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

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Abstract

We highlight the importance of signaling effects in determining whether public policy should be implemented at a decentralized or centralized level. For example, although a public policy may have the same direct effect if enacted at a state or county level, people may perceive these policies differently, leading to different indirect effects. We explore this mechanism using the patchwork of mask mandate orders in the U.S. from April to September 2020. State-wide mask mandates stimulate economic activity while also reducing COVID-19 case growth. Surprisingly, county-level mask mandates generally have the opposite effect, depressing economic activity. We argue that different unintended signaling effects can explain these differences in policy effects: households infer from county mask mandates that infection risks have increased in their local area and, therefore, socially distance more and spend less. In contrast, state mask mandates do not lead to similar local inferences, and thus overall, they stimulate the economy.

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1 Introduction

To what degree should public policy be decentralized? This is both a classic question of governance and a topic of ongoing controversy. An obvious case in point has been the nation's response to the COVID-19 pandemic, during which crisis management has been almost entirely decentralized and dominated by the state and local 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 traditional view on devolution of policy responsibility focuses on the tension between better coordination with centralization and better adaptation to local conditions with decentralization, see Oates (1999), and Alonso, Dessein and Matouschek (2008). We introduce a new component to this debate focused on private information revelation of policy. We show that this information channel is empirically relevant and, in our context, favors centralization.[1]

We empirically analyze the effects of various county- and state-level mask orders to understand how decentralization impacts the effectiveness of these policies. We evaluate these policies in terms of health and economic outcomes. Mask mandates provide a particularly useful lens for studying policy devolution for at least two reasons. First, due to the absence of a national mask mandate, there has been wide variation in when states and counties adopted mask mandates. This variation offers a rich natural laboratory for analyzing mask orders at different levels of government. Second, mask orders (like most public policies) have direct effects and additionally indirectly change behavior through information revelation. The direct effect of these policies is to limit transmission because the virus spreads largely through respiratory droplets. The indirect effects consist of two potentially conflicting economic forces. On the one hand, mask mandates boost consumer confidence due to lower infection risks and thereby stimulate the economy. On the other hand, mask mandates increase consumer risk assessments, as the government would only put a policy in place if infection risk is high.

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

This effect reduces economic activity. Whether a policy should be enacted at a centralized or decentralized level depends on the balance of the two indirect effects.

We find that state mask mandates stimulate the economy. In contrast, county mask mandates do not. The difference in effects by the level of government can be understood as resulting from the two conflicting economic forces. Specifically, mask mandates at both levels signal that economic activity is safer—stimulating the economy. However, when a county enacts a mask mandate, people also believe the risk from COVID-19 in their local area is higher—dampening economic activity. We find both of these effects are empirically large, such that state mask mandates stimulate the economy, but county mandates do not.

To quantify the effects of mask orders, 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 order. If local governments anticipate a substantial rise in local cases, they are more likely to impose mask orders. As a result, a simple regression of case growth on mask orders might show that the imposition of mask orders is positively correlated with case growth, even if the true effect of masks is to limit the spread of COVID-19. Deliberate policy induces an upward bias in the correlation of mask orders and case growth and a downward bias in the estimate of the effectiveness of the mask order 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 variation in dates at which states and counties enacted mask orders and the discontinuous nature of mask orders. States and counties imposed mask restrictions at varying points from April 2020 through September 2020 (in our sample). In addition, mask orders 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 this initial approach are robust to using alternative approaches, such as parametric and non-parametric regression-discontinuity designs (RDD). Furthermore, we offer estimates of longer-run impacts using Synthetic Control and Differences-in-Differences methods.

We also note that mask orders sometimes are accompanied by other restrictions, such as limits on gatherings or school and restaurant closures. When we control for these other restrictions using data from Killeen, Wu, Shah, Zapaishchykova, Nikutta, Tamhane, Chakraborty, Wei, Gao, Thies and Unberath (2020), our estimates do not change substantially. In addition, these types of measures typically directly reduce mobility and economic activity and are by themselves unlikely to explain our results.

We combine high-frequency (daily) data on economic activity, COVID-19 case growth, and mask mandates on the county level. Economic activity is measured using cellphone-GPS-based mobility data from Google and credit card transaction data from Safegraph/Facteus. To put the mobility data in context, a 10-percentage point increase in mobility is associated with a two percentage point reduction in state unemployment rates (Yang, Looney, Gaulin and Seegert, 2020a). COVID-19 case growth has been calculated based on county-level data maintained by *The New York Times*. To measure mask mandates, we manually collected county-level data on mask mandates issued either by the state or the county. To provide additional direct evidence on mask orders on economic activity, we also conducted a representative consumer survey for the state of Utah.

We establish three sets of main results with our data. First, our analysis of state mask mandates reveals that these mandates are effective policy tools. Specifically, we show that state mask mandates dampened COVID-19 case growth while stimulating consumer confidence, as measured by increased mobility and credit card spending. We find evidence for these effects immediately after states enact mask mandates and up to two months after implementation. We estimate a one percentage point increase in mobility in response to state mask mandates, which is roughly associated with a 0.2 percentage point reduction in state unemployment. We find that a state mask mandate immediately reduces the growth of new cases and that this effect persists over the next 2–3 months. Ultimately, the mandate results in a reduction of 10 new cases per day per 100,000 people compared to before the mandate. These estimates emphasize that mask mandates can persistently promote economic activity while at the same time safeguarding public health. Our results, therefore, suggest that policymakers do not face a trade-off between lives and livelihoods in combating COVID-19.

Second, we fail to find similar policy effects for mask mandates implemented by counties. While COVID-19 case growth decreases immediately after counties implement a mask mandate, mobility, and credit card spending decrease. In accordance with the traditional view on decentralization of public policy, one might think that a county mask order is more effective than a state mask order because counties are better at evaluating their local conditions. However, our findings suggest a key economic argument against county mask orders: they may decrease consumer confidence by revealing to people that the risk in their immediate neighborhood has increased. As a result, people reduce their mobility, spending, and demand for local businesses.

Third, we provide direct evidence for consumer responses to mask mandates and COVID case growth. In a purpose-built representative survey 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 15% higher likelihood of going out to a 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. This evidence is consistent with the stimulative effects of state mask mandates.

Our study builds on an illustrious federalism literature by considering how information inferences may change the optimal level of government that should implement a policy.

This question of devolution was first posed in an early normative literature (Musgrave, 1959; Oates, 1972; Gordon, 1983). A subsequent branch of this literature considers what level of government should raise redistributive income taxes and concludes that the answer depends on the level of consumer mobility across states (Boadway, Marchand and Vigneault, 1998; Gordon and Cullen, 2012). Another branch considers the distortions to policies due to competition between governments—both horizontal and vertical (Goodspeed, 1998; Keen and Kotsogiannis, 2002). We also consider the case in which a government has goals divergent from social efficiency (Brennan and Buchanan, 1980; Wildasin, 1986; Qian and Weingast, 1997; Oates, 2005; Brueckner, 2006). We also incorporate insights from Boadway, Pestieau and Wildasin (1989), who consider the role of information in a federalist setting. Finally, our empirical setting of COVID-19 demonstrates 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 mobility, cases, and fatalities from COVID-19. Chernozhukov et al. (2020) focus on state-level data and therefore do not contrast the differences between county and state-level and county-level mask mandates. Furthermore, Chernozhukov et al. (2020) primarily focus on employer mask mandates for employees 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. Decentralization of policy responsibility is often justified through potential advantages such as local adaptability. Centralization, in contrast, is often justified through promises of increased efficiency. We argue that the optimal level of devolution may change due to differences in signal strength created by implementing a policy at different levels of government. 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 a firm issues equity to finance an investment project, it unintentionally signals the quality of the project (Myers and Majluf, 1984). As a final example, 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, we believe that such 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 orders. In contrast, centralized crisis management avoids such a trade-off and both saves lives and preserves—even boosts—economic livelihoods.

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. 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 August, 37 states plus Washington D.C. enacted state-wide mask-wearing mandates. Additionally, from April to September, 136 counties across 29 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 our sample period. The sample period spans the 90 days before and after a state- or county-level mask mandate was put in place (offset by one week before the mask mandate to account for announcement effects). Our primary proxies of economic activity are consumer mobility,[2] which measures the amount of traffic at economically relevant activities (e.g., retail shopping and transportation) as a percentage of the prior year's activity levels, and consumer spending from Facteus, with which we calculate average spending per person per month.[3] Over the sample period (90 days before and after a mask mandate), the average mobility is 14% lower than for the equivalent period

[2] Our mobility 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).

[3] The Facteus data contains credit card spending data from multiple payment processing companies but only covers a subset of processed spending. This data provides spending based on residence and, unfortunately, 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 * 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.

in 2019, reflecting the general reduction in economic activity observed throughout much of 2020 (Yang et al., 2020a). However, the average mobility around county-level mandates is 23% lower than the equivalent period in 2019. Similarly, spending per month is 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 states around the mandates was 10 per 100,000 residents, while the equivalent number around county mandates was 15. 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 78% of the county mandates have above-median event-day-0 infection rates, compared to only 49% 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, whereas state mandates, which affect a larger region, may not reflect such acute statistics.

The counties enacting mandates are more urban (*Urban county* is 76% compared to 23% for state mandates) and adopt those mandates earlier in time (*Early county* is 66% compared to 55% for state mandates). According to survey data published by *The New York Times* (*Comply county*), 87% of residents covered by a county mandate reported compliance with a mask mandate, a figure substantially higher than the 67% under state mandates. Lastly, the counties enacting mask mandates are slightly more liberal, with only 43% being majority conservative. However, 82% counties under state mandates are conservative (*Red county*, based on 2016 presidential vote). Similarly, 77% of the counties with county mandates occurred in conservative states, and 65% of counties with state mandates occurred in conservative states. Liberal counties also adopted mandates earlier than their conservative counterparts, as we see in Table A.1, which shows a –0.2 correlation coefficient between *Early county* and *Red county*. In Figure 1, we show the geographic dispersion of counties and states with mask mandates. There is 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 2, 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 1). 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 case counts and economic activity.

3 Empirical Methodology

Our goal is to quantify the effects of mask orders on economic and health conditions. Unfortunately, government policy, whether at the state or county level, 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, as we have noted, after imposition of the mask mandate, 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 orders and the number of new cases.

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

$$Y_{i,\tau_i+1} = \beta_0 \cdot \mathbb{1}(\tau_i > 0) + g_i(\tau_i) + \epsilon_{i,\tau_i+1}, \tag{1}$$

where $\mathbb{1}(\tau_i > 0)$ is an indicator for the imposition of a mask mandate, and $g_i(\tau_i)$ is a continuous function. In specification (1), time is measured in "event time" for every county i, i.e., relative to the time when the mask mandate was imposed at time $\tau_i = 0$.[4] We also note

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

that event time zero is typically defined a week before the mandate's actual implementation to account for anticipation effects (although for a delayed discovery physical transmission process like virus infections, the intervention might instead lead to a response after some delay). The continuous function $g_i(\tau_i)$ can capture a variety of time-varying omitted variables. Chief among these are unobserved expectations of local government officials at time τ_i, given by $g(\tau_i) = \gamma_i \cdot E_{G,i,\tau_i}[Y_{i,\tau_i+1}]$. Note that the imposition of the mask mandate $\mathbb{1}(\tau_i > 0)$ and government expectations $E_{G,i,\tau_i}[Y_{i,\tau_i+1}]$ 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

$$\frac{1}{N}\sum_i (Y_{i,t_i+1} - Y_{i,t_i}) = \beta_0 + g_i(\tau_i+1) - g_i(\tau_i), \tag{2}$$

where $g_i(\tau_i+1) - g_i(\tau_i) \approx 0$, 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 for a subset of the data.[5] Second, we implement a non-parametric regression-discontinuity design approach in Subsection 6.2. Third, we pursue a global regression-discontinuity design approach in Subsection 6.3, which flexibly controls for the function $g_i(\tau_i)$. Both of these approaches allow

[5] We use county-level data on county-level stay-at-home orders, school and restaurant closures, and restrictions on gatherings and other policies by Killeen et al. (2020). Unfortunately, this data ends in early July 2020, which is why we do not report results here. For our sample, until July 2020, baseline results are unchanged if we include dummy controls for these policies. Additionally, if we project these policies forward, our main results remain unchanged. We are currently working to extend the county sample to report the main results with a full set of controls for these county-level policies. Preliminary results with the limited sample from Killeen et al. (2020) are available upon request.

us to control for continuous, time-varying unobservables in event-time, which correspond to $g_i(\tau_i)$. Finally, in Subsections 6.2 and 6.2, we report estimates from placebo tests using variation in location and time.

4 Empirical Evidence

In this section, we study the effects of a mask mandate and how they vary when a state or a county establishes the order. We report three different measures at the county level. *Mobility*, the change in consumer activity with respect to the same day last year, *Cases/pop*, the daily new cases per 100,000 population, and *Spend/month*, daily credit card spending per person (scaled to the monthly level for economic interpretation).

A county mandate likely provides more information about contagion risk to people because the county's case rates probably drive the government decision. In contrast, the decision to enact a state-wide mandate weighs information from the entire state. As such, it provides an attenuated signal about the contagion risk in any given county. Therefore, we expect to observe a stronger information signal from enacting county mandates than state mandates, leading individuals to update their beliefs about contagion rates more so for county mandates. As a result, we expect this signal to reduce or even overwhelm the mask-mandate's direct effect to minimize risk and increase economic activity.

