Information Revelation of Decentralized Crisis Management: Evidence from Natural Experiments on Mask Mandates

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Abstract

We highlight the role of information revelation for the decentralization of public health policy. State mask mandates stimulate the economy through reduced voluntary social distancing while also lowering COVID-19 case growth, thus providing a counterexample to the widely-held view that there is a policy trade-off between lives and livelihoods. Surprisingly, county-level mask mandates have the opposite effect, increasing social distancing. This is consistent with a model of devolution under information revelation. Households infer from county mask mandates that infection risks have increased locally, and therefore, socially distance more. In contrast, state mask mandates do not lead to similar local inferences.

JEL: I15, I18, J68

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To what degree should public policy be decentralized? This is both a classic question of governance and a topic of ongoing debate. An obvious case in point is the response to the COVID-19 pandemic in the United States, where crisis management has been almost entirely decentralized and relegated to the state and county levels. A natural question remains: Should the response to the pandemic be set at the state level or further delegated to the county level? The devolution literature focuses on the tension between better coordination with centralization and better adaptation to local conditions with decentralization (see Oates, 1972; Strumpf and Oberholzer-Gee, 2002; Knight, 2004). We introduce a new component to this debate focused on the implications of policy source for private information revelation. We show that this information channel is empirically relevant and, in our context, favors centralization of public health policy.[1]

We develop a model with two levels of government to explore how the optimal level of centralization depends on private information revelation. To fit our setting, we consider a model with one higher level of government (the state) and two lower levels of government (counties). We study the outcomes under two regimes; centralization and decentralization. Under centralization, the state enacts a (state-wide) mask mandate if the county-level COVID-19 case rate in either county is greater than some threshold. Under decentralization, county governments enact a mask mandate when the COVID-19 case rate is greater than some threshold. In both regimes, individuals learn about infection risks from government actions and then choose their optimal levels of voluntary social distancing and spending. We classify "risky activity" as refraining from social distancing, and doing activities such as eating out or going to a retail store that may expose you to COVID-19.

Individuals interact with governments in a game with private information, where governments move first. Governments have private information on COVID-19 case rates, while individuals begin with a prior belief about COVID-19 case rates and update it based on government policy. Individuals face a trade-off between risky activity and their health (see

[1] We focus on state and county levels of government because these are the relevant levels in the U.S., from which our data comes.

Philipson, 2000). Individuals have a higher likelihood of catching COVID-19, and having their health decrease, as their risky activity and county-level COVID-19 cases increase. We define risky activity as the amount of shopping and consumption individuals do at brick-and-mortar stores where risks are higher than e-commerce or going to parks. In other words, risky activity is economic activity that occurs outside of the home. For concreteness, we proxy for activity using cell phone activity/mobility data at retail establishments. Note that in our model there is no strategic interaction between governments, though such copycatting/yardstick competition as in Case, Rosen and Hines Jr. (1993), Besley and Case (1995), Solé-Ollé (2006), or Buettner and von Schwerin (2016) is likely important and could be added.[2]

The key insight from our model is that individuals update their beliefs about the risk associated with an activity differently, depending on whether mask mandate policies are centralized or decentralized. Individuals infer that infection risks have increased when mask mandates are imposed. However, their belief updates more in the case that their county enacts the mandate than if their state does. This difference in updating beliefs occurs because the information used by the county government to make the county-level mandate is more specific than the information used by the state government to make the state-level mandate. Said differently, individuals learn more about the risks in their location when policies are enacted at a lower level of government.

The model produces clear predictions on how decentralization shapes the effects of mask mandates on activity. Specifically, the model predicts that the direct effect of enacting a mask mandate will increase risky activity because masks limit the transmission of the virus. Therefore, individuals may feel safer doing group activities, such as going to restaurants or retail stores, making it more likely that they indeed pursue such activities. On the other hand, the model also predicts an indirect (information) effect, which increases consumer risk

[2]We present our information effect in the simplest possible model to highlight its effect. See Agrawal, Hoyt and Wilson (2020) for a review of the literature on local government policy, decentralization, and other federalism mechanisms that should be considered in a larger model necessary to derive the optimal level of government devolution.

assessments, as the government would typically only institute a policy if infection risk is high. This indirect effect reduces economic activity. The net effect of enacting a mask mandate is ambiguous because it depends on whether the direct or information effect dominates. When it comes to state versus county imposed mandates, the model is more precise. State imposed mandates would have the same direct effect but a smaller indirect information effect leading to greater post-mandate activity compared with a county mandate.

This analysis has important health policy implications. Specifically, in the case of mask mandates, the information effect suggests policies should be implemented at a higher level of government. Doing so will improve health outcomes (by decreasing transmission) while increasing activity in contrast to more local policy implementation. Our analysis also suggests that mask-mandates – under centralized implementation – can lead to Pareto-improvements instead of forcing a trade-off between lives and livelihoods. Whether the underlying information effect is of practical importance, however, is an empirical question. We investigate whether the information effect is large enough in practice to cause observable differences in activity between policies enacted at different levels of governments.

We empirically analyze the effects of various county- and state-level mask mandates to understand how decentralization impacts the effectiveness of these policies. To quantify the effects of mask mandates, we need to address a fundamental problem in identifying the dynamic effects of deliberate government policy (Romer and Romer, 2004, 2010). Specifically, policymakers' expectations are unobservable and are simultaneously correlated with policy decisions and future values of the outcome variables. For example, consider a regression of growth in locally reported COVID-19 cases on local decisions to adopt a mask mandate. If local governments anticipate a substantial rise in local cases, they are more likely to impose mask mandates. As a result, a simple regression of case growth on mask mandates might show that the imposition of mask mandates is positively correlated with case growth, even if the true effect of masks is to limit the spread of COVID-19. In this case, deliberate policy induces an upward bias in the correlation of mask mandates and case growth and a downward bias in the mask mandate's effectiveness to limit COVID case growth.

We use several strategies to address this fundamental identification problem of deliberate policy. Our main approach is an event-study approach, which exploits the discontinuous nature of mask mandates and the variation in dates at which states and counties enacted mask mandates. States and counties imposed mask restrictions at varying points from April 2020 through September 2020 (in our sample). In addition, mask mandates typically led to a sudden jump in the fraction of people wearing a mask in public. At the same time, COVID-19 prevalence and unobserved expectations of policymakers change smoothly. As a result, the event-study method estimates the immediate impact of mask mandates on economic activity and COVID-19 case growth.

We confirm that our results from our event-study approach are robust to using alternative approaches, such as parametric and non-parametric regression-discontinuity designs (RDD). All of these approaches rely on time variation within counties relative to when the mask mandate is enacted. Accordingly, our estimates are not confounded by permanent differences in the types of counties or states that enact mask mandates or large changes within counties over time. One such concern could be that political orientation of county officials determined mask policy and that liberal country officials tend to be elected in densely populated areas in which COVID-19 spread faster. However, an event study approach focuses on the change in risky activity days after this county enacted a mask mandate, relative to the control group of risky activity in the same county days before the mask mandate. In other words, this strategy will remove any direct effects that the political orientation of the county has, on activity.[3] The identifying assumption we rely on is the absence of an unobserved county-level factor that changed at the same time as the mask mandate and caused a discontinuous change in behavior, conditional on time-fixed effects. The estimates from these different methods alleviate some concern of potential confounding factors of unobserved expectations

[3] We also investigate whether liberal counties respond differently than conservative counties. In our model we show that differences between these counties are likely due to differences in the effectiveness of mask mandates.

and selection.

We also note that mask mandates sometimes are accompanied by other restrictions, such as limits on gatherings or school, restaurants, and bar closures. We control for these other policies using data from Killeen, Wu, Shah, Zapaishchykova, Nikutta, Tamhane, Chakraborty, Wei, Gao, Thies and Unberath (2020) as well as additional hand-collected data. These types of policies simultaneously reduce risky activity and case growth and are by themselves unlikely to explain our state mask mandate results, as well as the differences between state and county mandates. Furthermore, we offer estimates of medium-run impacts using differences-in-differences and synthetic control methods. Finally, we also provide estimates using updated methods that account for heterogeneous treatment effects in two-way fixed effect specifications (Athey and Imbens, 2021; Sant'Anna and Zhao, 2020; Callaway and Sant'Anna, 2020; De Chaisemartin and d'Haultfoeuille, 2020b; De Chaisemartin and D'Haultfœuille, 2020a; Goodman-Bacon, 2021; Sun and Abraham, 2020; Borusyak, Jaravel and Spiess, 2021; Gardner, 2021).[4] Wooldridge (2021) provides an extensive overview connecting this discussion with alternative estimators. In our context, the estimates from these updated methods support our baseline estimates.

Another important fact about voluntary social distancing in this period is that people reduced their risky activity before government lock-down policies in the spring of 2020 (Chetty, Friedman, Hendren and Stepner, 2020; Yang, Looney, Gaulin and Seegert, 2020a). This fact demonstrates that risky activity is responsive to information not just government policy. We consider whether the information conveyed to individuals from state and county government policies differs sufficiently to cause noticeable differences in behavior.

We use several data sources and surveys to investigate this information effect. We measure risky activity as movement to locations of economic activity, such as restaurants, retail stores, transportation hubs etc. using high-frequency (daily) cellphone-GPS-based data from Google. To put the risky activity data in context, a 10-percentage point increase in risky

[4]For an extensive discussion and a variety of methods used to confirm our baseline results, see Appendix C.

activity is associated with a two percentage point reduction in state unemployment rates (Yang et al., 2020a). We combine that data with manually collected county-level data on mask mandates issued by states and counties to estimate our main empirical results. We complement this analysis by conducting a purpose-built household and business survey to investigate the information effect mechanism. Finally, we use credit card transaction data from Safegraph/Facteus and health data on COVID-19 case growth maintained by *The New York Times* to provide additional context of the effects of mask mandates.

We establish three sets of main results with our data. First, we estimate a one and a half percentage point increase in risky activity in response to state mask mandates relative to a county mandate, which is roughly associated with a 0.3 percentage point reduction in state unemployment. Associated with this finding, we find that activity remains higher after a state mandate than a county mandate, that credit card spending increases by about $40 a month more after a state mandate, and that state mandates result in a reduction of 3 new cases per day per 100,000 people relative to a county mandate. These estimates emphasize that mask mandates can persistently promote economic activity while at the same time safeguarding public health.

Second, we provide direct evidence for consumer responses to mask mandates using purpose-built representative surveys of consumers and businesses for the state of Utah. We find that consumers are highly responsive to information on both confirmed cases and the adoption of mask mandates. In response to a 10% reduction in confirmed cases, households report a 12% higher likelihood of going out to a retail store. This evidence is consistent with the notion that people perceive higher COVID-19 case growth as implying higher infection risks; see also Yang et al. (2020a). At the same time, people report a 49% higher likelihood of going out to a store if their state enforces a mask mandate, supporting the hypothesis that state mask mandates stimulate economic activity. Similarly, businesses report large responses to their revenues from changes in COVID-19 cases and mask-wearing and policies. Specifically, businesses report revenues would decrease by 29% if new COVID-19 cases increased by 20% and that revenues would increase by 9.6% if their state enforced a mask mandate. Combined, the survey evidence suggests that the information effect is plausibly large enough in practice to explain the changes in activity we observe after a state and county mask mandate.

Third, our model does not only help us to understand these baseline empirical results, but can also be used to derive additional predictions about the interaction of state or county mask mandates with cross-sectional differences across counties in terms of mask compliance, prior risk beliefs, and population. For example, the model predicts counties with higher prior risk beliefs will increase activity less after a state and county mandate than counties with lower prior risk beliefs. Using COVID-19 case counts as a proxy for risk beliefs, we find evidence consistent with this comparative static. We consider differences based on mask compliance in counties, political affiliation, and differences across urban/rural areas. The model also allows for a third layer of predictions and tests based on differences in responses using cross-sectional variation. Finally, we show that risky activity increases most after a state mask mandate in counties with relatively small populations relative to the state population—precisely where we expect the information effect from a state mandate to be smallest.

Our study builds on a federalism literature that discusses the optimal level of centralization with its foundation in Oates (1972) and his decentralization theorem.[5] The basic argument is that if the central government has to provide a single level of the private good to all citizens who have heterogeneous preferences and there are no externalities it will be better to decentralize because local governments can provide different levels of consumption that better match the local preferences. When providing a public good, with an externality from economies of scale, there exists a tradeoff between matching local preferences and efficiency.

The subsequent literature has considered the optimal devolution of different policies con-

[5]See also Musgrave (1959) and Gordon (1983) for early formations of the normative question of devolution.

sidering similar trade-offs between central and local governments. For example, a large literature on redistributive taxation considers devolution allowing for people (and businesses) to move across jurisdictions—in essence shopping for a tax-expenditure bundle (Goodspeed, 1989; Boadway, Marchand and Vigneault, 1998; Gordon and Cullen, 2012). As another example, many welfare programs are decentralized because local governments may have more information about their specific needs (McGuire, 1997).

This literature has not considered the role of information conveyed to individuals from their policies—the focus of this paper. We add this additional consideration for devolution in a model that abstracts from many externalities that also affect the optimal devolution of COVID-19 policies. Therefore, the goal of our paper is not to derive the optimal level of government devolution but to demonstrate a new economic mechanism that could change the answer to this question when it is appropriately considered.

Our focus on information effects of policies complements many other aspects of the federalism literature that are highly relevant for COVID-19 policies. For example, early on in the pandemic many state governments found themselves competing with other states for necessary supplies. The existence of this type of competition would suggest the need for more centralization (Goodspeed, 1998; Keen and Kotsogiannis, 2002). In contrast, competition across governments can be positive if it spurs innovation—something needed in dealing with new challenges created by COVID-19 (Hoyt, 1990; Strumpf and Oberholzer-Gee, 2002; Garcia-Milà, McGuire and Oates, 2018; Hines Jr, 2020). There are many ways to have a hybrid approach between centralization and decentralization (Calsamiglia, Garcia-Milà and McGuire, 2013). For example, early on Oates (1972) suggested matching grants from the central level to correct for externalities. These type of budget hybrid models come, however, with other considerations including fly paper effects (Hines and Thaler, 1995), budgetary spillovers (Boadway, Pestieau and Wildasin, 1989; Case et al., 1993; Dye and McGuire, 1997), and divergent goals of governments (Brennan and Buchanan, 1980; Wildasin, 1986; Qian and Weingast, 1997; Oates, 2005; Brueckner, 2006). Finally, there also exists political economy considerations (Kollman, Miller and Page, 2000; Besley and Coate, 2003). A complete model of devolution of COVID-19 policies would need to consider all of these aspects. Our analysis adds to this list and reiterates that American federalism remains robust (Bednar, Eskridge and Ferejohn, 2001; Gordon, Huberfeld and Jones, 2020).

We also contribute to the recent literature on the role of different crisis management policies on health outcomes and economic activity during the COVID-19 pandemic. See, for example, Acemoglu, Chernozhukov, Werning and Whinston (2020), Allcott, Boxell, Conway, Gentzkow, Thaler and Yang (2020), Brzezinski, Kecht and Dijcke (2020), Gros, Valenti, Schneider, Valenti and Gros (2020), Berger, Herkenhoff and Mongey (2020), Stock (2020), Gaulin, Seegert and Yang (2020), Samore, Looney, Orleans, Tom Greene, Delgado, Presson, Zhang, Ying, Zhang, Shen, Slev, Gaulin, Yang, Pavia and Alder (2020), Yang, Seegert, Gaulin, Looney, Orleans, Pavia, Stratford, Samore and Alder (2020b) and Yang et al. (2020a). The paper closest to our study is Chernozhukov, Kasahara and Schrimpf (2020), which uses a structural-equation model to quantify the effects of different policies, including shutdowns and masks, on activity, cases, and fatalities from COVID-19. Chernozhukov et al. (2020) focus on state-level data and therefore do not contrast the differences between state-level and county-level mask mandates. Furthermore, Chernozhukov et al. (2020) primarily focus on employer mask mandates instead of broad public mask mandates. Another related paper is Mitze, Waelde, Kosfeld and Rode (2020), which uses a synthetic-control method to quantify the impact of mask mandates across German states. This paper primarily focuses on changes in case growth and analyzes neither the economic impact of mask mandates nor the question of the optimal governmental level of implementation. We add to this literature an investigation of how institutional design and delegation of policy responsibility affect the effectiveness of policy tools in combating COVID-19 and the related economic crisis.

Our paper argues that the revelation of private government information matters for questions of decentralization of public policy. These unintended information revelation effects exist in a variety of policy-relevant contexts. For example, when the IRS decides to mandate new disclosures, it also provides information about its audit technology (Konda, Patel and Seegert, 2020). Similarly, when the Federal Reserve lowers interest rates to stimulate the economy, it may also be signaling a higher risk of recession to investors (Romer and Romer, 2000). Indeed, these information effects are relevant for decentralization decisions surrounding other public policies, such as stimulus policy, health policy, and various types of crisis management. Under each of these policies, unintended information revelation can undermine the policy's intended goal, and this effect can differ by level of government. Our quantification of information effects shows that decentralized crisis management implies a trade-off between lives and livelihoods for mask mandates. In contrast, centralized crisis management avoids such a trade-off and saves lives and preserves–even boosts– economic livelihoods.

1 Model

1.1 Model description

We consider a model of government policy with two levels of government to explore an information channel that influences which level of government should implement certain policies. To match our setting, we call the higher level of government the state and the lower level of government counties. We consider one state government and two counties, which are under the state. Each county j has a characteristic c_j. In our setting, we refer to c_j as a COVID-19 case rate. Each county consists of n_j individuals, which we index by i.

We consider two regimes. In the first, a county government enacts a mask mandate $\gamma_j \in \{0, 1\}$ when the COVID-19 rate is greater than some threshold r. In the second, the state enacts a mask mandate $\Gamma \in \{0, 1\}$ when the COVID-19 rate in either county is greater than some threshold r. In both regimes, individual i chooses her level of risky activity and spending (hereafter, activity) $a_i \in [0, \infty)$.

