Mask Mandates Increased COVID-19 Deaths in Kansas

Bryan C. McCannon
Illinois Wesleyan University
United States of America

Mark Wilson
Saint Bonaventure University
United States of America

Abstract
We ask whether face mask mandates were effective at reducing COVID-related deaths. We explore data from Kansas, as local political leaders had the ability to opt out of the governor's statewide mandates. Exploiting the staggered adoption of mandates across counties across time, we estimate a difference-in-differences model. We present evidence that the adoption of a mask mandate *increased* the number of COVID-related deaths. In our preferred specification, two additional deaths arise every three weeks a county imposes a mask mandate. We explore threats to identification in making our causal claim and use total confirmed cases and cell phone tracking data to explore the mechanism.

I. Introduction
COVID-19 devastated the world. To date, almost seven million people have died.[1] During the height of the pandemic, radical steps were taken to combat the virus's spread. These actions seriously infringed personal liberty (Boettke and Powell 2021; Miozzi and Powell 2023a,b). Policies included widespread business and institution closures, stay-at-home orders, and mask mandates. Further, important discrepancies arose across political jurisdictions. Differences in which policies were implemented, their duration, and the restrictions' extensiveness existed. Given the pandemic's uniqueness, these policies were implemented with little hard evidence at the time to support them. One important policy was the mandate to wear face masks.

   While a serious knowledge problem exists when political actors attempt to make policy during such a unique event (Coyne et al. 2021),

a rationale for the policy was that covering the mouth and nose with a mask filters particles including water droplets, which can carry the coronavirus. Thus, it can be argued that wearing a face mask serves as a public good and, under the presumption of a benevolent social planner, the regulatory intervention mandating their use should be welfare enhancing.

We ask whether the hypothesized benefit to face mask mandates can be verified observationally. There are reasons for doubt.[1] For one, a biological theory has been proposed known as the *Foegen Effect* (Fögen 2022). It notes that the face mask captures exhaled water droplets that the wearer re-inhales. The "deep re-inhalation of hypercondensed droplets or pure virions caught in facemasks as droplets can worsen prognosis and might be linked to long-term effects of COVID-19 infection … The virions spread (because of their smaller size) deeper into the respiratory tract. They bypass the bronchi and are inhaled deep into the alveoli, where they cause pneumonia instead of bronchitis … Moreover, the Foegen Effect could increase the overall viral load because virions that should have been removed from the respiratory tract are returned."[2] Second, an economic theory known as the *Peltzman Effect*, or the *offsetting-behavior hypothesis*, may very well apply (Peltzman 1975). The idea of the Peltzman Effect is that safety improvements act to reduce the expected cost of risky behavior. Consequently, a rational decision-maker can be expected to increase their risk taking. In this application, if people view themselves as being safer when wearing a face mask, they will increase their exposure to the virus by spending time around others in public places.[3]

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[1] See Leeson and Rouanet (2021) for a discussion of numerous avenues of political failure in COVID-19 policy, and see Sutter (2022) for libertarian alternatives.

[2] Chan et al. (2020) provides supporting laboratory evidence that viral loads of masked hamsters compared to unmasked hamsters.

[3] Research on the Peltzman Effect in other applications is extensive. Peltzman's (1975) original research focused on automobile accidents after safety equipment, such as seat belts, was mandated. In health, examples include risk factors for and precautions against skin cancer (for example, shade, clothes, lotion) (Dickie and Gerking 1997) and diet when adding medication to a high-cholesterol diagnosis (Mancino and Kuchler 2009). In sports, it has been documented in the form of aggressive driving and wrecks in NASCAR (Pope and Tollison 2010) and has been extended to strategic offsetting behavior in the movement of the three-point line in basketball (McCannon 2011). Others have hypothesized an offsetting behavior related to COVID-19 safety (Iyengar et al. 2021; Mackolil and Mackolil 2021; Trogen and Caplan 2021; Falahi et al. 2022). Relatedly, Andersson et al. (2021) provide evidence that anticipation of vaccines reduced social distancing.

Together, they suggest that face mask mandates could be ineffective at reducing the virus's harmful impact. If evidence fails to document the hypothesized benefit to the mandate, then coupled with the serious infringement on personal liberty, the appropriateness of the mask mandate can be called into question.

Kansas provides an ideal setting. As with most states in the US, regulatory restrictions were implemented at the state level. On July 3, 2020, the governor of Kansas imposed a statewide mask mandate. In Kansas, though, individual counties were allowed to opt out of the statewide mandate. Many counties did. A few, though, rescinded their opt-out and implemented the mandate weeks later. Further, a second statewide mask mandate was ordered in late November 2020. Many counties that had opted out of the first order chose not to opt out the second time. Quite a few counties never implemented the mandate. This setting provides intrastate variation in policy uptake and timing. We exploit this variation to evaluate whether COVID-19 deaths are affected by the mask mandate.

Our primary result is that the mask mandate's effect was to *increase* the number of COVID-related deaths. In our preferred estimate, we show that the mask mandate led to approximately 0.66 more COVID-related deaths each week per county. We show this effect to be robust to identification concerns including common trends, representativeness of treated units, and biases arising from staggered adoption.

We go further and try to explore the mechanism at play. We show that the mask mandate is associated with an increase in confirmed cases. As these data are notoriously noisy, our finding is only suggestive evidence that heightened exposure due to behavioral changes may be at work. Further, we leverage cell phone tracking data. We find evidence that the implementation of a mask mandate increases movement. The number of visitors to restaurants and bars increased when a mask mandate was implemented.[4] This provides suggestive evidence that offsetting behaviors eroded the mask mandate's effectiveness.

