Economic and Health Impacts of Social Distancing Policies during the Coronavirus Pandemic

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Abstract

Many US states and local areas have implemented stay-at-home and business closure policies to reduce the spread of coronavirus. We estimate the effects of these policies on social distancing and economic outcomes using event study designs, and we estimate the effects on disease transmission using a regression framework derived from an epidemiological model. We find that stay-at-home orders reduce the rate of disease transmission by 5–10 percent, and also reduce measures of economic activity by about the same amount in the short-run. A decomposition exercise suggests that the ratio of health benefits to economic impacts is similar for the social distancing induced by policy and the largely voluntary social distancing that preceded it.

1

1 Introduction

Many US states have imposed and are now gradually removing regulations intended to reduce the spread of the novel coronavirus. In March and April of 2020, 42 states imposed stay-at-home orders, and many states and local areas closed non-essential businesses, closed schools, or implemented related policies. There is a large debate about the benefits and costs of these regulations: many argue that they are essential to reduce the spread of the virus (Baker, Brown, and Eckhouse 2020; Mehta 2020), while many others argue that they impose intolerable economic burdens and infringe on personal liberties (Epstein 2020; Friedersdorf 2020). Attendant to this debate is the question of whether policymakers should mandate social distancing or instead rely on voluntary actions by individuals, for example as Sweden has done.

This paper asks two questions. First, what are the effects of stay-at-home orders and business closures on social distancing, economic outcomes, and disease transmission? Estimating these effects can be challenging because of endogenous policy implementation (e.g., states may impose stay-at-home orders precisely because they see disease transmission rising) and because the non-linearity of virus spread can easily generate biases in mis-specified models. Second, how does the ratio of health benefits to economic impacts differ for mandatory and voluntary social distancing? Most social distancing policies are blunt instruments that leave limited discretion for individuals and businesses to weigh benefits and costs on a case by case basis. It is possible that more flexible voluntary responses could achieve similar health benefits more efficiently.

We begin by using event study designs to estimate the short-run effects of stay-at-home orders on social distancing and economic outcomes. We measure social distancing using visits to points of interest (POIs) such as shops, parks, hospitals, and other places measured in cell phone location data aggregated by a company called SafeGraph. We measure consumer spending using debit card transactions from the financial services company Facteus, and we measure small business employment using data from the payroll service provider Homebase. We find that POI visits drop by 16 percent on the day a stay-at-home order is implemented. Consumer spending drops by roughly 5 percent, and employment drops by 5–10 percent.

We estimate the effects of stay-at-home orders on disease transmission in the context of an SIRD (Susceptible, Infected, Recovered, Deceased) epidemiological model (Kermack and McKendrick 1927). Infected people infect Susceptible people at rate β_t and recover at rate γ. The contact rate R—the number of people to whom one Infected person transmits the virus—is β_t/γ.

If a stay-at-home order is modeled as a proportional effect $\tau \in [-1,0]$ on the contact rate, then we can estimate τ in a linear regression framework with the natural log of the number of new cases on the left-hand side. We find that stay-at-home orders reduce the contact rate by 7–11 percent for different plausible values of γ. Using simulation models, we show that non-model-based regression frameworks used in some other recent papers to estimate the effects of stay-at-home orders can produce biased results.

We use these estimates to break down the share of social distancing, health, and economic outcomes that can be attributed to stay-at-home orders versus voluntary and other policy responses. Consistent with contemporaneous work, we find that much of the reduction in POI visits pre-dates stay-at-home orders (Brzezinski et al. 2020; Chetty et al. 2020; Gupta et al. 2020; Villas-Boas et al. 2020). We calculate that only 16 percent of observed social distancing in mid-April 2020 was caused by stay-at-home orders, 12-14 percent of observed reductions in economic activity as of mid-April (measured by small business employment) was attributable to the short-run effects of stay-at-home orders, as was 11–34 percent of the reduction in contact rate. Thus, the ratio of disease transmission benefits to short-run impacts on our economic outcomes is similar for the social distancing induced by the stay-at-home policy and the social distancing induced by voluntary behavioral changes and other policies.

We emphasize a number of important caveats. Our GPS-based social distancing measure captures overall movement patterns without distinguishing activity with a high vs. low risk of virus transmission. Our measures of economic cost only capture two dimensions of policies' overall economic impact, and these only imperfectly. Our measure of health impact relies on the assumptions of our SIRD model, is overall relatively imprecise, and may be biased by factors including endogenous reporting of Covid cases. In each case, we are able to capture only short-term, on-impact effects. We provide a set of data points that speak to the benefits and costs of social distancing policies but stop well short of a comprehensive welfare analysis.

Our work connects to several active research areas. First, a series of recent papers uses GPS data from SafeGraph or similar providers to quantify social distancing and estimate the effects of stay-at-home orders and other policies (Allcott et al. 2020; Barrios and Hochberg 2020; Chen et al. 2020a; Engle et al. 2020; Painter and Qui 2020; Villas-Boas et al. 2020). Second, several recent papers have studied the effects of stay-at-home policies on economic outcomes (Baker et al. 2020; Bartik et al. 2020; Chen et al. 2020b; Chetty et al. 2020; Kong and Prinz 2020). Third, another set of papers quantifies the effects of regulation on health outcomes (Childs et al. 2020; Flaxman et al.

2020; Fowler et al. 2020; Friedson et al. 2020; Greenstone and Nigam 2020; Lasry et al. 2020). In the epidemiological literature, there are a set of what economists might call "structural" models that use Bayesian techniques to estimate the contact rate R; these estimates often pay less attention to identifying the causal effect of policies on R (Cori et al. 2013; Thompson et al. 2013). In the economics literature, there are a set of papers that use reduced form event study approaches to estimate the effects of policies on some measure of disease transmission, but many of these papers are not closely tied to structural models. Our paper forms a bridge between these two lines of work by deriving reduced form equations—which are useful for causal analysis—from structural epidemiological models. A final set of papers looks at the effects of social distancing in other countries (Chudik, Pesaran, and Rebucci 2020; Fang, Wang, and Yang 2020) or other time periods (Barro 2020; Barro, Ursúa, and Weng 2020).

Section 2 presents the data, Sections 3–5 present the estimated effects of stay-at-home policies on social distancing, economic outcomes, and health outcomes, respectively, and Section 6 presents estimates of the share of total changes attributable to stay-at-home policies.

2   Data

2.1   Policy Data

Due to the decentralized policy response of states, cities, and counties, there is no single resource documenting non-pharmaceutical interventions (NPIs) in the United States. To get the best coverage of the universe of NPIs, we combine policy data from four sources: Keystone Strategy, a crowdsourcing effort from Stanford and University of Virginia, Hikma Health, and the New York Times. We explore both stay-at-home and business closure policies in this paper.

The New York Times has been tracking "shelter-in-place", "stay-at-home", "healthy-at-home", etc. policies enacted at the city, county and state level. We use the dates reported by the NYT for our stay-at-home policy.

Keystone Strategy curates the enforcement dates of a series of NPIs for all states and for all counties with at least 100 confirmed cases. The relevant NPIs from Keystone's data are shelter-in-place (SIP) and closure of public venues (CPV) policies. Keystone considers an "order indicating that people should shelter in their home except for essential reason" as a SIP intervention and a "government order closing gathering venues for in-person services" as a CPV intervention. We will use Keystone's SIP and CPV dates for our stay-at-home and business closure policies respectively.

The crowdsourced data collected by a group from Stanford and University of Virginia solicits policy and personal information from survey participants in an online form. We use the "lockdown" and "business closed" dates for counties from this data.

Hikma Health, a non-profit working on data systems and analysis for healthcare providers, has carried out their own crowdsourcing effort to document NPIs. We use the county "shelter date" and "work date" from Hikma Health in our construction of stay-at-home and business closure policies respectively.

Given that none of the sources have entirely overlapping policy data, we define both our stay-at-home and business closure policies by sequentially assigning enforcement dates when data is available in the order: NYT, Keystone, Stanford/Virginia crowdsource, and Hikma Health. Once a state enacts a policy, the counties inherit the policy of the state. In Appendix Table A1, we provide summary statistics reporting the share of county policies from each source. The distribution of the timing of each county's first order is shown in Figure 1.

2.2 Social Distancing Data

The data on social distancing behaviors come from SafeGraph, a data company that aggregates anonymized location data from about 45 million mobile devices and numerous applications in order to provide insights about physical points of interest (POIs).[1] POIs include restaurants, coffee shops, grocery stores, retail outlets, hospitals and many other business establishments. For each POI, SafeGraph reports the daily number of unique device visits along with information on the POI's industry and location.[2] The data we use span January 1, 2020 to May 2, 2020. For each county, we construct the total number of visits to POIs in that county for a given day.

2.3 Economic Data

Our analysis uses economic data from two sources. We incorporate spending on approximately 10 million debit cards in data from Facteus, a financial data provider that directly partners with banks. This sample consists of traditional debit cards issued by banks, general purpose debit cards issued by merchants, payroll cards issued by employers, and government alimony disbursement

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[1]To enhance privacy, SafeGraph excludes census block group information if fewer than five devices visited an establishment in a month from a given census block group. More detailed documentation of SafeGraph's data can be found at https://docs.safegraph.com/docs.

[2]For data prior to March 2020, SafeGraph excludes POIs with fewer than 5 visits in the month. For subsequent data, SafeGraph excludes POIs with no visits in a given week.

cards. Lower- and middle-income individuals are represented more heavily in this data than in the US population. The data used in our analysis spans January 1, 2020 to May 2, 2020. We construct the total number of transactions and dollar amount spent by cards from a given home county on a given day.[3]

We also source information on employment from Homebase, a company providing scheduling and time tracking software to over 60,000 small businesses.[4] For each day, we analyze the number of work hours and individuals employed by Homebase partner firms. Our analysis again aggregates by county and day and spans Homebase data from January 1, 2020 to May 2, 2020.

2.4   Health Data

We pull case and death counts by day at the county level from a continually updated repository by the New York Times that aggregates reports from state and local health agencies. For all dates up to the first available data, we assume no cases nor deaths. We collect state-level testing and hospitalization data from the Covid Tracking Project.

2.5   County-Level Demographic Data

We supplement the policy and outcome data with data on county characteristics. For our measure of county partisanship, we use the Republican vote share in the 2016 presidential election (MIT Election Data and Science Lab 2018).

3   Effects on Social Distancing Outcomes

We first present descriptive evidence of trends in POI visits over time. Panel A of Figure 2 plots trends in average daily POI visits for weeks from January 29, 2020 to April 22, 2020 normalized relative to their January 29, 2020 value. Daily POI visits fell by 64 percent between the weeks of January 29 and April 1, with much of this change occurring mid-March. Panel B compares these patterns for counties with a stay-at-home order by March 31 and those without such an order. Both sets of counties exhibit similar patterns—counties with a stay-at-home order in March experiencing a 64 percent decrease whereas those without a stay-at-home order experiencing a 61 decrease.

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[3]To protect privacy, Facteus injects a small amount of mathematical noise into key record attributes. This has very minimal impact on aggregate data. More information on this differential privacy procedure can be found at https://www.facteus.com/products/data-products/.
[4]Additional information regarding Homebase's data can be found at https://joinhomebase.com/data/.

Figure 3 maps the geographic distribution of social distancing and public policy responses across counties. Panel A shows the percent change in SafeGraph visits from the week beginning January 29, 2020 to that of April 8, 2020, which is the week with the fewest number of POI visits. Panel B visualizes variation in the effective start date of stay-at-home orders across counties. We see a geographic correlation between stronger social distancing and earlier public orders, and both series also correlate with other factors such as population density or virus exposure.

To estimate the causal effect of these stay-at-home orders, we estimate the following event-study specification

$$Y_{it} = \mu_i + \delta_t \otimes G_i + \sum_{k=-21, k \neq -1}^{k=21} \omega_k \mathbf{1}_{\{t-T_i=k\}} + \varepsilon_{it} \tag{1}$$

where Y_{it} is the log of POI visits in county i during time t, μ_i is a county fixed effect, and δ_t is a date fixed effect possibly interacted with indicators for county characteristics G_i (e.g., partisanship and urban status), and $\mathbf{1}_{\{t-T_i=k\}}$ is an indicator for the days relative to the first stay-at-home order T_i.[5] Standard errors are clustered at the state level. Earlier and later time periods are pooled in the $k = -21$ and $k = 21$ time indicators respectively.

Figure 4 presents our main results for social distancing outcomes. Panel A shows that stay-at-home orders decrease POI visits by 17.2 percent (se = 1.5) by the day after the order's effective start date ($k = 1$). The pre-trend in Panel A indicates that, prior to the stay-at-home order, individuals in treated counties had already begun reducing their POI visits more than individuals in untreated counties.

Panel B interacts the date fixed effects with indicators for the interaction between being an urban county and being a Republican county as defined by 2016 presidential vote shares. We expect these to be key predictors of policy timing—the urban indicator as a proxy for disease risk and the vote share measure as a proxy for policy preferences. In this specification, there are minimal trends during the lead-up to the policy enactment—suggesting the interaction between partisanship and being urban is sufficient to explain differential distancing between treated and non-treated counties. Upon impact, there is a drop in POI visits of 16.1 percent (se = 1.3) by the day after the order's effective start date ($k = 1$) that persists and is relatively stable throughout the window of analysis.

In Appendix Figure A2, we show that estimated treatment effects are similar when restricting to smaller counties whose caseloads are less likely to directly affect the state's decision to issue a

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[5]For counties without a stay-at-home order in our sample, $\mathbf{1}_{\{t-T_i=k\}}$ is always set to zero.

stay-at-home order.

For mandatory business closures, Appendix Figure A3 estimates a 8.8 percent (se = 1.6) decrease in POI visits.

4 Effects on Economic Outcomes

We also examine the effect of stay-at-home orders on consumer spending and employment outcomes. We follow the event-study specification in equation (1).

Figure 5 presents our main results for consumer spending using the log of total spending in our Facteus debit card sample. Panel A estimates that stay-at-home orders decrease consumer debit spending by 7.2 percent (se = 1.0) by the day after an order's effective start date ($k = 1$). Prior to the stay-at-home order, we see little evidence of differential pretrends, and spending remains consistently low in the period following the order. Panel B provides similar conclusions when interacting the date fixed effects with indicators for the interaction between being an urban county and being a Republican county as defined by 2016 presidential vote shares. Appendix Figure A4 shows that the log number of debit transactions fell by 5.8 percent (se = 0.7) after the order's effective start date—suggesting similar effects on the intensive and extensive margins of consumer spending.