When the mask mandates' direct effect is larger than the information channel, we expect mobility and spending to increase because these activities have now become safer. When enacting a mandate is accompanied by a larger information shock, we expect increased mobility and spending to be attenuated and potentially decrease. We expect cases to fall after a mandate; the extent, however, may differ by level of government due to enforcement.

In Table 2 and Figure 3, we report estimates of how the three dependent variables change over the 25 days before and after a county or state mandate. We measure mobility and spending one week before the mask mandate to account for leakage of information. We measure the new case rates ten days after to account for testing delays (of three to four days on average) and symptom onset (of two to fourteen days).[6] Our findings are not sensitive to this timing. We report estimates with different timing and time polynomials to account for potential information leakage and mechanical testing lags as an alternative. The sample in the first three columns comprises all counties in a state with a state mandate that did not already have a county mandate. The sample in the last three columns comprises counties that enacted a county mandate (when no state mandate was already in place). We include county-fixed effects, as well as calendar-day-fixed effects.

From Table 2, we find that when a mask mandate is ordered by the state, mobility and spending increase by 2.7% and 2.0% of their means, respectively, which is consistent with the direct effect being stronger than the information effect. Specifically, in columns (1) and (3), we report that mobility increases by 0.39 percentage points and spending increases by $23.89 per person per month, respectively. This evidence suggests that people believe that they face less risk when engaging in economic activity with the mask mandate in place. Simply put, they go out and consume more. Meanwhile, cases per population decrease, despite the increased mobility. In Figure 3, we show that after the mandate, new cases appear to stop increasing entirely, suggesting that the mask mandate reduces the spread of the disease. Together, this evidence suggests that state-wide mask mandates achieve their economic and health goals.

As expected, the information channel appears to be stronger for county-level mask mandates. After a county enacts a mask mandate, we observe a decrease in mobility of 0.51 percentage points or 2.2% relative to the mean. This reaction is consistent with people updating their risk assessment of COVID-19 and partially or fully quarantining. Spending does not increase as it does under the state mandate, and Figure 3 shows that the downward trend in spending continues relatively unaffected. The number of new cases per 100,000 people decreases substantially after the mandate; column (5) reports that cases decreased by

[6] See 50-State COVID-19 project covidstates.org and CDC cdc.gov/coronavirus/2019-ncov/symptoms-testing/symptoms.html.

two people per 100,000, a 13% decrease relative to the mean. Consistent with this finding, in Figure 3, we show a significant decrease in the growth of new cases. This strong public health impact likely combines the effect of increased mask-wearing and the substantial decrease in mobility observed after county mask orders.

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 channel is significant enough, as observed in county mandates, the information effect dominates, and the net impact on economic activity is negative.

5 Treatment Effect Heterogeneity

This section considers heterogeneous responses across several dimensions that affect the response to mask mandates. For example, prior beliefs about COVID-19 contagion risk likely depended on information people received before the mask mandate. The information people receive, in turn, is partially influenced by news consumption habits (e.g., conservative or liberal sources). People's prior beliefs were also likely affected by whether the cases were initially high or low and whether the mandate came early in the pandemic or later. Similarly, people's priors are likely affected by externalities from other people's choices, which varies based on the local population density.

While we present one interaction effect at a time here, we show in Appendix B that our results are broadly unchanged if we jointly analyze all interaction effects together.

5.1 Differences by political affiliation counties

In Table 3, we report the effect of state and county mask orders on mobility, case growth per population, and spending per person, interacted with an indicator variable for conservative counties.[7] We denote conservative counties as *red* for short. We designate a county as conservative if it recorded more votes for the Republican presidential candidate in 2016 than the Democratic one, and liberal otherwise. The estimates are similar for other political affiliation measures, including vote shares in different elections and google searches for partisan phrases such as "plandemic."

We find that mobility increases after a state-enforced mask order, but substantially less so in conservative counties. Specifically, in column (1), we report that the coefficient on the interaction between state mask mandate and conservative county is roughly -0.40, and the coefficient on state mask mandate is about 0.71, both relatively precisely estimated. This evidence suggests that mobility increases following a state mandate but less so in conservative counties than liberal ones. This result is consistent with the presence of an information effect in state mandates, albeit weaker than the information effect in the county mask mandates. In other words, this evidence is consistent with conservatives, who previously believed the risks from COVID-19 were low, after the mask mandate believing the risk is high—leading them to go out and spend less, while liberals, who previously thought the risks were high, now start going out and spending with a new confidence that comes with the mandate.[8] For county mask mandates, column (4) shows that this information effect is strongly driven by red counties and is strong enough to reduce mobility. Column (4) also shows that liberal counties see a slight increase in mobility in response to county mask mandates, though this is imprecisely estimated. These findings suggest that people in liberal counties either had a higher prior risk assessment or did not view the county-level mask mandate as providing much new information about local infection risks.

Confirmed cases decrease substantially less in conservative counties in the days immediately following state-level mask mandates, which we report in column (2) of Table 3. Specifically, the coefficient on state mask is -2.58, and the coefficient on the interaction be-

[7]We also report figures for non-parametric regression-discontinuity design estimates in Figure A.1.

[8]We consider this a reasonable assumption. For example, at a Trump campaign rally in February, President Trump said, "Now the Democrats are politicizing the coronavirus ... This is their new hoax."

tween state mask and conservative county is 2.07, both estimated relatively precisely. One possible explanation for this pattern is that people in conservative counties respond to state mandates less and are therefore more mobile and continue to get infected.

There is no discernible difference in spending per person in conservative and liberal counties following a mask mandate. In columns (3) and (6) of Table 3, we report that spending increases following a state mask order but does not following a county order. The interaction effect for conservative counties, while typically positive, is small economically and statistically insignificant.

We provide additional estimates of political affiliation in appendix Table A.2. In this table, we report estimates allowing differing responses for conservative or non-conservative counties in conservative states or non-conservative states. We find that counties—either liberal or conservative—in conservative states decrease their mobility more than conservative counties, whether in conservative or liberal states. Although state mask mandates still imply increased mobility for conservative counties in blue states, mobility responses are almost zero for (liberal or conservative) counties in conservative states. Furthermore, Table A.2 shows that case growth tends to fall less in conservative states and conservative counties. This evidence is consistent with conservative states having a greater threshold to enforce a mask mandate and, therefore, a larger information revelation effect when the mandate is enforced.

5.2 Differences across urban and rural counties

In Table 4, we report the effect of mask mandates separately for urban and rural counties[9]. We designate a county as urban if more than 50% of its population lives in an urban area, and rural otherwise. The externality created by going out to stores or restaurants without a mask is greater in urban areas than rural because contagion risk increases with population density. As a result of this relative difference in the direct effect, we expect that mask mandates will increase mobility and spending and decrease case rates more in urban areas

[9] We also report results from non-parametric regression-discontinuity design estimates in figure A.2

than in rural areas.

The evidence across urban and rural counties is consistent with our predictions of differences in the direct channel. In Column (1) of Table 4, we report that mobility increases less in urban counties following a state mask mandate as shown by the coefficient -0.295. This evidence is consistent with the view that infection risk is generally higher in denser areas, which implies that people will be more hesitant to be mobile, even if other people wear masks.

Following county-level mask mandates, mobility decreases more in urban counties. Regarding case growth, we find that both state and county mask mandates effectively dampen COVID-19 spread. It should be noted that urban counties see a consistent reduction in case growth by about 2.7 cases per 100K, while case growth reduction from state mask mandates is somewhat less effective for rural counties.

Consistent with our mobility estimates, we also document in Table 4 that urban counties see a strong increase in credit card spending in response to state mask mandates. In contrast, they see a slight decrease by around \$8 per month (\$26.58 – \$35.52).

5.3 Differences across counties with high and low number of cases

A mask mandate might reveal especially high levels of infection risk in counties with a high initial level of case counts. This prediction follows from the pitfalls of monitoring infection risks of contagious diseases with exponential growth dynamics. Initially, case numbers are very low, and the risks of exponential growth in infectious people are underestimated. However, with higher base rates, the same exponential growth rate implies a non-linear increase in the number of newly infected people. This non-linear dynamic can result in sudden shifts in risk assessment, as risks are deemed negligible in the initial weeks but then appear suddenly very imminent as the number of new cases rises higher. Therefore, a given infection growth rate might signal disproportionately higher infection risk if the currently known base rate of cases is high.

Consistent with our predictions, we find that mobility increases in response to state mask mandates are very muted in counties with a high number of cases. Specifically, in Columns (1) and (4) of Table 5, we report that the interaction between high case county and state mask mandate (in Column 1) and high case county and county mask mandate (in Column 4) are negative, large, and relatively precisely estimated.[10] In the case of county mask mandates, the effect on mobility in high case counties is large enough to reduce overall mobility. The negative interaction effects are consistent with the view that mask mandates in general signal particularly high risks in counties with high initial levels of confirmed COVID-19 cases.

Columns (2) and (5) of Table 5 show that both state and county mask mandates are effective in dampening case growth, especially for counties with a high initial level of cases. The evidence on spending is more mixed. While state mandates stimulate credit card spending, they do so actually more in high case counties. The effect of state mask mandates in high case counties is almost double the effect with an increase by $\$31.23 = (\$16.25 + \$14.98)$ per month and person. In contrast, there is no significant effect of county mask mandates on credit card spending.

5.4 Differences across counties with earlier or later mandates

COVID-19 initially arrived in major global cities, such as the New York metropolitan area, the San Francisco Bay area, and Seattle-Tacoma metropolitan area. As a result of the disease's early geographic concentration, locations that imposed mask mandates early reveal much more information on infection risk. In contrast, by fall 2020, COVID-19 had spread from the coastal areas of the U.S. to almost all counties and had penetrated rural and even remote areas. Consequently, later adoption of mask mandates is likely to have revealed less information about infection risks, both because more time had passed and because the disease was more widespread.

If this is the case, then mobility in early counties with county-level mandates will decrease

[10] We also report results from non-parametric regression-discontinuity design estimates in Figure A.3.

more than mobility in late counties. This is exactly what we show in column (4) of Table 6.[11] At the same time, state mask mandates were effective in restoring confidence and allowing people to be mobile, as shown in column (1) of Table 6. This interpretation is reinforced by the results in column (3), which shows that early state mask mandates stimulated credit card spending by \$91(= \$139.93 − \$48.93) per month per person. No comparable significant effects result from early county mask mandates, as shown in column (6).

Additionally, column (2) highlights that early state mask mandates were very effective in reducing case growth, while estimates for the effects of early county mask mandates are noisier; see column (5).

5.5 Differences by compliance of mask wearing

In this section, we address the issue of compliance with mask orders. The reception and ability to enforce mask orders can vary widely. As a result, mask orders issued in counties with high compliance are more likely to reduce the spread of SARS-CoV-2 successfully, thereby boosting consumer confidence and reduced perceived risk.

To analyze the impact of varying compliance with mask mandates, we use county-level data gathered by *The New York Times* in cooperation with the survey firm Dynata. We designate a county as a comply county if at least 70% of people surveyed by *Times* reported that they always or frequently wear a mask, and a non-comply county otherwise. We report these heterogeneous effects in Table 7 and non-parametric regression-discontinuity design estimates in figure A.5.

In Column (1) of Table 7, we report that, after a state-level mask mandate, counties with more mask-wearing increased their mobility more than counties with less. This evidence is consistent with the view that higher compliance with state mask mandate reduces infection risk perception and enables people to be more mobile. In contrast, in Column (4), we report that, after a county-level mask mandate, counties with more mask-wearing saw decreased

[11] See also the confirming non-parametric regression-discontinuity design analysis of these effects in Figure A.4

mobility relative to counties with less mask-wearing. This result potentially stems from people in counties with higher mask-wearing compliance being more risk-averse to the virus.

For both state and county mask mandates, we find that case growth is significantly reduced for high mask-wearing compliance counties. Interestingly the reduction in case growth is even larger for county mask mandates than state mask mandates. This result is consistent with high compliance people being more risk-averse to infection risks.

6 Sensitivity of estimates

This section considers additional specifications and placebo tests to explore potential threats to identification. To identify the effects of mask orders, the estimates presented previously have relied on the staggered implementation of mask orders 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 provide two placebo tests to ensure that the mask mandate variation drives our results and not unobserved features of either the time or the treatment counties we analyze.

First, we provide a location placebo created by assigning treatment status to counties that did not enact mandates but are adjacent to counties that did. The placebo treatment assignment is chosen at the same time as the treatment assignment of the counties with mask mandates. This location placebo allows us to verify that our effects are not driven by effects associated with the treatment's timing per se. Second, we provide a timing placebo by assigning the timing of the mask enacted randomly 75 to 100 days before the actual mask mandate was enacted. This timing placebo is intended to confirm that our effects are not driven by effects associated with the locations of treatments per se. These placebo tests seek to uncover potential unobservable characteristics either across counties or time that could explain 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 confounding factors in the placebo tests.

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, that there are only minor systematic changes at the daily frequency, or $g_i(\tau_i + 1) - g_i(\tau_i) \approx 0$ in the notation of section 3—is overly strong. 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 $\mathbb{1}(\tau_i > 0)$. At the same time, COVID-19 prevalence and unobserved expectations of policymakers change in a continuous fashion, captured by the function $g_i(\tau_i)$.