We are by no means the first to study face masks. In fact, two other observational-data studies have also evaluated Kansas. They

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[4] We intentionally avoid conducting a welfare analysis, as costs and benefits can be controversial and difficult to quantify. For example, being able to enjoy a night out at such an establishment likely provides utility. A corresponding spread of the virus will likely be unpleasant to others.

suffer from important limitations. Fögen (2022) considers only the first mandate and ignores the second. Thus, later-treated counties and those that rescind their opt-out decision after public pressure are merged with those never adopting the mandate. Threats to identification are not evaluated thoroughly either. Nevertheless, he finds an increase in deaths. Van Dyke et al. (2020) only consider COVID-19 cases, not deaths, and consider only the immediate outcomes, as they end their analysis in August 2020. We contribute by taking seriously the identification threats and estimation biases that can arise in staggered-adoption, difference-in-differences empirical approaches. Further, we improve on these studies by fully leveraging the timing of the policy's rollout. Research has attempted to identify the effect of wearing a face mask in other contexts.

The shock of the pandemic led researchers to conduct meta-analysis studies of what effect face masks have with respect to other viruses. In their meta-analysis, Coclite et al. (2021) summarize by saying that the "published literature on the efficacy, effectiveness and acceptability of different types of face mask in preventing respiratory infections during epidemics is scarce and conflicting." Specifically, they summarize the observational studies as providing no statistically significant effect of wearing a face mask on the spread of disease. In a distinct meta-analysis, Aggarwal et al. (2020) summarize randomized control trials of face mask use in communities. They too find an overall insignificant effect of face mask use on illnesses. Sharma et al. (2020) conduct their own study and also fail to find the anticipated positive benefits to cloth masks. These meta-analyses do not include any results specifically studying the coronavirus, as none had been conducted as of late 2021.[5] One recent field study adds a treatment in which subjects are recommended to wear a face mask (along with the baseline request to engage in social distancing). Using an antibody test one month after the masking recommendation, no change in the rate of contracting COVID-19 could be found (Bundgaard et al. 2021). Given the strong evidence that masks filter water droplets/particles

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[5] Abaluck et al. (2021) are able to conduct a randomized control trial with face masks. Their goal is to assess how to persuade people to wear a face mask. The intervention included handing out free masks at community gathering places (for example, local mosques), leader endorsements, periodic monitoring and reminders, and monetary incentives. They show that these interventions increase face mask wearing (for a few weeks) and encouraged social distancing. They do not measure mortality and only measure disease spread through survey questions asking people whether they experienced COVID-like symptoms.

that can carry the coronavirus (Pan et al. 2021), it is a bit puzzling that field studies are often unable to document reductions in disease transmission.[6] The Foegen Effect and Peltzman Effect provide potential explanations, and we contribute by looking specifically at their potential relationships with COVID-related deaths.

Recent research produced after the pandemic's nadir has looked to quasi-natural experiments of face mask mandates. Karaivanov et al. (2021) evaluate the staggered adoption of mask mandates across Canadian provinces in the summer of 2020. Controlling for behavioral responses with cell phone tracking data, they provide evidence that mask mandates correspond to reductions in COVID-19's growth rate and an increase in reported mask wearing. Mitze et al. (2020) implement the synthetic control method to build synthetic versions of regions in Germany that adopted mask mandates before the nationwide mandate took effect. They show that the mandate reduced the growth rate of new cases. In an early study, Lyu and Wehby (2020) look across US states that adopt mask mandates prior to May 15 and provide evidence that the mandates reduced the growth rate. These studies focus on changes in the number of confirmed cases, while we explore deaths.

Finally, our work relates to a paper by Mulligan and Arnold (2022). They estimate the collateral health consequences of the pandemic. They find excessive deaths arising from alcohol/drug-induced causes, homicides, driving-related fatalities, diabetes- and obesity-related causes, and hypertension/heart disease due to COVID-related policies. Relatedly, Cantor et al. (2022) use cell phone tracking data and insurance claims to show that health care use decreased during the pandemic. COVID-19's impact on a variety of other well-being measurements has been considered. For one, empirical evidence has documented increases in domestic violence during the pandemic (Leslie and Wilson 2020; Bullinger et al. 2021). Markers of mental health worsened as well (Altindag et al. 2022). On the other hand, Brodeur et al. (2021) provide evidence that the reduced activity during the pandemic coincided with fewer automobile accidents and reduced pollution. The pandemic has even been shown to increase calories consumed (O'Connell et al. 2022). Thus, there is a growing literature exploring the consequences of COVID-inspired policy.

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[6] As a suggestive elaboration, Dave et al. (2021) show that shelter-at-home orders reduced the spread of COVID-19 but curiously did not have a statistically significant effect on mortality.

II. Methods

A. Background

On July 2, 2020, Kansas governor Laura Kelly issued Executive Order 20-52. It required that "any person in Kansas shall cover their mouth and nose with a mask or other face covering when they are . . . inside, or in line to enter, any public space." It went into effect July 3. The order was extended in Executive Order 20-68 on November 18, 2020. This second order went into effect November 23. Previously, though, the state's legislature had passed Kansas House Bill 2016 (signed June 8, 2020; effective June 9), which authorized county commissions to "issue an order related to public health that includes provisions that are less stringent" than the mask order if "implementation of the full scope of the provisions of the governor's executive order are not necessary to protect the public health and safety of the county." This provides a unique environment in which some counties in the state had mask mandates while others did not. Senate Bill 40 was signed by Governor Kelly on March 24, 2021, which revoked the statewide mandate on March 31.