Figure 6 presents our main results for employment using the log number of employees reporting positive hours in our Homebase sample. Panel A estimates a 8.7 percent (se = 3.3) reduction in the number of employees working in our Homebase sample on the day following the order's implementation. Unlike the consumer spending outcomes, employment shows a gradual decline much of which predates and then continues after the order's implementation. Panel B shows that including the partisanship and population density controls reduces these trends, particularly in the period after the order's implementation, and provides a similar estimate of a 7.6 percent (se = 2.9) decrease in employment. Appendix Figure A4 shows that the log of total work hours fell by 9.9 percent (se = 3.9) and log of total wages fell by 10.8 percent (se = 4.3) by the day after the order's effective start date.

Appendix Figure A5 examines the effect of mandatory business closures on our main economic outcomes. Panel A shows a 1.5 percent (se = 1.0) decline in consumer debit spending following a mandatory business closure relative to the day prior to the order's effective start date, which is a fifth of our analogous estimate for the effect of stay-at-home orders. In contrast, Panel B suggests

that employment at small businesses responds more strongly to business closures than to stay-at-home orders. We find a 11.4 percent (se = 2.9) decrease in the number of employees with positive work hours, which is nearly a third larger than our estimate following stay-at-home orders.

5 Effects on Health Outcomes

Figure 7 reports trends in cases, log cases, and log new cases by whether a county issued a stay-at-home order on or before March 25, issued an order after March 25, or has yet to issue an order. Each outcome is normalized relative to March 25. The figure highlights the fact that these different groups of counties have exhibited substantially different case dynamics—highlighting the value of a framework for thinking about the effect of stay-at-home orders.

5.1 SIRD Model

We start with a discrete-time SIRD model (Kermack and McKendrick 1927), suppressing notation for different geographies i. In outlining this model, we make the assumption that there are no health spillovers across geographies. Furthermore, we abstract from issues around testing and the endogeneity of stay-at-home order timing.

The population is defined by

$$S_t + I_t + R_t + D_t = N \tag{2}$$

where S_t, I_t, R_t, and D_t are the number of susceptible, infected, recovered, and deceased individuals at time t. Dynamics in the SIRD model are defined by the transition probabilities between states. The laws of motion are given by:

$$S_{t+1} - S_t = -\beta_t S_t \frac{I_t}{N} \tag{3}$$

$$I_{t+1} - I_t = \beta_t S_t \frac{I_t}{N} - \gamma I_t \tag{4}$$

$$R_{t+1} - R_t = (1 - \kappa)\gamma I_t \tag{5}$$

$$D_{t+1} - D_t = \kappa \gamma I_t \tag{6}$$

9

where β_t is the contact rate that governs the speed at which new infections propagate, γ is the rate at which infected individuals recover, and κ is the proportion of recovered individuals that die. We treat the recovery rate γ and death rate κ as fixed parameters, since there have been no significant developments in treatment.

We make the simplifying assumption that $S_t \approx N_t$ so that we can treat the ratio $\frac{S_t}{N_t} = 1$. As of May 5, less than 0.5 percent of the US population has or had a confirmed case—making this approximation reasonable. This allows us to replace equations (3) and (4) with

$$S_{t+1} - S_t = -\beta_t I_t \tag{7}$$

$$I_{t+1} - I_t = \beta_t I_t - \gamma I_t. \tag{8}$$

Defining the total number of cases to be $C_t = I_t + R_t + D_t$ and combining equations (5), (6), and (8), we get that cases evolve as follows

$$C_{t+1} - C_t = \beta_t I_t. \tag{9}$$

Furthermore, we can write

$$I_t = (C_t - C_{t-1}) + (1 - \gamma)I_{t-1}. \tag{10}$$

Given initial conditions C_0, I_0, the contact rate β_t, and the recovery rate γ, equations (9) and (10) define the dynamics of cases overtime.

5.2 Estimation Framework

The key parameter of interest for policymakers is the contact rate β_t. As social distancing increases, the contact rate decreases—yielding fewer new cases. The contact rate β_t is proportionally related to the reproduction number R_{0t} as $R_{0t} = \beta_t / \gamma$. A proportional effect on the contact rate β_t will have the same proportional effect on the reproduction number R_{0t}.

We suggest it is natural to think of stay-at-home orders as a proportional effect $\tau \in [-1, 0]$ on the contact rate. That is, the contact rate is $\beta_t(1 + \tau)$ under a stay-at-home order as opposed to β_t.

Taking logs of equation (9), we get

$$\log(C_{t+1} - C_t) = \log(\beta_t) + \log(1 + \tau T_i) + \log(I_t).$$

We then use an event-study framework to estimate the impact of treatment T_i

$$\log(C_{i,t+1} - C_{it}) = \alpha \log(I_{it}) + \delta_t \otimes G_i + \xi_{it} + \sum_{k=-14, k\neq-1}^{k=21} \omega_k \mathbf{1}_{\{t-T_i=k\}} + \varepsilon_{it}, \qquad (11)$$

where, relative to equation (1), ξ_{it} is an indicator for the non-binned event-study window.[6]

The ω_k coefficients can be interpreted as estimates of τ. Even though I_t is not directly observed, given initial conditions $C_0 = I_0$ and γ, no additional data is required to construct the time-path for I_t beyond the growth in cases. Below, we use values for γ suggested by the epidemiology literature and examine robustness to alternative values. Note that one test of the model and its assumptions is whether $\hat{\alpha} = 1$.

In the appendix, we show that this estimator performs well when estimated on data simulated from a SIRD model.

5.2.1 Other Methods of Estimation in the Literature

Several previous attempts at estimating the effect of stay-at-home orders have not been model driven. For example, Dave et al. (2020) use the log of confirmed cases as the outcome in an event-study framework with state-specific linear trends. Lin and Meissner (2020) also use a similar event-study specification with log cases on the left-hand side.[7] These estimators can produce unexpected results when the data comes from a SIRD data-generating process.

In the appendix, we show that the Dave et al. (2020) estimator exhibits substantial pretrends and fails to recover the estimated treatment effect when estimated on simulated data. We also apply the estimator to real, state-level data as in Dave et al (2020). We qualitatively replicate their results when using the 7-day pre-period event window. When using a more complete 14-day or 21-day pre-period event window, the estimator produces null results with substantial pretrends—mirroring the estimates from this estimator when using data simulated from a SIRD model. To gain intuition

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[6]The event-study window indicator is required for normalization when geography fixed effects are excluded. We drop geography fixed effects because they bias estimates in our simulations.

[7]Friedson et al. (2020) use a synthetic control estimator with log cases on the left-hand side to estimate the effect of California's stay-at-home order. Fowler et al. (2020) and Courtemanche et al. (2020) use a difference-in-difference specification with $\log(C_{t+1}) - \log(C_t)$ on the left-hand side, which gives $\log(C_{t+1}) - \log(C_t) = \log(\beta_t I_t + C_t) - \log(\beta_{t-1} I_{t-1} + C_{t-1})$. Given the nonlinear dynamics of the SIRD model, synthetic control or matching estimates may perform better when the model structure is not accounted for parametrically.

for the poor performance of these estimators, equation (9) suggests we can write log cases as

$$\log(C_{t+1}) = \log(\beta_t I_t + C_t). \tag{12}$$

Therefore, the ω_k coefficients from an event study with log cases on the left-hand side are going to pick up differential trends in a nonlinear function of β_t, I_t, and C_t across treated and non-treated units rather than differential trends in $\log(\beta_t)$ alone.[8]

5.3 Results

A key input into the estimation process is γ which is the inverse of the average infectious period for COVID-19. We report estimates using a range of values for γ. On one extreme, we set $\gamma = 0$ which implies $I_t = C_t$ or an infinite infectious period. On the other extreme, we set $\gamma = 1/4$ which implies an average infectious period of 4 days. Early indications in the literature suggested an infectious period of 4.4 to 7.5 days (Anderson et al. 2020). As of May 8, 2020, the CDC website recommends home isolation until at least 10 days have passed since symptoms first appeared, whereas the UK NHS recommends a minimum of 7 days.[9] We view the range of $\gamma = 1/4$ (average of four day infectious period) and $\gamma = 1/12$ (average of twelve day infectious period) as limits to the range of likely values.

Table 1 reports our estimates using case data and stay-at-home orders. To reduce instances where $\log(C_{t+1} - C_t)$ is undefined, we aggregate to the state-day level and restrict the sample to state-days with at least 10 cases. We restrict the sample to either never treated states or states which are observed for at least 8 days before and 20 days after the order. Because of the imprecision of the estimates, we aggregate treatment indicators in the event study specification. As the table highlights, γ is positively correlated with the estimated treatment effects of stay-at-home orders on case prevalence.

Using likely values of γ, we find a negative estimated effect of stay-at-home orders on case prevalence though these estimated effects have wide confidence intervals. Setting $\gamma = 1/8$, which

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[8]Note that even under the simplifying assumption that $C_t = I_t$ (which implies $\gamma = 0$), rewriting equation (12) still gives

$$\log(C_{t+1}) = \log(1 + \beta_t) + \log(C_t).$$

[9]https://www.cdc.gov/coronavirus/2019-ncov/if-you-are-sick/steps-when-sick.html and https://www.nhs.uk/conditions/coronavirus-covid-19/what-to-do-if-you-or-someone-you-live-with-has-coronavirus-symptoms/staying-at-home-if-you-or-someone-you-live-with-has-coronavirus-symptoms/

implies an average infectious period of 8 days, our baseline estimates in Panel A suggest that stay-at-home orders decreased the contact rate β_t (i.e., the rate of new cases) by 10.0 percent (se = 3.6) relative to their pre-order levels.

Consistent with the data coming from a SIRD data generating process, we cannot reject the hypothesis that $\alpha = 1$ across our baseline specifications in Panel A of Table 1. Furthermore, $\hat{\alpha}$ is closest to 1 for the specifications using more likely values of γ.

Interacting time fixed effects with indicators for the interaction between whether a state is above median in 2016 Republican vote share and population density, as reported in Panel B of Table 1, gives qualitatively similar conclusions.

Figure 8 reports the full event-study plot for $\gamma = 0$ which sets $I_t = C_t$. The appendix reports the full event studies for the other values of γ.

Our estimates come with several caveats. First, there is measurement error in confirmed cases. Adapting the method outlined above, the appendix reports positive point estimates for the effect of stay-at-home orders on the log of new deaths.[10] Second, the onset of the stay-at-home orders may be prompted by information that future cases or deaths are likely to exhibit substantial increases. This could lead to upward bias in our estimated effects. On the other hand, stay-at-home orders may be issued in places where people are more likely to respond to the pandemic and so may exhibit higher levels of counterfactual social distancing. Lastly, a final issue is that some counties issue stay-at-home orders prior to the state-level orders. To the extent this is occurring, it will likely attenuate estimates.

6 Decomposition

In this section, we compute the share of the overall change in each outcome during our time period attributable to stay-at-home and business closure orders. Our estimates in Sections 3–5 focus on the effect of stay-at-home and business orders on social distancing, economic, and health outcomes in percent change. However, many of these outcomes have also experienced secular trends over our time period as individuals make voluntary behavioral changes (e.g., Figure 2). A 20 percent effect on an outcome that has already decreased 50 percent is only a 10 percent decrease relative

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[10]Note that we can write
$$\log(D_{i,t+1} - D_{it}) = \log(\kappa\gamma) + \log(1 + \beta_t - \gamma) + \log(I_{t-1})$$
which we can utilize in the same event study framework where ω_k are estimates for the impact of the stay-at-home order on $\log(1 + \beta_t - \gamma)$.

to baseline levels.

Understanding the effect relative to baseline levels is important for understanding the relative costs associated with different policies. For example, consider the hypothetical scenario where stay-at-home orders account for 20 percent of the overall amount of social distancing and 60 percent of the overall losses in employment, while the voluntary changes in behaviors and other government policies account for the remaining 80 percent of social distancing and 40 percent of employment losses. In this scenario, the employment cost per unit of social distancing is six times larger for stay-at-home orders than the average employment cost across voluntary behavioral changes and other government policies.

To understand these relative costs, we start by computing the average total percent reduction in the outcome as

$$\text{Total}\Delta = \frac{Y_T - Y_0}{Y_0} \tag{13}$$

where Y_t is weighted average of the level of the outcome in week t taken over geographies that enact the corresponding order during our time period, $t = 0$ is the first week of February, and $t = T$ is the third week of April. We average across days in the week when computing Y_t to remove any day-of-week effects.[11]

Next, we compute the average policy-induced change relative to baseline levels as

$$\text{Policy}\Delta = \frac{1}{N} \sum_i w_i \frac{\omega_k \times Y_{\mathcal{T}(i)}}{Y_0} \tag{14}$$

where ω_k is the estimated treatment effect from Sections 3, 4, 5, w_i are geography population weights that sum to N, and $\mathcal{T}(i)$ is the period prior to the order's implementation for geography i. We account for uncertainty induced by the estimation of ω_k in the standard errors, and treat the values of Y_t as given. We set $k = 1$ in our baseline specification and examine robustness to alternative assumptions.

Figure 9 presents the ratio PolicyΔ/TotalΔ for our social distancing, employment and health outcomes.[12] We compute this estimate separately for counties enacting stay-at-home orders and

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[11]Most states do not have any cases in early February, so the pre-period levels cannot be estimated from the data when examining the overall change in the contact rate β_t. Since we must choose γ for each specification and the reproduction number R_0 is is β_t/γ, we can recover β_t in the pre-period by specifying R_0. Anderson et al. (2020) and D'Arienzo and Coniglio (2020) provide an overview of estimates of the initial reproduction rate R_0, ranging from 2.5 to 3.5. Our preferred reproduction rate is $R_0 = 3.0$. We then compute Y_T and $Y_{\mathcal{T}(i)}$ using equation (9) and the assumed γ value; we use the assumed R_0 when (9) is undefined in our data.

[12]Debit card transactions and total spending, according to the Facteus data, did not have a similarly strong decrease

14

counties enacting business orders. Table 2 reports TotalΔ and PolicyΔ individually for the stay-at-home orders, the business closure orders, and the simultaneous implementation of both orders.

We estimate that stay-at-home orders explain 16.4 percent of the change in POI visits, 13.6 percent of the change in total wages, 11.6 percent of the change in total employment, and 17.6 percent of the change in the contact rate β_t when setting $\gamma = 1/8$ and $R_0 = 3.0$. Our estimates suggest that stay-at-home orders have a wage cost per unit of social distancing that is 0.80 times as large as the average cost across voluntary behavioral changes and other government orders and has a relative employment cost that is 0.67 times as large.[13]

In contrast to the stay-at-home orders, the employment cost per unit of social distancing for business closures is relatively high. We estimate that business closure orders explain 10.8 percent of the change in POI visits, but 23.8 percent of the change in total wages and 18.7 percent of the change in total employment. Our estimates suggest that business closure orders have a wage cost per unit of social distancing that is 2.58 times larger than the average cost across voluntary behavioral changes and other government orders and has a relative employment cost that is 1.90 times greater.[14]

One issue is that business order closures often precede stay-at-home orders and the relative cost of stay-at-home orders may be different if considered in isolation. To partially address this, we also compute the employment costs per unit of social distancing when restricting to counties for which the stay-at-home and business closure orders were implemented simultaneously. The results are shown in Table 2.[15] They suggest that simultaneous stay-at-home and business closure orders have a smaller employment and spending cost per unit of social distancing.