Governments and individuals have different sets of information. The state and county governments perfectly observe the case rates in each jurisdiction. On the other hand, individuals in each jurisdiction have no information beyond their prior and will update given what they observe and correct conjectures of the rule being used by government(s). To fix ideas, let individuals have a prior that the case rate in each county is uniformly distributed between α and 2α, where $\alpha \in (r/2, r)$. Defining the prior in this way has the advantage that increasing α increases the prior and leaves the coefficient of variation constant.[6]

Individuals value their activity and their health, which is decreasing in activity when COVID-19 cases are greater than zero. The amount an individual's health decreases with activity is increasing with cases and aggregate activity in their county, $A_j = \sum_{i \in j} a_i$, and decreasing if the county or state enacts a mask mandate. To fix ideas, consider the following utility function

$$U_i(a_i | c_j, A_j, \Gamma, \gamma_j) = a_i + \beta \frac{A_j E[c_j]}{1 + \theta(\Gamma + \gamma_j)} log(1 - a_i), \tag{1}$$

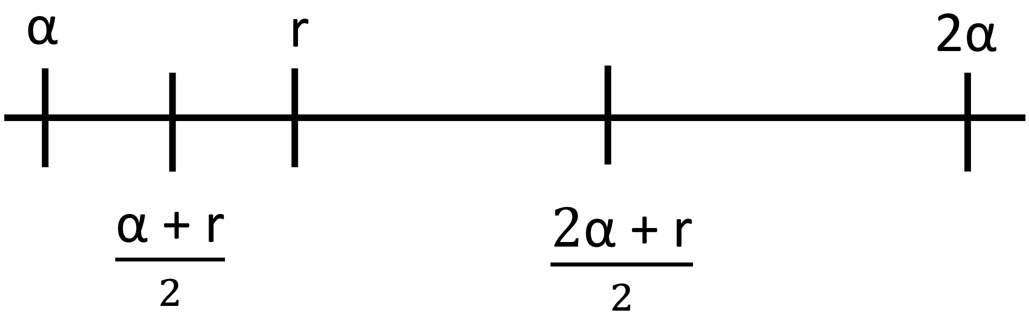
where β provides the utility weight on health relative to activity and θ quantifies the effectiveness of the mask mandates.

1.2 Equilibrium beliefs and activity

Consider how beliefs update depending on policy devolution. Under decentralization, counties enact a mandate if its case rate is above r and the expected case rate if a mandate is enacted is $E[c_j] = \frac{2\alpha + r}{2}$. Similarly, if a county does not impose a mandate the expected county case rate is $E[c_j] = \frac{\alpha + r}{2}$. We highlight these expected case rates in Figure 1.

[6] With a uniform distribution between α and 2α, the coefficient of variation is $\sqrt{1/12}\alpha/((1.5\alpha) = \sqrt{1/12}/1.5$.

Figure 1: Prior and posterior beliefs



Under centralization, the state enacts a mandate if at least one county in the state has a case rate above r. After observing a state mandate, the expected case rate in a county increases—but in a different way than if the county had imposed the mandate. The expected value of the case rate is a convex combination of the expected value if the case rate in your county is greater or less than r. We use Bayes' Rule to calculate the probability that the case rate in county j is greater than the threshold r conditional on observing the state enacts a mask mandate.[7] Specifically,

$$\begin{aligned} E[c_j|\Gamma = 1] &= P(c_j > r|\Gamma = 1)E[c_j|c_j > r] + (1 - P(c_j > r|\Gamma = 1))E[c_j|c_j < r] \\ &= \frac{\alpha^2 + r\alpha + r^2}{2r}. \end{aligned}$$

The expected case rate if the state enacts a mask mandate is less than the expected rates if a county enacts a mandate and greater than if there is no mandate; $E[c_j|\Gamma = 0, \gamma_j = 0] <$

[7]For the derivations of Bayes' Rule see Appendix Appendix A.2.

$E[c_j|\Gamma=1] < E[c_j|\gamma_j=1]$. These cases are summarized below

$$E[c_j] = \begin{cases} \frac{\alpha+r}{2}, & \text{if } \gamma_j = 0 \ \& \ \Gamma = 0 \\ \frac{\alpha+r}{2} + \frac{\alpha^2}{2r}, & \text{if } \Gamma = 1 \\ \frac{\alpha+r}{2} + \frac{\alpha}{2}, & \text{if } \gamma_j = 1, \end{cases}$$

or alternatively by $E[c_j] = \frac{\alpha+r}{2} + \Gamma\frac{\alpha^2}{2r} + \gamma\frac{\alpha}{2}$. Note that the state and county cannot simultaneously enact a mask mandate in our model such that either $\Gamma = 1$, $\gamma_j = 1$, or $\Gamma = 0 \ \& \ \gamma_j = 0$.

An individual's equilibrium activity is a function of the policy enacted, Γ and γ, the expected case rate in their county, $E[c_j]$, the population within their county, n_j, and the effectiveness of the policy θ. The equilibrium value of activity is found by taking the derivative of the utility function in equation (1), setting it equal to zero, finding the best response activity given the activity of everyone else in their county, and finally finding the intersection of the best response functions (see Appendix A.1 for details). The resulting activity is given by

$$a_i = \left(1 + \beta\frac{n_j E[c_j]}{1+\theta(\Gamma+\gamma_j)}\right)^{-1}. \quad (2)$$

1.3 Policy effectiveness

This section investigates how activity differs across regimes where the state or county enacts a mask mandate. Our results are summarized in two propositions. We investigate additional model implications in Section 8 and Appendix A.4.

Proposition 1 (State and County Mandate Comparison)**.** *Activity is higher with a state mandate than a county mandate.*

$$a_i(\Gamma = 1) > a_i(\gamma_j = 1). \quad (3)$$

Proof. See Appendix A.3. □

This comparison between state and county implementation highlights the information tradeoff. The direct effect of the mask mandate, given by θ, is the same for both. The difference is driven by how individuals update their infection risk assessment of COVID-19. Individual beliefs are less strongly updated in response to a state mandate than in response to a county mandate. This difference leads individuals to have higher activity after a state implements a mask mandate than if the county does.

To further highlight the information effect, we show that the activity after a state and county mandate can move in different directions, not just different magnitudes. Specifically, enacting a mask mandate has two effects, a direct effect given by the effectiveness of mask mandate θ and an information effect given by the updated expected case rate. Activity increases after a mask mandate if the direct effect is larger than the information effect. Similarly, activity decreases after a mask mandate if the direct effect is smaller than the information effect. Proposition 2 shows that activity increases after a state mandate and decreases after a county mandate if the direct effect is bounded between the information effect after a state and county mask mandate.

Assumption A (Regularity assumption on the effectiveness of masks)**.** *The effectiveness of masks at lowering transmissions is bounded between the percent increase in expected case rate after a state and county mask mandate* $\theta \in (\%\Delta E[c_j|\Gamma = 1], \%\Delta E[c_j|\gamma_j = 1])$.[8]

Proposition 2 (State and County Mandates Relative to No Mandate)**.** *Under assumption A, the highest activity occurs after a state mandate, the lowest activity occurs after a county mandate, and between these two is activity without a mandate.*

$$a_i(\Gamma = 1) > a_i(\Gamma = 0) = a_i(\gamma_j = 0) > a_i(\gamma_j = 1). \tag{4}$$

Proof. See Appendix A.3. □

[8]The percent change in expected cases is given by $\%\Delta E[c_j|\Gamma = 1] = (E[c_j|\Gamma = 1] - E[c_j|\Gamma = 0])/E[c_j|\Gamma = 0]$ and $\%\Delta E[c_j|\gamma_j = 1] = (E[c_j|\gamma_j = 1] - E[c_j|\gamma_j = 0])/E[c_j|\gamma_j = 0]$.

This ordering demonstrates the practical importance of the information effect because it can cause policies to have different effects if undertaken at different levels of government. In this case, the information effect moves in the opposite direction of the direct effect. The information effect lowers activity due to increasing the expected case rate (and therefore risk). The direct effect increases activity by decreasing transmission. The direct effect is the same whether the state or county implements the mandate—the information effect, however, differs. Under assumption A, activity decreases after a county mandate but increases after a state mandate because the information effect is larger than the direct effect after a county mandate but smaller than the direct effect after a state mandate.

2 Motivating Stylized Facts

The United States federal government left the determination of public safety measures in response to the COVID-19 pandemic up to the individual states and counties throughout 2020. Riverside County, California, imposed the first mask mandate at the beginning of April 2020. The first state mask mandate was put in place by New York state on April 17, and the most recent state mandate in our sample was put in place by Mississippi at the beginning of August. From the beginning of April to the beginning of September 2020, 34 states plus Washington D.C. enacted state-wide mask-wearing mandates. Additionally, from April to September, 221 counties across 33 states put in place county-wide mask-wearing mandates. This patch-work approach provides heterogeneity in the timing and the level of government policy interventions. This heterogeneity offers a natural laboratory to study the public's reactions to public safety regulations.

Table 1 presents the summary statistics for the 25 days before a state- or county-level mask mandate was enacted. Our primary proxy of economic activity,[9] measures the amount of traffic at economically relevant activities (e.g., retail shopping and transportation) as a

[9] Our activity proxy comes from Google's cellphone-location based mobility data, a daily-frequency comparison of mobility relative to the same calendar day in the prior year; for more detail, see Yang et al. (2020a).

percentage of the prior year's activity levels. Over the sample period, the average activity is 14% to 16% lower than for the equivalent period in 2019, reflecting the general reduction in economic activity observed throughout much of 2020 (Yang et al., 2020a). We also consider consumer spending from Facteus, with which we calculate as average spending per person per month.[10] Similar to activity, we find spending per month is slightly lower in the county-level mandate sample.

The information conveyed by the news of a mandate might differ depending on whether it comes from the county or the state. For example, the daily new infection rate in counties around state mandates was 12 per 100,000 residents, while the equivalent number around county mandates was 25. This evidence suggests that the difference in information revealed may be partially due to true differences in new cases. The increased rate for county mandates is also reflected in the variable *High county*, which shows that 77% of the county mandates have above-median event-day-0 infection rates, compared to only 53% for state-level mandates. This evidence is consistent with the idea that county mandates were put in place in response to heightened infection rates and depressed economic activity in that county. In contrast, state mandates, which affect a larger region, may not reflect such acute statistics.

The counties enacting mandates are more urban (*Urban county* is 62% compared to 23% for state mandates) and adopt those mandates before June 30, 2020 (*Early county* is 55% compared to 63% for state mandates). According to survey data published by *The New York Times* (*Comply county*), 3% of residents covered by a county mandate reported compliance high compliance with a mask mandate, compared to 4% under state mandates. Lastly, the counties enacting mask mandates are slightly less liberal, with 45% being majority

[10]The Facteus data contains credit card spending data from multiple payment processing companies but only covers a subset of processed spending. Unfortunately, this data provides spending based on residence and does not distinguish between online versus in-person spending. To derive an economically interpretable coefficient of "spending per person per month," we calculate the full-sample average number of transactions from Facteus per population, which is 0.04 daily transactions per person, or, assuming roughly one credit card transaction per person per day (a conservative estimate), the Facteus data comprises 1/0.04 or 1/25 of daily spending or $1/(30 \times 25)$ of monthly spending. However, we have no reason to believe there are selection issues in this consumer spending proxy. To arrive at our final proxy for spending/month, we multiply our spending per person by 30×25.

liberal. However, only 18% of counties under state mandates are liberal (*Blue county*, based on the 2016 presidential vote). Liberal counties also adopted mandates earlier than their conservative counterparts. In Table A.1 we report correlations for all variables used in the analysis. We find a 0.22 correlation coefficient between *Early county* and *Blue county* for counties that enacted mandates.

We also consider announcement effects and timing surrounding case reporting. In general, our results are not sensitive to using the date the mandate was announced or enacted because these typically were within a few days of each other. Reported cases are necessarily a lagged variable due to delayed discovery of physical transmission processes like virus infection and reporting delays. We do not find substantial differences with other plausible assumptions. See Section 6.4.

In Figure 2, we show the geographic dispersion of counties and states with mask mandates. There is a significant overlap between county-level mask mandates and state-level mask mandates. In these cases, we consider the order which was put in place earlier (e.g., Riverside County, California). In Figure 3, we show two maps at the county level. The first is the county-level active coronavirus cases per 100,000 people as of August 1st. The second is the map of counties and states with any mandate ever in place (the combination of counties in Figure 2). The counties with the highest case rates, shown in darker red, seem to also be in counties that never had a mask mandate. Our analysis below goes beyond this type of correlation to determine the policy impact of mask mandates on economic activity and case counts.

3 Empirical Methodology

Our goal is to test the model's main prediction that the information effect is large enough to cause differences in activity following a state or county mask mandate. Unfortunately, whether at the state or county level, government policy is not random and depends on current and expected future conditions. For example, a local government imposes a mask mandate when it expects higher case growth. In this case, the rate of new cases may very well increase, but concluding from this increase that the mandate has a positive effect would be misleading because we cannot observe the counterfactual. This effect imposes a spurious upward bias into the correlation of mask mandates and the number of new cases.

Consider the following regression for county j to formalize this potential endogeneity,

$$Y_{j,\tau_j+1} = \beta \cdot \mathbb{1}(\tau_j > 0) + g_j(\tau_j) + \epsilon_{j,\tau_j+1}, \tag{5}$$

where $\mathbb{1}(\tau_j > 0)$ is an indicator for the imposition of a mask mandate, and $g_j(\tau_j)$ is a continuous function. In specification (5), time is measured in "event time" for every county j, i.e., relative to the time when the mask mandate was imposed at time $\tau_j = 0$.[11] The continuous function $g_j(\tau_j)$ can capture a variety of time-varying omitted variables. Principal among these omitted variables are unobserved expectations of local government officials at time τ_j, given by $g(\tau_j) = \gamma_j \cdot E_{G,j,\tau_j}\left[Y_{j,\tau_j+1}\right]$. Note that the imposition of the mask mandate $\mathbb{1}(\tau_j > 0)$ and government expectations $E_{G,j,\tau_j}\left[Y_{j,\tau_j+1}\right]$ are correlated, and, at the same time, government expectations are also directly correlated with future outcomes of the dependent variable, because they are expectations of this variable.

We use an event-study approach to address this endogeneity issue. The event-study approach builds on the fact that mask use can be adopted immediately, which is similar to the ability of stock prices to jump in response to new information, see Khotari and Warner (2006). As a result, one can measure average outcomes across counties within a short time window in event time as

$$\frac{1}{N}\sum_j \left(Y_{j,t_j+1} - Y_{j,t_j}\right) = \beta + g_j(\tau_j + 1) - g_j(\tau_j), \tag{6}$$

[11] In other words, given calendar time t and given that $t_{m,j}$ is the date of the mask mandate for county j, then $\tau_j = t - t_{m,j}$.

where $g_j(\tau_j + 1) - g_j(\tau_j) \approx 0$, which captures the key assumption of only minor systematic changes at the daily frequency within the time window analyzed. We verify the robustness of our results to this assumption in four ways. First, we control for other county-level restrictions such as stay-at-home orders and school or restaurant closures. Second, we implement a non-parametric regression-discontinuity design approach in Subsection 6.1. Third, we pursue a global regression-discontinuity design approach in Subsection 6.1, which flexibly controls for the function $g_j(\tau_j)$. Both of these approaches allow us to control for continuous, time-varying unobservables in event-time, which correspond to $g_j(\tau_j)$. Finally, in Subsections 6.2 and 6.3, we report estimates from placebo tests using variation in time and location.

We test the model predictions of the information effect in Proposition 1 by testing whether there is a difference in activity after a state or county enacts a mask mandate. An observation is a county day. The event study specification we use has activity as the dependent variable. The independent variables include *State mandate*, which is an indicator that equals one if and when the state enacted a mask mandate $\mathbb{1}(\Gamma = 1)_{\tau,j}$ and zero otherwise, and *Mask mandate*, which is an indicator that equals one if and when there is a mask mandate (enacted by the county or state), $\mathbb{1}(\Gamma = 1|\gamma_j == 1)_{\tau,j}$ and zero otherwise. In addition, we include county fixed effects λ_j, week fixed effects λ_t, and controls $x_{k,j,t}$ for whether retail, restaurants, bars, schools have restrictions and whether there are restrictions on gatherings of 10 or more people, 50 or more people, or thresholds more than 50 people, or stay at home orders. This specification is given by

$$\begin{aligned} \text{Activity}_{j,\tau_j} =& \beta_0 + \beta_1 \mathbb{1}(\Gamma = 1)_{\tau,j} + \beta_2 \mathbb{1}(\Gamma = 1|\gamma_j = 1)_{\tau,j} \\ &+ \sum_k \delta_k x_{k,j,t} + \sum_w \delta_w \lambda_w + \sum_j \delta_j \lambda_j + \varepsilon_{j,\tau_j+1}, \end{aligned} \tag{7}$$

where λ_w indicates week fixed effects and ε_{j,τ_j+1} is an error term. The coefficient of interest is β_1, which identifies the differences in activity after a state enacts a mask mandate relative to a county mandate. Specifically, the test is that the sign of the coefficient on state mandate is positive. The theory has no prediction on the magnitude. The key insight from Proposition 1 is that activity will be more positive after a state mandate than a county mandate because both the same positive direct effect, but the negative information effect is larger for a county mandate than a state mandate.

The coefficient β_2 identifies the effect of a mask mandate enacted by a county on activity. The sum of β_1 and β_2 identifies the effect of a mask mandate enacted by a state on activity.