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

$$
\begin{aligned}
E\left[Y_{i,\tau_i+1}\middle| -\Delta < \tau_i < 0\right] \approx& E\left[Y_{i,\tau_i+1}(0)\middle|\tau_i = 0\right] \\
E\left[Y_{i,\tau_i+1}\middle|0 \leq \tau_i < \Delta\right] \approx& E\left[Y_{i,\tau_i+1}(1)\middle|\tau_i = 0\right],
\end{aligned}
\tag{3}
$$

where, as we will show in the estimation section Δ is typically 25 days, using optimal bandwidth selection methods such as Imbens and Kalyanaraman (2011) and Calonico, Cattaneo and Titinuik (2014). The jump in outcome Y_{i,τ_i+1} at $\tau_i = 0$, therefore, identifies the immediate mask impact

$$
\beta_0 = E\left[Y_{i,\tau_i+1}(1)\middle|\tau_i = 0\right] - E\left[Y_{i,\tau_i+1}(0)\middle|\tau_i = 0\right]. \tag{4}
$$

We also consider parametric versions of specification in equation (1), by using flexible polynomial functions to control for $g_i(\tau_i)$. 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 setups 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 our economic variables, especially mobility and spending because these are flow variables that can adjust almost immediately. Additionally, we use changes in case growth precisely to reduce the influence of persistence in this data.

Table 8 reports the result of the non-parametric regression-discontinuity design in event time. Almost all of our main results are qualitatively similar in this specification, confirming that our event-study approach is not systematically biased. We note that the economic effects are generally larger in magnitude in the non-parametric regression-discontinuity design specification, while the county mask mandate effects tend to be noisier than in our baseline event study result recorded in Table 2.

Table 9 reports additional parametric regression-discontinuity design estimates using either first- or third-order polynomials to control for continuous unobservables in event time. Encouragingly, these results are quantitatively very close to our main results in Table 2 for linear event-time controls and somewhat stronger for third-order polynomials.

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

6.2 Location Placebo Tests

Placebo tests provide a check on the likelihood that unobservable characteristics could drive the results. If our specifications measure the true effect of mask mandates, we should find no impact if we assign treatment to counties that did not enact a mask mandate and rerun our specifications. We select all counties without a county or state mandate that are 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 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 average time.

In Table 10, we report these placebo tests for both our event-study design (in equation (2)) and regression-discontinuity design (in equation (4)). In panel A, we find no statistically significant effects on mobility, cases per 100,000 people, or spending per person per month with state or county mask mandates. In panel B, we similarly find no evidence of a discontinuity in these placebo counties when the true counties are enacting their mask mandates. The lack of effects is encouraging, especially because of spillover effects that likely exist due to the proximity. To provide a further check, we next consider a placebo test in time.

6.3 Timing Placebo Tests

This section complements the previous section by reporting estimates from a placebo test across time in Table 11. Once again, panels A and B provide estimates from our event-study and regression-discontinuity designs (in equations (2) and (4)). If our specifications measure mask mandates' true effects, we should find no impact if we assign the mask mandate to occur substantially before the true mandate took place. In this placebo test, we use the 2,188 counties with a county or state mandate and assign their mandate to occur randomly between 75 and 100 days before the relevant mandate occurred.

We find no evidence of an effect around these placebo mask mandates. For example, in column (1) of panels A and B, the change in mobility before and after the placebo time is -0.001, with a standard error of 0.082 using the event-study design and 0.069 with a standard error of 0.440 in the regression-discontinuity design. In contrast, in Table 2, we report the change in mobility around the true implementation date is 0.391 with a standard error of 0.068.

7 Extensions: survey evidence, medium-run estimates, and transactions

In this section, we provide additional evidence on the effect of mask mandates on mobility from a survey we conducted and provide medium-run estimates using a differences-in-differences specification. Finally, we provide additional information on how economic activity changed after a government enacted a mask mandate by investigating changes in the number of transactions and spending per transaction.

7.1 Survey Evidence

In this section, we report evidence from the October and November 2020 Utah Consumer Sentiment Survey. The survey samples 400 Utah residents a month and is designed to be similar to the Michigan Surveys of Consumers, which includes 500 completed interviews per month. For the questions we are interested in, the sample was recruited by sending participants a letter and paying participants $10. The sample is the universe of addresses in Utah and sampled based on prior nonresponse rates that provide a final sample representative of Utah (Samore et al., 2020; Yang et al., 2020b). This sampling has been shown to have minimal nonresponse sample selection Gaulin et al. (2020). In appendix Appendix A, we provide more details on recruitment, nonresponse, and sample balance.

In Table 12, we report 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 seven scenarios: "The number of confirmed cases fell by 10%," "The number of confirmed cases fell by 50%," "The number of confirmed cases fell by 90%," "Half of the people were wearing a mask," "Everyone was wearing a mask," "The store enforced 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 are wearing a mask in their locality. In response to a drop in confirmed cases of 10%, 50%, and 90%, participants report they would increase their likelihood of going to a store by 13%, 30%, and 57%, respectively. These correspond to elasticities between 0.6 and 1.3. Similarly, respondents report that they would be substantially more likely to go out to a store if everyone was wearing a mask (51% more likely), the store enforced wearing a mask (50% more likely), and the state enforced wearing a mask (49% 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 that mask mandates increase mobility. Mobility decreased sharply in response to COVID-19. As a result, spending has decreased and shifted from local services toward goods that can be shipped to one's door. These survey responses suggest that people believe the risk of going out to a store is substantially less if people are wearing masks—and, as a result, increases their likelihood of going out to a store.

These survey responses also 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 mobility. This highlights the importance of government messaging of policy initiatives.

7.2 Medium-run estimates

To extend our analysis to the medium-run, we rely on synthetic control methods developed by Abadie, Diamond and Hainmueller (2010, 2015) and Abadie and Gardeazabal (2003). 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 $g_i(\tau_i + T) - g_i(\tau_i) \approx 0$, becomes clearly untenable. Instead, synthetic control methods allow us to flexibly control for time-varying functions $g_i(\tau_i + T)$, which capture variables such as unobserved expectations of local government officials.

We construct synthetic control counties to control for time-varying unobservable characteristics. Specifically, we construct control counties by weighting counties that do not have a county or state mask mandate to match counties with a state or county mandate

$$\tilde{Y}_{i,\tau_i+1} = \sum_{j=1}^{N} w_j \cdot Y_{j,\tau_i+1} \tag{5}$$

where j indexes counties without mask mandates. Weights w_j are chosen to match pre-treatment characteristics of county i, which eventually imposes a mask mandate. For weights

w_j, we employ entropy weights as proposed by Hainmueller (2012). We generate these weights matching the mean on county population, mobility, change in cases, and spending in a pre-period.

Given our measurement in equation (5) is successful, we measure $Y_{i,\tau_i+T}(0) = g_i(\tau_i+T) + \varepsilon_{i,\tau_{i+T}}$, so we can correctly identify

$$\beta_T = E\left[Y_{i,\tau_i+T}(1) - Y_{i,\tau_i+T}(0)\middle|\mathbb{1}(\tau_i > 0), \{w_j\}\right]. \tag{6}$$

In Table 13 and Figure 4, we report the medium-run effects of mask mandates on mobility, cases per 100,000 people, and spending per person per month, separately for state and county mandates. Consistent with our short-run estimates in Table 2, we find that mobility continues the same trend as in the short-term—increasing after state mandates but decreasing following county mandates. We report as much in columns (1) and (4) of Table 13. For state mandates, medium-run mobility responses are about 9.4% higher than short-run increases, representing a moderate increase over the two months analyzed here. In contrast, the decline in mobility in response to county mask mandates in column (4) of Table 13 is four times higher in the medium run than in the short run, documented in column (4) of Table 2.

Despite this pattern in mobility, we find, as reported in columns (2) and (5) of Table 13, that cases per 100,000 people are substantially lower after a state enacts a mask mandate than if a county does. The quantitative impact of state mandates is similar in the short and medium run. In contrast, while the impact of county mandates on case growth is larger in the short run than that of state mandates, as shown in column (5) of Table 2, in the medium run, the case-growth effect becomes insignificant and even changes sign.

Finally, in the medium term, spending increases more after a state enacts a mask mandate than in the short-run. As shown in column (3) of Table 13, the stimulating effect of state mask mandates substantially increases over time. The medium-run spending effect of state mask mandates is 37% higher than the short-run spending effect in column (3) of Table 2. Therefore, the medium-run analysis reinforces the ability of state mask mandates to restrict case growth while simultaneously stimulating the economy. As in our short-run analysis, we fail to find similar benefits for county mask mandates.

In conclusion, state mandates achieve their main policy goals by increasing mobility and spending while reducing virus cases. In contrast, county mandates are ineffective at increasing economic activity and do not seem to lower case counts in the medium-run.

Additionally, in Appendix C, we show that the main medium-run results of state mask mandates—persistent beneficial effects for public health as well as economic activity—also emerge if we use a completely different identification strategy. In Appendix C, we deploy a differences-in-differences approach that uses adjacent counties to treatment counties as the control group while assigning as the event date for the control group the date of mask mandates for treatment counties. This strategy tends to strengthen our results, a surprising outcome given that the presence of local spillovers is likely to attenuate the effects. We discuss this point in detail in the appendix.

We interpret these results as evidence that county mask mandates' information revelation effects erode the potential economic benefits as consumers revise their perceived infection risk upward.

7.3 Number of transactions and spending per transaction

In this subsection, we briefly discuss how the number of transactions and spending per transaction change around implementing a mask mandate. We expect the number of transactions to be related to economic activity. For example, transactions may increase if people are shopping at more stores. Spending per transaction may also indicate elevated economic activity—especially if the number of transactions increases and spending per transaction increases.

In Figure A.6, we show the number of transactions and spending per transaction 25 days before and after a mask mandate. The left two panels indicate that the number of transactions and the spending per transaction increase in response to a state mask mandate. This evidence is consistent with our other estimates of economic activity increasing around the state mandate. The right two panels indicate that a county mandate did not produce a large change in the percent of transactions occurring and produced only a small increase in spending per transaction. Again, this evidence is consistent with our conclusion that mask mandates enacted at the county-level fail to stimulate the economy.

8 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 orders, 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:

$$Y = \gamma \cdot \Gamma + \delta \cdot B. \tag{7}$$

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:

$$\hat{\gamma} = \gamma + \delta \cdot \frac{Cov(P, B)}{Var[P]}. \tag{8}$$

For concreteness, consider the case in this paper—mask mandates. The first effect is that mask mandates increase mobility by making mobility safer ($\gamma > 0$) and 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 ($Cov(P, B) \neq 0$), which will decrease mobility for higher infection rates ($\delta < 0$).

Why does this omitted-variables bias matter if the net effect is driven by the same government policy? 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 $Cov(P, B) = 0$ and leading to overall economic effects being dominated by $\gamma > 0$. On the other hand, decentralized county-level mask mandates are very likely to reveal information about local risks, so that $Cov(P, B) > 0$.

9 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 implications for policy decisions surrounding COVID-19.

We document that state mask mandates during the 2020 COVID-19 pandemic reduced case growth and promoted economic activity, as measured by mobility 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, while generally successful at slowing case growth, also depressed economic activity. The stark contrast between the effects of state and county mask mandates should be a cautionary tale for policymakers.

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. This paper also shows the importance of mask-wearing in terms of economic activity. Mask mandates enforced at the state level increased consumer confidence and led to increased economic activity. Evidence from our survey 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.

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 imply a trade-off between lives and livelihoods on the county level and why they do not indicate such a trade-off on the state-level. 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
Mobility	-14.4	-14	15.7	295,373	-23.4	-24	15.8	19,347
Cases/pop	10.3	2.98	27.1	308,748	15.4	8.45	22.0	18,672
Spend/month	1,211	776	1,700	304,541	879	710	705	18,800
Red county	0.82	1	0.38	340,344	0.43	0	0.50	19,600
Red state	0.65	1	0.48	340,344	0.77	1	0.42	19,600
Urban county	0.23	0	0.42	340,344	0.76	1	0.43	19,600
High county	0.49	0	0.50	340,344	0.78	1	0.42	19,600
Early county	0.55	1	0.50	340,344	0.66	1	0.47	19,600
Comply county	0.67	1	0.47	340,344	0.87	1	0.34	19,600

NOTE.— This table presents summary statistics for all variables used in the analysis. We consider values from day -90 to day 90, where day 0 is a week 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. *Mobility* is relative to mobility 2019 using cell phone data in percent. *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. *Red county* is an indicator equal to one if the Republican party got more votes than the Democratic party on the 2016 Presidential Election, and zero otherwise. *Red state* is an indicator equal to one if the Republican party got more votes than the Democratic party in 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 were a mask always or frequently, and zero otherwise.