In the appendix, we also consider the case where we set $k = 20$ for estimating the treatment effects or where we estimate treatment effects from fitting a linear pretrend in the two weeks prior to the order's implementation and computing the estimated treatment effect as the difference between the extrapolated pretrend and the treatment effect at $k = 20$.[16] The qualitative conclusions regarding the relative efficiency of stay-at-home orders is unchanged across these alternative estimators.

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between February and April. As a result, the decomposition of this small reduction (or increase in the case of total spending) into a voluntary and mandatory portion is more difficult to interpret. We note that Farrell et al. (2020) find a decrease in consumer spending when using data sources other than Facteus' debit card panel.

[13] That is, $\frac{.136}{.164} / \frac{1-.136}{1-.164} = 0.80$ and $\frac{.116}{.164} / \frac{1-.116}{1-.164} = 0.67$.

[14] That is, $\frac{.238}{.108} / \frac{1-.238}{1-.108} = 2.58$ and $\frac{.187}{.108} / \frac{1-.187}{1-.108} = 1.90$.

[15] There is not a sufficient number of counties to estimate the effect of stay-at-home orders on counties that have not received a prior business order closure.

[16] In computing the standard errors for this specification, we account for the additional variance induced by the extrapolation by adding the variance from the predicted value from the linear fit.

7 Conclusion

We use event studies and a model-driven regression framework to provide new estimates of the causal impact of stay-at-home and business closure orders. We find that these orders were responsible for roughly 16 percent of the total social distancing that occurred between early February and late April, 2020. They were responsible for roughly similar shares of spending and employment losses and of health benefits in the form of reduced transmission.

Consistent with other recent findings, these results suggest that the overall impact of policy changes has been modest compared to that of the largely voluntary efforts that preceded them. This implies that we may not see large immediate changes as policies are relaxed, and that public messaging and other factors that shape the path of voluntary responses going forward may be even more important than the timing of policy relaxations. Our findings also imply that while the health benefits of these policies have been modest, their economic costs appear modest as well, at least on the dimensions of spending and employment that we measure. We thus do not find any clear evidence that the greater discretion of voluntary social distancing allows for a significantly more efficient response.

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Figure 1: Distribution of Timing of First Government Order

Note: Figure shows the distribution of government order effective start dates over time and across counties. Each bar represents the number of counties (y-axis) for which the first order of a given type went into effect on the date specified (x-axis). Stay-at-home and business closing orders are shown in blue and orange bars respectively. See Section 2.1 for detail on data sources and processing.
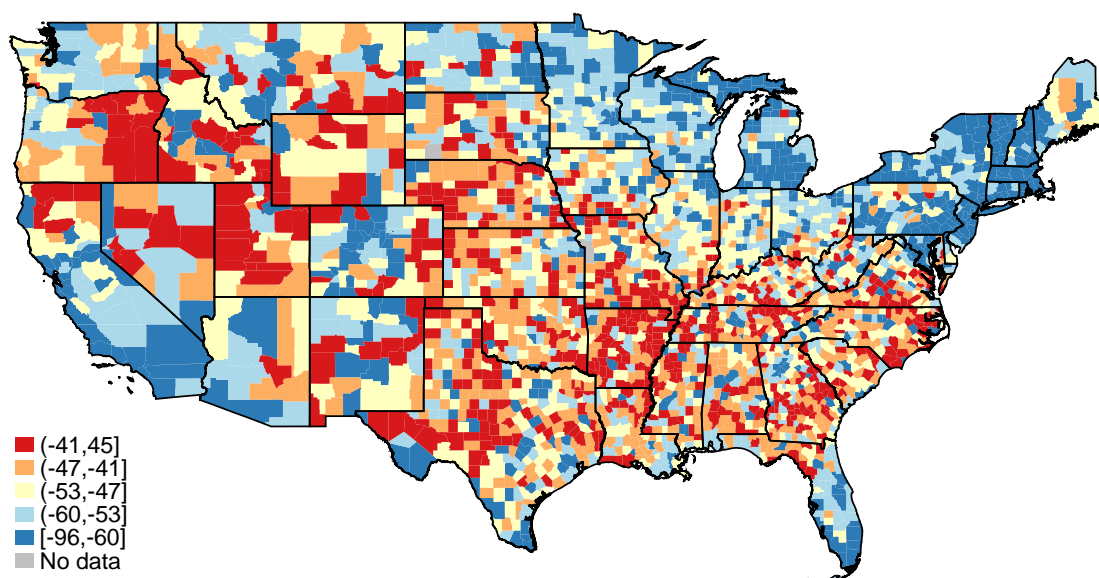
20

Figure 2: Trends in Social Distancing

Panel A: Overall

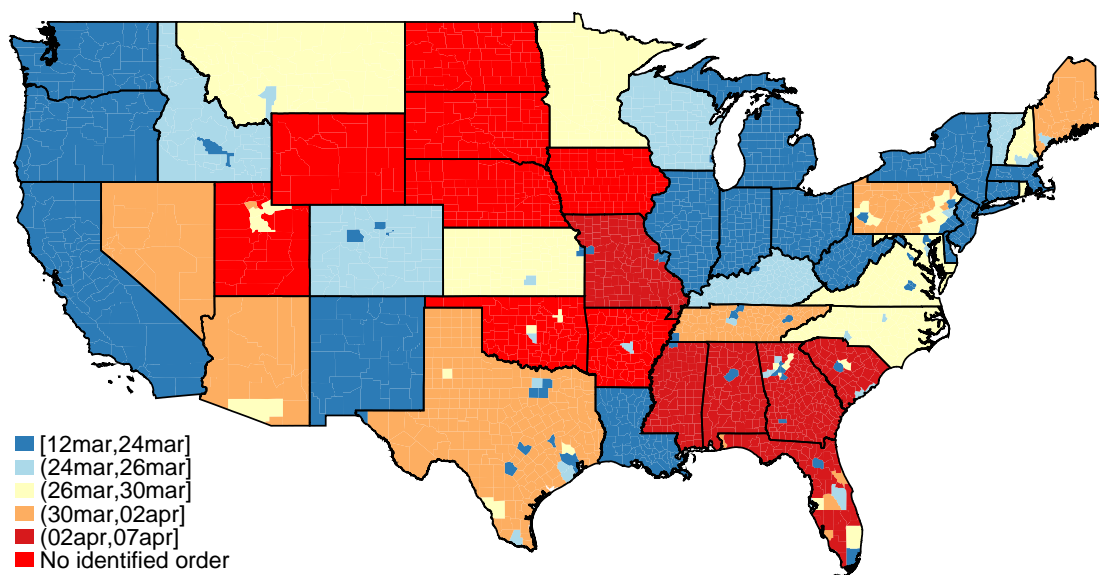*Panel B: By Government Order Status*
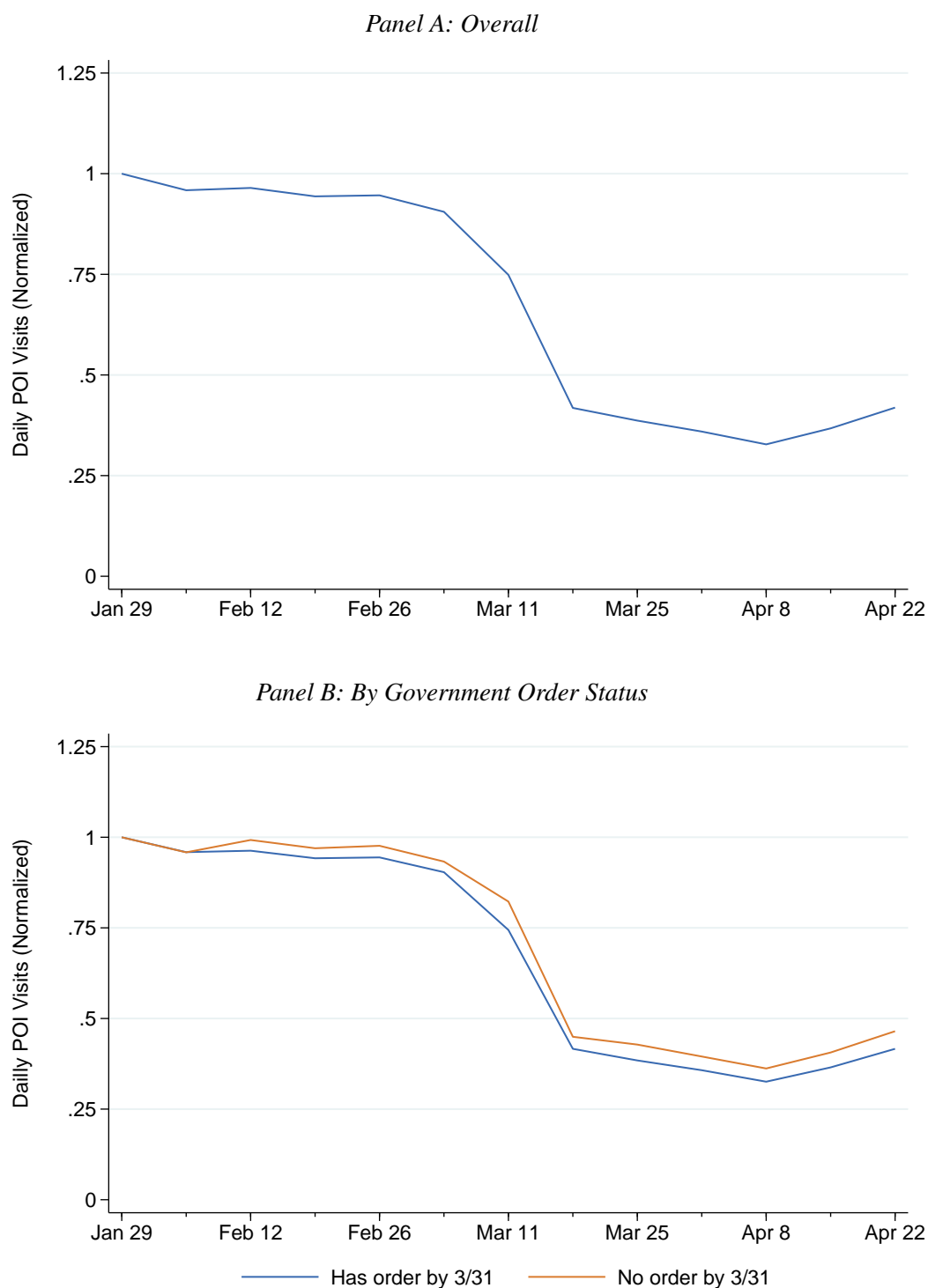
Note: Figure plots trends in average daily POI visits overtime normalized to one for the week of January 29, 2020. Panel A plots the population weighted average number of POI visits across counties. Panel B is the same except that it separates counties by whether they have a stay-at-home order by March 31, 2020. For both panels, the normalization occurs after averaging across counties and days.

Figure 3: Geographic Variation in Social Distancing and Public Policy

Panel A: % Change in SafeGraph Visits

- (-41,45]
- (-47,-41]
- (-53,-47]
- (-60,-53]
- [-96,-60]
- No data

Panel B: Stay-at-Home Order Effective Start Date

- [12mar,24mar]
- (24mar,26mar]
- (26mar,30mar]
- (30mar,02apr]
- (02apr,07apr]
- No identified order

Note: Figure shows the U.S. geographic distribution of social distancing and public policy responses. Panel A shows for each county the percent change in aggregate visits between the week beginning January 29, 2020 and the week beginning April 8, 2020. Blue shading denotes a more negative percent change in visits during the latter week relative to the former. Red shading indicates an increase or a smaller decrease in visits. These visits are sourced from SafeGraph's mobile device location data. Panel B shades U.S. counties by the effective start date for the earliest stay-at-home order issued (see Section 2.1 for sources). Blue shading indicates an earlier order, while red shading indicates that an order was issued later or was never issued. This figure reproduces parts of Figure 2 from Allcott et al. (2020).

22

Figure 4: Effect of Stay-at-Home Orders on Social Distancing
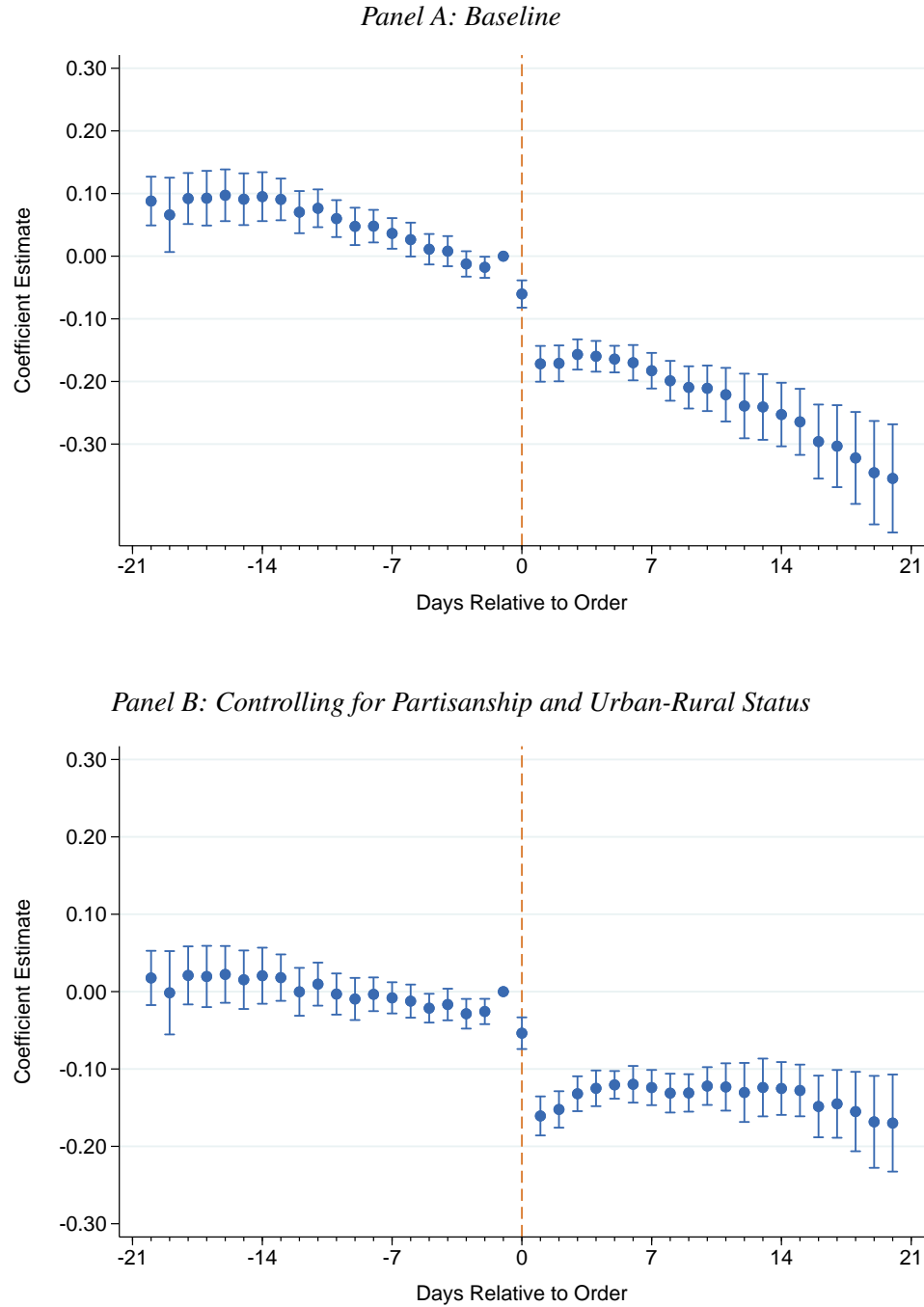
Panel A: Baseline

Panel B: Controlling for Partisanship and Urban-Rural Status

Note: Figure plots estimated treatment effects ω_k from the event-study specification outlined in equation (1) using the log of total POI visits in a county for a given day. Panel A only includes county fixed effects μ_i and date fixed effects δ_t. Panel B is the same as Panel A, except that it interacts the date fixed effects with indicators for the interaction between being an urban county and being a Republican county. Counties are weighted by population in the regression. Standard errors are clustered at the state level.