We test for treatment heterogeneity based on our model's predictions in Section 8. We also report estimates in Appendix C that use recent model extensions as discussed in Athey and Imbens (2021); Sant'Anna and Zhao (2020); Callaway and Sant'Anna (2020); De Chaisemartin and d'Haultfoeuille (2020b); De Chaisemartin and D'Haultfœuille (2020a); Goodman-Bacon (2021); Sun and Abraham (2020); Borusyak et al. (2021); and Gardner (2021).

4 Empirical Evidence

In this section, we study the effects of a mask mandate and how they vary when a state or a county enacts the mandate. We follow the model and focus on *risky activity* at a county level, which we measure as the change in consumer movement to retail locations, transportation hubs etc, in 2020 relative to the same day in 2019. In section 7, we consider *risky activity* over the medium run, *Spend/month*, daily credit card spending per person (scaled to the monthly level for economic interpretation), and *Cases/pop*, the daily new cases per 100,000 population.

We start by showing the raw data in the form of a bin scatter in event time. Figure 4 shows the difference in risky activity between counties with a state mandate (treatment group) and a county mandate (control group) around the time of adopting the mandate (normalized to $t = 0$). As the figure shows, before the mask mandate, activity in counties with a state mandate is similar to, and not statistically different from, counties with a county mandate. After the mask mandate, risky activity increases for counties with a state mask mandate relative to those with a county mandate. Specifically, right at the threshold, depicted by a red vertical line, there is a level shift up in activity, denoted by the shift up of the horizontal red line to the right of the threshold relative to the line to the left.[12]

Proposition 1 states that risky activity will be greater after a state enacts a mask mandate than if a county enacts it. We provide a direct test of this proposition in the first row of Table 2, which reports the coefficient on state mask mandate from equation (7).

We find that risky activity is greater after a state mandate relative to a county mandate, consistent with proposition 1. Specifically, the coefficients in row one of Table 2 are positive and precisely estimated across all specifications; standard errors are clustered at the county level. The columns in Table 2 vary the controls. Columns (1), (2), and (3) report estimates with no controls, basic controls, and expanded controls, respectively. Basic controls include indicator variables for restrictions to retail, restaurants, and bars. Expanded controls include variables for stay-at-home rules, school closings, and restrictions on gatherings of 10 or more people, 50 or more people, or thresholds of more than 50 people. Column (4) adds county fixed effects, and column (5) adds week fixed effects.

The magnitudes suggest that activity is 1.6 to 9.3 percentage points greater if a state mandate enacts a mask mandate instead of a county. While there is a large difference in the magnitude, they all provide evidence consistent with Proposition 1, that risky activity is higher if a state rather than a county implements a mask mandate.

In Figure 5, we show estimates of the effect of a state mask mandate, relative to a county mask mandate, on risky activity using a panel event study design following Freyaldenhoven, Hansen and Shapiro (2019). These designs have several advantages relative to the binned scatter plot, presented in Figure 4. First, the top panel of figure 5 presents estimates of counties with a state mask mandate relative to counties with county mandates in event time. The figure is reassuring, since it highlights the absence of any significant difference in pre-treatment trends and shows that the effects after adoption are persistent and not

[12] Appendix Figure A.1 provides the sensitivity of the estimates to the exact threshold date by running falsification tests of a range of dates around the actual threshold.

just temporary. Second, the bottom of figure 5 follows suggestions by Freyaldenhoven et al. (2019) and flexibly estimates the pre-treatment trends and then subtracts this pre-treatment trend from the post-treatment estimates. Instead of using the absence of a significant pre-treatment trend as justification to ignore any possible common trends between treatment and control groups, this method has the advantage that it is robust to the possibility that there unobserved common trends that are too subtle to be reliably ruled out at standard levels of statistical significance. We show in the bottom of Figure 5 that our results become even stronger if we use this method. These results show the robustness of our main results from the raw data binning in Figure 4 and the regression results in Table 2.

Proposition 2 states that under certain assumptions (Assumption A), the information effect can be sufficiently large to cause activity to increase after a state enacts a mask mandate and decrease after a county enacts one. The second row of Table 2 provides a direct test of whether activity decreases after a county enacts a mask mandate. The sum of the first and second rows provides the overall effect of activity after a state mandate. We report the Wald p-values for whether this sum is zero at the bottom of the table.

We find evidence consistent with Proposition 2: activity decreases after a county enacts a mask mandate and increases after a state does. Specifically, the coefficients in row two of Table 2 suggest that activity decreases 1.4 to 8.0 percentage points after a county enacts a mask mandate. This reaction is consistent with people updating their risk assessment of COVID-19 and partially or fully quarantining. The Wald p-values reported at the bottom of Table 2 suggests that we can reject that the overall activity effect after a state enacts a mask mandate is zero with relatively high precision.

In sum, we find that, although county-level mask mandates may seek to decrease health risks and potentially benefit consumer confidence, they also provide a strong signal that increases consumers' perceived risk of contracting COVID-19. When this indirect information channel is significant enough, as in this case with county mask mandates, the net effect on activity is negative.

5 Survey Evidence

In this section, we report evidence from two separate purpose-built representative consumer and business surveys for the state of Utah. We use these surveys to test whether the information effect is large enough to plausibly cause the differences between state and county mask mandates we observed in the previous section. Specifically, we quantify how responsive people are to COVID-19 conditions and mask mandates.

5.1 Consumer Survey

The consumer survey samples 400 Utah residents per month, and we designed it to be similar to the Michigan Survey of Consumers, which includes 500 completed interviews per month. The sample is recruited based on addresses by sending participants a letter. We provide a $10 gift card incentive for participation in an online survey. The sample comprises the universe of addresses in Utah and is sampled based on prior nonresponse rates to provide a final sample that is representative of Utah (Samore et al., 2020; Yang et al., 2020b). This sampling method has been shown to have minimal nonresponse sample selection (Gaulin et al., 2020). In Appendix B.1, we provide more details on recruitment, nonresponse, and sample balance.

In Panel A of Table 3, we report household responses to the question "How much more or less likely (as a percent) would you be to go out to a store if ..." with the following four scenarios: "The number of confirmed cases fell by 10%," "The number of confirmed cases fell by 90%," "Everyone was wearing a mask," "The state-enforced wearing a mask." This question is meant to determine how responsive people are to the perceived risks associated with COVID-19.

Overall, participants report that they are very responsive to the number of confirmed cases and how many people wear masks in their locality. In response to a drop in confirmed cases of 10%, and 90%, participants report they would increase their likelihood of going to a store by 12%, and 57%, respectively. These correspond to elasticities between 0.6 and 1.2. Similarly, respondents report that they would be substantially more likely to go out to a store if everyone wore a mask (49% more likely), and the state-enforced wearing a mask (48% more likely). Despite participants recognizing that a state mask mandate would not ensure compliance, a state-enforced mask mandate substantially increases their willingness to go out to a store.

These survey responses suggest the potential for a large information effect. Participants report that they are very responsive to the COVID-19 environment. Therefore, if mask mandates also update their beliefs of this environment, they may reduce activity. This evidence highlights the importance of government messaging of policy initiatives.

5.2 Business Survey

The business survey includes over 10,000 responses from businesses across Utah in late November 2020, where participants entered a lottery for different prizes. These prizes included ten $1,000 gift cards, ski jackets, and kayaks. The sample comprises the universe of firms registered with the State of Utah. Appendix B.2 provides more details.

Panel B of Table 3 reports how monthly revenues would change in the following five different scenarios: "New case counts increased by 20%," "No new COVID-19 cases," "Everyone wore a mask," "Your county enforced a mask mandate," "The state enforced a mask mandate."

First, businesses report that their revenues are very responsive to COVID-19 cases—consistent with the consumer survey. In response to a 20% increase in new COVID-19 cases, businesses report their revenues would decrease by 29%. Similarly, they report that their revenues would increase 27% if there were no new cases.

Second, businesses believe that their revenues would increase with mask-wearing and policies that encouraged mask-wearing. Specifically, businesses report that their revenues would increase 13% if everyone wore a mask. Businesses also report that their revenues would increase by roughly 9% if there were mask mandates enforced at the county or state level.

Third, businesses report that revenues would be 1.2 percentage points or 14% higher if a state-enforced a mask mandate than if a county enforced the mask mandate. This evidence is consistent with our previous findings and the information effect. Specifically, consumers are more willing to go out to stores as a result of a state mandate rather than a county mandate.

6 Sensitivity of estimates

This section considers additional specifications and placebo tests to explore potential threats to the identification. To identify the effects of mask mandates, the estimates presented previously have relied on the staggered implementation of mask mandates and the smoothness of potential confounding factors around the time of implementation. Our identification strategy relied on two key features. First, mask mandate policies vary both across time and across counties. Second, our event-study approach relied on the assumption that continuous unobservable effects are negligible within our short-run time windows.

This section stress tests these two features in reverse order. First, we address the potential impact of continuous unobservables on our event-study design. We extend these estimates by considering a (non-parametric and parametric) regression-discontinuity design. Second, we perform placebo tests by (i) assigning the timing of the mask mandate randomly 25 to 100 days after the actual mask mandate was enacted and (ii) assigning treatment status to counties that did not enact mandates but are adjacent to counties that did. These placebo tests seek to uncover potential unobservable characteristics across counties or time, explaining our previous findings.

We find that our estimates are not sensitive to different identifying assumptions in the regression-discontinuity design, and we find no evidence of sufficiently important confounding factors in the placebo tests. Finally, we also report estimates with different thresholds and find the estimates are not sensitive to different timing.

6.1 Regression-Discontinuity Design for the Immediate Impact of Mask Mandates

In this section, we address the potential concern that the key assumption of our event-study approach is overly strong. Specifically, the concern is that there are large systematic changes at the daily frequency, or $g_j(\tau_j + 1) - g_j(\tau_j) \approx 0$. One way to address this issue is to use a regression-discontinuity design, which allows for any continuous omitted variable in a flexible way.

For our regression-discontinuity design, we start with a non-parametric approach where the mask mandate provides the source of a regression discontinuity. The key assumption is that a mask mandate generally leads to a sudden jump in the fraction of people wearing a mask, captured by the discontinuous change $\mathbb{1}(\tau_j > 0)$. At the same time, COVID-19 prevalence and unobserved expectations of policymakers change in a continuous fashion, captured by the function $g_j(\tau_j)$.

Let $Y_{j,\tau_j+1}(0)$ and $Y_{j,\tau_j+1}(1)$ denote outcome of county j without and with mask mandates respectively. A non-parametric approach to the regression discontinuity optimally chooses a bandwidth Δ, such that

$$\begin{aligned} E\left[Y_{j,\tau_j+1}\middle| -\Delta < \tau_j < 0\right] &\approx E\left[Y_{j,\tau_j+1}(0)\middle|\tau_j = 0\right] \\ E\left[Y_{j,\tau_j+1}\middle|0 \leq \tau_j < \Delta\right] &\approx E\left[Y_{j,\tau_j+1}(1)\middle|\tau_j = 0\right], \end{aligned} \tag{8}$$

where, as we will show in the estimation section, Δ is around 15 days, using optimal bandwidth selection methods such as Imbens and Kalyanaraman (2011) and Calonico, Cattaneo and Titinuik (2014). The jump in outcome Y_{j,τ_j+1} at $\tau_j = 0$, therefore, identifies the immediate mask impact

$$\beta_0 = E\left[Y_{j,\tau_j+1}(1) \middle| \tau_j = 0\right] - E\left[Y_{j,\tau_j+1}(0) \middle| \tau_j = 0\right]. \tag{9}$$

We also consider parametric versions of the specification in equation (5), by using flexible polynomial functions to control for $g_j(\tau_j)$. This specification is especially useful for analyzing interaction effects because it allows us to report comparable estimates across specifications while also allowing us to control for location- and time-fixed effects.

Being aware of recent criticisms of using regression-discontinuity designs with time as a forcing variable, such as in Hausman and Rapson (2018), we caution the reader not to overweight the importance of these estimates relative to our main results. However, we also note that our setting is different from many of the contexts discussed in Hausman and Rapson (2018) in at least two respects. First, Hausman and Rapson (2018) focus on settings without cross-sectional variation, where estimation bandwidths are often extended to increase power. In contrast, we have rich cross-sectional variation, such that we can rely on cross-sectional asymptotics instead of time-series asymptotics as criticized by Hausman and Rapson (2018). Second, Hausman and Rapson (2018) caution that dependent variables might exhibit persistence. We maintain that this is less of an issue for activity because it is a flow variable that can adjust almost immediately.

Table 4 reports the result of the regression-discontinuity design in event time. Our main results are qualitatively similar in these specifications, lowering the probability that our event-study approach is systematically biased. The estimates are similar across columns. We report the regression discontinuity estimates for state mandates in columns (1)–(3) and county for columns (4)–(6). In columns (1) and (4), we report estimates with a flexible time trend, and in columns (2) and (5) and (3) and (6) estimates with a linear and cubic polynomial, respectively. Encouragingly, these results are quantitatively close to our main results.

Overall, the assumption that continuous unobservables only have a small impact on outcomes is reasonably robust.

6.2 Timing Placebo Tests

Placebo tests provide a check on the likelihood that unobservable cross-sectional characteristics could be an alternative explanation for the reported estimates. If our specifications measure the true effect of mask mandates, we should find no impact in these placebo tests. In this placebo test, we use the 2,188 counties with a county or state mandate and assign their mandate to occur randomly between 25 and 100 days after the relevant mandate.

In Panels A and B of Figure 6, we report 99 placebo estimates, in gray, and our baseline estimate, in black, sorted from smallest estimate to largest estimate. Panel A reports the state mandate coefficient from our specification in Table 2 column (5). Similarly, Panel B reports the mask mandate coefficient. Each placebo estimate represents a different random assignment of event times across all counties that ever enact a mandate.[13] We find no evidence of an effect around these placebo mask mandates. Specifically, the largest estimate in magnitude in both panels is our baseline estimate. In addition, none of the 99 placebo estimates in either panel are statistically significant from zero. Said differently, when using random dates for the event time, the effect size is small and not precisely estimated. These tests suggest our findings are not driven by unobservable cross-sectional confounding factors or by chance.

6.3 Location Placebo Tests

This section complements the previous section by reporting estimates from a placebo test across locations. We select all counties without a county or state mandate adjacent to a county that enacts one (or is in a state that enacts one). Initially, 2,188 counties have a county or state mandate, and with this designation, we have 370 placebo counties. We assign

[13] Each county receives a county-specific event time for each of the 99 randomizations.

the time of the mask mandate for these placebo counties to be the adjacent county's time, and if there are multiple adjacent counties with mask mandates, we take the first time. We expect some spillover effects from the mask mandate to adjacent counties, but these effects likely bias us toward finding an effect due to these spillovers. For example, people in adjacent counties may become more mobile to shop in the nearby county with a mask mandate due to increased safety.

In Panels C and D of Figure 6, we report 99 placebo estimates, in gray, and our baseline estimate, in black, sorted from smallest estimate to largest estimate. Each placebo estimate represents a different random selection of adjacent counties. Panel C reports the state mandate coefficient from our specification in Table 2 column (5). Similarly, Panel D reports the county mask mandate coefficient. Our baseline result is the greatest effect with smaller confidence intervals, suggesting this size is not due to chance or other confounding factors. The sometimes positive and relatively precise estimates in panel C in the adjacent counties are consistent with some spillovers across counties. The relatively smaller size of these estimates suggests that other confounding factors are unlikely to lead to our estimate.

6.4 Threshold timing

Finally, we also test for sensitivity of estimates to the exact threshold date by running falsification tests of a range of dates around the true threshold. The response to the mask mandate could differ from the date we use for several reasons, including anticipation effects, delayed information diffusion, and measurement error. To test whether our results are sensitive to this, we reran our baseline specification using different thresholds from five days before to five days after the true threshold, reported in Figure A.1. Encouragingly, we find the estimates are similar across all specifications and that the true threshold has the largest magnitude, suggesting that we are not incorrectly assigning the threshold.

7 Additional outcome variables: medium-run estimates, spending, and COVID-19 cases

In this section, we provide some additional context for our results on activity. First, we investigate whether the differences in activity that we observe in the short-run after a mask mandate persists in the medium-run. Second, we consider how spending and COVID-19 cases change following a mask mandate. These estimates are not directly related to the model but provide context for the effects of mask mandates broadly.

7.1 Medium-run estimates

We rely on synthetic control methods developed by Abadie, Diamond and Hainmueller (2010, 2015) and Abadie and Gardeazabal (2003) to extend our analysis to the medium-run. The basic identification challenge we face is that expectations of local government officials are unobservable, as discussed in Section 3. However, as we increase the time horizon T for estimation, the assumption that changes in continuous omitted variables are negligible $g_j(\tau_j+T) - g_j(\tau_j) \approx 0$, becomes clearly untenable. Instead, synthetic control methods allow us to flexibly control for time-varying functions $g_j(\tau_j + T)$, which capture variables such as unobserved expectations of local government officials.

We construct synthetic control counties by weighting counties that do not have a county or state mask mandate to match counties with a state or county mandate

$$\tilde{Y}_{j,\tau_j+1} = \sum_{l=1}^{N} w_l \cdot Y_{j,\tau_j+1}, \tag{10}$$

where l indexes counties without mask mandates. Weights w_l are chosen to match pre-treatment characteristics of county j, which eventually imposes a mask mandate. For weights w_l, we employ entropy weights as proposed by Hainmueller (2012). We generate these weights matching the mean on county population, activity, change in cases, and spending in a preperiod.