Many, but not all, counties opted out of the governor's mask mandate in July 2020. A few rescinded this opt-out decision over the course of the summer and into the fall. The second order in November 2020, though, led many counties that had initially opted out to decide to allow the statewide requirement to go into effect. Figure 1 depicts the proportion of the counties in the state with a mask mandate each week in 2020 and early 2021.

The governor's mandates and the county commissions' decision-making created an environment with regional and temporal variation in policy. Importantly, many counties never adopted the mask mandate. We exploit this variation to identify the mask mandate's effect on COVID-related deaths.

Furthermore, the two orders did not include any other safety measures. Thus, the orders did not include any other additional restrictions that would contaminate the causal identification.

B. Data

We are primarily interested in COVID-related deaths in the US. We use publicly available data from the Centers for Disease Control and Prevention (CDC). Daily cumulative COVID-19 deaths are provided for each county in the US. Data are reported starting January 22, 2020.

We consider various ending points for the panel in our analysis.[7] We limit attention to the 105 counties in Kansas.

Figure 1. Implementation of Mask Mandate in Kansas

Notes: The figure plots the proportion of Kansas's 105 counties that had a countywide mask mandate in effect in 2020 and 2021 (through July 2, 2021). The governor's first executive order went into effect on July 3, 2020, and the second executive order was implemented on November 23, 2020. The order expired on March 31, 2021.

The pandemic has not spared Kansas. Through December 31, 2021, a total of 6,673 people in Kansas died from the coronavirus. This represents 227.0 deaths per 100,000 people.[8] Information on county mask mandates in Kansas is collected primarily from the Kansas Health Institute (Shah et al., 2021), which provides an update of policies across the state approximately every six weeks in 2020 and 2021. Information is supplemented by searches in local newspapers. For each county in the state, we identify whether it

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[7] We accessed the data on May 30, 2022. Thus, data were available through May 25, 2022. For our primary data analysis, we convert the data to daily death counts and then aggregate them to the weekly level.

[8] This figure uses Kansas's 2020 US Census population in the denominator. As of May 25, 2022, the number had climbed to 8,397.

implemented a mask mandate between January 2020 and July 2021 and, if so, the beginning and end dates of the mandate.

The question we address is whether the mask mandate had the intended effect of reducing COVID-19 mortality. Figure 2 provides some visual evidence. In it, we consider cumulative COVID-19 deaths across the state. Time is recentered with $t = 0$ as the first week that a county adopted a mask mandate. Thus, positive values represent the number of weeks since the mandate's implementation and negative values represent the number of weeks prior. We recenter the time, recognizing that there is substantial variation in the date at which the mask mandate went into effect. We depict the sixteen weeks prior to the mandate and the sixteen weeks after the mandate.

Figure 2. Before and after the mandate
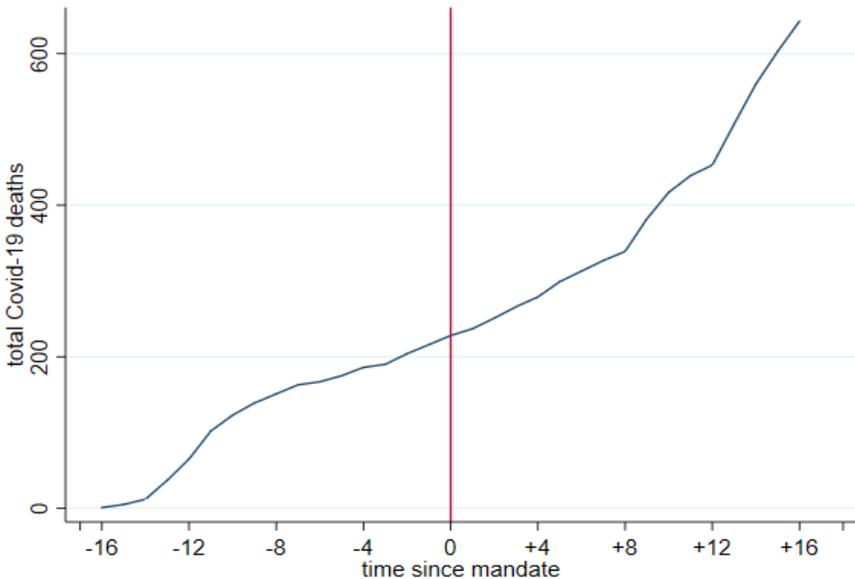
Notes: The figure plots the cumulative number of COVID-related deaths each week across the sixty-five counties in Kansas that implemented the mask mandate. Time is recentered so that $t = 0$ represents the week of the mandate's adoption in each county.

The mask mandate's implementation corresponds to an inflection point in the cumulative number of deaths. After a rapid rise early, the rate of increase in the number of deaths slows prior to the mandate. After the mandate's adoption, it begins to rise exponentially. As is to

be expected with mortality data, there seems to be a lag between the policy change and the eventual growth in deaths.

Figure 2 provides suggestive evidence that the mask mandate may have backfired. This is speculation, though, as we have not established a proper benchmark. The figure does not include time trends of those counties that did not adopt a mask mandate, and, importantly, the figure is unable to depict the counterfactual outcome of what these counties' COVID-19 experiences would have been had they not adopted the policy. An identification strategy is needed to estimate this counterfactual and pull out the policy's average treatment effect.