## Figure 5: Effect of Stay-at-Home Orders on Consumer Spending
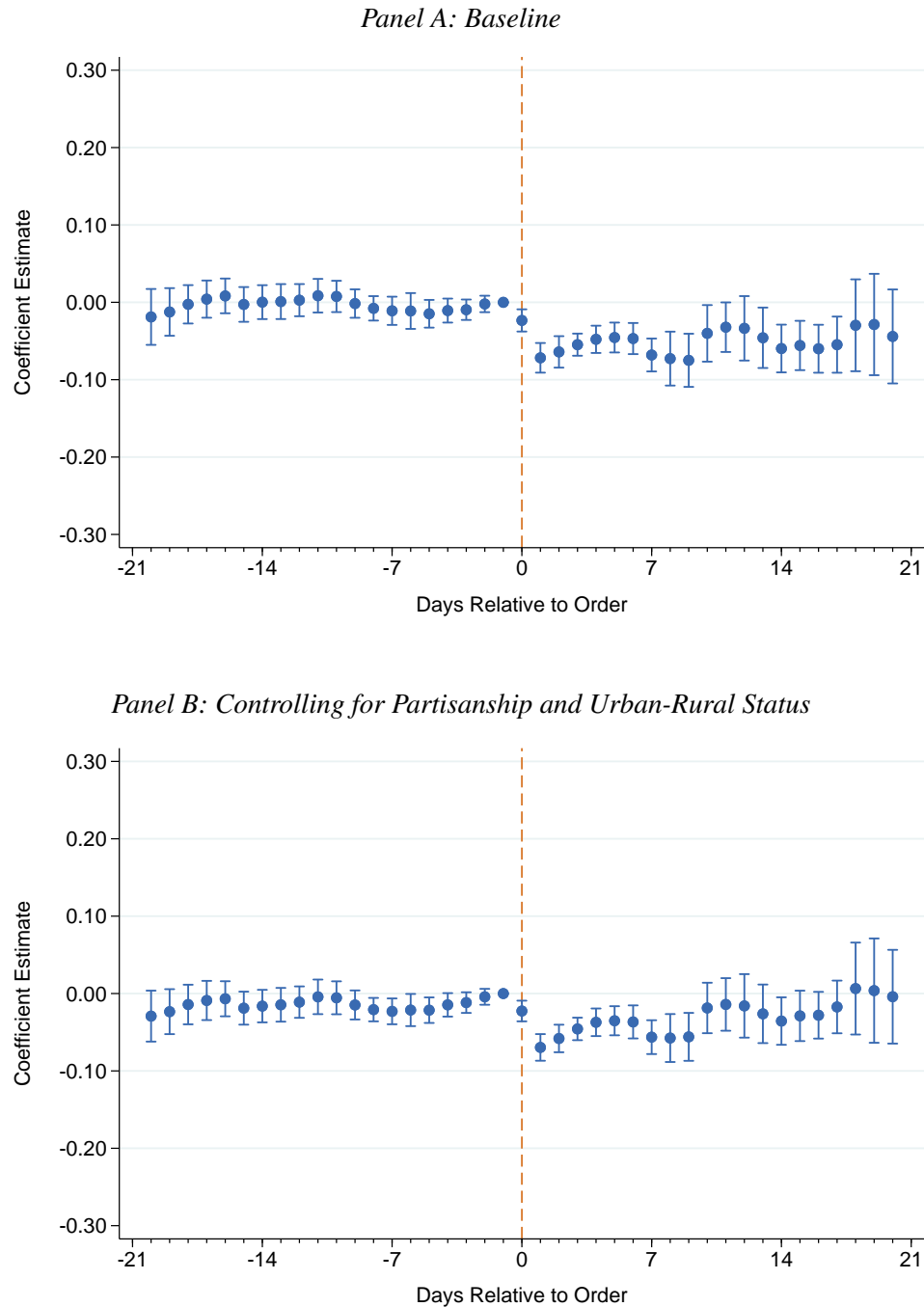
Panel A: Baseline

Panel B: Controlling for Partisanship and Urban-Rural Status

Note: Figure plots estimated treatment effects ω_k from the event-study specification outlined in equation (1) using the log of total spending in Facteus' debit card sample for a given home county and day. Panel A only includes county fixed effects μ_i and date fixed effects δ_t. Panel B is the same as Panel A, except that it interacts the date fixed effects with indicators for the interaction between being an urban county and being a Republican county. Counties are weighted by population in the regression. Standard errors are clustered at the state level.
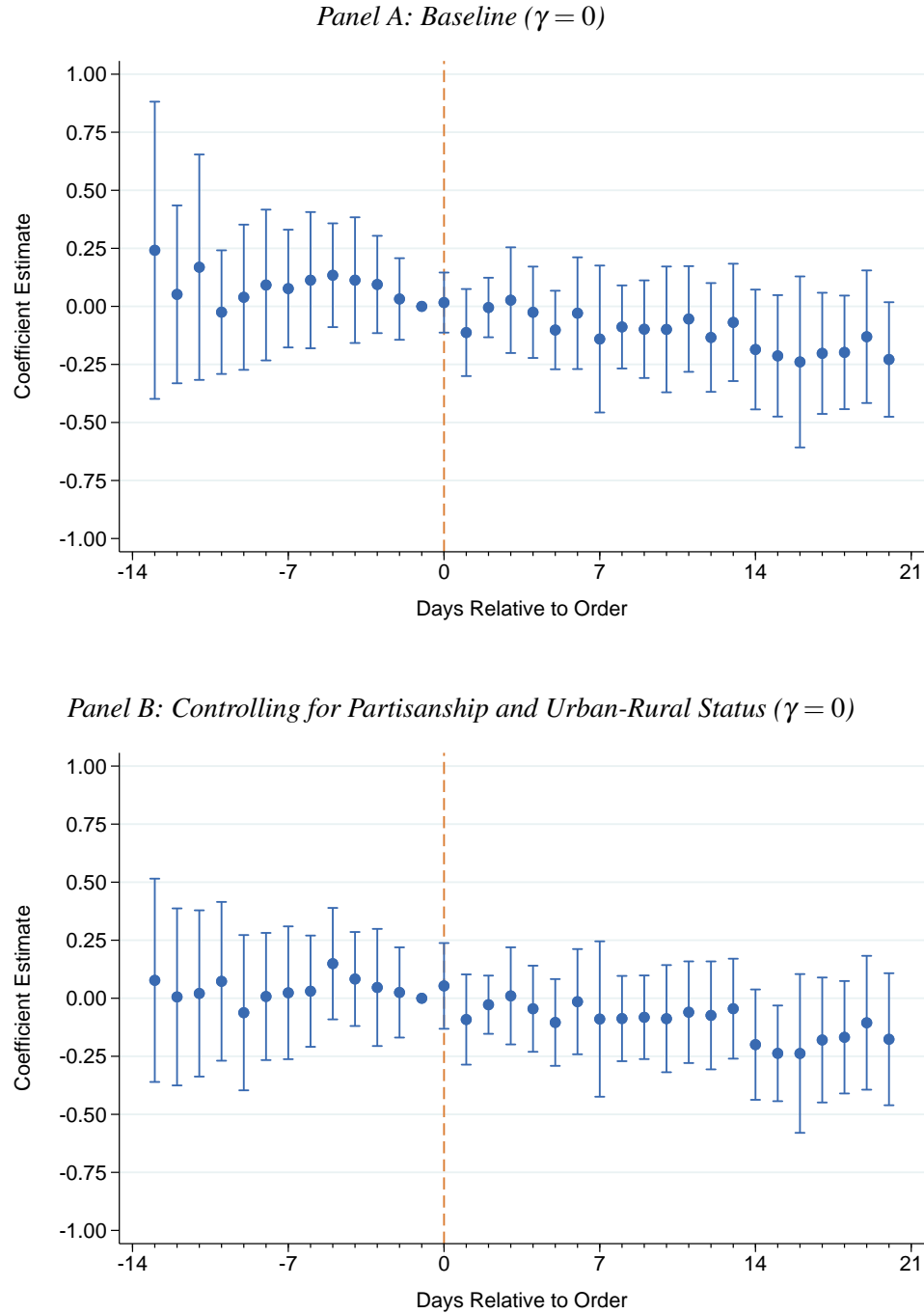
Figure 6: Effect of Stay-at-Home Orders on Employment
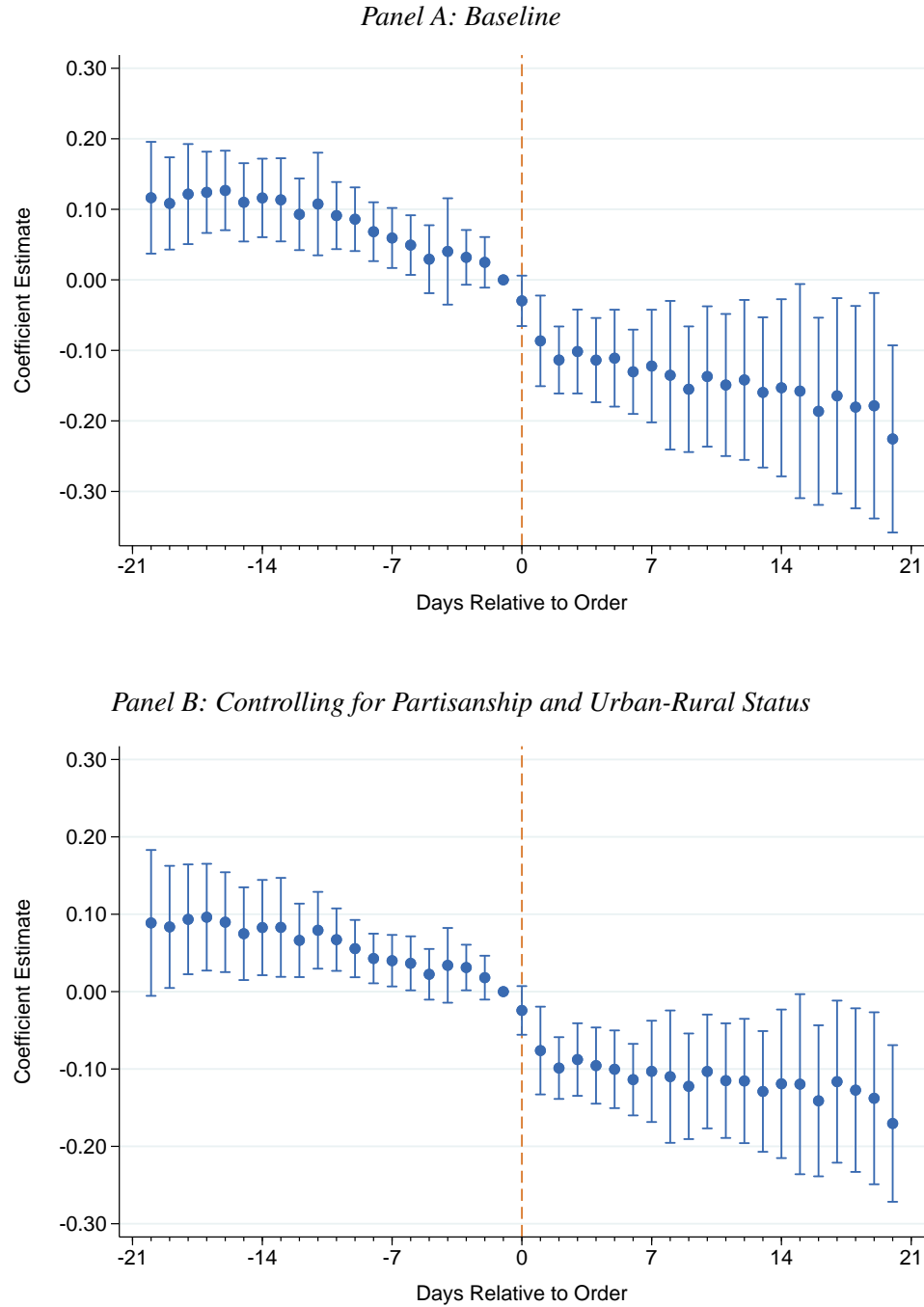
Panel A: Baseline

Panel B: Controlling for Partisanship and Urban-Rural Status

Note: Figure plots estimated treatment effects ω_k from the event-study specification outlined in equation (1) using the log number of individuals with positive work hours from the Homebase sample for a given county and day. Panel A only includes county fixed effects μ_i and date fixed effects δ_t. Panel B is the same as Panel A, except that it interacts the date fixed effects with indicators for the interaction between being an urban county and being a Republican county. Counties are weighted by population in the regression. Standard errors are clustered at the state level.