Given our measurement in equation (10) is successful, we measure $Y_{j,\tau_j+T}(0) = g_j(\tau_j + T) + \varepsilon_{j,\tau_j+T}$, so we can correctly identify

$$\beta_T = E\left[Y_{j,\tau_j+T}(1) - Y_{j,\tau_j+T}(0) \middle| \mathbb{1}(\tau_j > 0), \{w_j\}\right]. \tag{11}$$

In Table 5, we report the medium-run effects of mask mandates on activity in columns (1) and (2). Consistent with our short-run estimates in Table 2, we find that activity continues the same trend as in the short-term—increasing after state mandates and decreasing following county mandates. The state mandate coefficients suggest that the higher activity after a state mask mandate, relative to a county mandate, is relatively persistent 75 days after the mandate. Specifically, state mandate coefficients in columns (1) and (2) suggest that activity remains 1.1 to 1.4 percentage points higher after a mask mandate if a state rather than a county enacts it, compared to 1.5 percentage points in the short-run (column 5, Table 2). The mask mandate coefficient is small and not statistically significant.

7.2 Spending and COVID-19 cases

In this subsection, we briefly discuss how spending per month and cases per 100,000 people change around implementing a mask mandate. We expect spending to follow a similar pattern as activity. The expectation on cases, however, is ambiguous. Mask mandates decrease contagiousness but as we have seen also increases people's activity and chances of interacting with people. The net effect, therefore, is ambiguous.

We find that spending followed a similar pattern as activity after a mask mandate. Spending was \$32 to \$45 higher per month after a state enacted mandate relative to a county mandate (row one, columns 3 and 4 of Table 5). Spending after a county mandate was \$94 to \$102 lower (row two, columns 3 and 4 of Table 5). These estimates are consistent with the difference in activity we previously found. However, we caution against over-interpreting this result because the data is from credit card spending, which surely does not capture all spending.

We find that cases decreased more if a state rather than a county enacted the mandate. Specifically, we find that cases were lower by three people per 100,000 people after a state mandate relative to a county mandate (row one columns 5 and 6 of Table 5). This evidence suggests that although activity is higher after a state enacted mandate, relative to a county mandate, the increased activity does not increase contagion risk enough to overcome the direct benefits of wearing a mask. Outside of this paper's scope, there are many other important considerations as to why cases might be lower with a state enacted mandate, including more compliance with a state mandate than a county mandate and greater efficiency from spillovers from other counties within the state. Our finding is consistent with and adds to the growing literature on the effectiveness of mask mandates (Krishnamachari, Morris, Zastrow, Dsida, Harper and Santella, 2021) from a health perspective. Our main contribution is to show differences by state and county enacted mandates and that there does not seem to be a tradeoff between the economy and health—in fact, they reinforce each other.

8 Additional model predictions

In this section, we investigate how activity after a mask mandate differs with different parameters. These comparative statics vary the information effect and produce a series of testable implications. We then test these implications using differences across counties in their mask compliance, political affiliation, case counts, and urbanization.

8.1 Comparative Statics with respect to mask effectiveness, prior beliefs, and population

We consider differences in the tradeoff between the direct and information effect by considering differences with respect to the effectiveness of masks at reducing the externality θ, the prior beliefs α, and the strength of the externality given by the population in a county n_j.

Activity increases with the effectiveness of mask mandates $\partial a_i/\theta$ and decreases with prior beliefs $\partial a_i/\partial\alpha < 0$ and population $\partial a_i/\partial n_j < 0$, as seen in equation (2). Activity decreases with prior beliefs because the perceived risk is higher, and it decreases with population because the actual risk is higher (due to increased transmission). Activity increases with the effectiveness of mask mandates (if there is a mask mandate) because the actual risk decreases.

The magnitudes of these effects differ on whether there is a mask mandate and at what level of government. We summarize these points in Proposition 3.

Proposition 3 (Mandate effects with different risks)**.** *Activity increases faster with respect to the effectiveness of mask mandates (θ) with both a state and county mandate (1 and 2 below). Activity decreases faster with respect to prior beliefs (α) if there is a mask mandate at the state or county level (3 and 4 below). Finally, activity decreases faster with respect to population (n) with a county mandate (5 below).*

1. $\frac{\partial a_i}{\partial \theta}(\Gamma = 1) > \frac{\partial a_i}{\partial \theta}(\Gamma = 0)$
2. $\frac{\partial a_i}{\partial \theta}(\gamma_j = 1) > \frac{\partial a_i}{\partial \theta}(\gamma_j = 0)$
3. $\frac{\partial a_i}{\partial \alpha}(\Gamma = 1) < \frac{\partial a_i}{\partial \alpha}(\Gamma = 0)$
4. $\frac{\partial a_i}{\partial \alpha}(\gamma_j = 1) < \frac{\partial a_i}{\partial \alpha}(\gamma_j = 0)$
5. $\frac{\partial a_i}{\partial n_j}(\gamma_j = 1) < \frac{\partial a_i}{\partial n_j}(\gamma_j = 0)$

Proof. See Appendix A.3. □

The rate of change in activity with respect to the effectiveness of masks, θ, is greater with a mask mandate than without one. Intuitively, the effectiveness of masks does not impact activity if a mask mandate is not implemented, and it positively affects activity if there is a mask mandate. Therefore, the rate of change with respect to the effectiveness of mask mandates is greater with a mask mandate than without.

The rate of change in activity with respect to prior beliefs α is more negative with a mask mandate because the prior beliefs increase in the information effect more with a mask mandate than without. Specifically, the rate of change of expected cases with respect to prior beliefs is larger with a state or county mandate than without a mandate. Said differently, increasing the information effect (in this case through higher prior beliefs) decreases activity.

Finally, the rate of change in activity with respect to population n_j is more negative with a county mandate. Note that the prediction after a state mandate is ambiguous, which is why it is not stated in Proposition 3. After a county mandate, people's expectations of risk increase, amplifying the transmission effect of the greater population, causing this comparative static.

These comparative statics not only highlight the importance of the information effect, but they are also empirically testable. We also consider additional comparative statics that compares the rate of change of activity between state and county mask mandates in Appendix Appendix A.4. We also test whether activity increases more in counties with a small percent of their state's population after a state mandate because these counties should have the smallest information effect. Table A.3 provides evidence that these counties do experience the largest increase in activity, consistent with the information effect. The rest of this section provides empirical evidence consistent with these additional model implications.

8.2 Empirical model of comparative statics

We use differences across county characteristics to test the additional implications of the model. The characteristics we use (and are available) are not perfect measures of mask effectiveness, prior beliefs, and population because they encompass more than just these factors. Specifically, for mask effectiveness, we use survey evidence of mask compliance from the *New York Times* and political affiliation. We use the level of COVID-19 case rates to proxy for prior beliefs. Finally, for the population, we use whether a county is urban or rural. Despite these limitations, heterogeneity across counties based on these characteristics provides additional tests of the model.

Our empirical design follows Proposition 3. Specifically, we use activity as the dependent variable. We then include indicator variables for whether a county is under a mask mandate, the type of county (described above), and the interaction of the two. We run this specification separately in two subsets: counties with state enacted mandates and counties with county enacted mandates. We also include week fixed effects and county fixed effects in the OLS specification,

$$\begin{aligned}\text{Activity}_{j,t} = \beta_0 + \beta_1 \mathbb{1}(\text{mask mandate}) + \beta_2 \mathbb{1}(\text{mask mandate})_{j,t} \times \mathbb{1}(\text{county type})_j \\ + \sum_k \delta_k x_{k,j,t} + \sum_w \delta_w \lambda_w + \sum_j \delta_j \lambda_j + \varepsilon_{j,t},\end{aligned} \tag{12}$$

where j indicates county, t indicates day, and λ_w indicates week fixed effects. The coefficient of interest is β_2 on the interaction term. We also run the model with state and county mask mandates interacted with all county types k,

$$\begin{aligned}\text{Activity}_{j,t} = \beta_0 + \beta_1 \mathbb{1}(\text{state mask}) + \sum_k \beta_{2,k} \mathbb{1}(\text{state mask})_{j,t} \times \mathbb{1}(\text{county type})_{j,k} \\ + \beta_3 \mathbb{1}(\text{county mask}) + \sum_k \beta_{4,k} \mathbb{1}(\text{county mask})_{j,t} \times \mathbb{1}(\text{county type})_{j,k} \\ + \sum_k \delta_k x_{k,j,t} + \sum_t \delta_t \lambda_t + \sum_j \delta_j \lambda_j + \varepsilon_{j,t}.\end{aligned} \tag{13}$$

The coefficients of interest are $\beta_{2,k}$ and $\beta_{4,k}$, which are vectors of size $k = 4$, the number of county types, which include mask compliance, political affiliation, high/low case count, and urban/rural.

8.3 Comparative statics evidence

We report estimates from the specifications in equations (12) and (13) in Table 6. All specifications include expanded controls, county fixed effects, week fixed effects, and standard errors clustered at the county level. Overall, we find supportive evidence for Proposition 3.

We find that activity is greater in counties with higher mask effectiveness after a mask mandate is enacted. This finding is consistent with Proposition 3, parts 1 and 2. Specifically, we find that counties that report high compliance with wearing a mask, on a *New York Times* survey, have higher activity after a mask mandate than counties that report low compliance. This finding is true whether a state or county enacts the mandate, reported in columns (1) and (2), respectively. Similarly, we find that counties with a higher vote share for Democrats had higher activity after a mask mandate than counties with higher vote shares for Republicans, reported in columns (3) and (4). These estimates, however, are not precisely estimated.

We find that activity is lower in counties with higher COVID-19 risk beliefs after a mask mandate is enacted. These findings are consistent with Proposition 3, parts 3 and 4. Specifically, we find that counties with higher COVID-19 case rates have lower activity after a mask mandate than counties with lower case rates. This finding is true whether a state or county enacts the mandate, reported in columns (5) and (6), respectively. This evidence suggests that while activity is lower in counties with higher COVID-19 cases, mask mandates are also less effective at increasing activity in these counties.

Finally, we find that activity is lower in urban counties after a county enacts a mask mandate, consistent with part 5 of Proposition 3. In column (7), we report that mask mandates increase activity less in urban counties than rural counties.

We report estimates from a joint estimation in column (8) of Table 6. The first row corresponds to the first column and so forth. The joint analysis includes the interaction between urban and state enacted mask mandates, but is not reported due to the ambiguity of the theoretical prediction. The estimates in the joint model are generally similar to those in the subset analysis. For example, the estimate for changes in activity in counties with high mask compliance after a state enacted mask mandate is 1.365 in the subset analysis (column 1) and 1.364 in the joint analysis (row one, column 8).

8.4 Comparative statics survey evidence

In this section, we investigate whether survey evidence from household and business surveys supports the additional predictions of the model.

In the household survey, we test whether there are differences in the activity of households based on mask effectiveness θ, risk beliefs α, and population n. We proxy for mask effectiveness using household responses to how effective masks mandates are (column 1) and political affiliation based on vote shares in their county in 2016 (column 2). Additionally, we proxy for risk beliefs using households' optimism or pessimism for the future.[14] Finally, we proxy for population-based on whether their county population density is above or below 200 per square mile. Proposition 3 suggests that households should be more responsive with higher mask effectiveness and less responsive if they are more pessimistic. We provide estimates based on population, though the prediction is ambiguous based on Proposition 3.

The household survey evidence supports the predictions of Proposition 3. Across four different questions given in the rows of Panel A of Table 7, households report being more responsive for higher values of mask effectiveness and less responsive if they are more pessimistic. Specifically, the activity questions asked households how much more likely they would be to go out to a store if (1) the number of confirmed cases fell by 10%, (2) the number of confirmed cases fell by 90%, (3) everyone was wearing a mask, and (4) the state-enforced a mask mandate (rows in Panel A of Table 7). The estimates are relatively precisely estimated in column (1) and less so in columns (2) and (3). Overall, the sign of these estimates is consistent with the predictions of Proposition 3.

[14] Specifically, we designate someone has optimistic or pessimistic based on whether they consider that one month after the survey they expect the situation in Utah to lead to good or bad times, a question we used based on the design of the Michigan Survey of Consumers.

We also test the additional predictions in Proposition 3 using evidence from the business survey. We again test for differences across mask effectiveness, risk beliefs, and population, using the same proxies as before[15]. The business survey asks about businesses' expected changes in revenues in different scenarios related to cases and mask-wearing and mandates. Specifically, these questions asked businesses how their revenues would change if (1) new COVID-19 cases increased by 20%, (2) there were no new COVID-19 cases, (3) everyone was wearing a mask, and (4) the state-enforced a mask mandate, and (5) your county enforced a mask mandate (rows in Panel B of Table 7).

Proposition 3 suggests revenues will decrease in row 1 when cases increase and increase in rows 2–5 more for businesses in counties with higher mask effectiveness, reported in columns (1) and (2). Similarly, Proposition 3 suggests revenues will increase in row 1 and decrease in rows 2–5 more for businesses in counties with higher risk perceptions, reported in column (3). Finally, the predictions are ambiguous for population, but we report them in column (4).

The business survey evidence supports the predictions of Proposition 3. Across five different questions given in the rows of Panel B of Table 7, businesses report their revenues will change more for changes in mask effectiveness and will be less responsive in case of pessimism. The estimates are relatively precisely estimated in columns (1)–(3).

The survey evidence suggests that households and businesses believe their activity and, as a result, revenues are responsive to COVID-19 conditions and mask compliance and mandates. In addition, differences in reported changes in activity and revenues across counties are consistent with our additional predictions from the model. Taken together, this evidence suggests the information mechanism in the model is plausible and helps explain, at least partially, the differences in activity we observe after state and county mandates.

[15] Both surveys have slightly different questions for beliefs, Appendix B provides more information on the questions asked.

9 Discussion: Information Effects of Mask Mandates: Lessons for Public Policy

Government policies result in a variety of intended and unintended effects. We have shown how information revealed by government policies can have countervailing effects to their intended consequences. In the context of mask mandates, unintended information effects can attenuate or reverse the sign of the intended effect. In this section, we expand our discussion to the consequences of unintended-information effects for the evaluation of public policy.

Consider an empirical study that analyzes the effect of policy Γ on behavior or outcome Y, which is also influenced by unobserved beliefs of economic agents B:

$$Y = \beta_1 \cdot \Gamma + \beta_2 \cdot B. \tag{14}$$

If policy Γ affects beliefs B, then $Cov(\Gamma, B) \neq 0$ so that any policy evaluation that ignores the impact of the policy on beliefs results in classical omitted variables bias:

$$\hat{\beta}_1 = \beta_1 + \delta \cdot \frac{Cov(\Gamma, B)}{Var[\Gamma]}. \tag{15}$$

For concreteness, consider the case in this paper—mask mandates. The first effect is that mask mandates increase activity by making activity safer ($\beta_1 > 0$), thereby boosting consumer confidence. However, implementing a mask mandate can also signal risk perception by government officials. This signal affects people's prior beliefs of the true infection rates ($Cov(\Gamma, B) \neq 0$), which will decrease activity for higher infection rates ($\beta_2 < 0$).

Why does this omitted-variables bias matter if the same government policy drives the net effect? It matters because the relative importance of both effects can sometimes be understood as a consequence of which level of government deploys the policy. In our case, a centralized policy is unlikely to reveal higher risk assessments for specific counties, thereby implying $Cov(\Gamma, B) = 0$ and leading to overall economic effects being dominated by $\beta_1 > 0$. On the other hand, decentralized county-level mask mandates are very likely to reveal information about local risks, so that $Cov(\Gamma, B) > 0$.

10 Conclusion

This paper highlights the importance of information-revelation effects of public policy and its impact on whether or not a policy should be decentralized. This information channel has broad implications for devolution and immediate impact on policy decisions surrounding COVID-19.

We document that state mask mandates during the 2020 COVID-19 pandemic reduced case growth and promoted economic activity, measured by activity and credit card spending. This evidence highlights that slowing the growth of COVID-19 cases and fostering economic activity can be complements—not substitutes. Put differently, state mask mandates expand the policy frontier and show there is no conflict between public health and economic recovery.

Unfortunately, not all mask mandates are created equal. We also document that county mask mandates during the COVID-19 pandemic depressed economic activity relative to state mandates. The stark contrast between the effects of state and county mask mandates should be a cautionary tale for policymakers.

Mask mandates enforced at the state level increased consumer confidence and led to increased economic activity. Evidence from our surveys bolsters this claim. People report that they are nearly 50% more likely to go out to a store if their state enforces a mask mandate. In terms of the effectiveness of mask mandates, this paper adds to a growing consensus that increased mask-wearing slows the growth of new cases and is a critical tool in the public health arsenal.

Our findings provide a new consideration in determining what level of government is most effective at implementing different policies. An information treatment accompanies many, if not most, policy changes. For example, when the SEC changes firms' regulations, it sends these firms a signal about the relative importance of different activities. Similarly, when Congress passes a stimulus package, it signals the risk and severity of a recession.

In our context, an understanding of the information effects explains why mask mandates at state and county levels had starkly different effects. These insights, therefore, highlight how public policy can most effectively deploy mask mandates to save lives and, at the same time, stimulate the economy.

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Table 1: Summary Statistics

	Mandate							
	State				County			
	Mean	Median	Std Dev.	N	Mean	Median	Std Dev.	N
Activity	-14.18	-12.25	16.77	39828	-16.19	-13.67	17.66	4975
Spend/month	1321.92	832.99	1985.47	43275	1137.86	883.27	1038.87	5060
Cases/pop	12.05	4.25	26.03	44270	25.07	16.47	30.81	5085
Comply county	0.04	0	0.19	44578	0.03	0	0.16	5232
Blue county	0.18	0	0.38	44578	0.45	0	0.50	5232
High county	0.53	1	0.50	44578	0.77	1	0.42	5232
Urban county	0.23	0	0.42	44578	0.62	1	0.48	5232
Early county	0.63	1	0.48	44578	0.55	1	0.50	5232

NOTE.— This table presents summary statistics for all variables used in the analysis. We consider values from day 25 days before the mask mandate. First four columns for counties where the mandate was enacted by the state, while last four columns where the mandate was enacted by the county. *Activity* is relative to activity in 2019 in percent, using Google's cell phone data. *Cases/pop* is the new confirmed cases in a day per 100,000 based on county-level population from the Census Bureau. *Spend/month* is the average county credit card spending per person per month, scaled to the monthly level assuming one daily transaction per person on average. *Blue county* is an indicator equal to one if the Democratic party got more votes than the Republican party on the 2016 Presidential Election, and zero otherwise. *Urban county* is an indicator equal to one if urban areas include more than 50% of the county population, and zero otherwise. *High county* is an indicator equal to one if the number of cases on event-day 0 are above the median value of event-day 0 values, and zero otherwise. *Early county* is an indicator equal to one if the mandate was issued on or before June 30th, and zero otherwise. *Comply county* is an indicator equal to one if at 70% of people surveyed stated that they wore a mask always or frequently, and zero otherwise.