C. Estimation Strategy

We apply a difference-in-differences identification strategy. Specifically, we consider the number of deaths occurring in county c during week w, $D_{c,w}$, as the dependent variable. The indicator variable $Mandate_{c,w}$ is equal to 1 if the county is one that did not opt out of the statewide implementation. It only equals 1 for weeks in which it had a face mask mandate. Hence, we estimate a two-way fixed-effects regression:

$$D_{c,w} = a_0 Mandate_{c,w} + \tau_w + \varkappa_c + \epsilon_{c,w} \tag{1}$$

A total of 105 county fixed effects are included, $\varkappa_c$, as are week fixed effects, τ_w. The difference-in-differences estimated coefficient, $\hat{a}_0$, is the one of interest, as it identifies whether the difference in deaths between mandate counties and nonmandate counties grows, shrinks, or is unchanged when the mandate is in effect. Specifications differ in time period covered. Because of the use of high-frequency data, we are unable to include standard covariates, such as socioeconomic-status variables, as controls. As we are studying a relatively short period, they would be perfectly multicollinear with the cross-sectional fixed effects. After presenting the initial results, we explore the robustness of the result to heterogeneity across the state.

III. Results

A. Initial Result

To investigate the mask mandate, as stated, we consider a standard two-way fixed-effects model in which the treatment variable, $Mandate_{c,w}$, is equal to 1 for those periods that a countywide mask mandate is in place. The number of COVID-related deaths arising in each county in each week is the dependent variable. Table 1 presents the initial results.

Table 1. Mask mandates and COVID-19 deaths

time period =	Jan. 22, 2020 - Nov. 23, 2020 (1)	Jan. 22, 2020 - Dec. 31, 2020 (2)	Jan. 22, 2020 - March 31, 2021 (3)
Mandate	0.8129	0.6627	0.4457
	(0.3503) **	(0.2467) ***	(0.2727) +
	[0.0869] ***	[0.1077] ***	[0.1839] **
	$\{p = 0.001\}$	$\{p = 0.001\}$	$\{p = 0.001\}$
County Fixed Effects?	Yes	Yes	Yes
Week Fixed Effects?	Yes	Yes	Yes
R^2	0.364	0.348	0.259
AIC	13,762.2	20,692.6	36,663
N	4515	5145	6510
DV μ	0.2707	0.4519	0.7548

Notes: The dependent variable counts the number of reported COVID-19 deaths in each of Kansas's 105 counties each week. Specifications differ by the period covered. Each specification includes 105 county fixed effects and week fixed effects. Standard errors clustered at the county level are presented in parentheses (105 clusters), and unadjusted standard errors are presented in brackets; + 10.5%, * 10%, ** 5%, *** 1% level of significance. Inside the curly brackets are the p-values constructed from a random inference procedure in which the treatment variable is randomly permuted a thousand times.

The roll-out of the mask mandate is associated with an increase in the number of deaths each week in the counties that did not opt out of the statewide mandate. The first specification considers deaths arising only from the first order issued by the governor. The effect is quite large. The second column, which is our preferred specification, includes the effects of the numerous later-adopting counties. The third column extends the period until the end of the statewide mandate.[9] This is not an ideal specification, as it includes post-treatment observations with the never-treated and not-yet-treated observations as the omitted group. If the mask mandate continues to have effects in the weeks after its removal, this feature will cause the estimate to be downward biased. Nevertheless, specification (3) is informative and presents consistent results.

Table 1 presents both unadjusted standard errors, in brackets, and clustered standard errors, in parentheses. The clustering greatly expands the standard errors but fails to lead to the acceptance of the null hypothesis. The curly brackets summarize the results of a

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[9] A few counties continued their mandates into April.

random inference procedure. For each period, we permute the treatment variable and reestimate the two-way fixed-effects model with the false treatment storing the estimated difference-in-differences coefficient. We iterate this process a thousand times. The proportion of the permutations with an estimated coefficient greater in absolute value than the true value is the constructed *p*-value. As one can see, in each of the four thousand placebo estimates, the difference-in-differences coefficient is smaller than what is shown in table 1. Thus, we are unlikely to have a false positive.

Numerous sensitivity checks are conducted. For one, we conduct a leave-one-out process to assess our result's sensitivity to the exclusion of any one particular county. This is essential, as there is substantial heterogeneity across Kansas's counties. As population density, age distribution, and other important escalating factors vary across the state, there is a real possibility that an outlier may be driving the result. Similarly, we systematically drop observations from a particular week. This allows us to assess whether nuances in data reporting in any week matter or whether any spike in a particular period drives our result. The result is robust. The statistical significance of the result is always strong (with a *p*-value never exceeding 0.017), and the estimated treatment effect is essentially unchanged. Second, we engage in a winsoring process, modifying extreme values. This, among other things, addresses data corrections and updates in the CDC data. Again, the results are strengthened. Third, we consider specifications in which we add a lagged value of the outcome variable as a control and transform the dependent variable[10] to assess the change in the growth rate. The sign remains the same, and each specification produces a significant result. Fourth, we add information on city-level mandates, recoding the treatment variable to measure the proportion of the county subject to a mask mandate. Again, the coefficient's magnitude and statistical significance persist. Thus, the result presented in table 1 is robust. Instead, in what follows, we focus on population differences and threats to identification, asking to what degree the results presented identify the policy's causal impact.

B. Accounting for Population Differences

The initial results in table 1 use the total number of COVID-related deaths across a county in each week as the dependent variable. This is

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[10] Specifically, we use the inverse hyperbolic sine transformation, which approximates a log transformation but is defined for zero values.

potentially concerning, as population differs substantially across counties in Kansas. As an illustration, the mean county population in 2019 is almost four times as large as the median.