Table 2: Mobility, cases, and spending by state and county mask mandates

	Mandate					
	State			County		
	Mobility (1)	Cases/pop (2)	Spend/month (3)	Mobility (4)	Cases/pop (5)	Spend/month (6)
State mask	0.391***	-0.909***	23.892***			
	(0.078)	(0.341)	(6.712)			
County mask				-0.511**	-2.019*	-0.087
				(0.221)	(1.036)	(10.234)
Day fixed effects	✓	✓	✓	✓	✓	✓
County fixed effects	✓	✓	✓	✓	✓	✓
Adj. R-Square	0.917	0.209	0.930	0.939	0.526	0.921
Observations	87,300	99,437	97,801	5,787	5,779	5,754

NOTE.— This table reports changes in mobility, cases, and spending as a result from state and county mask orders. We consider a time period from day -25 to day 25, where day 0 is a week before the mask mandate for Mobility and Spend/month, and 10 days after the mandate for Cases/pop. *Mobility* is relative to mobility 2019 using cell phone data in percent. *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. *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. 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: Differences by political affiliation

	Mandate					
	State			County		
	Mobility (1)	Cases/pop (2)	Spend/month (3)	Mobility (4)	Cases/pop (5)	Spend/month (6)
State mask	0.707***	-2.581***	16.754			
	(0.209)	(0.941)	(14.877)			
State mask × Red county	-0.397	2.069**	8.819			
	(0.246)	(0.927)	(19.277)			
County mask				0.084	-2.145	-2.874
				(0.417)	(1.502)	(13.112)
County mask × Red county				-1.442*	0.304	6.698
				(0.825)	(2.808)	(19.315)
Day fixed effects	✓	✓	✓	✓	✓	✓
County fixed effects	✓	✓	✓	✓	✓	✓
Adj. R-Square	0.917	0.210	0.930	0.939	0.526	0.921
Observations	87,300	99,437	97,801	5,787	5,779	5,754

NOTE.— This table reports changes in mobility, cases, and spending as a result from state and county mask orders interacted with County level political leaning. We consider a time period from day -25 to day 25, where day 0 is a week before the mask mandate for Mobility and Spend/month, and 10 days after the mandate for Cases/pop. *Red county* is an indicator equal to one if the Republican party got more votes than the Democratic party on the 2016 Presidential Election, and zero otherwise. *Mobility* is relative to mobility 2019 using cell phone data in percent. *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. *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. 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 4: Differences by urban and rural counties

	Mandate					
	State			County		
	Mobility (1)	Cases/pop (2)	Spend/month (3)	Mobility (4)	Cases/pop (5)	Spend/month (6)
State mask	0.469***	-0.626	20.967***			
	(0.095)	(0.382)	(7.299)			
State mask × Urban county	-0.295	-1.230**	12.772			
	(0.189)	(0.568)	(8.729)			
County mask				1.205	-2.703	26.584
				(0.971)	(3.074)	(28.125)
County mask × Urban county				-2.274*	0.913	-35.524
				(1.183)	(3.871)	(33.684)
Day fixed effects	✓	✓	✓	✓	✓	✓
County fixed effects	✓	✓	✓	✓	✓	✓
Adj. R-Square	0.917	0.210	0.930	0.940	0.526	0.921
Observations	87,300	99,437	97,801	5,787	5,779	5,754

NOTE.— This table reports changes in mobility, cases, and spending as a result from state and county mask orders interacted with County level extent of Urban population. We consider a time period from day -25 to day 25, where day 0 is a week before the mask mandate for Mobility and Spend/month, and 10 days after the mandate for Cases/pop. *Urban county* is an indicator equal to one if urban areas include more than 50% of the county population, and zero otherwise. *Mobility* is relative to mobility 2019 using cell phone data in percent. *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. *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. 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 5: Differences by high and low case count counties

	Mandate					
	State			County		
	Mobility (1)	Cases/pop (2)	Spend/month (3)	Mobility (4)	Cases/pop (5)	Spend/month (6)
State mask	0.735***	1.021**	16.250*			
	(0.125)	(0.483)	(9.040)			
State mask × High county	-0.628***	-3.761***	14.982			
	(0.176)	(0.552)	(11.672)			
County mask				0.730	0.458	-2.230
				(0.780)	(1.849)	(31.467)
County mask × High county				-1.629*	-3.258	2.798
				(0.941)	(2.619)	(36.793)
Day fixed effects	✓	✓	✓	✓	✓	✓
County fixed effects	✓	✓	✓	✓	✓	✓
Adj. R-Square	0.917	0.211	0.930	0.939	0.526	0.921
Observations	87,300	99,437	97,801	5,787	5,779	5,754

NOTE.— This table reports changes in mobility, cases, and spending as a result from state and county mask orders interacted with County level case severity. We consider a time period from day -25 to day 25, where day 0 is a week before the mask mandate for Mobility and Spend/month, and 10 days after the mandate for Cases/pop. *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. *Mobility* is relative to mobility 2019 using cell phone data in percent. *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. *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. 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 6: Differences by early and late mandate counties

	Mandate					
	State			County		
	Mobility (1)	Cases/pop (2)	Spend/month (3)	Mobility (4)	Cases/pop (5)	Spend/month (6)
State mask	0.269**	0.388	-48.935***			
	(0.127)	(0.518)	(10.176)			
State mask × Early county	0.229	-2.484***	139.935***			
	(0.226)	(0.586)	(17.392)			
County mask				0.273	-0.735	-3.601
				(0.589)	(2.039)	(17.438)
County mask × Early county				-1.251	-2.051	5.621
				(0.852)	(2.662)	(23.528)
Day fixed effects	✓	✓	✓	✓	✓	✓
County fixed effects	✓	✓	✓	✓	✓	✓
Adj. R-Square	0.917	0.210	0.930	0.939	0.526	0.921
Observations	87,300	99,437	97,801	5,787	5,779	5,754

NOTE.— This table reports changes in mobility, cases, and spending as a result from state and county mask orders interacted with County level mandate timing. We consider a time period from day -25 to day 25, where day 0 is a week before the mask mandate for Mobility and Spend/month, and 10 days after the mandate for Cases/pop. *Early county* is an indicator equal to one if the mandate was issued on or before June 30th, and zero otherwise. *Mobility* is relative to mobility 2019 using cell phone data in percent. *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. *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. 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 7: Differences by compliance to wearing a mask

	Mandate					
	State			County		
	Mobility (1)	Cases/pop (2)	Spend/month (3)	Mobility (4)	Cases/pop (5)	Spend/month (6)
State mask	-0.021	0.024	18.048			
	(0.146)	(0.455)	(13.034)			
State mask × Comply county	0.595***	-1.409**	8.855			
	(0.177)	(0.568)	(14.700)			
County mask				1.897	6.204**	-2.817
				(1.280)	(3.038)	(21.079)
County mask × Comply county				-2.755*	-9.442***	3.129
				(1.425)	(3.490)	(22.421)
Day fixed effects	✓	✓	✓	✓	✓	✓
County fixed effects	✓	✓	✓	✓	✓	✓
Adj. R-Square	0.917	0.210	0.930	0.940	0.529	0.921
Observations	87,300	99,437	97,801	5,787	5,779	5,754

NOTE.— This table reports changes in mobility, cases, and spending as a result from state and county mask orders interacted with County level mark-wearing compliance, based on responses to a New York Times survey. We consider a time period from day -25 to day 25, where day 0 is a week before the mask mandate for Mobility and Spend/month, and 10 days after the mandate for Cases/pop. *Comply county* is an indicator equal to one if at 70% of people surveyed stated that they were a mask always or frequently, and zero otherwise. *Mobility* is relative to mobility 2019 using cell phone data in percent. *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. *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. 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 8: Regression Discontinuity Estimation in Event Time

	Mandate					
	State			County		
	Mobility	Cases/pop	Spend/month	Mobility	Cases/pop	Spend/month
	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.912***	-0.643	131.581***	-0.341	-2.174*	51.631
	(0.323)	(0.435)	(23.371)	(0.954)	(1.283)	(41.010)
Optimal BW	15.1	19.8	28.3	26.0	37.3	22.8
Observations	396,565	374,594	361,645	25,688	23,300	22,382

NOTE.— This table reports changes in mobility, cases, and spending in a regression discontinuity framework using event time as the forcing variable. We consider a time period from day -25 to day 25, where day 0 is a week before the mask mandate for Mobility and Spend/month, and 10 days after the mandate for Cases/pop. *Mobility* is relative to mobility 2019 using cell phone data in percent. *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. *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. 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 9: Parametric Event Study with Event Time Controls

Panel A: Linear Event time Control

	Mandate					
	State			County		
	Mobility (1)	Cases/pop (2)	Spend/month (3)	Mobility (4)	Cases/pop (5)	Spend/month (6)
State mask	0.385***	-0.916***	23.359***			
	(0.079)	(0.337)	(6.656)			
County mask				-0.576**	-2.207**	0.869
				(0.223)	(1.024)	(10.122)
Linear Event-time Controls	✓	✓	✓	✓	✓	✓
Day fixed effects	✓	✓	✓	✓	✓	✓
County fixed effects	✓	✓	✓	✓	✓	✓
Adj. R-Square	0.917	0.209	0.930	0.939	0.527	0.921
Observations	87,300	99,437	97,801	5,787	5,779	5,754

Panel B: Cubic Event time Control

	Mandate					
	State			County		
	Mobility (1)	Cases/pop (2)	Spend/month (3)	Mobility (4)	Cases/pop (5)	Spend/month (6)
State mask	0.855***	-2.049***	71.251***			
	(0.142)	(0.639)	(14.413)			
County mask				-1.007***	2.653	31.331
				(0.375)	(1.672)	(22.931)
Cubic Event-time Controls	✓	✓	✓	✓	✓	✓
Day fixed effects	✓	✓	✓	✓	✓	✓
County fixed effects	✓	✓	✓	✓	✓	✓
Adj. R-Square	0.917	0.210	0.930	0.939	0.528	0.921
Observations	87,300	99,437	97,801	5,787	5,779	5,754

NOTE.— This table reports changes in mobility, cases, and spending mirroring the specification in Table 2, with the addition of *Event Time* as a control variable interacted with the State and County mask variables. Panel A includes event time as a linear interacted control, and Panel B includes event time as a linear, second, and third order controls. We consider a time period from day -25 to day 25, where day 0 is a week before the mask mandate for Mobility and Spend/month, and 10 days after the mandate for Cases/pop. *Mobility* is relative to mobility 2019 using cell phone data in percent. *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. *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. 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 10: Location Placebo: Mobility, cases, and spending by state and county mask mandates

	Mandate					
	State			County		
	Mobility (1)	Cases/pop (2)	Spend/month (3)	Mobility (4)	Cases/pop (5)	Spend/month (6)
Panel A: Event Study						
State mask	0.339	0.813	18.849			
	(0.218)	(0.855)	(13.567)			
County mask				-0.001	1.612	-0.148
				(0.243)	(1.010)	(14.312)
Day fixed effects	✓	✓	✓	✓	✓	✓
County fixed effects	✓	✓	✓	✓	✓	✓
Adj. R-Square	0.902	0.289	0.892	0.886	0.294	0.915
Observations	9,582	11,611	11,437	7,472	8,133	8,087
Panel B: Regression Discontinuity						
	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	1.199	0.145	48.540	-0.050	0.630	0.615
	(0.802)	(0.855)	(39.644)	(0.798)	(1.166)	(49.346)
Optimal BW	19.2	30.6	27.3	22.8	27.1	25.4
Observations	43,663	42,675	41,324	34,145	30,865	29,683

NOTE.— This table reports results of location placebo for changes in mobility, cases, and spending as a result from state and county mask orders. Location placebos are defined as counties adjacent to counties with mask mandates, but not subject to mask mandates themselves. Event time for placebo counties is set to average event-time for adjacent counties with mask mandates. We consider a time period from day -25 to day 25, where day 0 is a week before the mask mandate for Mobility and Spend/month, and 10 days after the mandate for Cases/pop. *Mobility* is relative to mobility 2019 using cell phone data in percent. *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. *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. 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 11: Time placebo: Mobility, cases, and spending by state and county mask mandates

	Mandate					
	State			County		
	Mobility (1)	Cases/pop (2)	Spend/month (3)	Mobility (4)	Cases/pop (5)	Spend/month (6)
Panel A: Event Study						
State mask	-0.001	-0.139	-15.582			
	(0.116)	(0.421)	(11.483)			
County mask				-0.288	-0.451	-0.634
				(0.343)	(0.397)	(19.429)
Day fixed effects	✓	✓	✓	✓	✓	✓
County fixed effects	✓	✓	✓	✓	✓	✓
Adj. R-Square	0.894	0.222	0.860	0.944	0.365	0.908
Observations	71,530	59,556	47,843	5,006	4,800	4,033
Panel B: Regression Discontinuity						
	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.069	0.545	29.445	0.358	-0.257	-27.501
	(0.440)	(0.409)	(38.752)	(1.483)	(0.450)	(57.665)
Optimal BW	10.1	14.9	13.0	13.6	21.3	22.1
Observations	396,565	374,594	361,645	25,688	23,300	22,382

NOTE.— This table reports results of a time placebo for changes in mobility, cases, and spending as a result from state and county mask orders. Time placebos are determined by randomly reassigning an event time at least 75 days before the true event time. Event time for placebo counties is set to average event-time for adjacent counties with mask mandates. We consider a time period from day -25 to day 25. *Mobility* is relative to mobility 2019 using cell phone data in percent. *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. *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. 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 12: Survey Evidence

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%	12.62%
	(1.49)
The number of confirmed cases fell by 50%	29.62%
	(1.69)
The number of confirmed cases fell by 90%	56.82%
	(2.22)
Half of the people were wearing a mask	-13.48%
	(1.93)
Everyone was wearing a mask	50.72%
	(2.28)
The store enforced wearing a mask	49.73%
	(2.50)
The state enforced wearing a mask	48.76%
	(2.79)
Observations	305

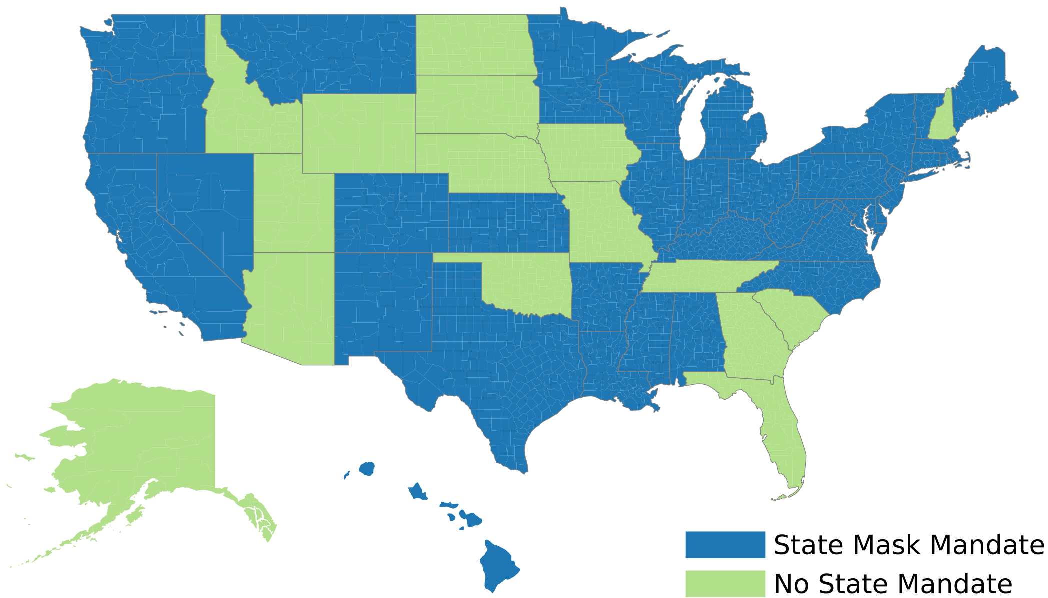
NOTE.— Survey evidence from the October and November Utah Consumer Sentiment Survey. Standard errors in parentheses.