Table 2: Activity by state and county mask mandates

	(1)	(2)	(3)	(4)	(5)
State mandate	5.107***	8.779***	9.258***	2.927***	1.579***
	(1.170)	(0.940)	(0.926)	(0.397)	(0.282)
Mask mandate	-3.269***	-7.481***	-8.009***	-2.198***	-1.422***
	(1.057)	(0.858)	(0.846)	(0.383)	(0.262)
Constant	-14.428***	-6.924***	-7.823***	-3.279***	-10.649***
	(0.356)	(0.368)	(0.751)	(1.088)	(0.402)
Basic controls		✓	✓	✓	✓
Expanded controls			✓	✓	✓
County fixed effects				✓	✓
Week fixed effects					✓
Adj. R-Square	0.006	0.267	0.296	0.841	0.880
Observations	93,173	93,173	93,173	93,163	93,147
Wald P-Values:					
H_0 : State + Mask =0	0.001	0.001	0.001	0.001	0.062

NOTE.— This table reports changes in activity as a result from mask mandates and state mandates. We consider a time period from day -25 to day 25. Basic controls includes indicator variables for whether retail, restaurants, and bars are restricted. Expanded controls adds restrictions to stay at home, school closings, and restrictions on gatherings of 10 or more people, 50 or more people, or thresholds more than 50 people. *Activity* is relative to activity in 2019 in percent, using Google's cell phone data. *State mandate* is an indicator equal to one if an order mandated at the state level is in place, and zero otherwise. *Mask mandate* is an indicator equal to one if an mask order is in place, and zero otherwise. Standard errors clustered at the county level are in parentheses. We denote statistical significance by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Survey Evidence

Panel A: Household Survey	
How much more or less likely (as a percent) would you be to go out to a store if:	
The number of confirmed cases fell by 10%	11.58%
	(0.88)
The number of confirmed cases fell by 90%	57.11%
	(1.13)
Everyone was wearing a mask	49.27%
	(1.19)
The state enforced wearing a mask	47.86%
	(1.41)
Observations	1,121
Panel B: Business Survey	
How would your business's monthly revenues change in the following scenarios as a percent if:	
New COVID-19 cases increased by 20%	-28.82%
	(0.39)
No new COVID-19 cases	26.70%
	(0.47)
Everyone wore a mask	13.08%
	(0.52)
Your state enforced a mask mandate	9.58%
	(0.63)
Your county enforced a mask mandate	8.42%
	(0.60)
Observations	5,009

NOTE.— Panel A shows survey evidence from a representative sample of Utah households from October 2020 to January 2021. Panel B shows estimates from a survey of the universe of Utah businesses. Standard errors in parentheses. Observations represent the maximum number of observations in a question.

Table 4: Sensitivity Analysis Using Regression Discontinuity Estimation in Event Time

	State			County		
	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.784***	0.693**	1.071***	-0.705**	-0.594	-0.989
	(0.097)	(0.272)	(0.312)	(0.296)	(0.768)	(0.870)
Flexible time trends	✓			✓		
RD Linear		✓			✓	
RD Cubic			✓			✓
Optimal BW	20	8	13	14	15	16
Observations	66,238	165,846	165,846	5,784	19,827	19,827

NOTE.— This table reports estimates from various models to control for time varying confounding factors. Columns (1) and (4) use flexible time trends. Columns (2), (3), (5), and (6) use parametric regression discontinuity designs, either linear or cubic. The first three columns report estimates for state mandates. The last three columns report estimates for county mandates. The dependent variable is the residual of activities after accounting for county fixed effects. Standard errors are clustered by county and reported in parentheses. We denote statistical significance by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Medium-run activity, Spending, and Cases

	Medium run		Spend/month		Cases/pop	
	(1)	(2)	(3)	(4)	(5)	(6)
State mandate	1.374***	1.070***	32.396**	45.494***	-3.502***	-3.199***
	(0.338)	(0.337)	(14.482)	(16.085)	(1.180)	(1.072)
Mask mandate	-0.361	0.013	-93.575***	-101.711***	3.473***	3.370***
	(0.351)	(0.346)	(15.129)	(16.289)	(1.131)	(1.055)
Constant	-2.611***	-2.173***	1240.184***	1130.981***	13.166***	29.163***
	(0.105)	(0.196)	(4.608)	(41.438)	(0.190)	(6.583)
Expanded controls		✓		✓		✓
County fixed effects	✓	✓	✓	✓	✓	✓
Week fixed effects	✓	✓	✓	✓	✓	✓
Adj. R-Square	0.025	0.060	0.882	0.883	0.323	0.324
Observations	161,355	161,355	35,579	35,579	22,440	22,440

NOTE.— Medium run represents activity relative to 2019 using cell phone data in percent. *Spend/month* is the average county credit card spending per person per month, scaled to the monthly level assuming one daily transaction per person on average. *Cases/pop* is the new confirmed cases in a day per 100,000 based on county-level population from the Census Bureau. For medium run we consider a time period from day -25 to day 75, whereas spending and cases are considered from day -25 to day 25. *State mandate* is an indicator equal to one if an order mandated at the state level is in place, and zero otherwise. *Mask mandate* is an indicator equal to one if an mask order is in place, and zero otherwise. Expanded controls includes indicator variables for whether retail, restaurants, and bars are restricted and also restrictions to stay at home, school closings, and restrictions on gatherings of 10 or more people, 50 or more people, or thresholds more than 50 people. Standard errors are clustered by county and reported in parentheses. We denote statistical significance by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Changes in activity by County Heterogeneity

	Mask effectiveness θ				Beliefs α		n	
	Comply		Blue		High Rate		Urban	
	State (1)	County (2)	State (3)	County (4)	State (5)	County (6)	County (7)	Joint (8)
Mask Mandate × County Type	1.365*	5.080***	0.080	1.009*	-0.885***	-1.401**	-1.215*	
	(0.744)	(1.681)	(0.257)	(0.546)	(0.190)	(0.655)	(0.655)	
State mask × Comply								1.364*
								(0.726)
County mask × Comply								4.134**
								(1.866)
State mask × Blue								0.245
								(0.266)
County mask × Blue								0.204
								(0.572)
State mask × High Rate								-0.835***
								(0.191)
County mask × High Rate								-1.109
								(0.677)
County mask × Urban								-1.005
								(0.657)
Expanded controls	✓	✓	✓	✓	✓	✓	✓	✓
County fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Week fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Adj. R-Square	0.877	0.904	0.877	0.903	0.877	0.903	0.903	0.880
Observations	82,887	10,276	82,887	10,276	82,887	10,276	10,276	93,163

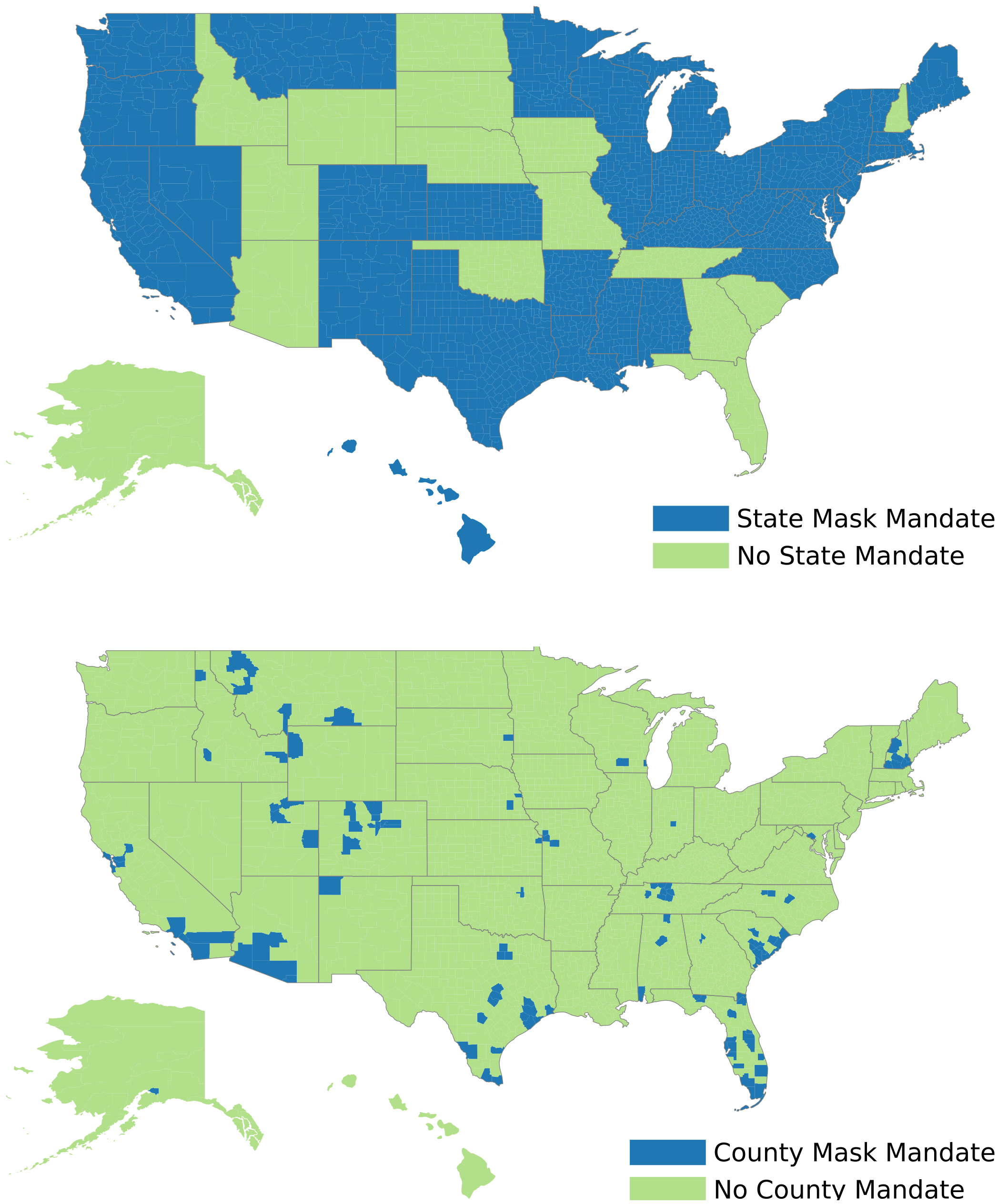
NOTE.— *Mask mandate* is an indicator equal to one if an mask order is in place, and zero otherwise. *State mask* is an indicator equal to one if an order mandated at the state level is in place, and zero otherwise. *County mask* is an indicator equal to one if an order mandated at the county level is in place, and zero otherwise. *Comply county* is an indicator equal to one if at 70% of people surveyed stated that they wore a mask always or frequently, and zero otherwise. *Blue county* is an indicator equal to one if the Democratic party got more votes than the Republican party on the 2016 Presidential Election, and zero otherwise. *High county* is an indicator equal to one if the number of cases on event-day 0 are above the median value of event-day 0 values, and zero otherwise. *Urban county* is an indicator equal to one if urban areas include more than 50% of the county population, and zero otherwise. Expanded controls includes indicator variables for whether retail, restaurants, and bars are restricted and also restrictions to stay at home, school closings, and restrictions on gatherings of 10 or more people, 50 or more people, or thresholds more than 50 people. Standard errors are clustered by county and reported in parentheses. We denote statistical significance by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Survey Evidence

Panel A: Household Survey				
How much more or less likely (as a percent) would you be to go out to a store if:				
	Mask effectiveness θ		Beliefs α	n
	Mandate	Blue	Perception	Population
	(1)	(2)	(3)	(4)
Number of confirmed cases fell by 10%	10.652***	-0.972	-8.250***	4.325
	(2.057)	(1.837)	(1.783)	(2.752)
Number of confirmed cases fell by 90%	29.109***	0.102	-1.186	2.086
	(2.541)	(2.354)	(2.327)	(3.539)
Everyone was wearing a mask	60.813***	4.733*	-1.975	4.864
	(1.946)	(2.490)	(2.466)	(3.807)
The state enforced a mask mandate	88.998***	10.184***	-0.762	13.869***
	(1.373)	(2.940)	(2.923)	(4.474)
Observations	873	1,020	1,065	1,020
Panel B: Business Survey				
How would your business's monthly revenues change in the following scenarios as a percent if:				
	Mask effectiveness θ		Beliefs α	n
	Mandate	Blue	Perception	Population
	(1)	(2)	(3)	(4)
New COVID-19 cases increased by 20%	1.667	-3.721***	-10.554***	-0.957
	(1.213)	(0.824)	(0.832)	(1.023)
No new COVID-19 cases	5.433***	3.983***	3.240***	2.416*
	(1.531)	(1.089)	(1.025)	(1.367)
Everyone wore a mask	42.181***	8.088***	-3.142***	6.756***
	(1.354)	(1.215)	(1.125)	(1.527)
Your state enforced a mask mandate	58.256***	11.879***	-6.293***	8.655***
	(1.146)	(1.454)	(1.356)	(1.808)
Your county enforced a mask mandate	54.136***	11.333***	-6.398***	8.483***
	(1.148)	(1.378)	(1.270)	(1.705)
Observations	2,733	3,945	4,799	3,945

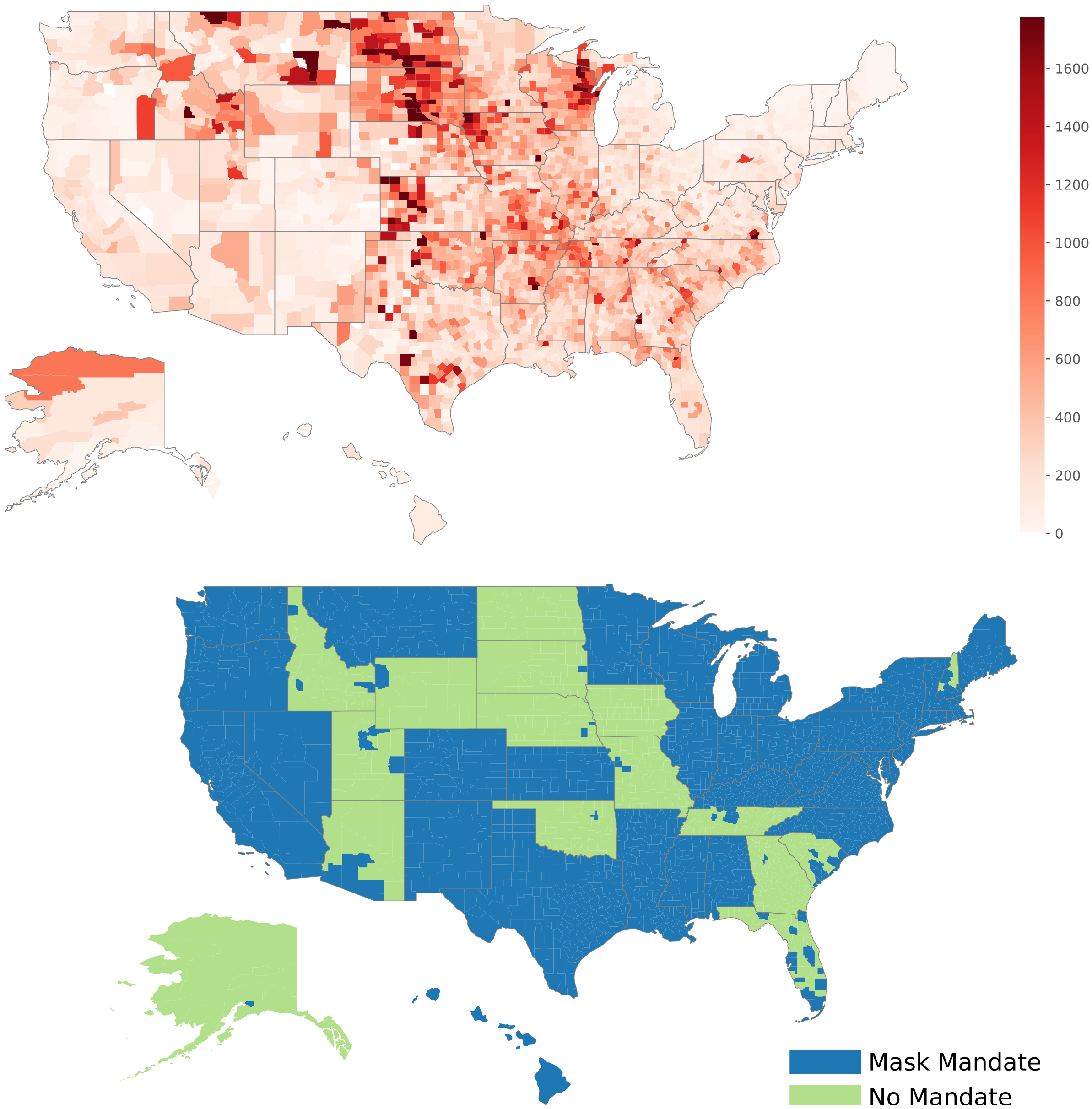
NOTE.— Panel A shows estimates from a survey of a representative sample of Utah households, while Panel B shows estimates from a survey of the universe of Utah businesses. For Panel A *Mandate* is an indicator equal to one if households would expect their activity by 60% or more if a state-level mask mandate where implemented and zero if it is by 40% or less, whereas for Panel B equals one if the business' owner thinks that a state-level mask mandate would increase their revenue and zero otherwise. *Blue* is an indicator equal to one if the Democratic party got more votes than the Republican party on the 2016 Presidential Election, and zero otherwise. For Panel A *Perception* is an indicator equal to one if they think that the business situation will be bad during the next 12 months, and zero otherwise, whereas in Panel B equals one if they think is going to be worse over the next six months and zero if it is going to be better. *Population* is an indicator equal to one if the county population density is higher than 200hab/sqmi, and zero otherwise. Standard errors in parentheses. Observations represent the maximum number of observations in a question.