25

Figure 7: Trends in COVID Cases by Order Timing
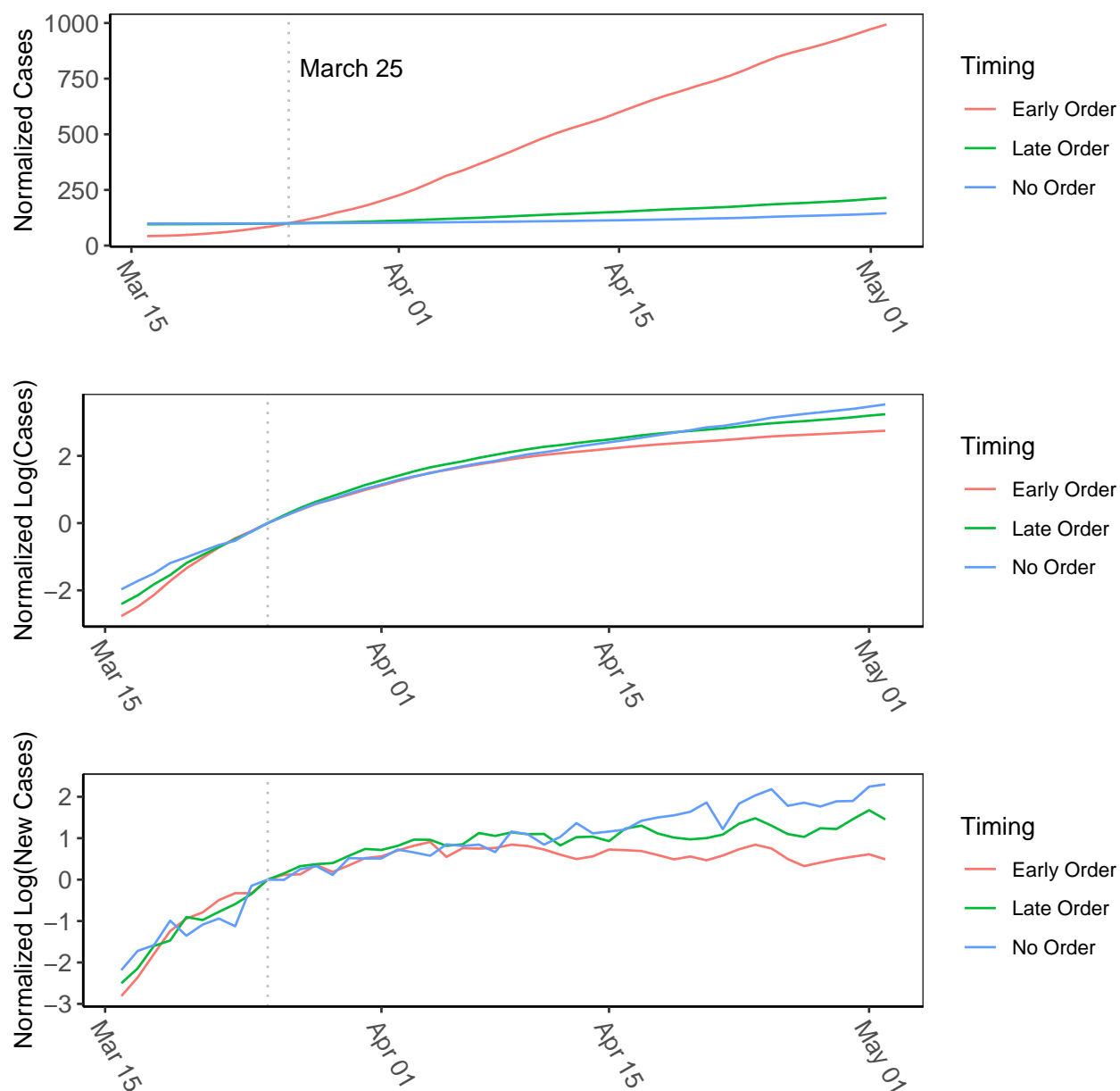
Note: Figure reports trends in cases, log cases, and log new cases by county order status. 'Early Order' indicates counties with a stay-at-home order on or before March 25, 2020. 'Late Order' indicates counties with a stay-at-home order after this point. 'No Order' indicates counties which did not issue a stay-at-home order during this sample period. The dashed vertical line indicates March 25, 2020. Cases are normalized to be 100 on March 25, 2020 for each county group. The log outcomes are normalized to be 0 on March 25, 2020 for each county group. The average is taking across counties in each group prior to taking logs or normalization.

26

Figure 8: Effect of Stay-at-Home Orders on Contact Rate

Panel A: Baseline ($\gamma = 0$)

Panel B: Controlling for Partisanship and Urban-Rural Status ($\gamma = 0$)

Note: Figure plots estimated treatment effects ω_k from the event-study specification outlined in equation (15) using the log of new cases in a state for a given day as the outcome. Panel A only includes date fixed effects δ_t and sets $\gamma = 0$ which implies $\log(I_{it}) = \log(C_{it})$. Panel B is the same as Panel A, except that it interacts the date fixed effects with indicators for the interaction between above median Republican and population density state indicators. States are weighted by population in the regression. Standard errors are clustered at the state level.

Figure 9: Benefits and Costs of Policies

Panel A: Social Distancing and Employment Outcomes (Counties)

Panel B: Contact Rate β_t (States)
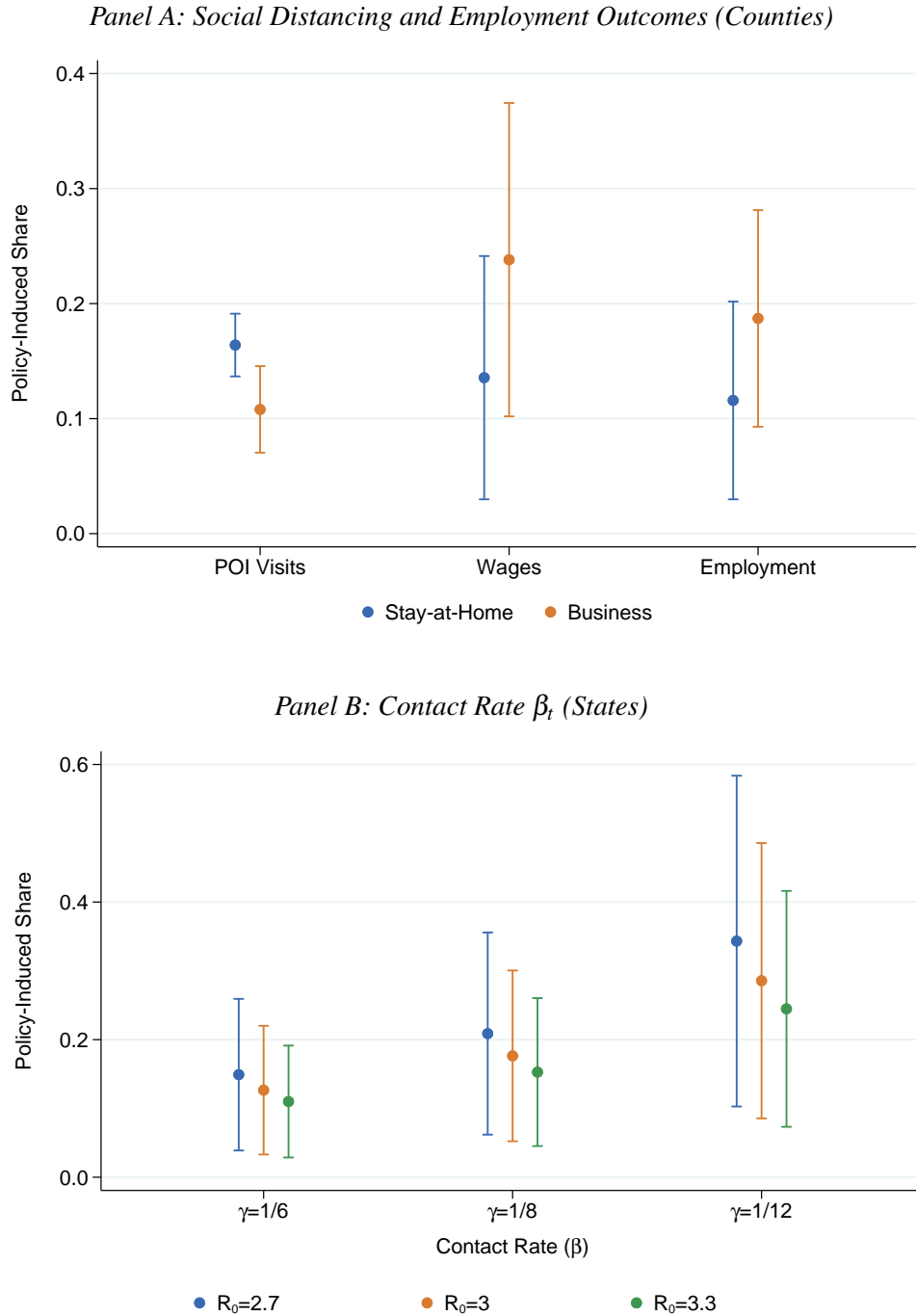
Note: Figure plots the share of the total change in each outcome that is attributable to a given policy PolicyΔ/TotalΔ following Section 6. Panel A reports estimates using stay-at-home orders and business closure orders for POI visits from SafeGraph, total wages from Homebase, and employment from Homebase. For each policy treatment, we restrict attention to the counties treated by the given policy. Panel B reports estimates of the policy-induced change in the contact rate β_t from stay-at-home orders. In both panels, the bars depict 95 percent confidence intervals. See Table 2 for additional details.

28

Table 1: Estimated Effects of Stay-at-Home Orders on Contact Rate

	(1) $\gamma = 0$	(2) $\gamma = 1/12$	(3) $\gamma = 1/8$	(4) $\gamma = 1/6$	(5) $\gamma = 1/4$
Panel A: Baseline					
Post-order	-0.136	-0.112	-0.100	-0.088	-0.067
	(0.055)	(0.040)	(0.036)	(0.033)	(0.031)
$\log(I_{it})$	0.979	0.996	0.997	0.996	0.989
	(0.024)	(0.017)	(0.014)	(0.012)	(0.011)
Panel B: Controlling for Partisanship and Urban					
Post-order	-0.095	-0.086	-0.079	-0.071	-0.056
	(0.063)	(0.045)	(0.040)	(0.036)	(0.033)
$\log(I_{it})$	0.945	0.976	0.983	0.985	0.984
	(0.032)	(0.024)	(0.021)	(0.018)	(0.014)
Clusters	46	46	46	46	46
Obs.	2397	2397	2397	2397	2397

Note: Table shows estimated coefficients from estimating an aggregated version of the event study in equation (15):

$$\log(C_{i,t+1} - C_{it}) = \alpha \log(I_{it}) + \delta_t \otimes G_i + \omega_0 E_{it} + \omega_1 (E_{it} \times \text{Post}_{it}) + \xi_{it}^0 + \xi_{it}^1 + \varepsilon_{it} \qquad (15)$$

where $E_{it} = \mathbf{1}_{\{-9 < t - T_i < 21\}}$, $\text{Post}_{it} = \mathbf{1}_{\{-1 < t - T_i < 21\}}$, $\xi_{it}^0 = \mathbf{1}_{\{t - T_i < -8\}}$, and $\xi_{it}^1 = \mathbf{1}_{\{t - T_i > 20\}}$. 'Post-order' reports $\hat{\omega}_1$, and 'log(I_{it})' reports $\hat{\alpha}$. Panel A does not interact time fixed effects δ_t with any state characteristics. Panel B interacts the time fixed effects with interactions between above median Republican and poulation density state indicators. Standard errors clustered by state are reported in parenetheses. Each column reports estimates for a different value of γ as reported in the header.

Table 2: Decomposing Changes in Distancing, Economic, and Health Outcomes

Panel A: Social Distancing and Economic Outcomes (Counties)						
	(1)		(2)		(3)	
	Stay-at-Home		Business		Both Orders	
	Total	Policy	Total	Policy	Total	Policy
POI Visits	-0.649	-0.106	-0.648	-0.070	-0.641	-0.156
		(0.009)		(0.012)		(0.017)
Homebase Wages	-0.611	-0.083	-0.610	-0.145	-0.594	-0.076
		(0.033)		(0.042)		(0.075)
Homebase Employment	-0.581	-0.067	-0.580	-0.108	-0.574	-0.084
		(0.025)		(0.028)		(0.065)
Facteus Debit Transactions	-0.035	-0.057	-0.035	-0.022	-0.037	-0.061
		(0.007)		(0.009)		(0.011)
Facteus Total Spending	0.190	-0.071	0.191	-0.016	0.177	-0.066
		(0.010)		(0.011)		(0.014)

Panel B: Contact Rate β_t (States)						
	(1)		(2)		(3)	
	$\gamma = 1/12$		$\gamma = 1/8$		$\gamma = 1/6$	
	Total	Policy	Total	Policy	Total	Policy
$R_0 = 2.7$	-0.536	-0.184	-0.586	-0.122	-0.604	-0.090
		(0.066)		(0.044)		(0.034)
$R_0 = 3.0$	-0.581	-0.166	-0.626	-0.110	-0.642	-0.081
		(0.059)		(0.040)		(0.031)
$R_0 = 3.3$	-0.617	-0.151	-0.658	-0.101	-0.673	-0.074
		(0.054)		(0.036)		(0.028)

Note: Table reports the total and policy-induced changes in various outcomes as outlined in Section 6. In Panel A, we consider the policy-induced changes of stay-at-home orders, business closures, and simultaneous stay-at-home and business closures. Each column restricts attention to the set of counties that receive a given treatment, with Column (3) restricting to counties in which business closure and stay-at-home orders went into effect on the same day. The treatment effects ω_k used in Panel A set $k = 1$. In Panel B, all specifications estimate the effect of stay-at-home orders using estimates from the state level and use the estimated treatment effect ω from Panel A of Table 1.

A Appendix

A.1 SafeGraph Data

The SafeGraph data construction largely follows Allcott et al. (2020). For the five boroughs of New York City, we assign cases and deaths based on their population share. We also include additional weeks of data.

A.2 SIRD Simulations and Estimation Details

A.2.1 Simulation

To generate data from a SIRD data generating process, we assume a death rate $\kappa = .008$ and an average infectious period of 10 days ($\gamma = .1$). We assume β_{it} evolves as $\beta_{it} = \gamma(\mathscr{R}_{i0}e^{\lambda_i t} + \mathscr{R}_1(1 - e^{\lambda_i t})) \times (1 + \tau T_{it})$ where $\mathscr{R}_{i0}$ is drawn from a normal distribution with mean of 2.4 and standard deviation of 0.1, $\mathscr{R}_1 = .95$, τ is either 0 or -0.1 depending on the simulation, T_{it} is drawn from a binomial distribution with size 150 and probability 40/150, and λ is drawn from a normal distribution with mean -0.08 and standard deviation 0.01. The exponential decay process for β_t follows Fernández-Villaverde and Jones (2020). The initial share of the population infected is drawn from an exponential distribution with rate 10,000. The size of the population is drawn from an exponential with rate 1/100,000. We then follow the laws of motion outlined in the main text, updating β_t each period. Note that in constructing the simulations, we do *not* use the $S/N \approx 1$ assumption but simulate data from the complete model. We simulate data for 200 geographies with 150 time periods.

