	0.03
Gyms closed	0.254***	0.061	0.017***	0.003	−0.202***	0.037
Spas closed	−0.053	0.097	0.002	0.005	0.025	0.06
Gatherings limited to 10	0.031	0.038	0.014***	0.003	−0.097***	0.028
No gatherings over 100	−0.061**	0.025	−0.009***	0.002	0.073***	0.018
No gatherings limit>100	−0.157***	0.029	−0.003	0.003	0.064***	0.024
Med.-risk bus. closed	0.094	0.289	0.005	0.016	−0.047	0.185
High-risk bus. closed	−0.488***	0.164	−0.015***	0.005	0.272***	0.086
Higher-risk bus. closed	0.542***	0.182	0.007	0.004	−0.230**	0.09
Highest-risk bus. closed	0.13	0.181	−0.002	0.004	−0.021	0.087
Reopenings reversed	−0.008	0.063	0.006	0.006	−0.01	0.054
Observations						82,619
Adj. R-squared						.202
Controls						Yes

This table shows results of a regression in which we regress 1-week-ahead changes in deaths per 10,000 population ($Growtht_{+1}$) on the policy dummies (*policy,* where all policy variables are defined in Table 1); the number of consecutive weeks policies have been in force (*Wks_policy*); the natural log of 1+ the number of weeks policies have been in force (*ln_wks_policy*); and county demographic variables, weather, and lagged fatality controls. The specification is identical to that in Table 4, except that we add *Wks_policy* and *ln_wks_policy*. The controls are estimated, but not reported in the table. The table reports results from a single regression, with estimated coefficients for *Policy*, *Wks_policy* and *ln_wks_policy* reported in columns 1, 2, and 3, respectively. All standard errors (in parentheses) are clustered at the county level. *$p<.1$; **$p<.05$; ***$p<.01$.

Table A.2
Weeks in force: Duration of policy interventions and week-ahead fatalities

Variables	(1) T = 4 Growth_{t+1}	SE	(2) T = 8 Growth_{t+1}	SE	(3) T = 12 Growth_{t+1}	SE	(4) T = 16 Growth_{t+1}	SE
Stay at home	−0.271***	0.076	−0.359***	0.093	−0.408***	0.099	−0.439***	0.100
State of emergency	−0.085	0.124	−0.100	0.160	−0.098	0.176	−0.090	0.183
Nursing accept pos.	0.293***	0.072	0.363***	0.091	0.385***	0.099	0.386***	0.102
No nursing visits	0.067	0.055	0.081	0.069	0.083	0.075	0.079	0.077
Employees masks	−0.011	0.029	−0.017	0.035	−0.022	0.037	−0.027	0.037
Masks recommended	0.059**	0.027	0.090***	0.033	0.114***	0.035	0.136***	0.036
Mandatory masks	−0.129***	0.025	−0.161***	0.032	−0.172***	0.034	−0.173***	0.035
Beaches or parks closed	−0.227***	0.069	−0.282***	0.086	−0.300***	0.092	−0.301***	0.093
No elective procedures	0.006	0.098	−0.031	0.112	−0.077	0.113	−0.128	0.113
Restaurants and bars closed	−0.148	0.090	−0.111	0.094	−0.033	0.085	0.062	0.080
Bars closed/rest. open	−0.163***	0.034	−0.190***	0.039	−0.187***	0.039	−0.171***	0.036
Gyms closed	−0.258***	0.049	−0.311***	0.060	−0.319***	0.063	−0.306***	0.063
Spas closed	0.048	0.078	0.071	0.095	0.088	0.100	0.103	0.099
Gatherings limited to 10	−0.099***	0.033	−0.100***	0.038	−0.079**	0.037	−0.048	0.035
No gatherings over 100	0.083***	0.022	0.091***	0.026	0.084***	0.026	0.069***	0.025
No gatherings limit>100	0.090***	0.028	0.115***	0.032	0.126***	0.031	0.130***	0.028
Med.-risk bus. closed	−0.056	0.238	−0.064	0.289	−0.061	0.300	−0.054	0.295
High-risk bus. closed	0.377***	0.120	0.475***	0.153	0.513***	0.168	0.524***	0.174
Higher-risk bus. closed	−0.344***	0.128	−0.452***	0.164	−0.510***	0.180	−0.545***	0.188
Highest-risk bus. closed	−0.040	0.125	−0.059	0.160	−0.074	0.177	−0.086	0.185
Reopenings reversed	0.007	0.064	0.025	0.074	0.044	0.074	0.065	0.071

The estimates in this table predict the additional impact of having a policy in place for all of the 4, 8, 12, and 16 weeks leading up to week t on week-ahead fatality growth. The estimates are based on the model that incorporates information on the number of weeks a given policy is in force, as estimated in Table A.1, columns 2 and 3, in the appendix. The values in the table correspond to β_1(weeks)+ $\beta_2$ln(weeks+1).