Figure 2: Mandatory Mask Mandates Map



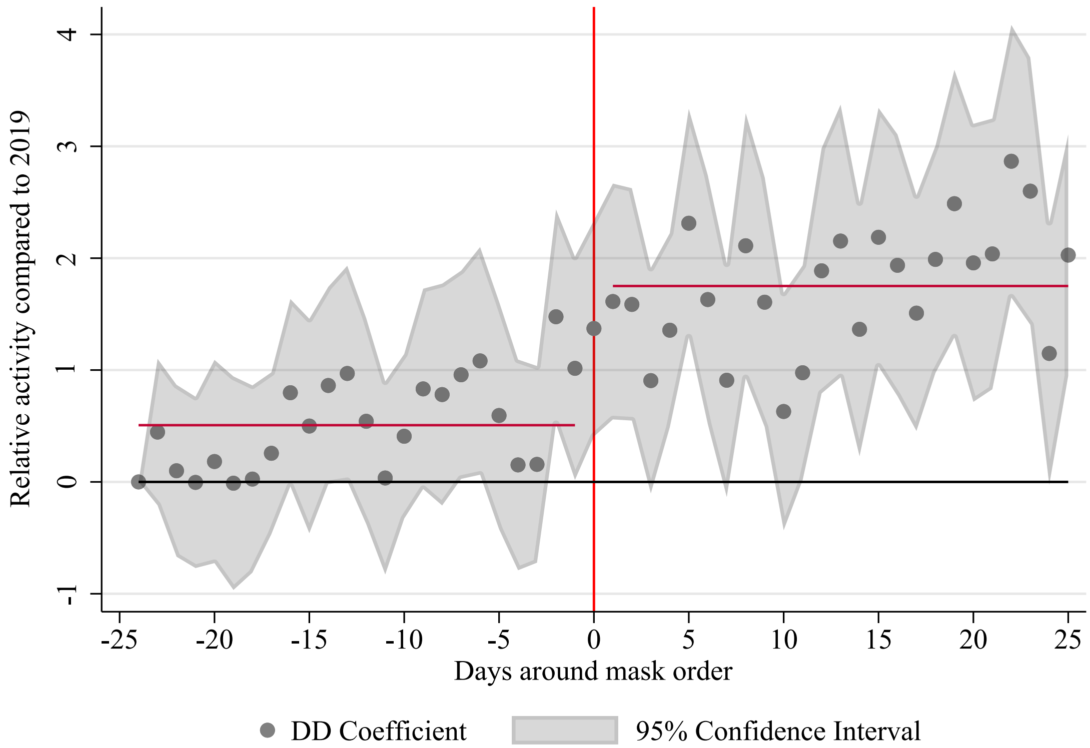
NOTE.— Figure 2 shows maps of the county-level mandate counties, and the state-level mandate states. Many of the county-level mandates come from states which eventually also had a state level mandate.

Figure 3: Covid-19 Case Rates in August



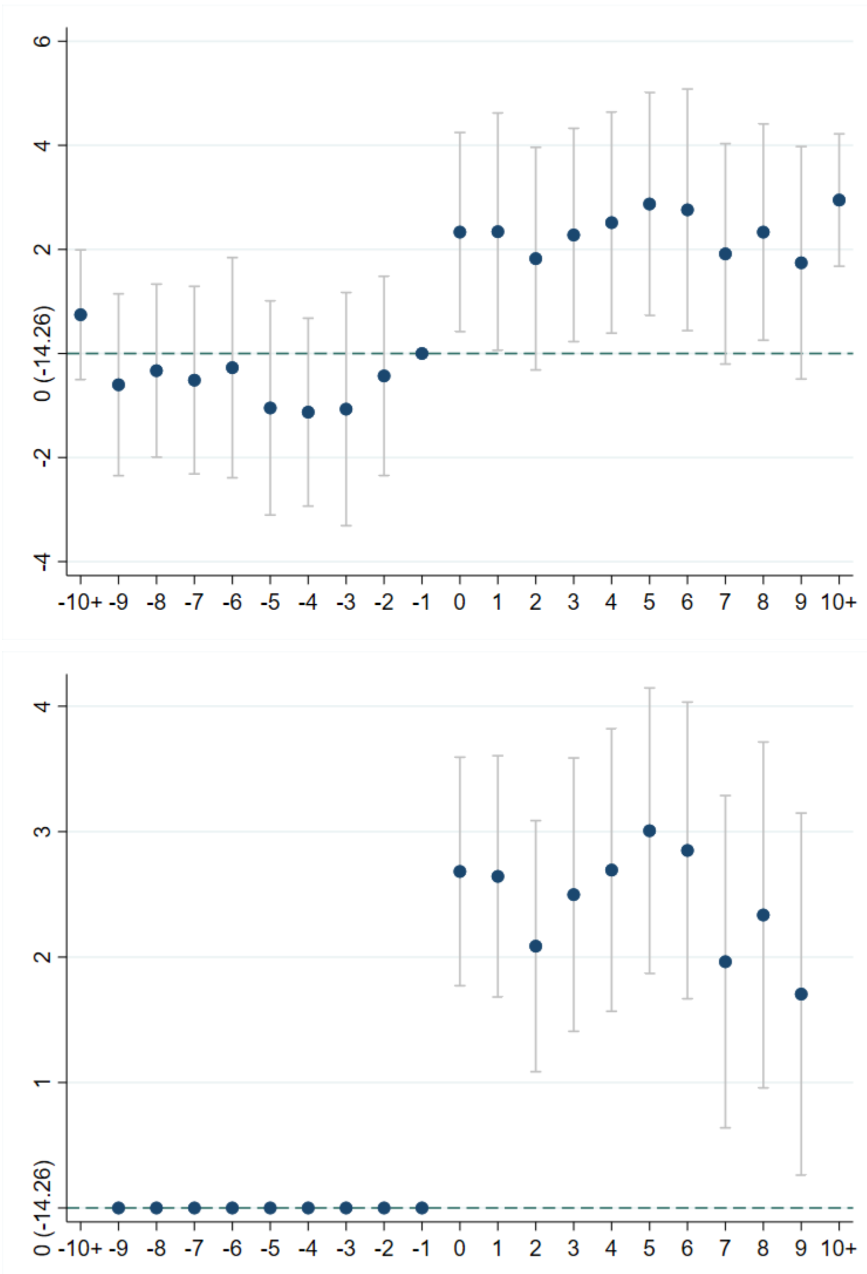
NOTE.— Figure 3 shows a map of the county-level active coronavirus cases per 100,000 people as of August 1st, and under that for comparison, the map of counties and states with *any* mandate in place (which is the combination of the State and County maps from Figure 2). Coronavirus case rate data come from the New York Times, and population statistics come from the 2019 Census estimate of county level residence.

Figure 4: Activity After a State Mandate



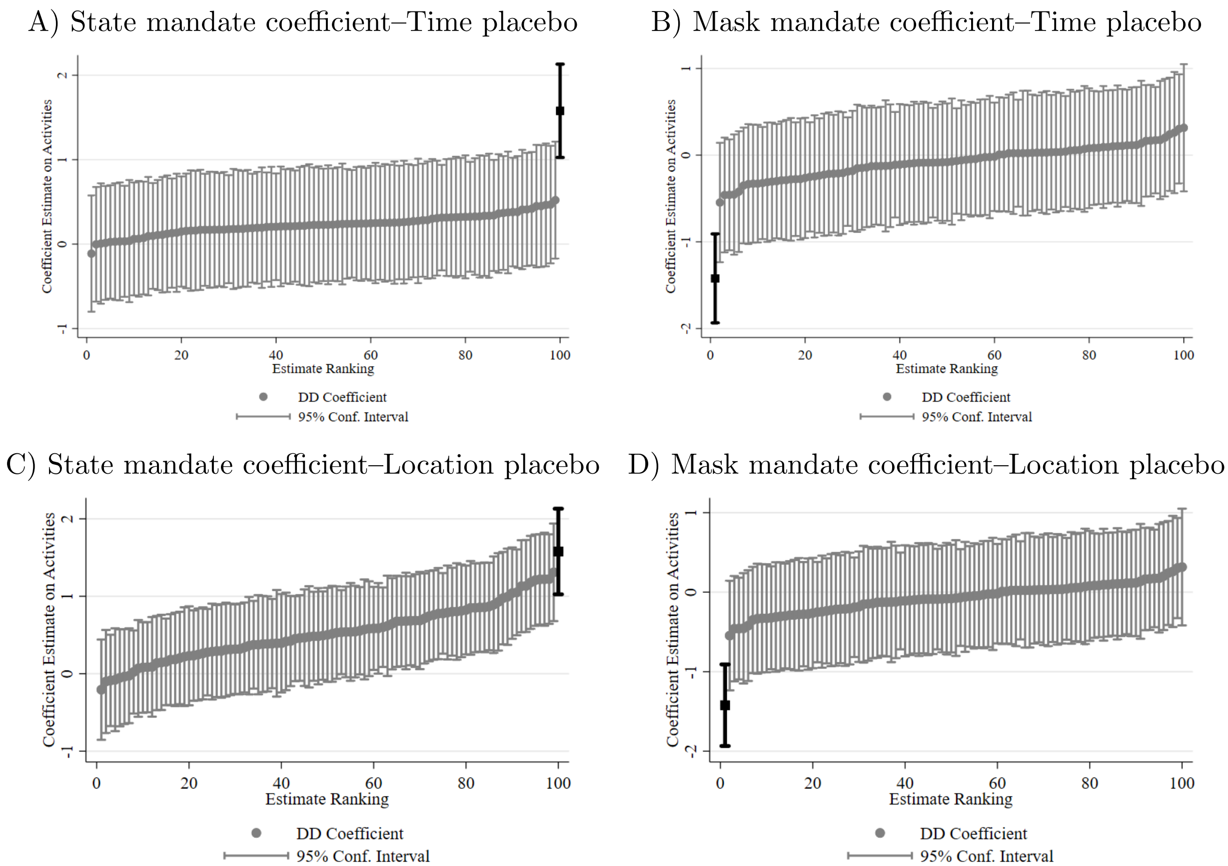
NOTE.— Activity compared to 2019 using cell phone data around a state-level mask mandate, relative to a county-level mask mandate.

Figure 5: Activity After a State Mandate Event Study



NOTE.— This figure estimates the effect of activity after a state mask mandate, relative to a county mask mandate, using a panel event study design following Freyaldenhoven et al. (2019). Panel A uses county and date fixed effects. Panel B allows for a pre-trend as in Dobkin, Finkelstein, Kluender and Notowidigdo (2018).

Figure 6: Time and location placebo tests



NOTE.— Figure 6 shows 100 coefficient estimates, 99 from a placebo test and 1 from our baseline estimate described in sections 6.2 and 6.3. Each placebo estimate represents a different random draw of event times.

Appendix A Model derivations

Appendix A.1 Utility

The utility function combines activity and health. For simplicity, we model health as a function of net activity, $1 - a_i$. We also model different risks associated with net activity. Specifically, the risk of higher activity is greater with a) the aggregate of activity in your county A_j and the case rate in your county c_j and decreases with mask mandate Γ and γ_j and its effectiveness θ.

$$U(a_i|c_j, A_j, \Gamma, \gamma_j) = a_i + \beta\frac{A_j E[c_j]}{1+\theta(\Gamma+\gamma_j)} log(1-a_i). \tag{16}$$

We derive the equilibrium level of activity by solving the first-order condition and replacing aggregate activity with its equilibrium level

$$\begin{aligned}
\frac{\partial U}{\partial a_i} : 1 - \beta\frac{A_j c_j}{1+\theta(\Gamma+\gamma_j)}\frac{1}{1-a_i} &= 0 \\
\beta\frac{A_j E[c_j]}{1+\theta(\Gamma+\gamma_j)} &= 1 - a_i \\
a_i &= 1 - \beta\frac{E[c_j]}{1+\theta(\Gamma+\gamma_j)} A_j
\end{aligned}$$

The first-order condition provides the best response given aggregate activity A_j. We use the following identity to solve for aggregate activity to substitute into the best response,

$$\begin{aligned}
A_j \equiv \sum_{i\in j} a_i &= n_j - n_j\beta\frac{E[c_j]}{1+\theta(\Gamma+\gamma_j)} A_i \\
A_j\left(1 + n_j\beta\frac{E[c_j]}{1+\theta(\Gamma+\gamma_j)}\right) &= n_j \\
A_j &= \frac{n_j}{1+n_j\beta\frac{E[c_j]}{1+\theta(\Gamma+\gamma_j)}}.
\end{aligned}$$

The equilibrium level of activity is a function of the population in the county n_j, the expected case rate $E[c_j]$, the effectiveness of mask requirements, θ, and whether a mask mandate is imposed by the state or county Γ and γ_j,

$$
\begin{aligned}
a_i &= 1 - \beta \frac{E[c_j]}{1+\theta(\Gamma+\gamma_j)} A_i \\
&= 1 - \beta \frac{E[c_j]}{1+\theta(\Gamma+\gamma_j)} \frac{n_j}{1 + n_j \beta \frac{E[c_j]}{1+\theta(\Gamma+\gamma_j)}} \\
&= 1 - \frac{n_j \beta E[c_j]}{1+\theta(\Gamma+\gamma_j) + n_j \beta E[c_j]} \\
&= \frac{1+\theta(\Gamma+\gamma_j) + n_j \beta E[c_j] - n_j \beta E[c_j]}{1+\theta(\Gamma+\gamma_j) + n_j \beta E[c_j]} \\
&= \frac{1+\theta(\Gamma+\gamma_j)}{1+\theta(\Gamma+\gamma_j) + n_j \beta E[c_j]} \\
&= \frac{1}{1 + \beta \frac{n_j E[c_j]}{1+\theta(\Gamma+\gamma_j)}}.
\end{aligned}
$$

Appendix A.2 Bayes' Rule

We use Bayes' rule to update beliefs on the county case rate given the state enacts a mask mandate. Specifically, the expected county case rate in this case is

$$
E[E[c_j]|\Gamma=1] = P(E[c_j] > r|\Gamma=1)E[E[c_j]|E[c_j] > r] + (1 - P(E[c_j] > r|\Gamma=1))E[E[c_j]|E[c_j] < r]. \tag{17}
$$

According to Bayes' Rule The probability that a county's case rate is greater than the threshold r given that the state enacts a mask mandate is

$$
P(E[c_j] > r|\Gamma=1) = \frac{P(\Gamma=1|E[c_j] > r)P(E[c_j] > r)}{P(\Gamma=1)}.
$$

Given the state rule for implementing a mask mandate, $P(\Gamma = 1|E[c_j] > r) = 1$. The unconditional probability that a counties case rate is above the threshold is $P(E[c_j] > r) = (2\alpha - r)/\alpha$. The state implements a mask mandate in two scenarios; the case rate is above the threshold in one of the counties and not the other and the case rate is above the threshold in both counties. We assume a common prior such that

$$
\begin{aligned}
P(\Gamma=1) &= 2\frac{2\alpha - r}{\alpha}\frac{r-\alpha}{\alpha} + \left(\frac{2\alpha - r}{a}\right)^2 \\
&= \frac{2\alpha - r}{\alpha}\frac{r}{\alpha}
\end{aligned}
$$

The conditional probability, using these expressions, is given by

$$
\begin{aligned}
P(E[c_j] > r|\Gamma = 1) &= \frac{P(\Gamma = 1|E[c_j] > r)P(E[c_j] > r)}{P(\Gamma = 1)} \\
&= \frac{(2\alpha - r)/\alpha}{\frac{2\alpha - r}{\alpha}\frac{r}{\alpha}} \\
&= \frac{\alpha}{r}
\end{aligned}
$$

We calculate the expected value of the case rate given that the case rate is greater than the threshold r noting that in this case the prior distribution shifts such that $E[c_j]$ is distributed uniformly between $[r, 2\alpha]$. The expected value in this case is $E[E[c_j]|E[c_j] > r] = (2\alpha + r)/2$. Similarly, the expected vale of the case rate given the case rate is less than the threshold r is $E[E[c_j]|E[c_j] > r] = (\alpha + r)/2$.

The expected county case rate, using these expressions, is

$$
\begin{aligned}
E[E[c_j]|\Gamma = 1] &= P(E[c_j] > r|\Gamma = 1)E[E[c_j]|E[c_j] > r] + (1 - P(E[c_j] > r|\Gamma = 1))E[E[c_j]|E[c_j] < r] \\
&= \frac{\alpha}{r}\frac{2\alpha + r}{2} + \left(1 - \frac{\alpha}{r}\right)\frac{\alpha + r}{2} \\
&= \frac{\alpha^2 + r\alpha + r^2}{2r}.
\end{aligned}
$$

Compare the expected county case rate after the state implements a mask mandate and after a county does.

$$
\begin{aligned}
E[E[c_j]|\Gamma = 1] = \frac{\alpha^2 + r\alpha + r^2}{2r} &\lessgtr \frac{2\alpha + r}{2} = E[E[c_j]|\gamma_j = 1] \\
\alpha^2 + r\alpha + r^2 &\lessgtr 2\alpha r + r^2 \\
\alpha &< r,
\end{aligned}
$$

where the last inequality is due to the assumption of the priors α. This comparison demonstrates the expected county case rate is greater if the county, rather than the state, implements the mask mandate.