A common empirical approach is to normalize the data by considering per capita outcomes. Unfortunately, this presents a challenge in this setting. Here, we are exploring a high-frequency data set by looking at weekly death counts. Measurements of population movements are conducted over longer time intervals (specifically, annual). Hence, population is perfectly collinear with county fixed effects. A further complication is that being time invariant, population is highly correlated with numerous other socioeconomic variables. Thus, dividing the dependent variable by a (time invariant) constant that is correlated with multiple other factors that have been shown to influence mortality will likely obfuscate the interpretation of the results.[11]

Nevertheless, it is important to address population differences. For one, we reestimate our results in table 1 but use population weighting. This procedure puts more weight on observations from more heavily populated counties and discounts observations from sparsely populated ones. The estimated treatment effects remain positive and highly statistically significant.

Further, we drop observations from the five counties with population sizes substantially greater than those in the rest of the state.[12] Reestimating the results in table 1 without them, the positive treatment effect and statistical significance persist.

In addition, we note that high-population counties could be emitting high death counts, which could be skewing the result. We address this by coarsening the dependent variable. Specifically, we instead consider a binary variable equal to 1 if a death occurred in a week in the county. This allows us to isolate the policy's extensive margin. Doing so, the possibility of a death's occurring at all is positive and highly statistically significant. Using our preferred time

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[11] A further complication is that the dependent variable includes numerous values of zero. In our robustness checks provided in the online supplement, we show that the finding is not sensitive to estimation strategies intended to account for data with prevalent zeroes (such as Tobit models). It matters also for a normalization. A per capita adjustment, by definition, does not affect observations of zero deaths. Only the proportion of the sample with nonzero values is adjusted. The left-censoring of the data leads to an inconsistent normalization.

[12] The ratio of the mean to the median county population drops from 3.94 to 2.10 with this deletion.

range (through December 31, 2020), the probability of a death's occurring in a week increases by 13.2 percentage points. Additionally, evaluating the intensive margin of the number of deaths in a week, conditional on there being at least one death, produces an essentially zero effect. These findings suggest that the effect is strongest for the creation of a death and the policy is not necessarily producing high volumes of deaths. Importantly, this provides confidence that it is not high death counts in high-population counties that are driving our result. All robustness checks discussed are provided in our online, supplemental appendix.

C. Threats to Causal Identification
To make a causal claim, a difference-in-differences identification strategy rests on the assumption that the treated counties follow the same time trend as the control counties. While we are unable to test whether the counterfactual outcome—the COVID-19 deaths in the treated counties had they not been treated—follows a time trend that is parallel to the control counties after the mandate goes into effect, we can test whether the two groups of counties are following a common trend prior to the mandate. If they do not have parallel trends, then the difference-in-differences coefficient may be capturing the divergent time paths rather than the policy's causal effect. Further, it is informative to assess the treatment effect's timing after the mandate's implementation. It allows us to ask whether the effects were gradual or immediate.

We recenter time around the week of the mask mandate's implementation in each county. Hence, $t = 0$ is the week a county adopted a mask mandate. We consider the four months prior and the four months after. This includes the beginning of COVID-19's presence in the US until the weeks prior to the end of the second mask mandate. Thus, the event study identifies whether the counties that mandated face masks experienced more deaths each week than those that did not. Specifically, we estimate the following:

$$D_{c,w} = \sum_{-16, w \neq -1}^{+16} \gamma_w \mathbb{1}_w \times Mandate_c + \sum_{-16, w \neq -1}^{+16} \tau_w + \kappa_c + \epsilon_{c,w}. \qquad (2)$$

Time period $t = -1$ is the omitted period. Observations from counties that never adopted a mask mandate are coded as $t = -1$ so that they are included in the reference group. The thirty-two estimated $\hat{\gamma}_w$'s identify, for each week both before and after the mask mandate, whether the counties treated with the mask mandate

experience different COVID-19 fatalities. Figure 3 graphically depicts the regression results.

Figure 3. Dynamic effects
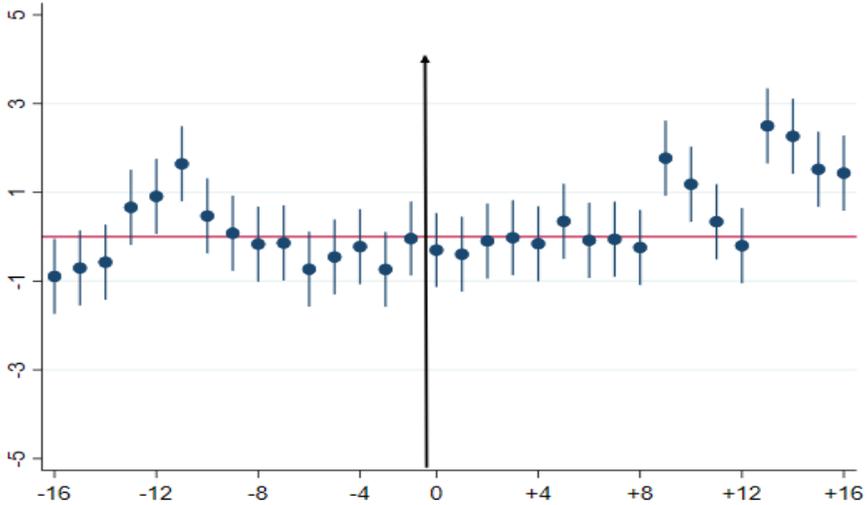
Notes: Weekly COVID-19 deaths for each county-week observation are the dependent variable. The coefficient estimates for the interaction between being a treated county (that is, having a mask mandate) and the week are presented, along with the 95% confidence interval. Time is recentered so that t = 0 is the first week of the mask mandate (July 3, 2020). Only periods with full coverage are included; t ∈ [−16, +16]. Period t = −1 is omitted.