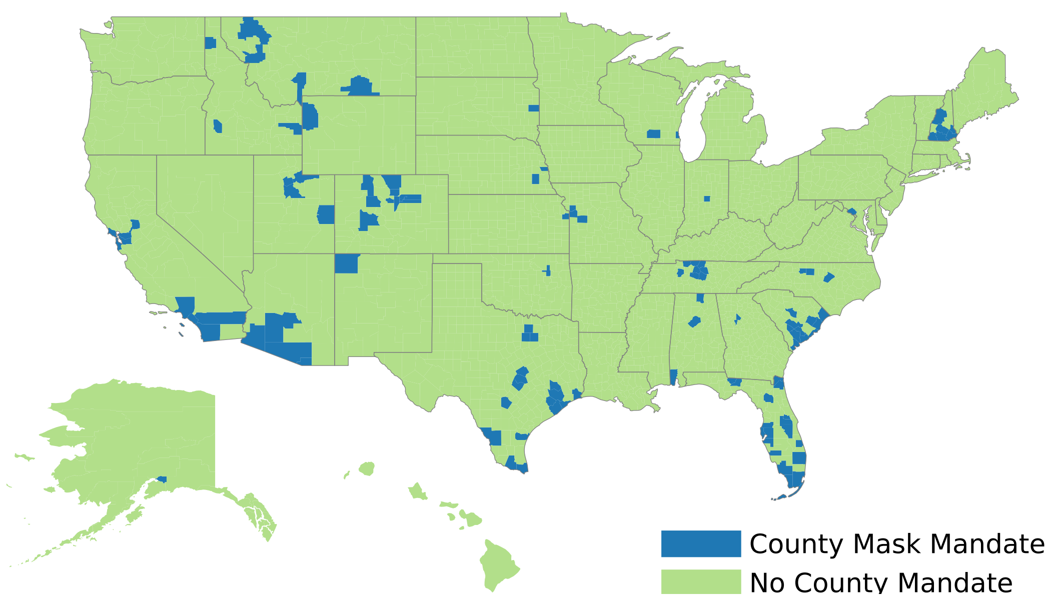
Table 13: Synthetic Controls in the medium-run

	Mandate					
	State			County		
	Mobility (1)	Cases/pop (2)	Spend/month (3)	Mobility (4)	Cases/pop (5)	Spend/month (6)
State mask	0.428*	-0.814***	32.809***			
	(0.221)	(0.301)	(4.373)			
County mask				-2.246***	0.947	30.499
				(0.614)	(1.902)	(22.471)
Day fixed effects	✓	✓	✓	✓	✓	✓
County fixed effects	✓	✓	✓	✓	✓	✓
R-Squared	0.056	0.022	0.054	0.12	0.057	0.092
Observations	153,172	157,812	160,360	8,173	7,980	8,100

NOTE.— This table reports changes in mobility, cases, and spending using synthetic control methods developed by Abadie et al. (2010, 2015) and Abadie and Gardeazabal (2003) and calculate entropy weights following Hainmueller (2012). The control group are all counties that are not subject to a mask mandate. We consider a time period from day -25 to day 75, where day 0 is a week before the mask mandate for Mobility and Spend/month, and 10 days after the mandate for Cases/pop. *Mobility* is relative to mobility 2019 using cell phone data in percent. *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. *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. Standard errors clustered at the county level are in parentheses. We denote statistical significance by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

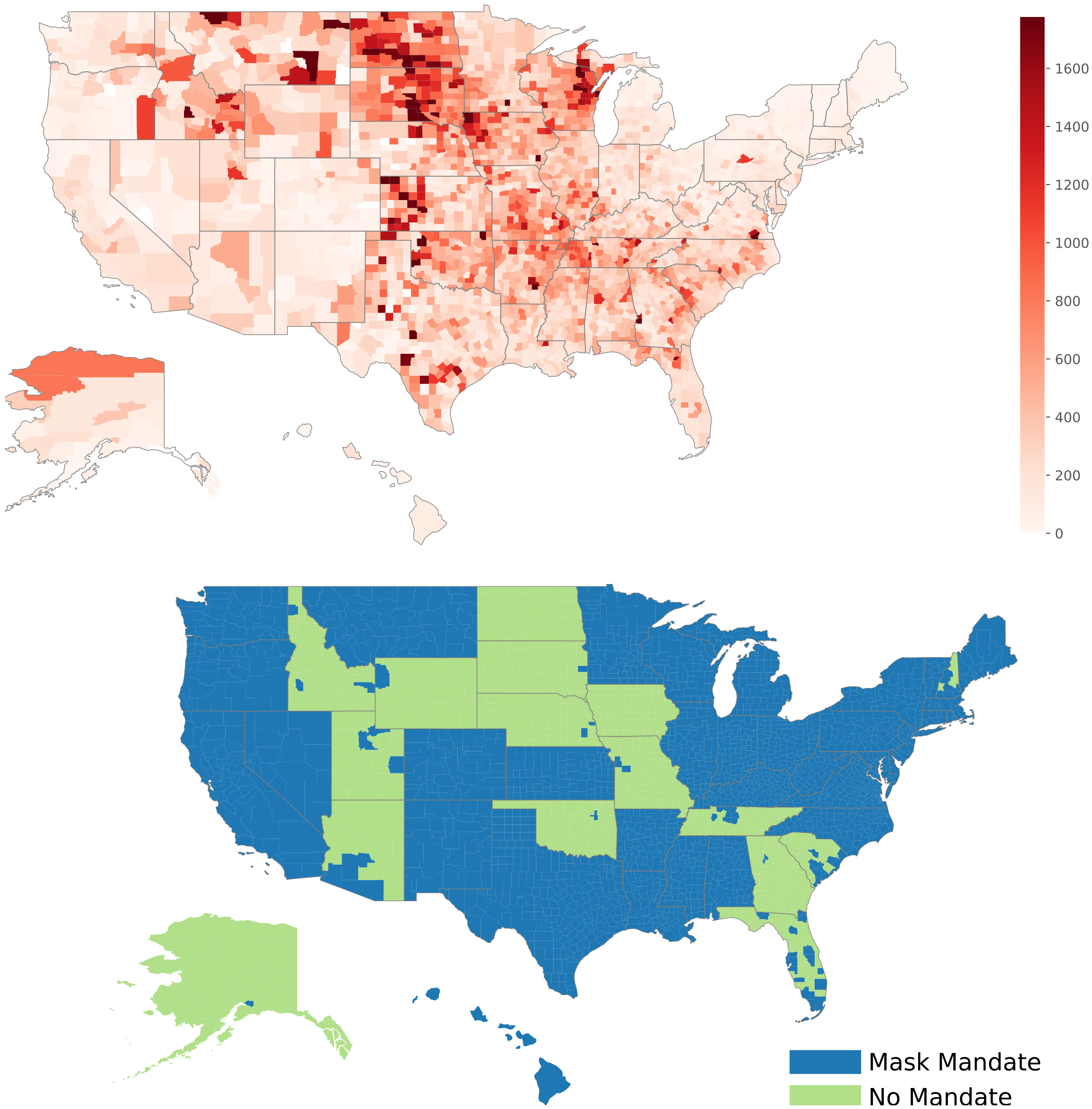
Figure 1: Mandatory Mask Mandates Map





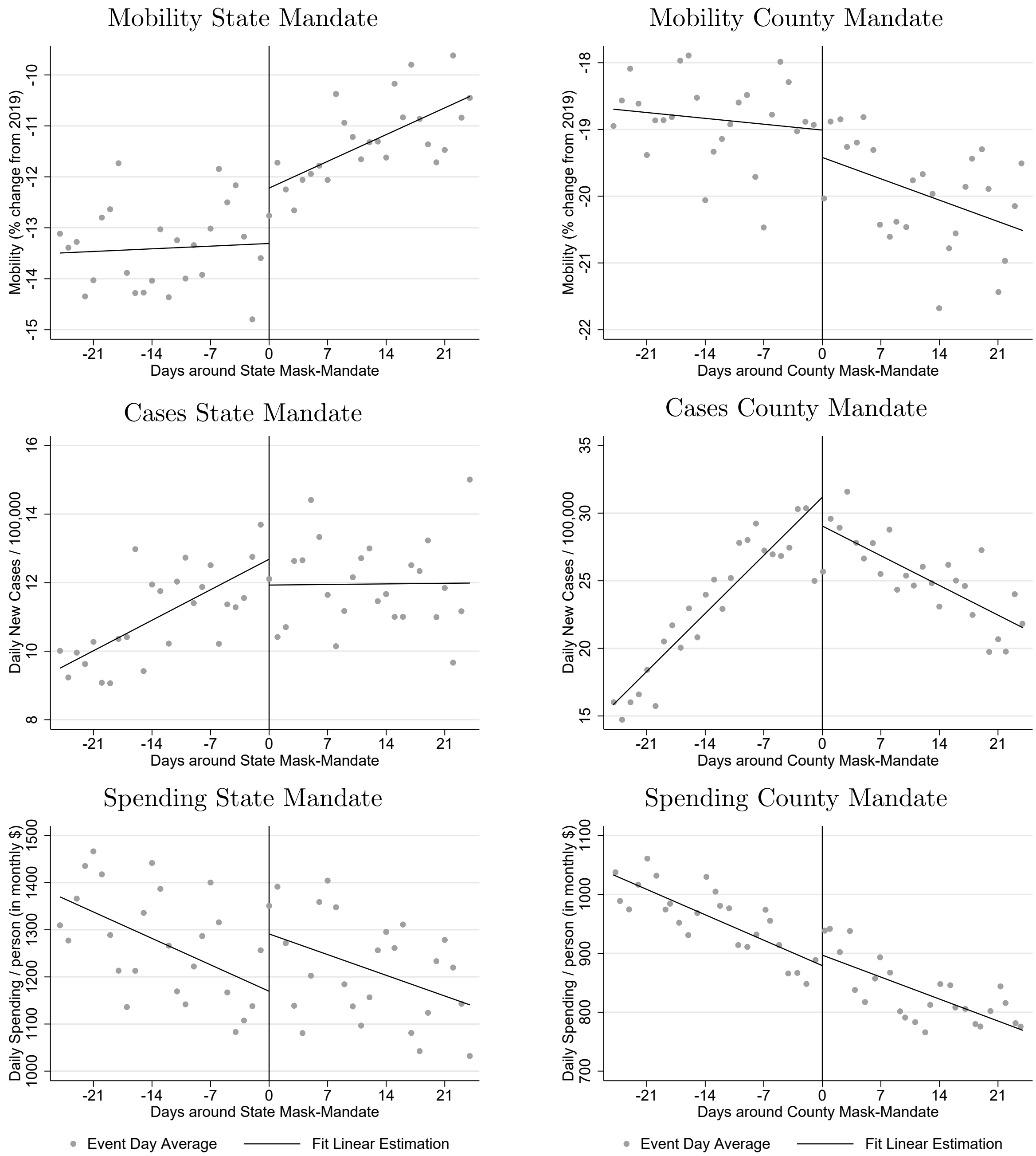
NOTE.— Figure 1 shows maps of the county-level mandate counties, and the state-level mandate states. Many of the county-level mandates come from states which eventually also had a state level mandate.