Appendix A.3 Proposition proofs

Proposition 1 (State and County Mandate Comparison)**.** *Activity is greater with a state mandate than a county mandate.*

Proof. Activity differs after a state or county enacts a mask mandate because individuals update their priors differently. The direct effect that masks have on lowering risk is the same.

The proof consists of comparing activity in the two regimes and determining which is larger. This comparison is aided by remembering that the threshold for a mandate to be enacted is assumed to be within individuals' priors, such that $r > \alpha$. Consider activity in different regimes,

$$\begin{aligned}
a_i(\Gamma=1) = \frac{1}{1+\beta\frac{n_j}{1+\theta}\frac{\alpha^2+r\alpha+r^2}{2r}} &> \frac{1}{1+\beta\frac{n_j}{1+\theta}\frac{2\alpha+r}{2}} = a_i(\gamma_j=1) \\
\frac{2\alpha+r}{2} &> \frac{\alpha^2+r\alpha+r^2}{2r} \\
2\alpha r + r^2 &> \alpha^2+r\alpha+r^2 \\
r &> \alpha \\
a_i(\Gamma=1) &> a_i(\gamma_j=1).
\end{aligned}$$

□

Proposition 2 (State and County Mandates Relative to No Mandate)**.** *Under assumption A, the highest activity occurs after a state mandate, the lowest activity occurs after a county mandate, and activity without a mandate is in between.*

$$a_i(\Gamma=1) > a_i(\Gamma=0) = a_i(\gamma_j=0) > a_i(\gamma_j=1). \tag{18}$$

Proof. This proof compares activity with and without a state and county mandate. These comparisons, with the Assumption A, provides the ordering, noting that activity without a state mandate is the same as activity without a county mandate. In both scenarios, individuals update that the true case rate is below r. Without a state mandate, individuals also know that the other county has a case rate below r, but this does not add any information about the case rate in their county. The last inequality in both cases obtain from Assumption A.

Assumption A bounds the effectiveness of mask orders between the percent increase in expectations of cases from a state and county mask order. Specifically, $\theta \in (\%\Delta E[c_j|\Gamma=1], \%\Delta E[c_j|\gamma_j=1])$.[16]

$$\begin{aligned}
a_i(\Gamma=1) = \frac{1}{1+\beta\frac{n_j}{1+\theta}\frac{\alpha^2+r\alpha+r^2}{2r}} &\lesseqgtr \frac{1}{1+\beta n_j\frac{\alpha+r}{2}} = a_i(\Gamma=0) \\
\frac{\alpha+r}{2}(1+\theta) &\lesseqgtr \frac{\alpha^2+r\alpha+r^2}{2r} \\
(\alpha r+r^2)(1+\theta) &\lesseqgtr \alpha^2+r\alpha+r^2 \\
\theta &> \frac{\alpha^2}{\alpha r+r^2} = \%\Delta E[c_j|\Gamma=1]
\end{aligned}$$

[16]The percent change in expected cases is given by $\%\Delta E[c_j|\Gamma=1] = (E[c_j|\Gamma=1]-E[c_j|\Gamma=0])/E[c_j|\Gamma=0]$ and $\%\Delta E[c_j|\gamma_j=1] = (E[c_j|\gamma_j=1]-E[c_j|\gamma_j=0])/E[c_j|\gamma_j=0]$.

$$
\begin{aligned}
a_i(\gamma_j = 1) = \frac{1}{1 + \beta \frac{n_j}{1+\theta} \frac{2\alpha + r}{2}} &\lesseqgtr \frac{1}{1 + \beta n_j \frac{\alpha + r}{2}} = a_i(\gamma_j = 0) \\
\frac{\alpha + r}{2}(1 + \theta) &\lesseqgtr \frac{2\alpha + r}{2} \\
(\alpha + r)(1 + \theta) &\lesseqgtr 2\alpha + r \\
\theta &< \frac{\alpha}{\alpha + r} = \%\Delta E[c_j | \gamma_j = 1]
\end{aligned}
$$

□

Proposition 3 (Mandate effects with different risks)**.** *Activity increases faster with respect to the effectiveness of mask mandates (θ) with both a state and county mandate (1 and 2 below). Activity decreases faster with respect to prior beliefs (α) if there is mask mandate at the state or county level (3 and 4 below). Finally, activity decreases faster with respect to population (n) with a county mandate (5 below).*

1. $\frac{\partial a_i}{\partial \theta}(\Gamma = 1) > \frac{\partial a_i}{\partial \theta}(\Gamma = 0)$
2. $\frac{\partial a_i}{\partial \theta}(\gamma_j = 1) > \frac{\partial a_i}{\partial \theta}(\gamma_j = 0)$
3. $\frac{\partial a_i}{\partial \alpha}(\Gamma = 1) < \frac{\partial a_i}{\partial \alpha}(\Gamma = 0)$
4. $\frac{\partial a_i}{\partial \alpha}(\gamma_j = 1) < \frac{\partial a_i}{\partial \alpha}(\gamma_j = 0)$
5. $\frac{\partial a_i}{\partial n_j}(\gamma_j = 1) < \frac{\partial a_i}{\partial n_j}(\gamma_j = 0)$

Parts 1 and 2 of proposition 3 characterize the change in activity with respect to the effectiveness of mask mandates θ. Activity increases with the effectiveness of mask mandates because it decreases the risk. Part 5 and 6, then, follows directly from this increase and noting that the derivative when there is no mask mandate is zero.

$$
\begin{aligned}
\frac{\partial log(a_i(\gamma_j = 1))}{\partial \theta} &= \frac{\beta n_j \frac{1}{(1+\theta)^2} \frac{2\alpha + r}{2}}{1 + \beta n_j \frac{1}{1+\theta} \frac{2\alpha + r}{2}} > 0 \\
\frac{\partial log(a_i(\Gamma = 1))}{\partial \theta} &= \frac{\beta n_j \frac{1}{(1+\theta)^2} \frac{\alpha^2 + r\alpha + r^2}{2r}}{1 + \beta n_j \frac{1}{1+\theta} \frac{\alpha^2 + r\alpha + r^2}{2r}} > 0 \\
\frac{\partial log(a_i(\Gamma = 0))}{\partial \theta} &= 0
\end{aligned}
$$

Parts 3 and 4 of proposition 3 considers the rate of change of activity with respect to prior beliefs. Activity decreases with prior belief because people believe the risk is higher.

$$\frac{\partial log(a_i(\gamma_j = 1))}{\partial \alpha} = -\frac{\beta n_j \frac{1}{1+\theta}}{1 + \beta n_j \frac{1}{1+\theta} \frac{2\alpha + r}{2}} < 0$$
$$\frac{\partial log(a_i(\Gamma = 1))}{\partial \alpha} = -\frac{\beta n_j \frac{1}{1+\theta} \frac{2\alpha + r}{2r}}{1 + \beta n_j \frac{1}{1+\theta} \frac{\alpha^2 + r\alpha + r^2}{2r}} < 0$$
$$\frac{\partial log(a_i(\Gamma = 0))}{\partial \alpha} = -\frac{\frac{1}{2}\beta n_j}{1 + \beta n_j \frac{\alpha + r}{2}} < 0$$

Part 3 of proposition 3 states that activity decreases faster with respect to prior beliefs with a state mask mandate than without a mask mandate.

$$\frac{\partial a_i}{\partial \alpha}(\Gamma = 1) < \frac{\partial a_i}{\partial \alpha}(\Gamma = 0)$$
$$-\frac{\beta n_j \frac{1}{1+\theta} \frac{2\alpha + r}{2r}}{1 + \beta n_j \frac{1}{1+\theta} \frac{\alpha^2 + r\alpha + r^2}{2r}} < -\frac{\frac{1}{2}\beta n_j}{1 + \beta n_j \frac{\alpha + r}{2}}$$
$$(2\alpha + r)\left(1 + \beta n_j \frac{\alpha + r}{2}\right) > (1+\theta)r + \beta n_j \frac{\alpha^2 + r\alpha + r^2}{2}$$
$$\frac{\alpha(\beta n_j(\alpha + 2r) + 4)}{2r} > \theta$$
$$\frac{\alpha(\beta n_j(\alpha + 2r) + 4)}{2r} > \frac{\alpha}{\alpha + r} > \theta$$

Part 4 of proposition 3 states that activity decreases faster with respect to prior beliefs with a county mask mandate than without a mask mandate.

$$\frac{\partial a_i}{\partial \alpha}(\gamma_j = 1) < \frac{\partial a_i}{\partial \alpha}(\Gamma = 0)$$
$$-\frac{\beta n_j \frac{1}{1+\theta}}{1 + \beta n_j \frac{1}{1+\theta} \frac{2\alpha + r}{2}} < -\frac{\frac{1}{2}\beta n_j}{1 + \beta n_j \frac{\alpha + r}{2}}$$
$$2 + \beta n_j(\alpha + r) > 1 + \theta + \beta n_j(2\alpha + r/2)$$
$$1 + \beta n_j(r/2) > \frac{\alpha}{\alpha} > \frac{\alpha}{\alpha + r} > \theta$$

Parts 5 of proposition 3 characterize the change in activity with respect to population with a county mask mandate and without a mandate. activity decreases with population because the risk is higher.

$$\frac{\partial log(a_i(\gamma_j = 1))}{\partial n_j} = -\frac{\beta \frac{1}{1+\theta} \frac{2\alpha+r}{2}}{1 + \beta n_j \frac{1}{1+\theta} \frac{2\alpha+r}{2}} < 0$$
$$\frac{\partial log(a_i(\gamma_j = 0))}{\partial n_j} = -\frac{\beta \frac{\alpha+r}{2}}{1 + \beta n_j \frac{\alpha+r}{2}} < 0$$

Part 5 of proposition 3 states that activity decreases slower with respect to population with a county mandate than without a mask mandate.

$$\frac{\partial a_i}{\partial n_j}(\gamma_j = 1) < \frac{\partial a_i}{\partial n_j}(\gamma_j = 0)$$
$$-\frac{\beta \frac{1}{1+\theta} \frac{2\alpha+r}{2}}{1 + \beta n_j \frac{1}{1+\theta} \frac{2\alpha+r}{2}} < -\frac{\beta \frac{\alpha+r}{2}}{1 + \beta n_j \frac{\alpha+r}{2}}$$
$$\frac{2\alpha + r}{2} > (1+\theta)\frac{\alpha + r}{2}$$
$$\frac{\alpha}{\alpha + r} > \theta$$

where the last inequality is consistent with assumption A.

Appendix A.4 Additional Model Implications

In this section, we provide additional model implications than those in the text by comparing the heterogeneous changes to activity between a state and county mask mandate. These implications expand on the comparisons in the text, where we compare activity changes with a mask mandate (either state or county) with no mask mandate. First, the model implies that the rate of change of activity with respect to population is less negative with a state mandate than a county mandate. This implications follows directly from proposition 3. Second, the model implies that the rate of change of activity with respect to prior beliefs is less negative with a county mandate than a state mandate.

Finally, the model implies that the rate of change of activity with respect to the effectiveness of masks is greater with a county mask mandate than a state mandate.

These additional implications are summarized below:

1. $\frac{\partial a_i}{\partial \theta}(\Gamma = 1) < \frac{\partial a_i}{\partial \theta}(\gamma_j = 1)$.

2. $\frac{\partial a_i}{\partial \alpha}(\Gamma = 1) < \frac{\partial a_i}{\partial \alpha}(\gamma_j = 1)$

3. $\frac{\partial a_i}{\partial n_j}(\Gamma = 1) > \frac{\partial a_i}{\partial n_j}(\gamma_j = 1)$

First, we prove these implications and then we test them in the data.

The first additional implication compares the rate of change of activity with respect to the effectiveness of mask mandates.

$$
\begin{aligned}
\frac{\partial a_i}{\partial \theta}(\Gamma = 1) <& \frac{\partial a_i}{\partial \theta}(\gamma_j = 1) \\
\frac{\beta n_j \frac{1}{(1+\theta)^2} \frac{\alpha^2 + r\alpha + r^2}{2r}}{1 + \beta n_j \frac{1}{1+\theta} \frac{\alpha^2 + r\alpha + r^2}{2r}} <& \frac{\beta n_j \frac{1}{(1+\theta)^2} \frac{2\alpha + r}{2}}{1 + \beta n_j \frac{1}{1+\theta} \frac{2\alpha + r}{2}} \\
\frac{\alpha + r}{2} + \frac{\alpha^2}{2r} <& \frac{\alpha + r}{2} + \frac{\alpha}{2} \\
\alpha <& r
\end{aligned}
$$

The second part of the additional implications compares the rate of change of activity with respect to prior beliefs.

$$
\begin{aligned}
\frac{\partial a_i}{\partial \alpha}(\Gamma = 1) <& \frac{\partial a_i}{\partial \alpha}(\gamma_j = 1) \\
-\frac{\beta n_j \frac{1}{1+\theta} \frac{2\alpha + r}{2r}}{1 + \beta n_j \frac{1}{1+\theta} \frac{\alpha^2 + r\alpha + r^2}{2r}} <& -\frac{\beta n_j \frac{1}{1+\theta}}{1 + \beta n_j \frac{1}{1+\theta} \frac{2\alpha + r}{2}}
\end{aligned}
$$

Consider the numerator and denominator separately.

$$
\begin{aligned}
-\beta n_j \frac{1}{1+\theta} \frac{2\alpha + r}{2r} <& -\beta n_j \frac{1}{1+\theta} \\
\frac{2\alpha + r}{2r} >& 1 \\
2\alpha >& r
\end{aligned}
$$

$$
\begin{aligned}
1 + \beta n_j \frac{1}{1+\theta} \frac{\alpha^2 + r\alpha + r^2}{2r} <& 1 + \beta n_j \frac{1}{1+\theta} \frac{2\alpha + r}{2} \\
\frac{\alpha + r}{2} + \frac{\alpha^2}{2r} <& \frac{\alpha + r}{2} + \frac{\alpha}{2} \\
\alpha^2 <& \alpha r \\
\alpha <& r
\end{aligned}
$$

Finally, The third part of the additional implications compares the rate of change of activity with respect to population.

$$
\begin{aligned}
\frac{\partial a_i}{\partial n_j}(\Gamma = 1) >& \frac{\partial a_i}{\partial n_j}(\gamma_j = 1) \\
-\frac{\beta \frac{1}{1+\theta} \frac{\alpha^2 + r\alpha + r^2}{2r}}{1 + \beta n_j \frac{1}{1+\theta} \frac{\alpha^2 + r\alpha + r^2}{2r}} >& -\frac{\beta \frac{1}{1+\theta} \frac{2\alpha + r}{2}}{1 + \beta n_j \frac{1}{1+\theta} \frac{2\alpha + r}{2}} \\
\frac{\alpha}{r} \left(1 + \beta n_j \frac{1}{1+\theta} \frac{2\alpha + r}{2} \right) <& 1 + \beta n_j \frac{1}{1+\theta} \frac{\alpha^2 + r\alpha + r^2}{2r} \\
\beta n_j \frac{1}{1+\theta} \left(2\alpha^2 + r\alpha - \alpha^2 - r\alpha - r^2 \right) <& 2r - 2\alpha \\
\beta n_j \frac{1}{2} \left(\frac{\alpha^2 - r^2}{r - \alpha} \right) <& 1 + \theta \\
-\beta n_j \frac{1}{2} (\alpha + r) <& 1 + \theta
\end{aligned}
$$

We report tests of these additional model predictions in Table A.2. Specifically, we estimate the following model using OLS:

$$
\begin{aligned}
\text{Activity}_{j,t} = & \beta_0 + \beta_1 \mathbb{1}(\text{mask mandate}) + \beta_2 \mathbb{1}(\text{mask mandate})_{j,t} \times \mathbb{1}(\text{county type})_j \\
& + \sum_j \lambda_j + \sum_W \lambda_W + \varepsilon_{j,t},
\end{aligned} \tag{19}
$$

where j indicates county, t indicates day, and W indicates week. The coefficient of interest is β_2 on the interaction term. We cluster the standard errors at the county level.

The top row reports the direct test of all three additional model predicts about whether activity after a state mandate, relative to a county mandate, is greater or smaller. In the first two columns, we report tests using mask compliance from the *New York Times* survey and political affiliation as proxies for mask effectiveness θ. Consistent with the first prediction, activity is greater after a state mandate, relative to a county mandate, in counties with greater mask effectiveness. The estimate in both cases is positive, though it is not precisely estimated using political affiliation. In the third column, we report the test across counties with different ex ante beliefs α. We, again, find supportive evidence of the additional predictions that there is less activity after a state mandate, relative to a county mandate, in counties with higher ex ante beliefs, those with higher case counts. Finally, the model predicts less activity in urban counties after a state mandate, relative to a county mandate. In column four, we find supportive evidence for this prediction.

Appendix B Survey data

Appendix B.1 Household survey

The household survey evidence we report in Section 5 is from a short survey we conducted in conjunction with the State of Utah on consumer sentiment.[17] Participants were recruited by mail and included a link to a survey. Figure A.2 shows the recruitment letter. We selected addresses from a database of all addresses in the state of Utah. We recruited a representative sample of Utah by selecting addresses based on nonresponse rates from previous surveys the HERO project conducted (Gaulin et al., 2020). For example, we over sampled census block groups with lower income and a higher percentage of Hispanics to ensure our resulting survey was representative of the population of Utah.

We pooled the answers from the October to the January waves of the survey. Participants were paid $10 to take the survey starting in the November wave. Figure A.3 shows the survey questions that we employ in Panel A of Tables 3 and 7 as shown to the participants. We start with a sample of 1,756 surveys and drop surveys that do not have a response in any of the four scenarios analysed, leaving a final sample of 1,271 surveys.