The effect of the mask mandate is essentially zero for the first eight weeks after its implementation. After that, the difference is large and significant. Thus, the mandate has a delayed effect. This is to be expected, as there will naturally be a lagged effect between the beginning of a behavioral change and the eventual mortality outcome.

The dynamic effects depicted in figure 3 provide visual evidence that the two groups are following parallel trends prior to the mandate. For the two months (eight weeks) prior to the treatment, $t = -9$ to $t = -2$, the confidence intervals all include zero. Further, we conduct a test for whether they are jointly equal to zero. For the eight weeks prior, the coefficients are jointly insignificant ($F = 1.00$; $p = 0.43$). Therefore, we argue that parallel trends hold. Even if one took a skeptical stance on common trends, the point estimates in the periods just prior to treatment are for the most part negative and declining. Thus, the counties that adopted the mask mandate had slightly lower

COVID-19 death rates prior. Thus, if the difference-in-differences estimate were problematic, it would be downward biased. A second threat to identification is bias arising from staggered policy adoption. In the presence of heterogeneous treatment effects and staggered policy adoption, the two-way fixed-effects model might not be able to recover an interpretable causal estimate. Goodman-Bacon (2021) shows that the difference-in-differences coefficient includes a comparison between the later-treated observations and the earlier-treated ones. If the treatment effect varies over time, this opens up the possibility of a biased estimate, as the measurement is affected by the change in the treatment effect over time and becomes sensitive to the time window studied (and where the treatments fall within it).

Figure 4. Correcting bias arising from staggered policy adoption
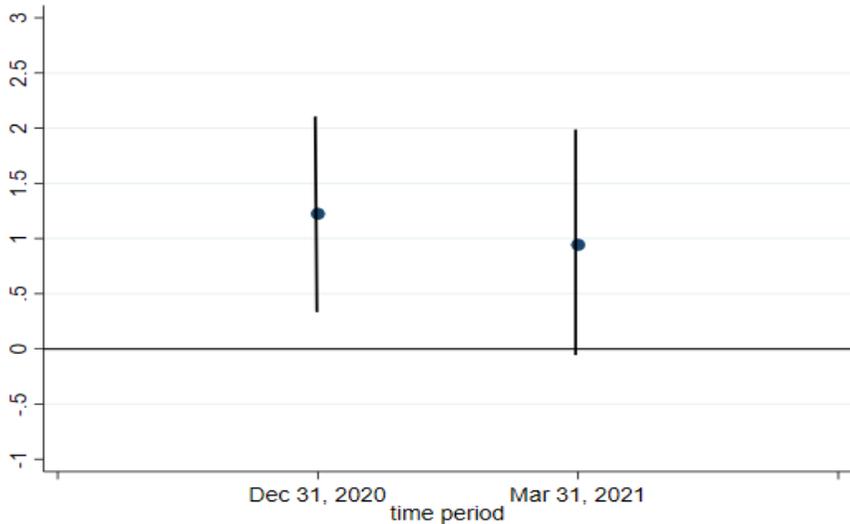
Notes: The weighted average of the group treatment effects with the never-treated counties as the control group is presented, along with the 95% confidence intervals. Specifications differ in the period covered, with the first being January 22, 2020, to December 31, 2020, and the second extending to March 31, 2021.

Callaway and Sant'Anna (2021) provide a solution. They propose to separately estimate the treatment effect for each cohort (defined by the timing of the policy implementation). Each cohort's treatment effect is estimated relative to the never-treated units. A weighted average of these group treatments creates an average treatment effect. Figure 4

presents the average treatment effects. Estimating each group's effect relative to the never-treated counties, the average treatment effect is larger than what was reported for the two-way fixed-effects estimation.

A third threat to identification is the treatment's nonrandom assignment. A concern is that those counties that chose to implement the mask mandate are not similar to those that chose to opt out. This is especially concerning in this application, as the disease's spread is correlated with factors such as population density and the age distribution of a region. Hence, there may be a selection bias.

To investigate this, we collect county-level socioeconomic data from the 2020 American Community Survey and the 2019 County Health Rankings & Roadmaps program. We collect twenty-four variables that describe counties. These include population, population density, and proportions of the population under sixteen and over sixty-five. We also collect demographic information on median age and on gender and racial distributions. Regarding economic variables, we gather information on median household income, educational attainment, and unemployment, labor force participation, and poverty rates. Multiple health-related variables are included: life expectancy, diabetes rate, obesity rate, smoking rate, and the number of primary care physicians per capita. Finally, we include information on mobility measured by visits and visitors at bars and restaurants (described in detail in the upcoming section).

With these we estimate a linear probability model in which never adopting the mandate is the dependent variable. Each observation's fitted value becomes its propensity score. We use inverse probability of treatment weighting to account for differences between those that adopt the mandate and those that do not (Chesnaye et al. 2022). In this method, observations of counties opting out of the mandate are

weighted according to the inverse of the propensity to opt out, $\frac{1}{\hat{p}}$, where $\hat{p}$ is the estimated propensity score from the first-stage regression. Observations from counties not opting out of the mandate are weighted by $\frac{1}{1-\hat{p}}$.[13] This weighting produces a pseudo-population that is more balanced in the twenty-four covariates. Based on its characteristics, if a county adopting a mandate looks more similar to those

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[13] We also engage in a nearest-neighbor propensity-score-matching exercise, restricting the sixty-five counties that adopt the mandate to the subsample of forty that are observationally similar to the forty that opt out. The results from this exercise produce similar findings.

that opt out, then it receives a greater weight so that it makes up a greater share of the pseudo-population, while an adopting state that does not look like the opt-out counties receives a relatively smaller weight, leading to its making up a smaller share of the pseudo-population.[14] After applying the weights, the method is designed to achieve improved balance between the counties that chose to follow the governor's policy and those that chose not to.