Figure 2: Covid-19 Case Rates in August



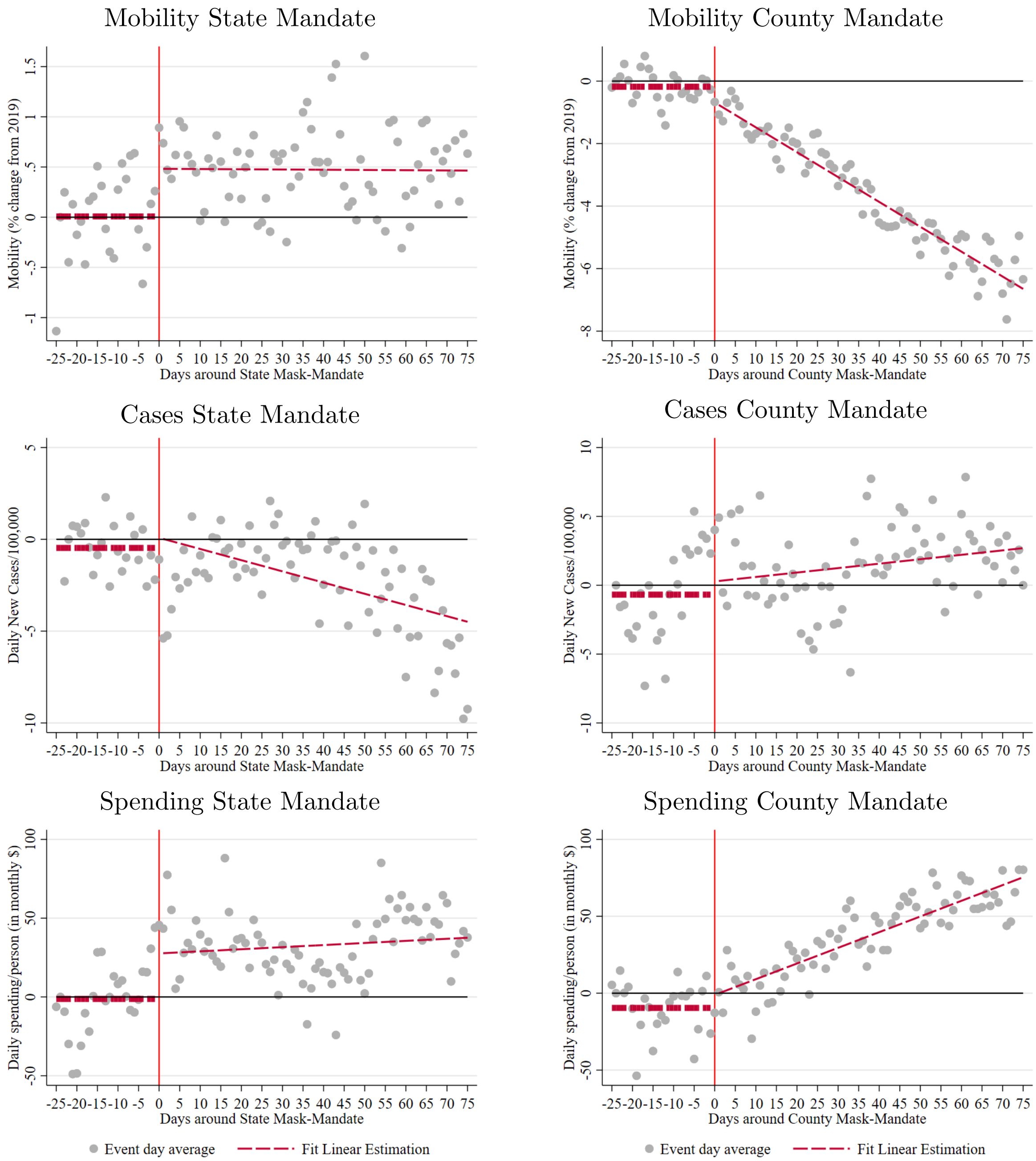
NOTE.— Figure 2 shows a map of the county-level active coronavirus cases per 100,000 people as of August 1st, and under that for comparison, the map of counties and states with *any* mandate in place (which is the combination of the State and County maps from Figure 1). Coronavirus case rate data come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

Figure 3: Mobility, cases, and spending around mask mandate for county or state mandates



NOTE.— Figure 3 shows how mobility, cases per 100,000 people, and spending per person per month change around the implementation of a mandate.

Figure 4: Medium-Run Synthetic Controls



NOTE.— Figure 4 shows how mobility, cases per 100,000 people, and spending per person per month change around the implementation of a mandate.

Appendix A Survey data

The survey evidence we report in Section 7.1 is from a short survey we conducted in conjunction with the State of Utah on consumer sentiment.[12] Participants were recruited by mail and included a link to a survey. 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 and the November waves of the survey. Participants were paid $10 to take the survey in the November wave.

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%	87%
Hispanic	14%	11%
Median income	$68,374	$50,000–$74,999
Bachelor's or higher	33%	50%

Appendix B Joint Estimation of Heterogeneous Treatment Effects

In this section we document that almost all of our analysis of treatment effect heterogeneity from section 5 is robust to joint estimation of all interaction effects. Since most of the analyzed effects remain qualitatively and quantitatively similar, we focus here and just three interactions that are affected by the joint estimation.

The comparison of table A.3 with the results in table 4 shows that the joint analysis of all interaction terms shifts some of the effects of urban counties. For example, responses of case growth to state mask mandates fall by about 60% and become insignificant in the joint analysis. At the same time, the response of case growth to county mask mandates turns strongly positive and significant. This suggests that the mobility decline through county mask mandates is insufficient to offset higher virus exposure in denser areas.

Additionally, the joint analysis of all interaction terms renders the response of early counties to state mask mandates insignificant. This is most likely related to the fact that early counties with mask mandates were primarily in urban and liberal areas.

[12]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/

Appendix C Differences-in-Differences Approach

This appendix extends our analysis to the medium-run using a difference-in-differences approach. In Table A.4 and Figure A.7, we report estimates from a differences-in-differences analysis. The short-run estimates rely on the identifying assumption that all omitted variables are continuous around the time of the mask mandate. This assumption becomes less tenable in the medium-run. To control for potential conflating factors, we rely on a control group of counties adjacent to counties that are ever subject to a county or state mask mandate. We assign an event time to the control counties equal to that of the adjacent county subject to a mask mandate. This differences-in-differences specification relies on the parallel trend assumption, which we investigate in Figure A.7. We find no evidence of differential trends for any of our dependent variables before a government enacts a mask mandate. A control group allows us to estimate up to 75 days after a government enacts a mask mandate.

We also note that control counties' geographic adjacency to treatment counties is likely to bias our estimates against finding results. This bias is the result of possible spillovers across adjacent counties. For example, suppose as a result of a mask mandate in county A, people in the adjacent county B also increase their mobility. Our differences-in-differences strategy can only identify the difference between the mobility increase in county A and county B, which is smaller than the absolute value for the mobility increase in county A. Similar arguments can be made for spillovers in spending or case growth.

The impact of state mask mandates on mobility and case growth is $4-5\times$ larger than the immediate short-run impact we report in Table 2. Specifically, in column (1) of Table A.4, we report that mobility is 1.3 percentage points higher after a state enacts a mask mandate and cases are lower by five people per 100,000. These results confirm that the treatment effects of state mask mandates are persistent and accumulate over time, as would be the case if mask mandates can consistently impact the trajectory of COVID-19 infection risk.

The opposite dynamic is at work in counties with county-only mask mandates. We report, in the last three columns of Table A.4, that mobility strongly increases over the medium-run, after immediately dropping as documented in Table 2. This transitory negative effect is consistent with the view that information effects are transient in nature and that the medium-run effects of mask orders are qualitatively the same for state and county mask mandates.

Table A.1: Correlations

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: State Mandates								
(1) Mobility	1							
(2) Cases/pop	-0.00	1						
(3) Spend/month	0.00	0.04*	1					
(4) Red county	0.26*	-0.04*	0.03*	1				
(5) Red state	0.12*	0.08*	0.21*	0.26*	1			
(6) Urban county	-0.20*	0.02*	-0.14*	-0.33*	-0.16*	1		
(7) High county	0.01*	0.08*	0.01*	-0.11*	0.07*	0.19*	1	
(8) Early county	-0.11*	-0.06*	-0.10*	-0.08*	-0.23*	0.06*	-0.18*	1
(9) Comply county	-0.15*	-0.01*	-0.02*	-0.23*	-0.21*	0.25*	-0.05*	0.37*
Panel B: County Mandates								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(1) Mobility	1							
(2) Cases/pop	0.13*	1						
(3) Spend/month	0.16*	0.04*	1					
(4) Red county	0.28*	0.03*	-0.03*	1				
(5) Red state	0.18*	0.21*	0.25*	0.35*	1			
(6) Urban county	-0.15*	0.01	0.03*	-0.03*	0.06*	1		
(7) High county	-0.01	0.16*	0.05*	0.12*	0.41*	0.08*	1	
(8) Early county	-0.20*	0.01	-0.02	-0.10*	-0.18*	0.15*	-0.14*	1
(9) Comply county	-0.30*	-0.01	0.02*	-0.17*	-0.20*	0.38*	-0.08*	0.26*

NOTE.— This table presents correlations for all variables used in the analysis when the mandate was ordered by the state. We consider values from day -90 to day 90, where day 0 is a week before the mask mandate for Mobility and Spend/month, and 10 days after the mandate for Cases/pop. Definitions are from Table 1. * denotes the correlation is significant at the 1% level.

Table A.2: Differences by political affiliation extended

	State Mandate		
	Mobility	Cases/pop	Spend/person
	(1)	(2)	(3)
State mask	1.138***	-3.555***	-4.727
	(0.310)	(0.808)	(24.068)
State mask × Red county	-0.224	2.119**	31.343
	(0.375)	(0.910)	(27.455)
State mask × Red state	-1.030**	2.283	52.468
	(0.432)	(1.996)	(34.327)
State mask × Red state × Red county	0.154	-0.962	-54.015
	(0.471)	(2.048)	(33.416)
Day fixed effects	✓	✓	✓
County fixed effects	✓	✓	✓
Adj. R-Square	0.917	0.210	0.930
Observations	87,300	99,437	97,801

NOTE.— From day -25 to day 25, where day 0 is a week before the mask mandate. Red county is a dummy with value 1 if the Republican party got more votes than the Democratic party on the 2016 Presidential Election on that county and 0 if the Democratic party got more votes than the Republican party. Red state is a dummy with value 1 if if the Republican party got more votes than the Democratic party on the 2016 Presidential Election on that state and 0 if the Democratic party got more votes than the Republican party.

Table A.3: Mobility, cases, and spending by state and county mask mandates

	Mandate					
	State			County		
	Mobility (1)	Cases/pop (2)	Spend/month (3)	Mobility (4)	Cases/pop (5)	Spend/month (6)
State mask	0.795**	1.740	-73.477***			
	(0.318)	(1.307)	(25.086)			
State mask × Red county	-0.502*	1.425	15.635			
	(0.276)	(1.014)	(21.528)			
State mask × Urban county	-0.459**	0.448	11.317			
	(0.221)	(0.707)	(12.067)			
State mask × High county	-0.599***	-3.799***	20.878*			
	(0.184)	(0.576)	(11.972)			
State mask × Early county	0.028	-2.757***	143.021***			
	(0.239)	(0.633)	(18.995)			
State mask × Comply county	0.619***	-0.779	-4.487			
	(0.189)	(0.598)	(15.790)			
County mask				4.922***	8.698**	-3.003
				(1.619)	(4.336)	(41.614)
County mask × Red county				-1.541*	-0.465	8.536
				(0.794)	(2.686)	(19.056)
County mask × Urban county				-1.442	4.556	-43.610
				(1.175)	(4.015)	(36.012)
County mask × High county				-1.470	-3.973	7.898
				(0.894)	(2.466)	(33.439)
County mask × Early county				-1.026	-1.334	7.688
				(0.797)	(2.916)	(22.718)
County mask × Comply county				-2.226*	-11.572***	24.358
				(1.307)	(4.029)	(29.267)
Day fixed effects	✓	✓	✓	✓	✓	✓
County fixed effects	✓	✓	✓	✓	✓	✓
Adj. R-Square	0.917	0.211	0.930	0.941	0.531	0.921
Observations	87,300	99,437	97,801	5,787	5,779	5,754

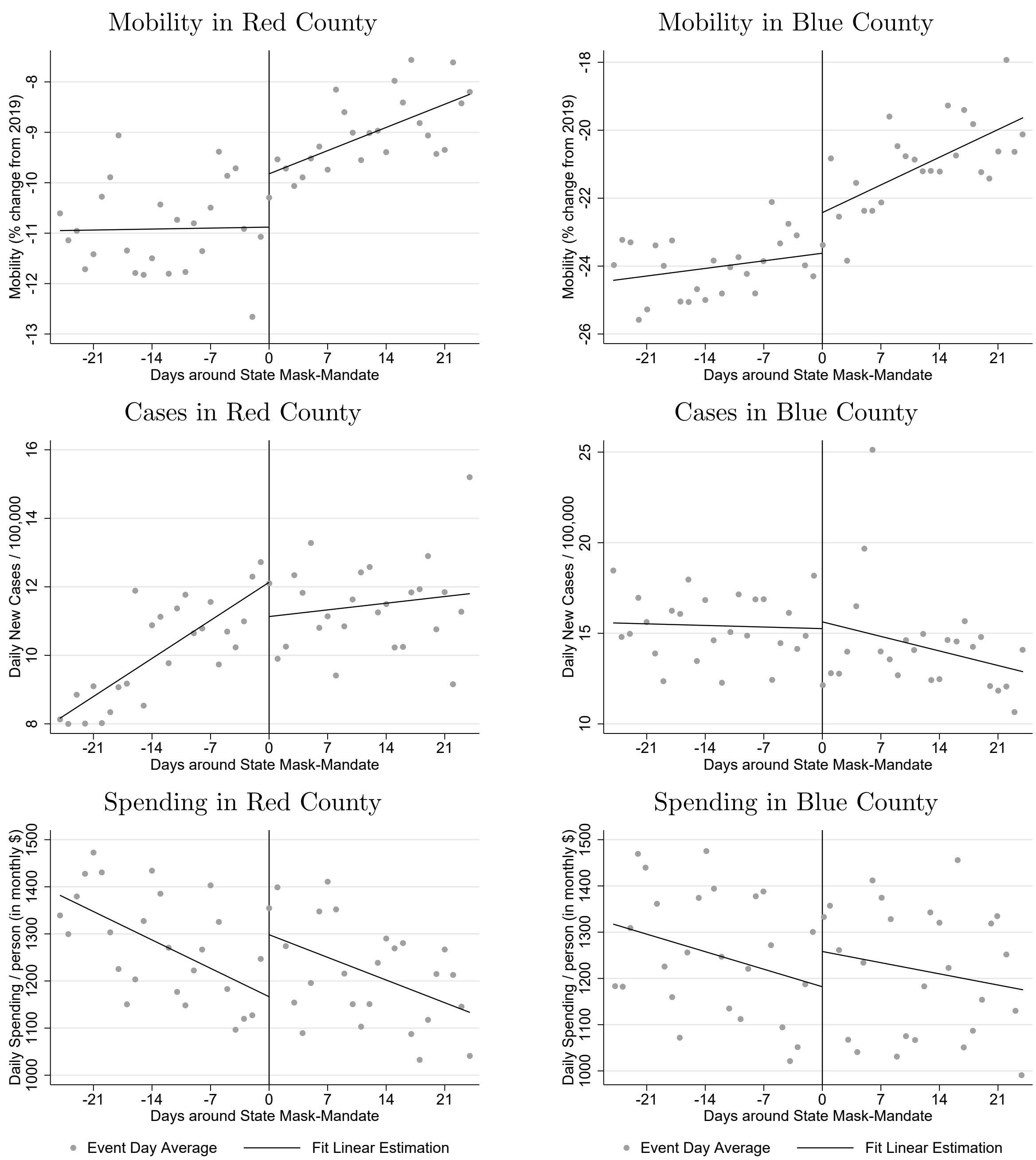
NOTE.— This table reports changes in mobility, cases, and spending as a result from state and county mask orders. We consider a time period from day -25 to day 25, where day 0 is a week before the mask mandate for Mobility and Spend/month, and 10 days after the mandate for Cases/pop. *Mobility* is relative to mobility 2019 using cell phone data in percent. *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. *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. 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.4: Differences-in-Differences in the medium-run