Utah as a state is more white, less Hispanic, has a higher median income, and slightly lower percentage with a bachelor's degree or higher than the US. Our survey participants had a similar white percentage and median income to Utah's population but were less Hispanic and had higher education attainment.

	Population	Survey
White	90%	88%
Hispanic	14%	8%
Median income	$68,374	$50,000–$74,999
Bachelor's or higher	33%	54%

Appendix B.2 Business survey

The business survey covers businesses registered with the State of Utah. Participants entered a lottery for different prizes. Figure A.4 shows the recruitment email.

Figure A.5 shows the survey questions that we employ in Panel B of Tables 3 and 7 as shown to the participants. We start with 10,726 surveys and we drop observations were all the questions of interest were not answered, this leave us with a sample of 8,769 surveys.

Most of the businesses in our sample are microbusinesses as the median number of workers is 3 and monthly revenue of $15,000 and most of the remaining them are small businesses, percentile 99 is 400 workers and monthly revenue of $8,000,000. This is fairly representative of the State of Utah as according to the 2020 Small Business Profile from the U.S Small Business Administration Office of Advocacy[18] 99.3% of Utah Businesses are small businesses

[17] As part of the HERO project, University of Utah faculty worked with the State of Utah to provide answers about COVID-19. https://eccles.utah.edu/utah-health-economic-recovery-outreach/

[18] https://cdn.advocacy.sba.gov/wp-content/uploads/2020/06/04144227/2020-Small-Business-Economic-Profile-UT.pdf

where the median small business is a non-employer firm. Looking at industries, the top three represented in our sample belong to the top half in employment numbers in Utah.

	Survey
Mean number of workers	127.7
Median number of workers	3
Mean monthly revenue	1,545,850
Median monthly revenue	15,000
Top 3 industries	Other Services
	Professional, Scientific and Technical Services
	Health Care and Social Assistance

Appendix C Heterogeneous Treatment Effects in Calendar Time

Our empirical analysis leverages the staggered enactment of mask mandates using variation across counties and time. A common method with this type of variation is a two-way fixed effects model with staggered interventions. Including county level and time fixed effects removes county-specific time averages and economic time trends. However, as pointed out by a recent econometric literature, exemplified by Sun and Abraham (2020), one potential issue of such two-way fixed effects approaches is that treatment effects can vary not only over event-time, but over calendar time. In our context, this would mean that the same policy has a different effect if implemented in March 2020 as opposed to being implemented in November 2020, which is plausible, given that COVID-19 infections as well as voluntary social distancing behavior varied over calendar time. If treated units can be considered to be in different groups (here "calendar time groups") and treatment effects differ across these groups, then a fixed effects estimator is potentially biased, as the treatment effect is calculated as a variance-weighted average effect instead of the population-fraction weighted average effect.

Several recent papers have provided extensions to the two-way fixed effect method to account for such (time-) heterogeneous treatment effects (Athey and Imbens, 2021; Sant'Anna and Zhao, 2020; Callaway and Sant'Anna, 2020; De Chaisemartin and d'Haultfoeuille, 2020b; De Chaisemartin and D'Haultfœuille, 2020a; Goodman-Bacon, 2021; Sun and Abraham, 2020; Borusyak et al., 2021; Gardner, 2021). Wooldridge (2021) provides an overview of this literature, extends it, and provides a series of suggestions for modeling treatment heterogeneity. In our setting, we re-estimate the treatment effects using the extensions in the literature. Specifically, we focus on estimates from methods proposed by De Chaisemartin and d'Haultfoeuille (2020b), De Chaisemartin and D'Haultfœuille (2020a), Sant'Anna and Zhao (2020), Callaway and Sant'Anna (2020), and Gardner (2021) because they provide aggregate estimates.

We report the estimates from these different methods in in Table A.4. Columns (1) and (2) report estimates from De Chaisemartin and d'Haultfoeuille (2020b) and De Chaisemartin and D'Haultfœuille (2020a), respectively. The first estimate is unbiased if there are heterogeneous but no dynamic effects and the second is consistent if there are heterogeneous and dynamic effects. Columns (3) and (4) report estimates from Sant'Anna and Zhao (2020) and Callaway and Sant'Anna (2020), respectively. These estimates use never-treated or not-yet treated units as controls to avoid bias and violations of the parallel trends assumption. Column (5) report estimates from Gardner (2021), who proposes a two-stage differences-in-differences model to consistently estimate the average treatment effect. Finally, we report placebo estimates from De Chaisemartin and D'Haultfœuille (2020a) in Column (6).

The estimates in Columns (1)–(5) are all positive and relatively precisely estimated, consistent with our baseline estimates. These estimates suggest that activity increased after a state enacts a mask mandate, relative to changes in activity after a county mask mandate. In contrast, the placebo estimate in Column (6) is negative and not statistically significant.

Table A.1: Correlations

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: State Mandates									
(1)	Activity	1.00							
(2)	Cases/pop	0.04	1.00						
(3)	Spend/month	0.02	0.04	1.00					
(4)	Comply county	-0.10	-0.04	-0.01	1.00				
(5)	Blue county	-0.21	-0.02	0.07	0.18	1.00			
(6)	High county	0.02	-0.01	0.15	-0.02	0.10	1.00		
(7)	Urban county	-0.14	-0.13	0.02	0.08	0.32	0.20	1.00	
(8)	Early county	-0.17	-0.03	-0.09	0.11	0.06	-0.26	0.10	1.00
Panel B: County Mandates									
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(1)	Activity	1.00							
(2)	Cases/pop	0.13	1.00						
(3)	Spend/month	0.17	0.10	1.00					
(4)	Comply county	-0.16	-0.09	-0.06	1.00				
(5)	Blue county	-0.25	0.08	-0.05	0.12	1.00			
(6)	High county	0.05	0.13	0.16	0.01	-0.11	1.00		
(7)	Urban county	-0.18	-0.19	-0.08	0.00	0.15	0.16	1.00	
(8)	Early county	-0.26	-0.22	-0.13	0.15	0.22	-0.05	0.31	1.00

NOTE.— This table presents correlations for all variables used in the analysis. We consider values for 90 days before the mask mandate was enacted. Definitions are from Table 1.

Table A.2: Additional model predictions

	Mask effectiveness θ		Beliefs α	n
	Comply	Blue	High Rate	Urban
	(1)	(2)	(3)	(4)
State Mandate × County Type	1.433*	0.116	-0.880***	-0.402**
	(0.743)	(0.257)	(0.189)	(0.182)
Mask mandate × County Type	4.132**	0.308	-1.224*	-1.159*
	(1.981)	(0.531)	(0.670)	(0.638)
Constant	-10.715***	-10.650***	-10.663***	-10.660***
	(0.397)	(0.403)	(0.403)	(0.405)
Basic controls	✓	✓	✓	✓
Expanded controls	✓	✓	✓	✓
County fixed effects	✓	✓	✓	✓
Week fixed effects	✓	✓	✓	✓
Adj. R-Square	0.880	0.880	0.880	0.880
Observations	93,163	93,163	93,163	93,163

NOTE.— *Activity* is relative to activity in 2019 in percent, using Google's cell phone data. We consider a time period from day -25 to day 25. *State mandate* is an indicator equal to one if an order mandated at the state level is in place, and zero otherwise. *Mask mandate* is an indicator equal to one if an mask order is in place, and zero otherwise. *Comply* is an indicator equal to one if at 70% of people surveyed stated that they were a mask always or frequently, and zero otherwise. *Blue* is an indicator equal to one if the Democratic party got more votes than the Republican party on the 2016 Presidential Election, and zero otherwise. *High rate* is an indicator equal to one if the number of cases on event-day 0 are above the median value of event-day 0 values, and zero otherwise. *Urban* is an indicator equal to one if urban areas include more than 50% of the county population, and zero otherwise. Basic controls includes indicator variables for whether retail, restaurants, and bars are restricted. Expanded controls adds restrictions to stay at home, school closings, and restrictions on gatherings of 10 or more people, 50 or more people, or thresholds more than 50 people. Standard errors clustered at the county level are in parentheses. We denote statistical significance by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3: Proportion of Population in County

	(1)	(2)	(3)	(4)	(5)
State × net pop	30.236*	21.934**	22.632**	6.744	5.452*
	(16.923)	(11.054)	(10.915)	(5.461)	(3.089)
Mask × net pop	-23.532*	-22.691**	-23.147***	-4.047	-4.486
	(12.207)	(8.982)	(8.904)	(4.730)	(2.816)
State mandate	-26.084	-13.925	-14.093	-3.527	-3.587
	(16.254)	(10.453)	(10.310)	(5.238)	(2.880)
Mask mandate	21.066*	15.776*	15.604*	1.609	2.794
	(11.458)	(8.322)	(8.243)	(4.497)	(2.604)
Constant	-71.781***	-54.309***	-57.487***	-3.247***	-10.601***
	(7.895)	(5.360)	(5.615)	(1.084)	(0.405)
Basic controls		✓	✓	✓	✓
Expanded controls			✓	✓	✓
County fixed effects				✓	✓
Week fixed effects					✓
Adj. R-Square	0.044	0.289	0.321	0.841	0.880
Observations	93,173	93,173	93,173	93,163	93,163

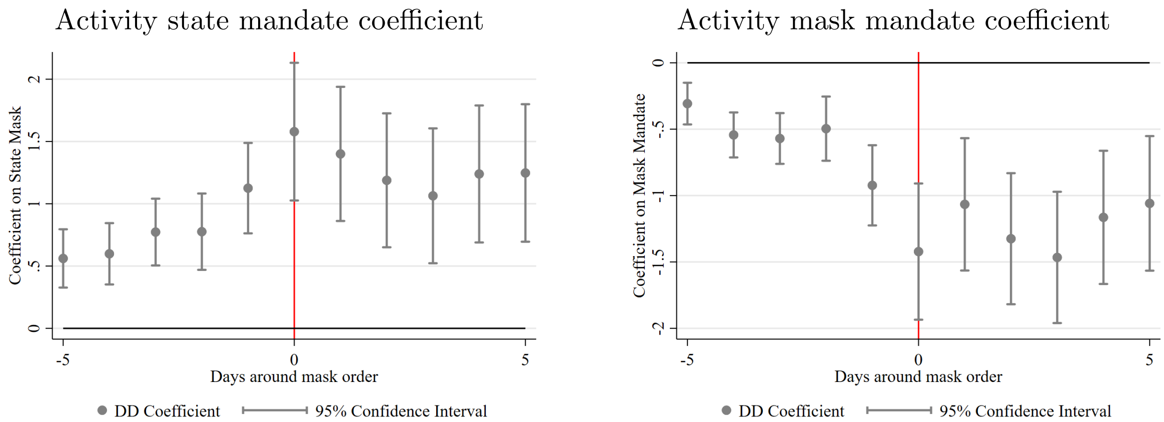
NOTE.— Net population (net pop.) is defined as the fraction of the state population not in your county. Said differently, state population net the population in your county. The dependent variable is activity.

Table A.4: Heterogeneous treatment effects

	De Chaisemartin d'Haultfoeuille (2020a) (1)	De Chaisemartin d'Haultfoeuille (2020b) (2)	Sant'Anna Zhao (2020) (3)	Callaway Sant'Anna (2020) (4)	Garnder (2021) (5)	Placebo (6)
State mask	0.409*	0.520**	1.396***	1.396***	2.333***	-0.145
	(0.223)	(0.216)	(0.216)	(0.216)	(0.422)	(0.158)
Mask mandate	✓	✓	✓	✓	✓	✓
County fixed effects	✓	✓	✓	✓	✓	✓
Week fixed effects	✓	✓	✓	✓	✓	✓
Observations	93,173	93,173	93,173	93,173	93,173	93,173

NOTE.— This table reports changes in activity as a result from mask mandates and state mandates using recent developments in heterogeneous treatment effects. Column (1) reports estimates from De Chaisemartin and d'Haultfoeuille (2020b). Column (2) reports estimates from De Chaisemartin and D'Haultfœuille (2020a). Column (3) reports estimates from Sant'Anna and Zhao (2020). Column (4) reports estimates from Callaway and Sant'Anna (2020). Column (5) reports estimates from Gardner (2021). Column (6) reports the placebo estimates from De Chaisemartin and d'Haultfoeuille (2020b). *Activity* is relative to activity in 2019 in percent, using Google's cell phone data. *State mandate* is an indicator equal to one if an order mandated at the state level is in place, and zero otherwise. *Mask mandate* is an indicator equal to one if an mask order is in place, and zero otherwise. Standard errors clustered at the county level are in parentheses. We denote statistical significance by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A.1: Sensitivity to different event time threshold



NOTE.— Coefficients from regression discontinuity estimations for different event dates around the actual event date. Left figure shows estimates for changes in activity around a state-level mask mandates, right figure for mask mandates.

Figure A.2: Household recruitment letter

MARRINER S. ECCLES INSTITUTE FOR ECONOMICS AND QUANTITATIVE ANALYSIS
THE UNIVERSITY OF UTAH

DAVID ECCLES SCHOOL OF BUSINESS

Spencer Fox Eccles Building,
1655 East Campus Center Drive,
Salt Lake City, UT 84112

Dear Utah Resident:

Your household has been selected to participate in an important effort to measure how the economy is doing in your community through a program developed by the University of Utah in collaboration with the Utah State Government. Your answers will help ensure Utah's economy keeps going strong.

Please fill out the survey online: The survey will ask for your Access Code, provided below.

Website: eccles.link/ConsumerSurvey

Access Code: [house_id]

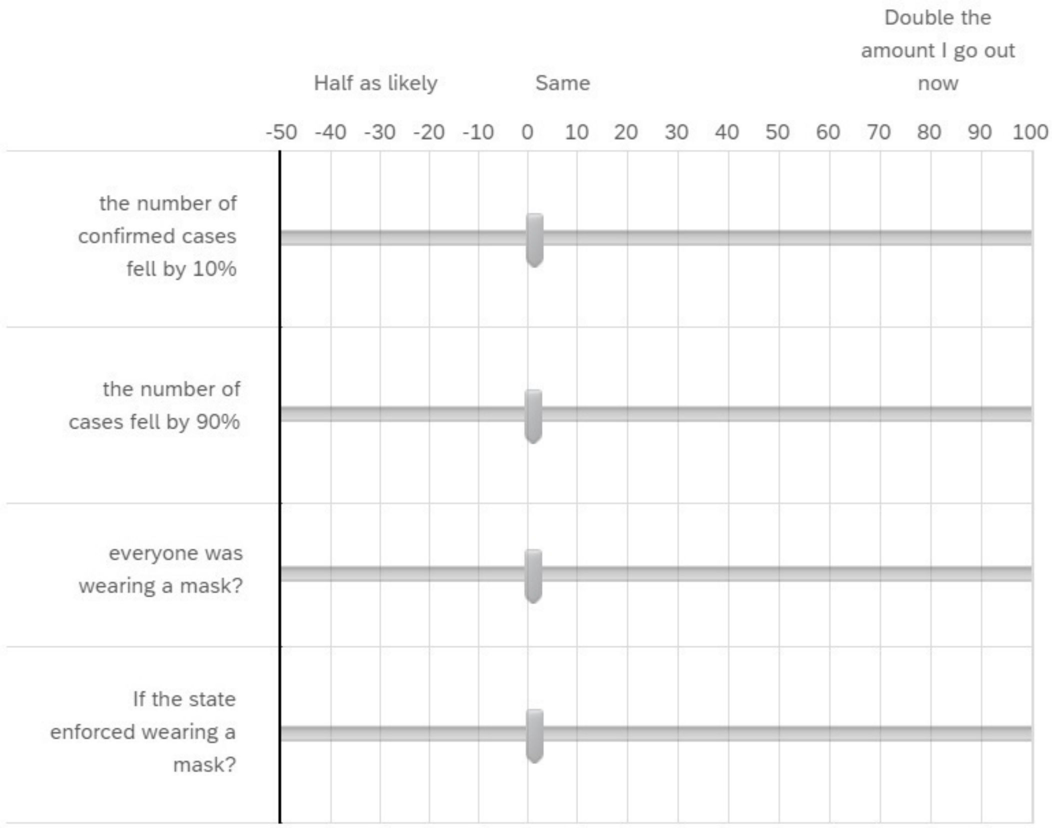
Answering the questions only takes a few minutes and is voluntary. You may choose not to answer any of the questions. **All information collected will remain confidential.**

As a thank you, you will receive a $10 gift card after completing the survey

Ultimately, the goal of this effort is to ensure Utah's economy remains strong. Our country faces an unprecedented crisis, and it is individuals like you that will help us get back on track. For more information regarding this program visit https://marriner.eccles.utah.edu/covid-research/ or email nathan.seegert@utah.edu.

Figure A.3: Household survey questions

How much more or less likely (as a percent) would you be to go out to a store if:



Regarding business conditions **in UTAH** as a whole - do you think that during the next 12 months we will have good times financially or bad times?

- ○ Good times
- ○ Bad times

Figure A.4: Business recruitment email

Dear Utah Business,

COVID-19 is an ongoing health and economic crisis in Utah, and a strong recovery is only possible with your help.

To aid in the state's decision-making, we would like to better understand the issues your business is facing and have prepared this survey (link below) to gather your feedback. We will use your input to develop economic initiatives, policies, and programs to continue to support our business community and residents.

Your input is important to us! As a token of appreciation for your help, you will be entered to win one of ten $1,000 gift cards, a kayak, skis, and other outdoor equipment.

Many thanks,

Follow this link to the Survey:
Take the Survey

Or copy and paste the URL below into your internet browser:
https://covid19testing.co1.qualtrics.com/jfe/preview/SV_eo0pbcP4ogoFLRH?Q_CHL=preview

Follow the link to opt out of future emails:
Click here to unsubscribe

Figure A.5: Business survey questions