Table A.3
Weeks in force: Policy interventions and week-ahead weekly new fatalities, less populous counties

Variables	(1) *Policy*	SE	(2) *Wks_policy*	SE	(3) *Ln_wks_policy*	SE
Stay at home	0.407***	0.108	0.007	0.006	−0.218***	0.067
State of emergency	0.024	0.203	0.005	0.005	−0.031	0.099
Nursing accept pos.	−0.444***	0.110	−0.017***	0.003	0.265***	0.059
No nursing visits	0.000	0.089	−0.004	0.003	0.038	0.047
Employees masks	0.022	0.036	−0.000	0.002	−0.012	0.025
Masks recommended	−0.105***	0.038	0.003*	0.002	0.033	0.023
Mandatory masks	0.164***	0.036	0.007***	0.002	−0.106***	0.022
Beaches or parks closed	0.343***	0.108	0.013***	0.005	−0.207***	0.063
No elective procedures	0.144	0.102	−0.015	0.012	0.047	0.090
Restaurants and bars closed	0.111	0.091	0.036**	0.015	−0.184*	0.096
Bars closed/restaurants open	0.166***	0.038	0.013***	0.004	−0.132***	0.034
Gyms closed	0.259***	0.070	0.017***	0.004	−0.206***	0.042
Spas closed	−0.077	0.110	0.000	0.005	0.044	0.067
Gatherings limited to 10	0.035	0.042	0.016***	0.004	−0.106***	0.032
No gatherings over 100	−0.067**	0.029	−0.010***	0.002	0.082***	0.020
No gatherings limit>100	−0.160***	0.033	−0.003	0.003	0.062**	0.027
Med.-risk bus. closed	0.145	0.327	0.007	0.017	−0.078	0.206
High-risk bus. closed	−0.536***	0.186	−0.017***	0.005	0.302***	0.097
Higher-risk bus. closed	0.708***	0.209	0.010**	0.005	−0.310***	0.103
Highest-risk bus. closed	0.108	0.204	−0.003	0.005	−0.002	0.098
Reopenings reversed	−0.005	0.069	0.005	0.007	−0.008	0.058
Observations						74,275
Adj. R-squared						.196
Controls						Yes

This table shows results of a regression in which we regress 1-week-ahead changes in deaths per 10,000 population ($Growtht_{+1}$) on the policy dummies (*Policy,* where all policy variables are defined in Table 1); the number of consecutive weeks policies have been in force (*Wks_policy*); the natural log of 1+ the number of weeks policies have been in force (*ln_wks_policy*); and county demographic variables, weather, and lagged fatality controls. The specification is identical to that in Table A.1, but we use the sample of less populous counties. The table reports results from a single regression, with estimated coefficients for *Policy*, *Wks_policy* and *ln_wks_policy* reported in columns 1, 2, and 3, respectively. All standard errors (in parentheses) are clustered at the county level. $^{*}p<.1$; $^{**}p<.05$; $^{***}p<.01$.

Table A.4
Weeks in force: Policy interventions and t-week-ahead fatalities (less populous counties)