We then follow the estimation process outlined in the main text and assume that γ is the true value used in simulations. We also show robustness to incorrectly specifying τ as half its true value and twice its true value.

A.2.2 Dave et al. (2020)

Dave et al. (2020; Figure 5) use an event-study specification with the log of confirmed cases on the left-hand side and geography-specific linear time trends, e.g.,

$$\log(C_{it}) = \mu_i + \mu_i \times t + \delta_t + \sum_{k=-21, k \neq -1}^{k=21} \omega_k \mathbf{1}_{\{t-T_i=k\}} + \varepsilon_{it} \qquad (16)$$

where $\log(C_{it})$ is the log of confirmed cases in geography i during time t, μ_i is a geography fixed effect, and δ_t is a date fixed effect, $\mathbf{1}_{\{t-T_i=k\}}$ is an indicator for the days relative to the first stay-at-

home order T_i, and $\mu_i \times t$ is a geography-specific linear time trend.[17]

A.2.3   Estimation Details

To implement our estimator, we proceed as follows:

1. We aggregate county data to the state level, and restrict to state-days with at least 10 cases.

2. We set C_{i0} to be the number of confirmed cases at that period in which at least 10 cases are confirmed and define $C_{i0} = I_{i0}$. We assume zero cases in counties for which none have been reported and constrain cumulative cases to be non-decreasing at the state level.

3. We restrict the set of counties to either (a) those never treated, or (b) those that were treated and have at least 8 days of pre-treatment observations and 20 days of post-treatment observations.

4. We then use equation (10) to define the full time path of I_{it} for each geography given γ.

5. We then estimate equation (15), setting $\log(C_{t+1} - C_t) = \log(1/2)$ when $C_{t+1} - C_t = 0$.

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[17]In their event-study specification, Dave et al. (2020) aggregate multiple post-treatment periods into a single treatment effect and include other control variables.

Appendix Figure A1: Relative Timing of Stay-at-Home Orders
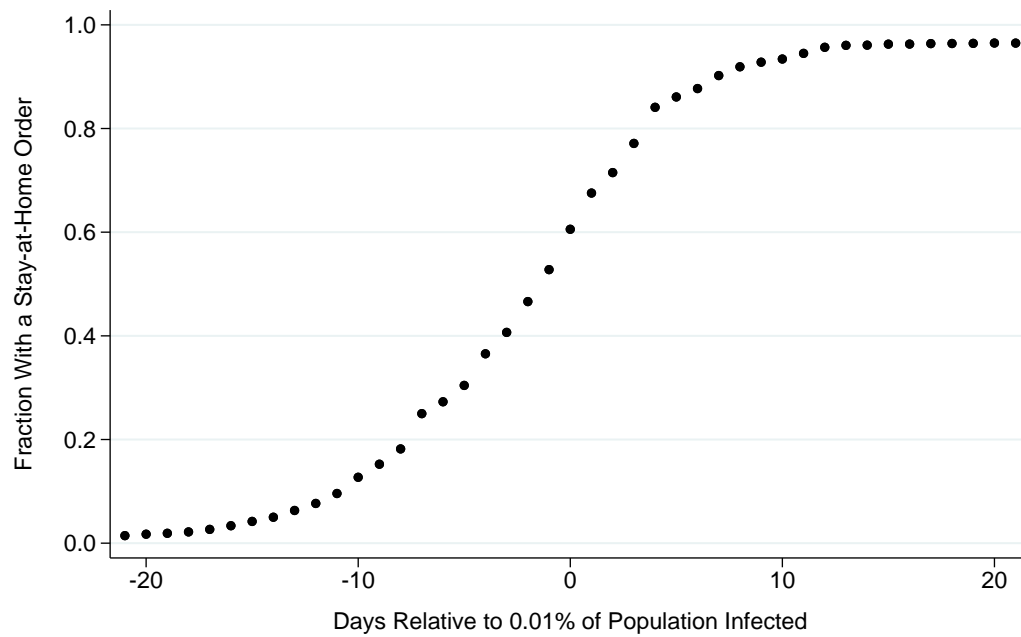
Note: Figure plots the population weighted share of counties with a stay-at-home order relative to the point in time in which at least 0.01% of the population has a confirmed case.

Appendix Figure A2: Effect of Stay-at-Home Orders on Social Distancing, Small Counties
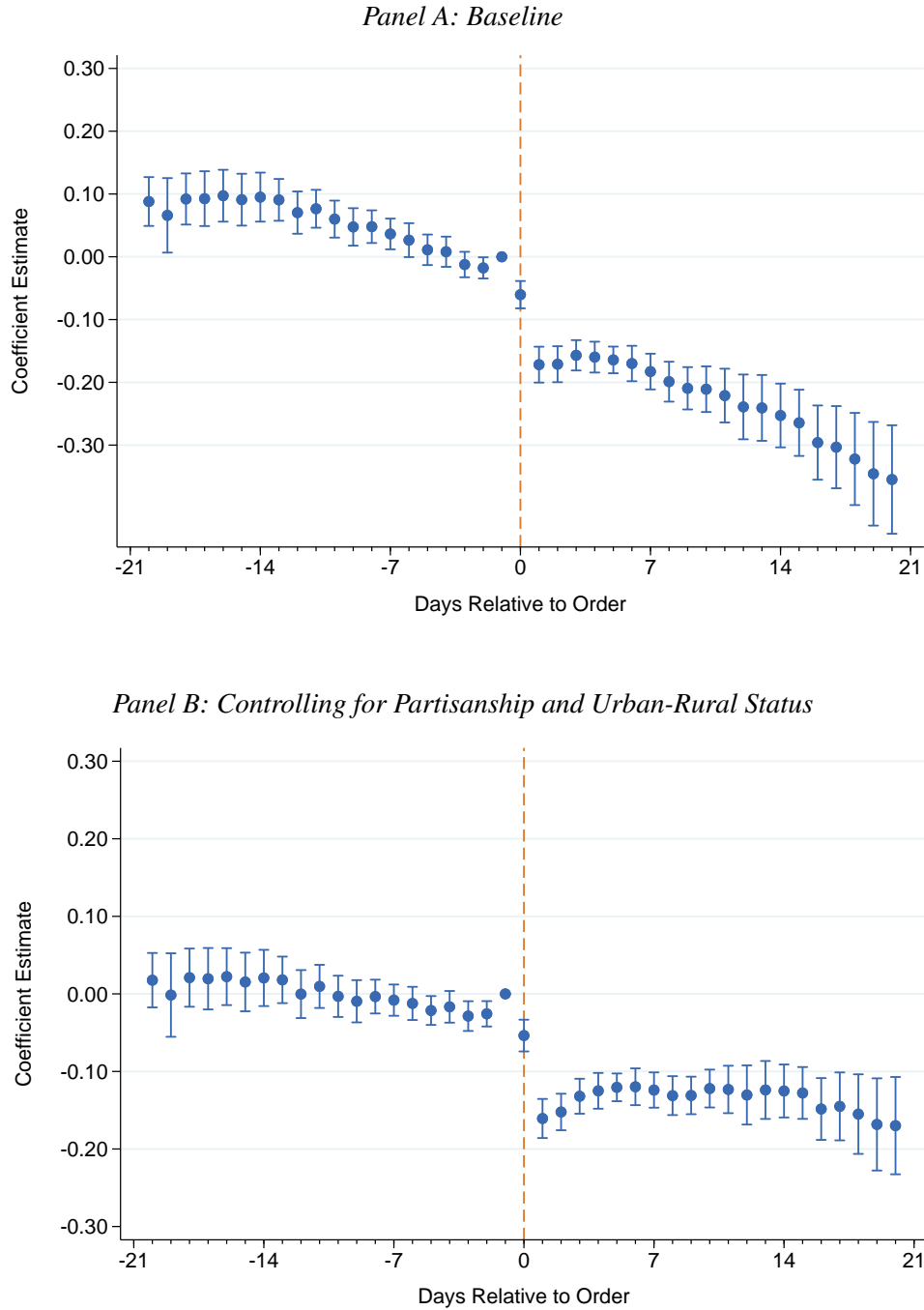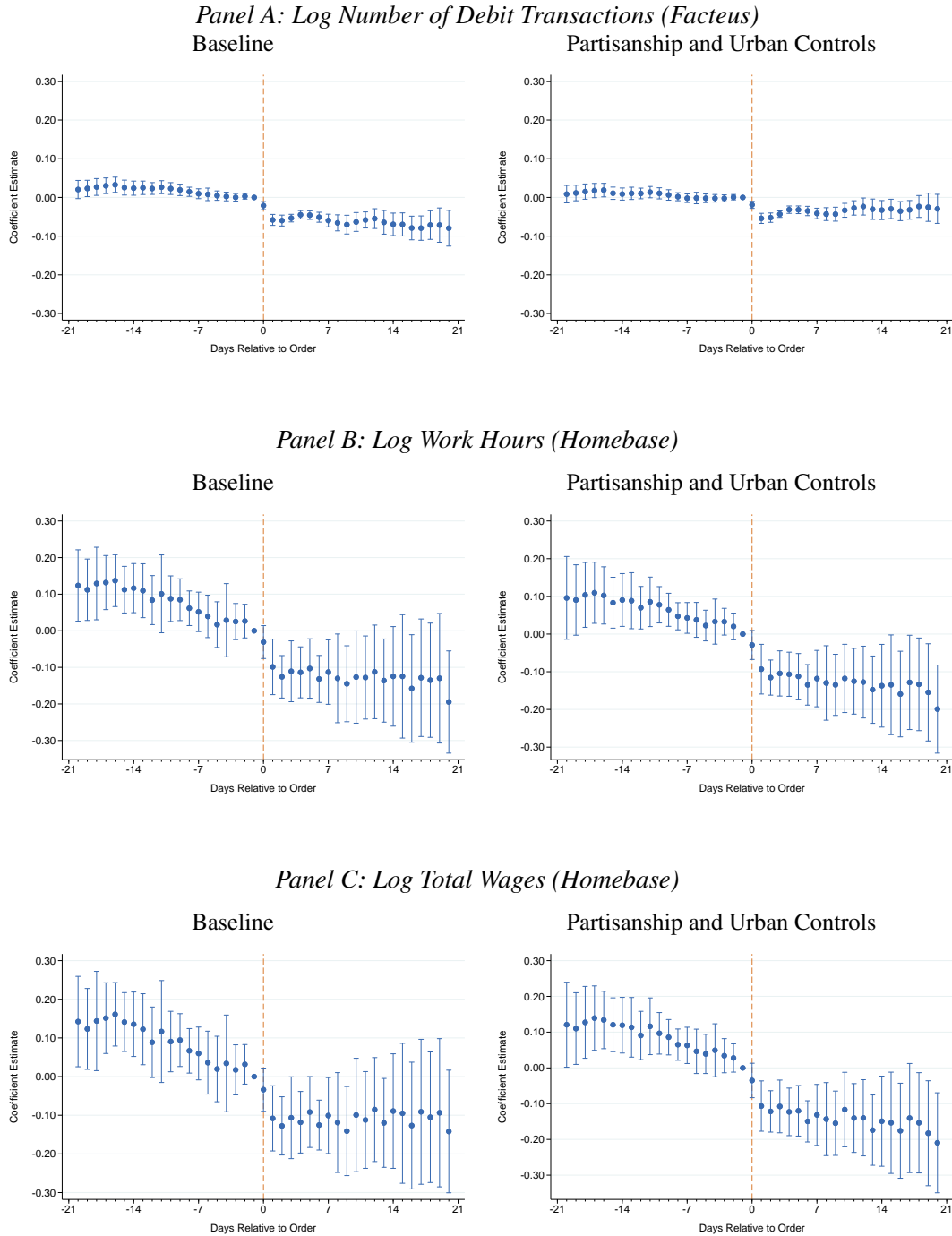
Panel A: Baseline

Panel B: Controlling for Partisanship and Urban-Rural Status

Note: Figure plots estimated treatment effects ω_k from the event-study specification outlined in equation (1) using the log of total POI visits in a county for a given day. Relative to Figure 2, the sample is restricted to counties that either (a) have a population of less than 10,000 and never experienced a separate county order, or (b) have a population of less than 50,000, never experienced a separate county order, and are not in the top four most populous counties for their state. Panel A only includes county fixed effects μ_i and date fixed effects δ_t. Panel B is the same as Panel A, except that it interacts the date fixed effects with indicators for the interaction between being an urban county and being a Republican county. Counties are weighted by population in the regression. Standard errors are clustered at the state level.