Table 2 presents the reestimation of table 1 but uses the inverse-probability-of-treatment-weighting weights to achieve a more representative control group.

Table 2. Inverse probability of treatment weighting:

time period =	Jan. 22, 2020 - Nov. 23, 2020 (1)	Jan. 22, 2020 - Dec. 31, 2020 (2)	Jan. 22, 2020 - March 31, 2021 (3)
Mandate	0.5744 [0.0752] ***	0.5630 [0.0879] ***	0.2804 [0.1669] **
County Fixed Effects?	Yes	Yes	Yes
Week Fixed Effects?	Yes	Yes	Yes
R²	0.326	0.327	0.253
AIC	11,754.1	18,181.9	34,893.8

Notes: The dependent variable counts the number of reported COVID-19 deaths in each of Kansas's 105 counties each week. Observations are weighted by the inverse probability of treatment. Specifications differ by the period covered. Each specification includes 105 county fixed effects and week fixed effects. Standard errors are presented in brackets; * 10%, ** 5%, *** 1% level of significance.

Given that the result persists when using weights to create an observationally equivalent control sample, is robust to correcting for bias arising from staggered policy adoption, satisfies common trends prior to adoption, and is insensitive to adjustments for population differences, we argue our result indicates a causal impact of the policy.

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[14] No county has an inverse-probability-of-treatment-weighting weight greater than 5. As 10 is a typical cutoff used in practice, no particular county makes up an outsized share of the pseudo-population.

D. Mechanism

Unpacking the underlying mechanism is valuable. First, we explore the infection rate. It is worth emphasizing the noisiness of the CDC data. Importantly, one must choose to take a test to be confirmed to have the virus. Thus, a person may feel symptomatic but choose to simply quarantine at home without a test. Additionally, who is forced to take tests uncovering asymptomatic cases (such as college students returning to campus) is nonrandom and likely varies across counties (such as between counties that host universities and other counties). Further, the confirmed case count can be affected by false positives and false negatives. Nevertheless, it is informative to look at the number of confirmed cases and its relationship with the mask mandate. We use the CDC's data on confirmed cases as the dependent variable in the two-way fixed-effects model. Table 3 presents the results.

Table 3. Mask mandates and COVID-19 confirmed cases

time period =	Jan. 22, 2020 - Nov. 23, 2020 (1)	Jan. 22, 2020 - Dec. 31, 2020 (2)	Jan. 22, 2020 - March 31, 2021 (3)
Mandate	142.17	91.41	55.96
	(66.71) **	(43.18) **	(28.08) **
County Fixed Effects?	Yes	Yes	Yes
Week Fixed Effects?	Yes	Yes	Yes
R^2	0.411	0.411	0.445
AIC	56,275.0	67,798.0	85523.8
N	4620	5250	6615
DV μ	28.28	42.91	45.71

Notes: The dependent variable counts the number of confirmed COVID-19 cases in each of Kansas's 105 counties each day. Specifications differ by the period covered. Each specification includes 105 county fixed effects and day fixed effects. Standard errors clustered at the county level are presented in parentheses (105 clusters); * 10%, ** 5%, *** 1% level of significance.

The mask mandate is associated with an increase in the number of COVID-19-related cases. This counterintuitive result can be taken as evidence in support of the Peltzman Effect. If residents perceive the mask mandate as making public interactions safer, then the lowered expected costs result in more social contact. This serves to increase the number of confirmed cases after the mandate's implementation. This observation does not necessarily rule out the Foegen Effect. The re-

inhalation of virions could lead one to be more likely to become symptomatic. This would lead to an increase in the likelihood of seeking out testing and, consequentially, being a confirmed case. Regardless of the mechanism, the implementation of the mask mandate is associated with an increase in confirmed cases, which, not surprisingly, corresponds to increased deaths.

This result should be viewed with caution. Conducting an event study, as was done in figure 3 for deaths, shows strong divergent time trends in the pre-period. The treated counties had lower confirmed case counts prior to implementation, but counts increased as they approached the policy's implementation, with essentially no difference after. Thus, the finding that there is a positive relationship between new cases and the mandate may be an artifact of these divergent trends. This finding also contrasts with the work of others, who have found benefits from the mandate in terms of case counts in other locations. Given the noisy data, conflicting results, and nonparallel trends, further evaluation of this phenomenon may be necessary.

The Peltzman Effect predicts that there will be a change in sheltering-at-home behavior. Cell phone tracking data are available. SafeGraph created aggregated, anonymized data on mobile-device movement. As a public service, SafeGraph provided its mobility data during the pandemic for no charge. We use information on the presence of cell phones at restaurants and bars in Kansas. For each week of the pandemic, SafeGraph records the number of visits to each restaurant and bar and each unique visitor to a restaurant and bar. We aggregate these data to the weekly level and aggregate all restaurants and bars within a county. This provides a county-by-week panel tracking both the number of visits and visitors to these establishments.[15] The two measurements differ when the same person visits an establishment more than once during the week. That individual is recorded as being one unique visitor making multiple visits. We consider both, as the number of visitors establishes how many people receive increased exposure, while the number of visits captures the intensity of the exposure. We use presence at restaurant and bar establishments, as

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[15] Cell phone tracking data have been used to study correlates with shelter-at-home compliance (Alexander and Karger 2023; Brodeur et al. 2021; Brzezinski et al. 2021; Dave et al. 2021; Luther 2021; Smith et al. 2022). See Hall and McCannon (2021) and McCannon (2021) for studies investigating the correlates with governors' decisions whether, and how quickly, to issue stay-at-home orders.

business-closure policies focused on these so-called nonessential businesses. We ask whether changes in movement coincide with adoption of mask mandates. We reestimate our two-way fixed-effects model substituting the movement data for the dependent variable. Table 4 presents the results.