Variables	(1) T = 4 Growth$_{t+1}$	SE	(2) T = 8 Growth$_{t+1}$	SE	(3) T = 12 Growth$_{t+1}$	SE	(4) T = 16 Growth$_{t+1}$	SE
Stay at home	−0.323***	0.089	−0.423***	0.110	−0.475***	0.116	−0.506***	0.118
State of emergency	−0.031	0.142	−0.030	0.182	−0.022	0.200	−0.012	0.209
Nursing accept pos.	0.359***	0.082	0.448***	0.104	0.478***	0.113	0.482***	0.116
No nursing visits	0.046	0.065	0.053	0.082	0.052	0.089	0.048	0.091
Employees masks	−0.020	0.033	−0.028	0.040	−0.033	0.042	−0.037	0.042
Masks recommended	0.067**	0.031	0.099***	0.038	0.125***	0.040	0.147***	0.041
Mandatory masks	−0.142***	0.029	−0.176***	0.036	−0.186***	0.039	−0.186***	0.040
Beaches or parks closed	−0.280***	0.086	−0.348***	0.107	−0.371***	0.115	−0.373***	0.117
No elective procedures	0.014	0.105	−0.020	0.121	−0.064	0.124	−0.113	0.124
Restaurants and bars closed	−0.153	0.098	−0.118	0.102	−0.042	0.093	0.052	0.087
Bars closed/rest. open	−0.159***	0.039	−0.184***	0.045	−0.179***	0.044	−0.161***	0.041
Gyms closed	−0.265***	0.056	−0.319***	0.069	−0.328***	0.073	−0.317***	0.073
Spas closed	0.073	0.088	0.100	0.108	0.118	0.114	0.131	0.113
Gatherings limited to 10	−0.108***	0.037	−0.108**	0.043	−0.085**	0.042	−0.051	0.039
No gatherings over 100	0.093***	0.025	0.102***	0.029	0.094***	0.029	0.078***	0.028
No gatherings limit>100	0.090***	0.032	0.116***	0.036	0.128***	0.035	0.134***	0.032
Med.-risk bus. closed	−0.097	0.268	−0.115	0.328	−0.116	0.344	−0.109	0.341
High-risk bus. closed	0.416***	0.136	0.524***	0.174	0.565***	0.190	0.577***	0.197
Higher-risk bus. closed	−0.459***	0.147	−0.601***	0.188	−0.675***	0.207	−0.718***	0.216
Highest-risk bus. closed	−0.016	0.141	−0.029	0.181	−0.043	0.200	−0.056	0.209
Reopenings reversed	0.008	0.069	0.025	0.081	0.043	0.081	0.062	0.078

The estimates in this table predict the additional impact of having a policy in place for all of the 4, 8, 12, and 16 weeks leading up to week t on week-ahead fatality growth. The estimates are identical to those in Table A.2, but the model is estimated using the sample of less populous counties. $^*p<.1$; $^{**}p<.05$; $^{***}p<.01$.

Table A.5
Weeks in force: Policy interventions and week-ahead weekly new fatalities, near-border counties

Variables	(1) *Policy*	SE	(2) *Wks_policy*	SE	(3) *Ln_wks_policy*	SE
Stay at home	0.661***	0.160	0.019**	0.007	−0.350***	0.099
State of emergency	0.339	0.497	0.010	0.011	−0.189	0.241
Nursing accept pos.	−0.381*	0.208	−0.020***	0.006	0.264**	0.107
No nursing visits	0.148	0.135	0.002	0.005	−0.057	0.076
Employees masks	0.020	0.056	0.002	0.004	−0.019	0.041
Masks recommended	−0.085	0.057	0.011***	0.003	−0.023	0.035
Mandatory masks	0.172***	0.043	0.011***	0.003	−0.129***	0.028
Beaches or parks closed	0.145	0.239	0.004	0.007	−0.079	0.130
No elective procedures	0.058	0.210	0.006	0.072	−0.035	0.327
Restaurants and bars closed	−0.093	0.156	0.006	0.038	0.018	0.208
Bars closed/restaurants open	0.132	0.080	0.020**	0.009	−0.130*	0.077
Gyms closed	0.042	0.201	0.007	0.009	−0.072	0.119
Spas closed	0.413	0.471	0.022	0.020	−0.261	0.279
Gatherings limited to 10	0.056	0.067	0.025***	0.009	−0.160**	0.063
No gatherings over 100	−0.107***	0.040	−0.011***	0.003	0.091***	0.031
No gatherings limit>100	−0.126***	0.048	0.005	0.004	0.005	0.037
Med.-risk bus. closed	0.085	0.524	0.026	0.019	−0.147	0.303
High-risk bus. closed	−0.656**	0.315	−0.021**	0.009	0.356**	0.167
Higher-risk bus. closed	0.628*	0.333	0.010	0.008	−0.291*	0.166
Highest-risk bus. closed	0.151	0.306	−0.002	0.007	−0.025	0.146
Reopenings reversed	0.067	0.164	−0.010	0.019	0.021	0.163
Observations						29,835
Adj. R-squared						.225
Controls						Yes

This table shows results of a regression in which we regress 1-week-ahead changes in deaths per 10,000 population ($Growtht_{+1}$) on the policy dummies (*Policy,* where all policy variables are defined in Table 1); the number of consecutive weeks policies have been in force (W*ks_policy*); the natural log of 1+ the number of weeks policies have been in force (*ln_wks_policy*); and county demographic variables, weather, and lagged fatality controls. The specification is based on that in Table A.1, but we use the sample of near-border counties, and we control for the policies of nearby county matches. The table reports results from a single regression, with estimated coefficients for *Policy*, *Wks_policy* and *ln_wks_policy* reported in columns 1, 2, and 3, respectively. All standard errors (in parentheses) are clustered at the county level. $^{*}p<.1$; $^{**}p<.05$; $^{***}p<.01$.