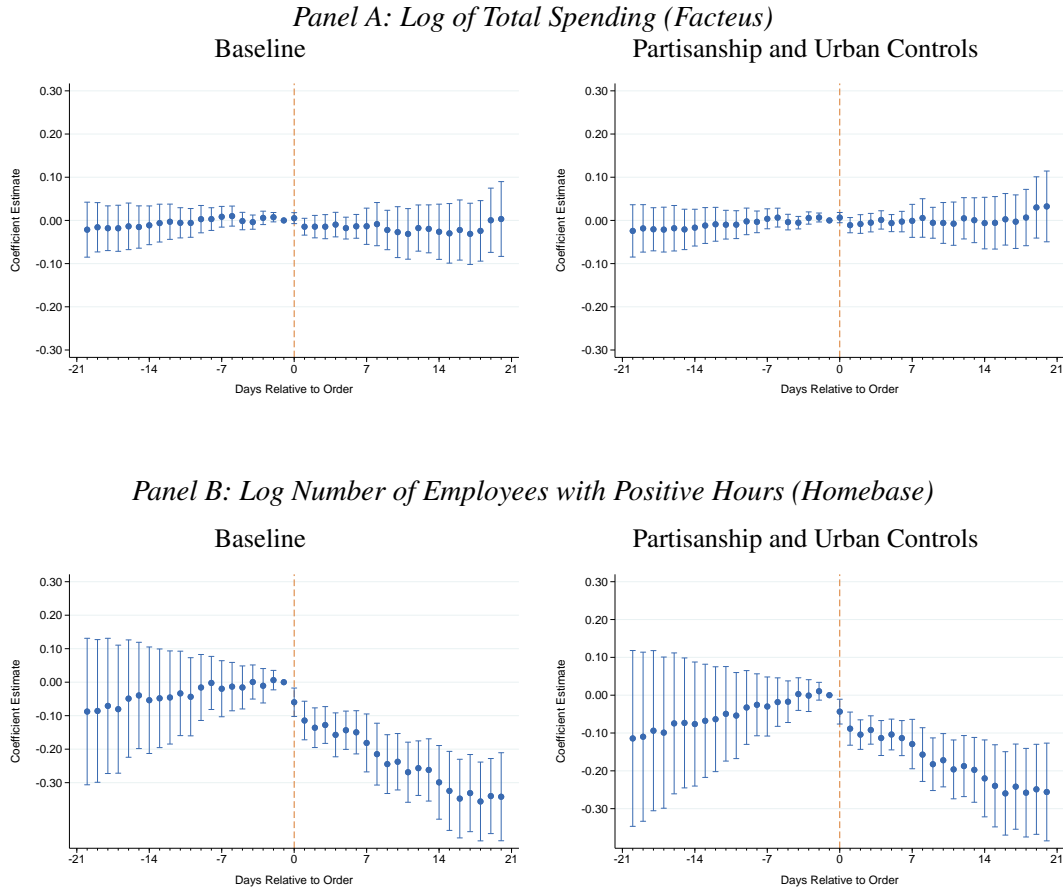Appendix Figure A3: Effect of Mandatory Business Closure Orders on Social Distancing
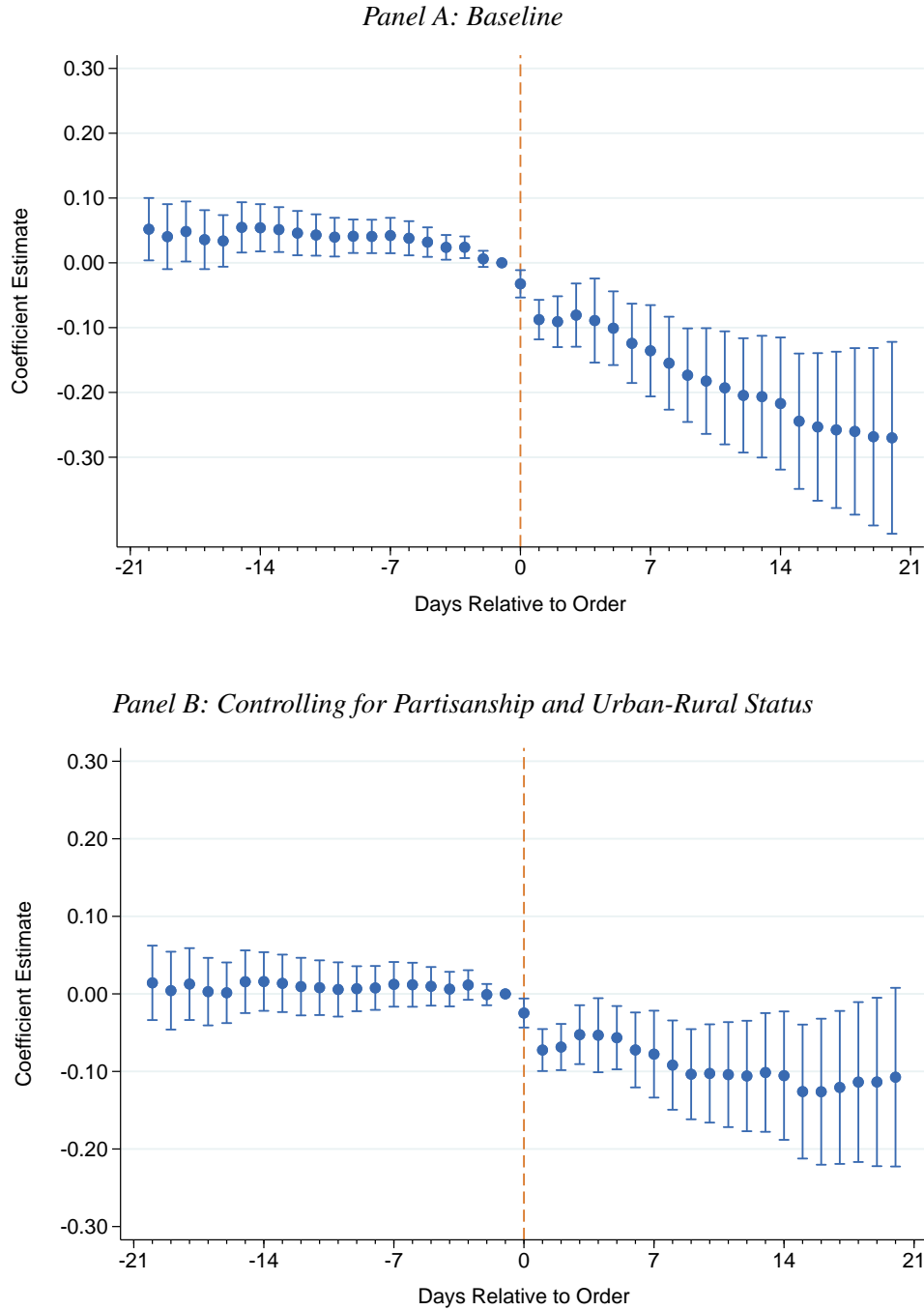
Panel A: Baseline

Panel B: Controlling for Partisanship and Urban-Rural Status

Note: Figure plots estimated treatment effects ω_k from the event-study specification outlined in equation (1) using the log of total POI visits in a county for a given day. Relative to Figure 2, business closures orders are used instead of stay-at-home orders. Panel A only includes county fixed effects μ_i and date fixed effects δ_t. Panel B is the same as Panel A, except that it interacts the date fixed effects with indicators for the interaction between being an urban county and being a Republican county. Counties are weighted by population in the regression. Standard errors are clustered at the state level.

Appendix Figure A4: Effect of Stay-at-Home Orders on Alternative Economic Outcomes
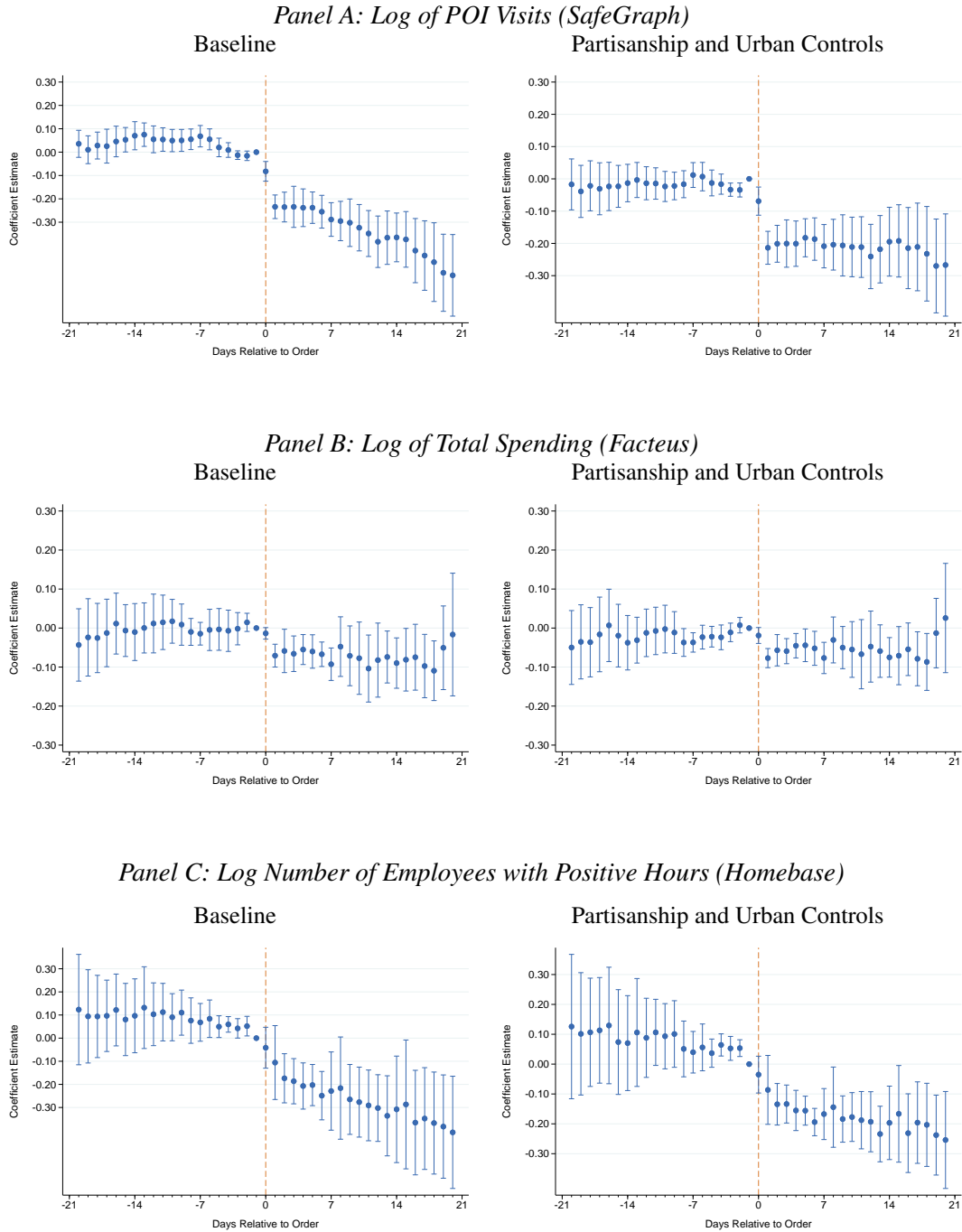
Panel A: Log Number of Debit Transactions (Facteus)

Baseline | Partisanship and Urban Controls

Panel B: Log Work Hours (Homebase)

Baseline | Partisanship and Urban Controls

Panel C: Log Total Wages (Homebase)

Baseline | Partisanship and Urban Controls

Note: Figure plots estimated treatment effects ω_k from the event-study specification outlined in equation (1). Relative to Figure 5, we use alternative economic outcome measures. Panel A includes as the dependent variable the log number of debit transactions in Facteus' debit card sample; Panel B uses the log number of work hours from Homebase; and Panel C gives the log total wages from Homebase for a given home county and day. All plots include county fixed effects μ_i and date fixed effects δ_t. The 'Partisanship and Urban Controls' interact the date fixed effects with indicators for the interaction between being an urban county and being a Republican county. Counties are weighted by population in the regression. Standard errors are clustered at the state level.

36

Appendix Figure A5: Effect of Mandatory Business Closure Orders on Main Economic Outcomes

Panel A: Log of Total Spending (Facteus)

Baseline   Partisanship and Urban Controls

Panel B: Log Number of Employees with Positive Hours (Homebase)

Baseline   Partisanship and Urban Controls

Note: Figure plots estimated treatment effects ω_k from the event-study specification outlined in equation (1). Relative to Figure 5, business closures orders are used instead of stay-at-home orders. Panel A includes as the dependent variable the log of total spending in Facteus' debit card sample for a given home county and day. Panel B uses the log number of individuals with positive work hours from the Homebase sample for a given county and day as the dependent variable. All plots include county fixed effects μ_i and date fixed effects δ_t. The 'Partisanship and Urban Controls' interact the date fixed effects with indicators for the interaction between being an urban county and being a Republican county. Counties are weighted by population in the regression. Standard errors are clustered at the state level.

Appendix Figure A6: Combined Effect of Mandatory Business Closure Orders and Stay-at-Home Orders

Panel A: Log of POI Visits (SafeGraph)

Baseline | Partisanship and Urban Controls

Panel B: Log of Total Spending (Facteus)

Baseline | Partisanship and Urban Controls

Panel C: Log Number of Employees with Positive Hours (Homebase)

Baseline | Partisanship and Urban Controls

Note: Figure plots estimated treatment effects ω_k from the event-study specification outlined in equation (1). Relative to Figure 4, we restrict to counties that implement their business closure and stay-at-home orders at the same time. Panel A uses the log of POI visits from SafeGraph for a given county and day as the dependent variable. Panel B includes as the dependent variable the log of total spending in Facteus' debit card sample for a given home county and day. Panel C uses the log number of individuals with positive work hours from the Homebase sample for a given county and day as the dependent variable. All plots include county fixed effects μ_i and date fixed effects δ_t. The 'Partisanship and Urban Controls' interact the date fixed effects with indicators for the interaction between being an urban county and being a Republican county. Counties are weighted by population in the regression. Standard errors are clustered at the state level.

Appendix Figure A7: Simulations

Panel A: Preferred estimator
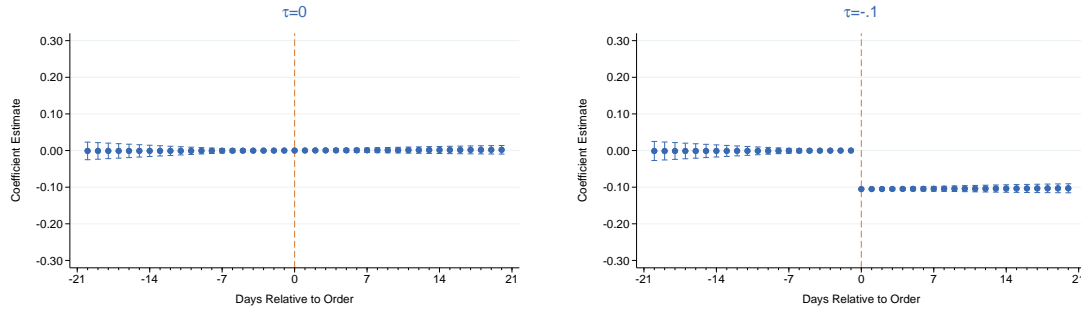
Panel B: Preferred estimator but not controlling for $\log(I_{it})$
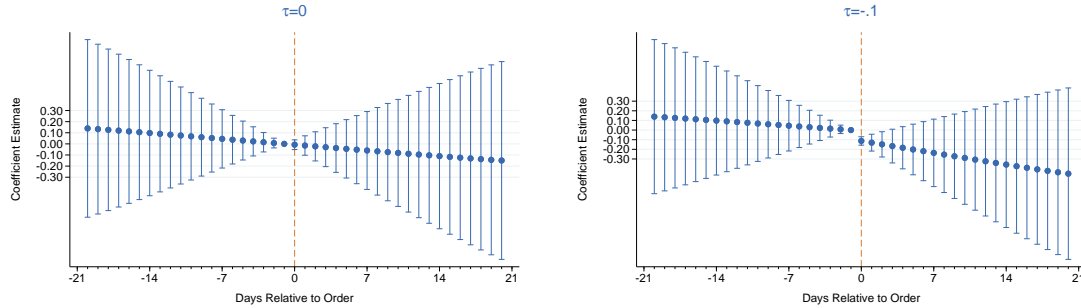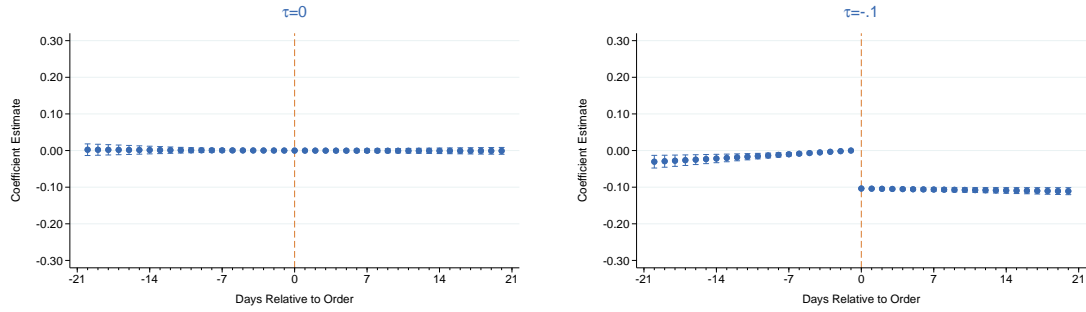