	Mandate					
	State			County		
	Mobility (1)	Cases/pop (2)	Spend/month (3)	Mobility (4)	Cases/pop (5)	Spend/month (6)
State mask	1.259**	-5.179***	10.592			
	(0.572)	(1.155)	(20.631)			
County mask				2.746***	-0.457	121.978***
				(1.024)	(1.726)	(33.270)
Day fixed effects	✓	✓	✓	✓	✓	✓
County fixed effects	✓	✓	✓	✓	✓	✓
R-Square	0.394	0.107	0.868	0.552	0.191	0.819
Observations	284,124	307,265	301,034	38,245	38,234	38,723

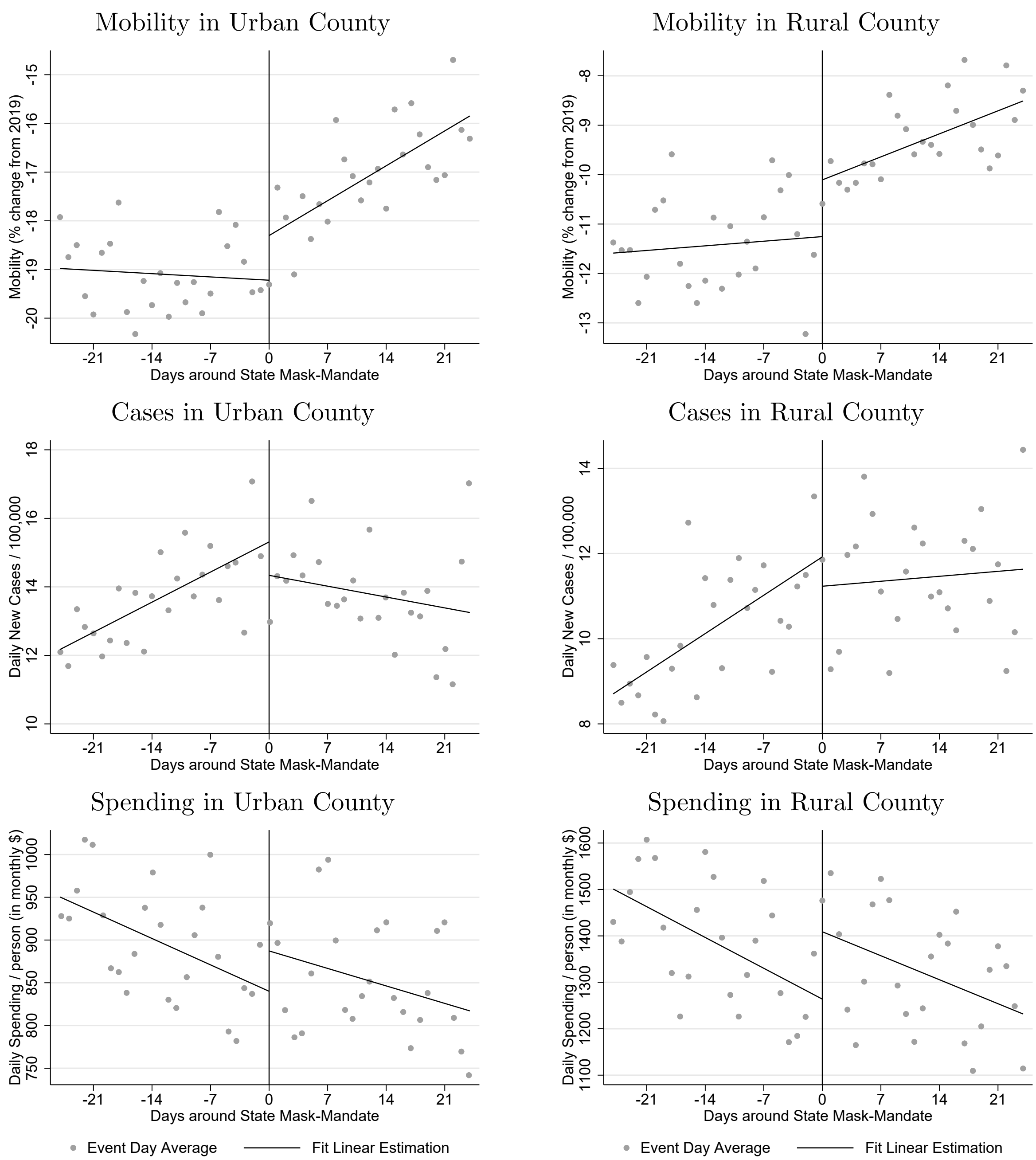
NOTE.— This table reports changes in mobility, cases, and spending in a differences-in-differences framework using event time and whether the county has a mask mandate as the two dimensions. The control group are all counties that are adjacent to treated counties that do not ever have a mask mandate. We consider a time period from day -75 to day 75, where day 0 is a week before the mask mandate for Mobility and Spend/month, and 10 days after the mandate for Cases/pop. *Mobility* is relative to mobility 2019 using cell phone data in percent. *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. *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. 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: Mobility, cases, and spending around mask mandate for county or state mandates by political affiliation



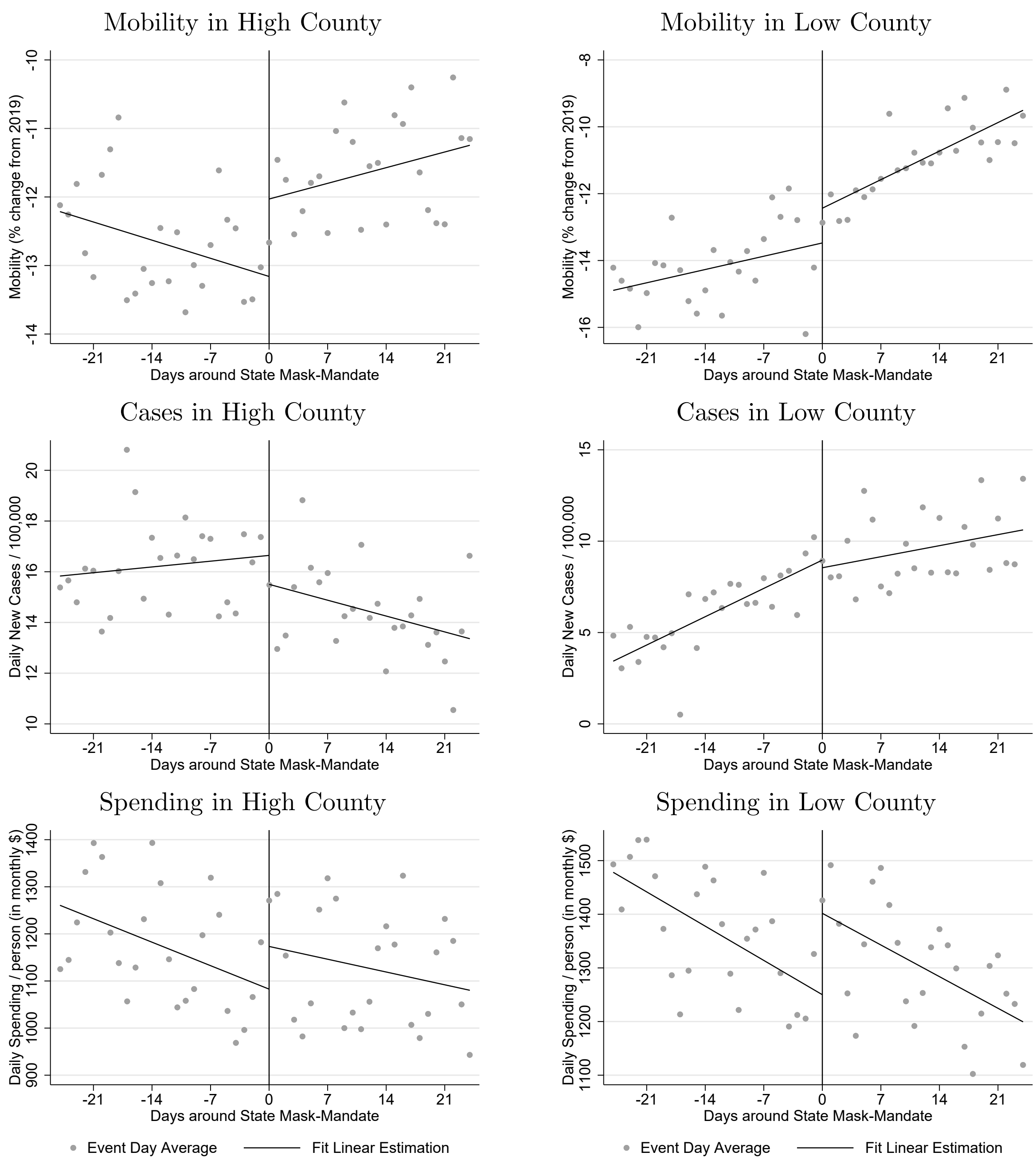
NOTE.— Figure A.1 shows how mobility, cases per 100,000 people, and spending per person per month change around the implementation of a state mandate, differentiated by average State political affiliation of the county (Red or Blue for majority conservative and majority non-conservative respectively.

Figure A.2: Mobility, cases, and spending around mask mandate for county or state mandates by urban vs rural



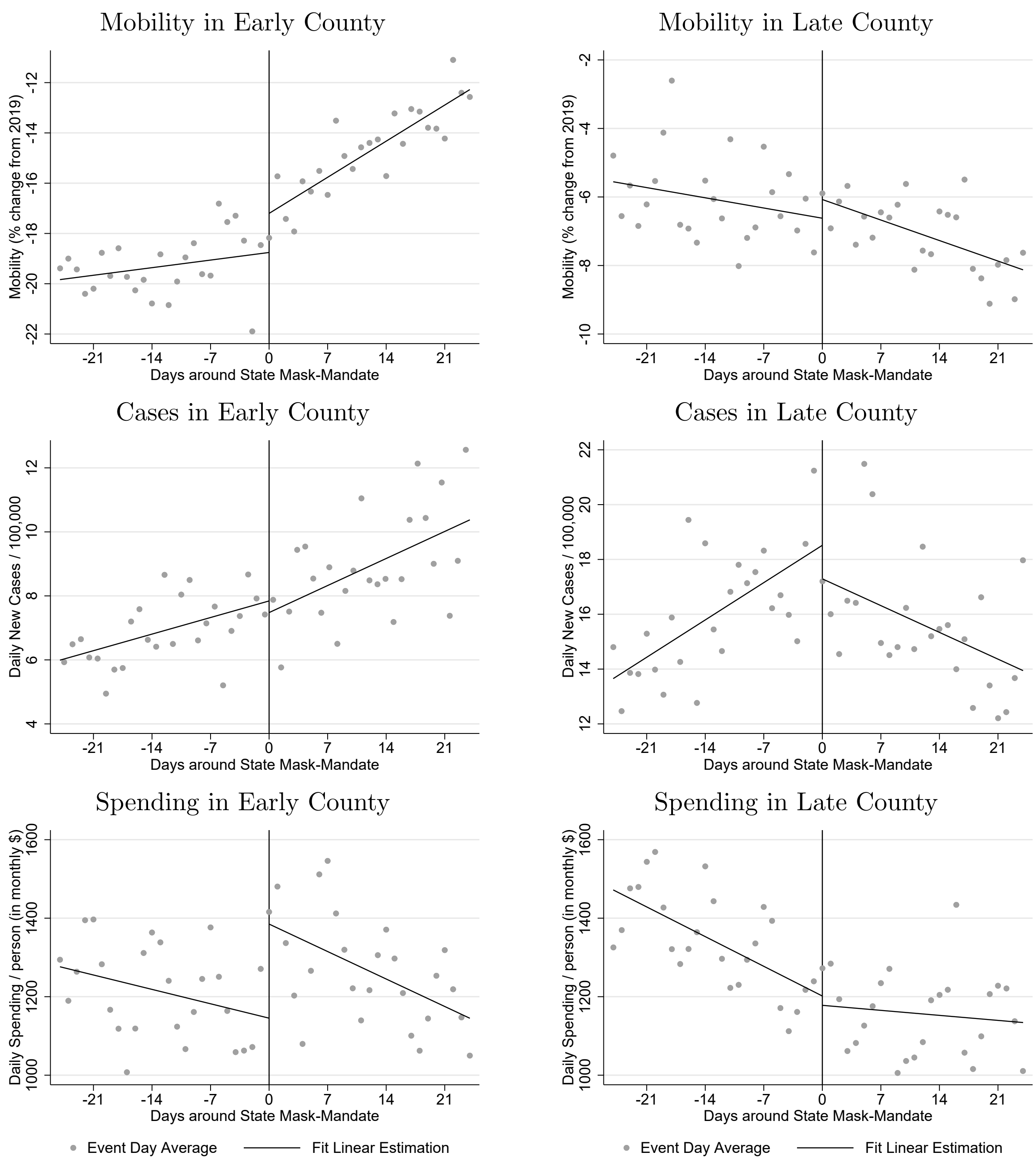
NOTE.— Figure A.2 shows how mobility, cases per 100,000 people, and spending per person per month change around the implementation of a state mandate, differentiated by whether the majority of population lives in an urban center.