Visits to restaurants and bars are correlated with the mask mandate. The imposition of a mask mandate *increases* the number of visits and visitors to these establishments. Considering column (2), this is an increase of 15.0 percent relative to the sample mean. While likely contrary to the objectives of the policy makers who imposed the mask mandate, this is strong evidence of a deleterious behavioral response as hypothesized by the Peltzman Effect. This finding does not rule out the Foegen Effect. Going to a restaurant or bar may very well mean that an individual who would have been staying at home, likely not wearing a face mask, is now wearing a face mask, as compliance is enforced at the establishment.

Table 4. Cell phone tracking

| | Jan. 22 - Dec. 31, 2020 | | Jan. 22 - March 31, 2021 | |
| | visits | visitors | visits | visitors |
	(1)	(2)	(3)	(4)
Mandate	485.92	454.65	317.01	285.32
	(254.68) *	(230.86) *	(181.69) *	(162.32) *
County Fixed Effects?	Yes	Yes	Yes	Yes
Week Fixed Effects?	Yes	Yes	Yes	Yes
R^2	0.942	0.943	0.950	0.950
AIC	97,824.0	95,386.4	122,578.3	119,957.2
N	5145	5145	6510	6510
DV μ	3842.73	3032.81	3907.77	3086.00

Notes: The dependent variable measures the counts from SafeGraph data. The first in each pair is the total number of visits to restaurants and bars in a county over the week. The second in each pair is the total number of unique visitors to restaurants and bars in a county over the week. Specifications differ by the period covered. Standard errors clustered at the county level are presented in parentheses (105 clusters); *** 1%, ** 5%, * 10% level of significance.

IV. Conclusion

We asked what effect the mask mandate had on COVID-related deaths in Kansas. The state legislature allowed individual counties to opt out of each of the governor's statewide mandates. This created variation in both the decision to opt out of the mandate and the timing of the policy's implementation. We leveraged this quasi-natural experiment and provided evidence that the mask mandate increased the number of COVID-related deaths in the state. We estimated that for every three weeks of having a mask mandate in place, the policy caused two additional deaths (and the effect could be as large as four additional deaths).

Our hope is that the results presented provide useful information for policy makers who are considering implementing a face mask mandate in the future. It is necessary to emphasize that our results do not necessarily imply that face masks are harmful. While masks have the direct effect of reducing the chances an infected individual transmits the virus to someone else, the Peltzman Effect suggests that the net harm identified here can be due to increased interactions between individuals, as perceived safety leads individuals to engage in more risky behavior. Our analysis focuses on the effect of a policy mandating their use and not on the immediate effect of wearing a mask. How the mandate changes people's behavior is an important dimension to consider. The Foegen Effect, on the other hand, suggests that there is a serious public good problem. While wearing the mask provides a social benefit, the increased net harm documented here suggests that the private costs of contributing to the public good may be higher than just the discomfort in mask wearing.

There are some important limitations to the data available that are worth emphasizing. For one, we are unable to evaluate compliance with mandates nor voluntary masking. In other words, we do not know the intensity of face mask wearing and how exactly the mandates changed behavior. Some residents in each treated county were wearing face masks prior to the mandate, and 100 percent compliance with the mandate cannot be expected. Thus, the marginal impact of wearing a face mask is not measurable. We focus on the impact of the policy. Stated differently, we estimate the policy's average treatment effect and not the average treatment on the treated. Further, residents in untreated counties may very well have changed their behavior. Some residents in counties without the mandate undoubtedly voluntarily wore face masks and may very well have increased their use with the governor's orders.

We expected that the mandate had a differential effect between treated and untreated counties, and our results suggests that it did. We provided results based on the existence of a mandate. We are unable to account for the actual magnitude of the treatment on mask wearing. We simply do not know how many Kansans wore face masks.

A second important limitation is that we cannot account for spatial spillover effects. Obviously, the virus moved quickly across space. Therefore, it is perfectly reasonable to presume that the implementation of a mask mandate in one county affects those in surrounding counties. Individual travel patterns between counties are not documented here. For example, a person may live in a county with a mandate but work or shop in an adjacent county that does not have a mandate (and vice versa). This might increase the spread from the mandate county to the no-mandate counties, causing a downward bias in the results. Relatedly, we do not have information on travel into and out of the state. While most of Kansas's neighbors (Missouri, Nebraska, and Oklahoma) did not have a statewide mandate, Colorado did. Accounting for travel would presumably improve the accuracy of the estimate.[16] The mandate's effect of increasing COVID-related deaths, whether it is travel into the mandate counties or travel by those in a mandate county to one without a mandate, would lead to a downward bias in the results, leading us to underestimate the harm caused by the mask mandate.

We strongly encourage future research to replicate our findings and apply our methods to other states or countries where variation in mask mandates exists. Also, we do not explore the public choice dimensions to policy making in this context. Given the lack of evidence of face masks' effectiveness at the time, the political motives for implementing such an extreme infringement are potentially interesting but are beyond the scope of the current study. Further, additional work may want to consider other COVID-19-related policies such as restrictions on going to public places—such as restaurants and bars—school policies, and vaccination availability and uptake.

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[16] Research has focused on the effect of international travel bans on reducing the spread of COVID-19 (Wells et al. 2020) and the closing of bars and restaurants on travel into and out of a city (Smith et al. 2022).

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