Table A.6
Weeks in force: Policy interventions and *t*-week-ahead fatalities, near-border counties

Variables	(1) T = 4 Growth$_{t+1}$	SE	(2) T = 8 Growth$_{t+1}$	SE	(3) T = 12 Growth$_{t+1}$	SE	(4) T = 16 Growth$_{t+1}$	SE
Stay at home	−0.487***	0.132	−0.617***	0.163	−0.669***	0.173	−0.686***	0.175
State of emergency	−0.263	0.345	−0.333	0.445	−0.361	0.491	−0.371	0.514
Nursing accept pos.	0.346**	0.150	0.423**	0.191	0.441**	0.209	0.433**	0.217
No nursing visits	−0.083	0.104	−0.108	0.130	−0.120	0.140	−0.126	0.143
Employees masks	−0.024	0.052	−0.029	0.063	−0.030	0.066	−0.029	0.065
Masks recommended	0.006	0.046	0.035	0.056	0.068	0.060	0.105*	0.060
Mandatory masks	−0.163***	0.037	−0.194***	0.044	−0.197***	0.047	−0.187***	0.047
Beaches or parks closed	−0.112	0.181	−0.144	0.229	−0.158	0.249	−0.165	0.257
No elective procedures	−0.032	0.257	−0.029	0.213	−0.018	0.212	−0.003	0.341
Restaurants and bars closed	0.054	0.192	0.090	0.181	0.122	0.146	0.152	0.147
Bars closed/rest. open	−0.128	0.088	−0.123	0.099	−0.090	0.095	−0.043	0.087
Gyms closed	−0.087	0.158	−0.101	0.195	−0.099	0.208	−0.091	0.209
Spas closed	−0.331	0.372	−0.396	0.460	−0.404	0.486	−0.385	0.485
Gatherings limited to 10	−0.156**	0.068	−0.149**	0.074	−0.106	0.069	−0.047	0.061
No gatherings over 100	0.103***	0.037	0.113***	0.044	0.103**	0.044	0.084**	0.042
No gatherings limit>100	0.026	0.044	0.047	0.051	0.067	0.050	0.087*	0.047
Med.-risk bus. closed	−0.133	0.417	−0.116	0.524	−0.067	0.567	−0.003	0.580
High-risk bus. closed	0.490**	0.233	0.616**	0.297	0.664**	0.324	0.676**	0.334
Higher-risk bus. closed	−0.429*	0.235	−0.560*	0.301	−0.627*	0.331	−0.665*	0.344
Highest-risk bus. closed	−0.047	0.209	−0.069	0.269	−0.084	0.297	−0.098	0.310
Reopenings reversed	−0.006	0.191	−0.033	0.218	−0.065	0.211	−0.099	0.191

The estimates in this table predict the additional impact of having a policy in place for all of the 4, 8, 12, and 16 weeks leading up to week *t* on week-ahead fatality growth. The estimates are identical to those in Table A.2, but the model is estimated using the sample of near-border counties. *$p<.1$; **$p<.05$; ***$p<.01$.

Table A.7
Summary of coefficients from Tables 4, 6, and 9 (4- and 6-week horizons)

	Table 4	Table 6	Table 9	**Overall**
Stay at home	+*	+*	+	+*
State of emergency	+	+		+
Nursing accept pos.				
No nursing visits	+	+		+
Employees masks	–	–	–	–
Masks recommended	+	+	+	+
Mandatory masks	–	–	−*	–
Beaches or parks closed	+			
No elective procedures	+	+	+	+
Restaurants and bars closed	–	–	–	–
Bars closed/restaurants open	−*	−*		−*
Gyms closed	−*	−*	–	−*
Spas closed	+	+	+*	+
Gatherings limited to 10		+		
No gatherings over 100	+	+	+	+
No gatherings limit>100	+*	+*	+*	+*
Risk level 1 closed				
Risk level 2 closed				
Risk level 3 closed	–	–		–
Risk level 4 closed			+	
Reopenings reversed	+	+	+*	+

The minus (−) and plus (+) signs indicate negative and positive estimated coefficients for the 4- and 6-week horizons (respectively) where at least one of the coefficients is statistically significant. * indicates a change in significance or sign as the horizon goes from shorter (1 to 2 weeks) to longer. ** indicates a sign switch between the 4- and 6-week horizon, where both coefficients are significant. The “Overall” column includes a symbol if at least two tables have the same symbol, and the third table does not have a countervailing symbol. For example, *Stay at home* has a-in the “Overall” columns since two tables have a-and the third does not have +. The Overall column is blank for *Masks recommended* since one table has a – and another has a +.

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