Figure A.3: Mobility, cases, and spending around mask mandate for county or state mandates by infection severity



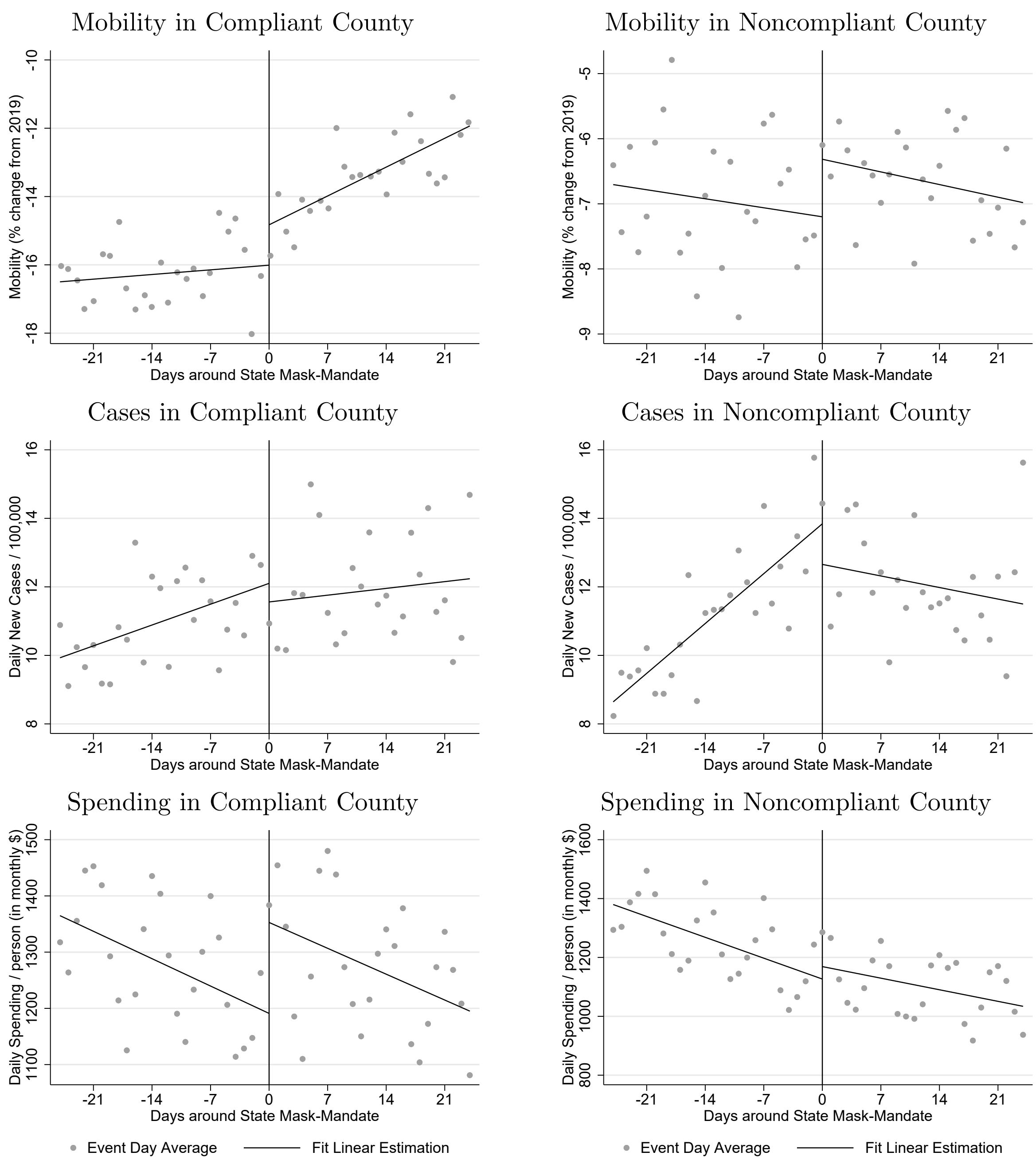
NOTE.— Figure A.3 shows how mobility, cases per 100,000 people, and spending per person per month change around the implementation of a state mandate, differentiated by whether the county had above median event-day 0 new cases per 100,000.

Figure A.4: Mobility, cases, and spending around mask mandate for county or state mandates by mandate timing



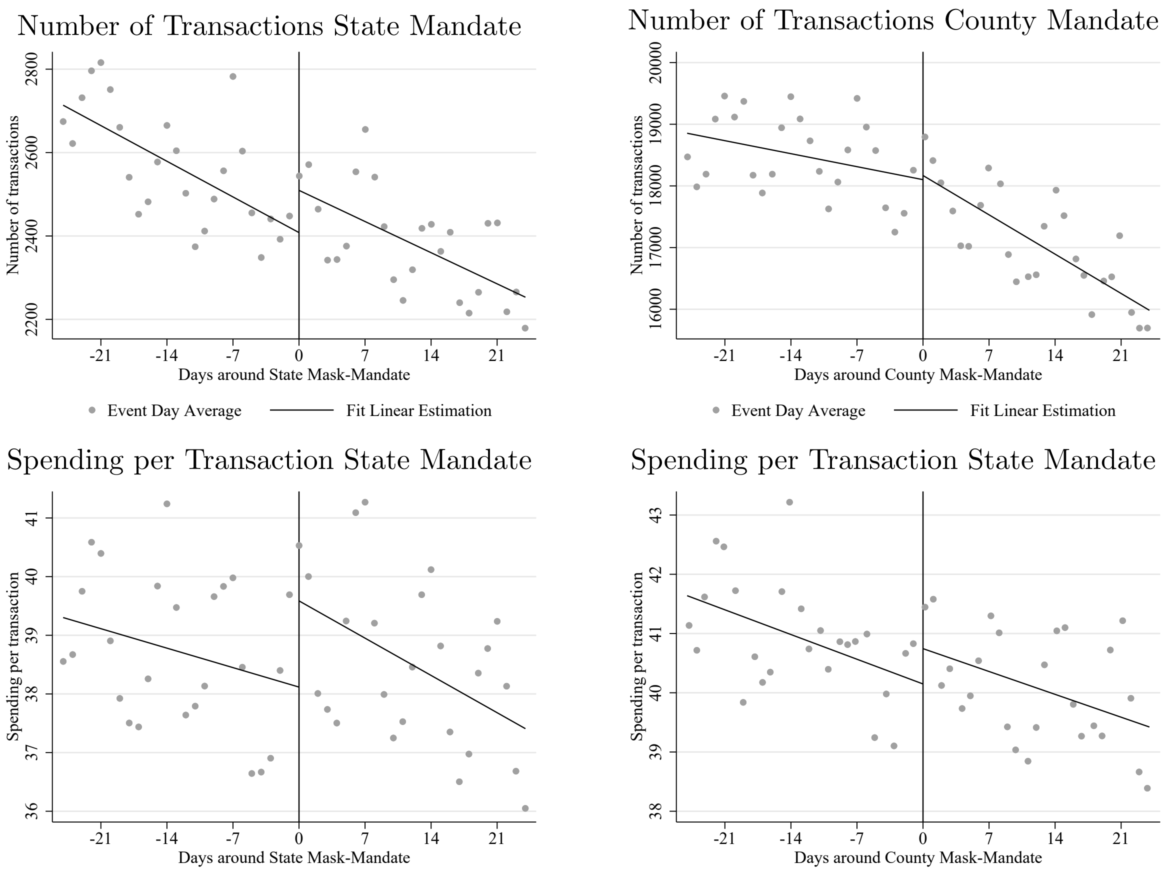
NOTE.— Figure A.4 shows how mobility, cases per 100,000 people, and spending per person per month change around the implementation of a state mandate, differentiated by whether the county put the mandate in place in June or before (Early) or after June (Late).

Figure A.5: Mobility, cases, and spending around mask mandate for county or state mandates by mandate compliance



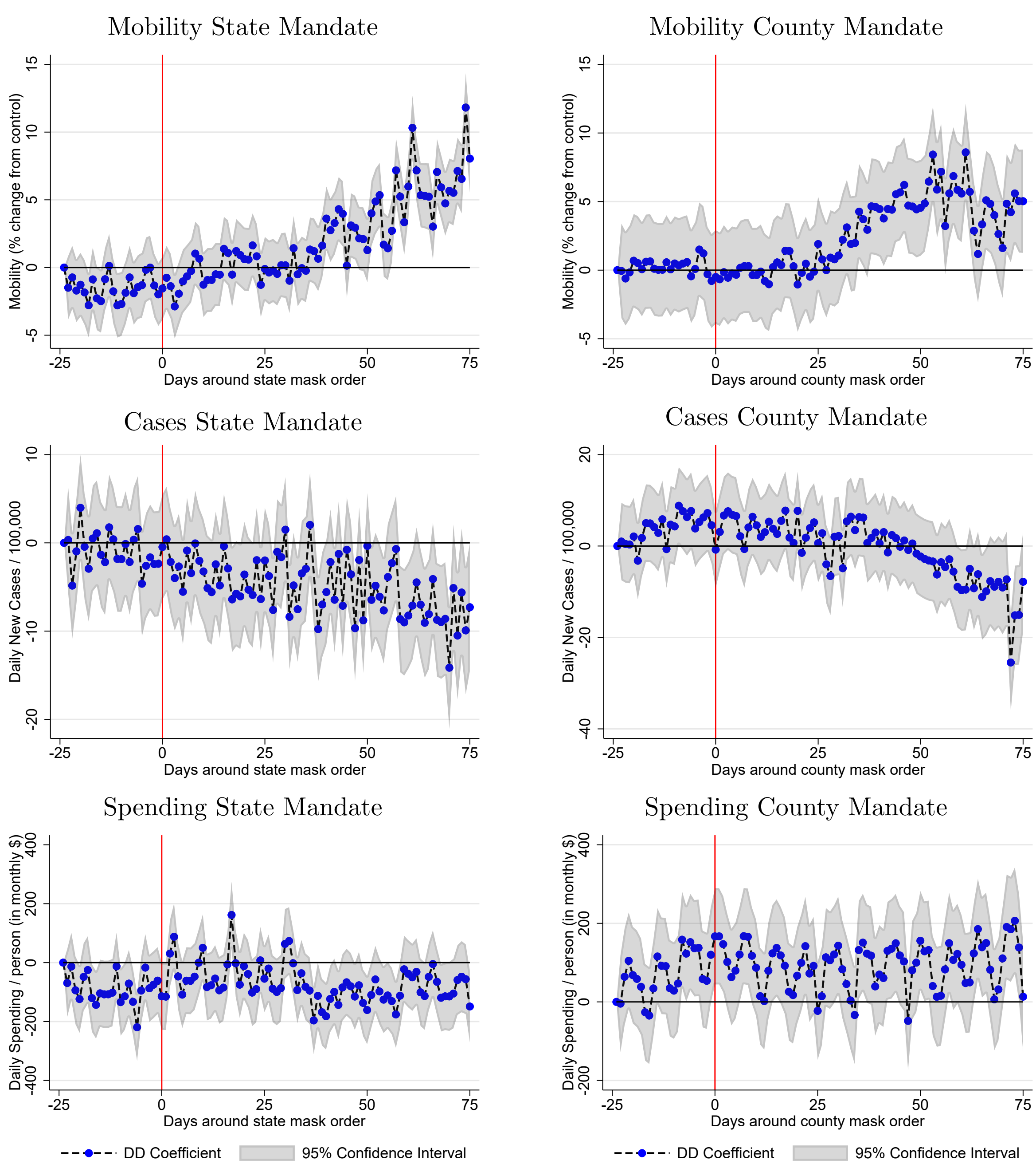
NOTE.— Figure A.5 shows how mobility, cases per 100,000 people, and spending per person per month change around the implementation of a state mandate, differentiated by whether more than 70% of survey respondents said they *always* or *frequently* wear masks, based on the New York Times published survey data.

Figure A.6: Number of transactions and spending per transaction for county or state mandates



NOTE.— Figure A.6 shows how the number of transactions and spending per transaction change around the implementation of a mask mandate.

Figure A.7: Mobility, cases, and spending around mask mandate for county or state mandates compared to adjacent non-mandate counties



NOTE.— Figure A.7 shows how mobility, cases per 100,000 people, and spending per person per month change around the implementation of a mandate compared to a set of geographically adjacent control counties.