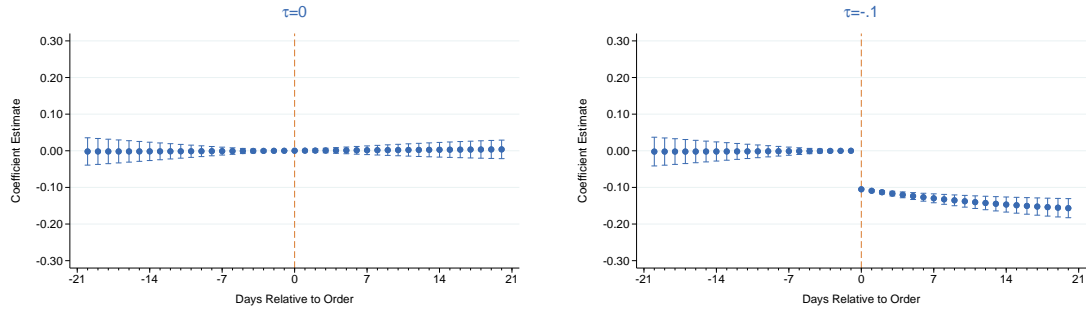### *Panel C: Log cases event study with linear time trends (Dave et al. 2020)*
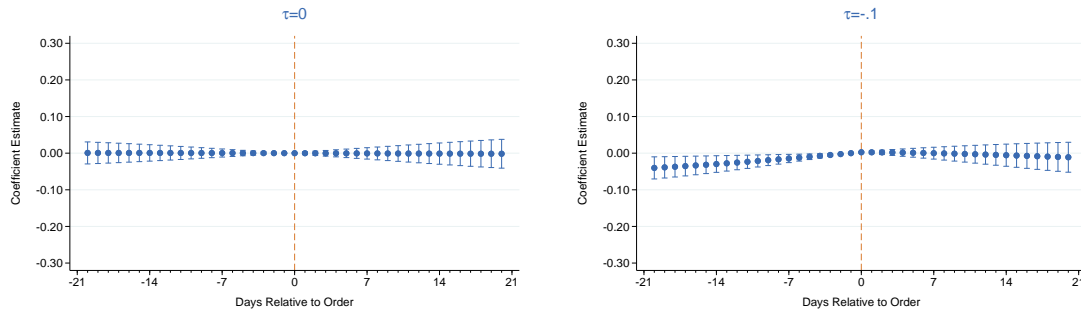
Note: Figure plots estimated treatment effects ωk from the event-study specification outlined in equation (11) using the data simulated from a SIRD model. The first column uses data simulated from a SIRD model where the true treatment effect of the stay-at-home order is $\tau = 0$. The second column uses data simulated from a SIRD model where the true treatment effect of the stay-at-home order is $\tau = -.1$. Panel A our preferred estimator. Panel B is the preferred estimator, but does not control for $\log(I_{it})$. Panel C is Dave et al. (2020) estimator that uses log cases on the left-hand side and controls for geography-specific linear time trends. Geographies are weighted by population in the regression. Standard errors are clustered at the geography level.

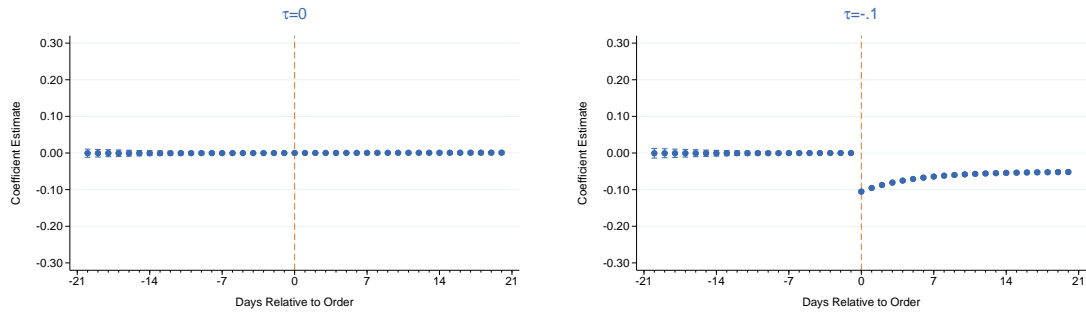## Appendix Figure A8: Simulations with Alternative Specifications

Panel A: Preferred estimator but adding geography FEs

*Panel B: Preferred estimator but setting γ at half its true value*

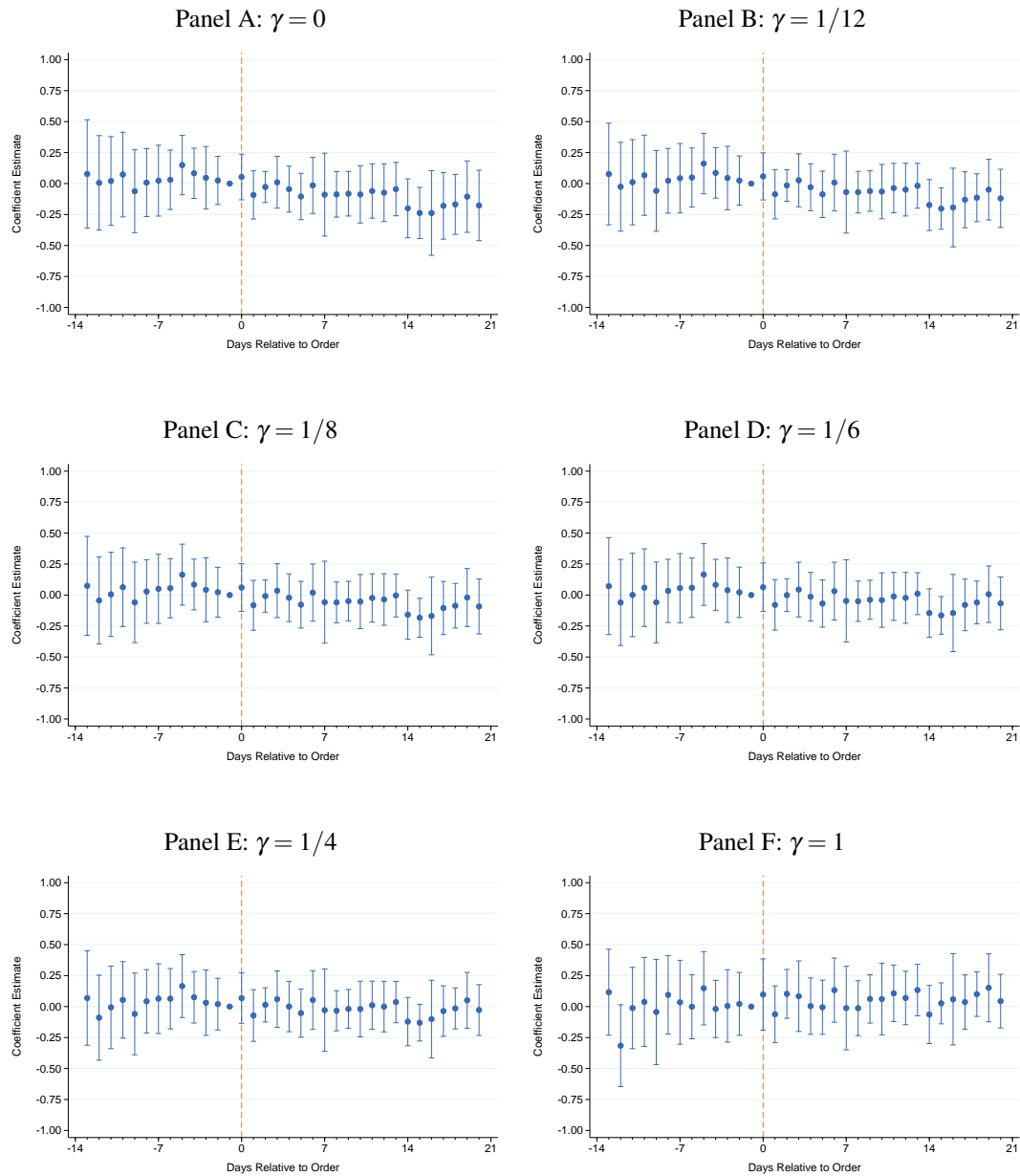*Panel C: Preferred estimator but setting γ at twice its true value*

Note: Figure plots estimated treatment effects ωk from the event-study specification outlined in equation (11) using the data simulated from a SIRD model. The first column uses data simulated from a SIRD model where the true treatment effect of the stay-at-home order is $\tau = 0$. The second column uses data simulated from a SIRD model where the true treatment effect of the stay-at-home order is $\tau = -.1$. Panel A is the preferred estimator, but adds geography fixed effects. Panel B is the preferred estimator, but sets γ to half its true value. Panel C is the preferred estimator, but sets γ to twice its true value. Geographies are weighted by population in the regression. Standard errors are clustered at the geography level.

Appendix Figure A9: Effect of Stay-at-Home Orders on Contact Rate, All γ Values
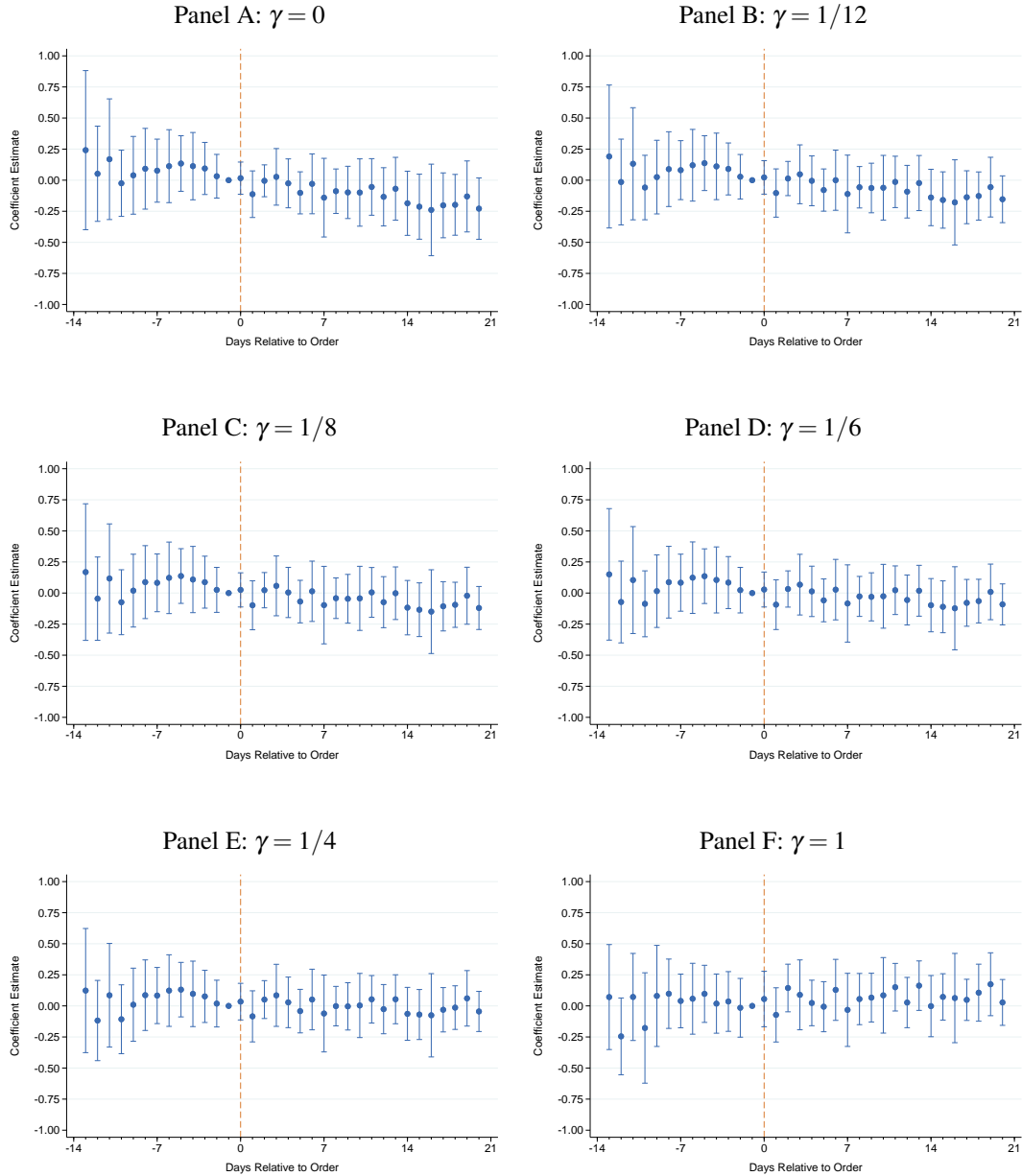
Note: Figure plots estimated treatment effects ω_k from the event-study specification outlined in equation (15) using the log of new cases in a state for a given day as in Panel A of Figure 8. Each panel uses a different value of γ. States are weighted by population in the regression. Standard errors are clustered at the state level.

Appendix Figure A10: Effect of Stay-at-Home Orders on Contact Rate, All γ Values and Controlling for Partisanship and Urban
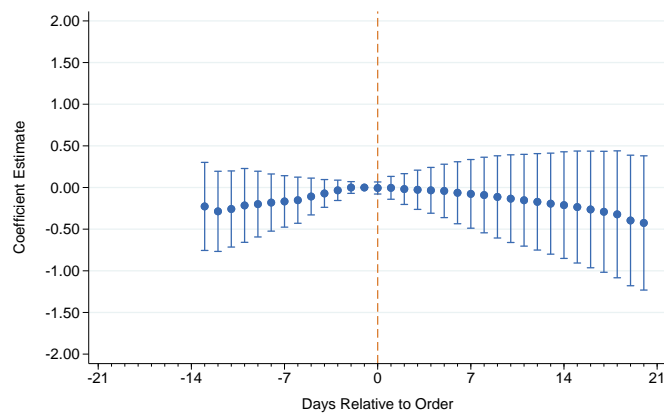

Note: Figure plots estimated treatment effects ω_k from the event-study specification outlined in equation (15) using the log of new cases in a state for a given day as in Panel B of Figure 8. Each panel uses a different value of γ. States are weighted by population in the regression. Standard errors are clustered at the state level.
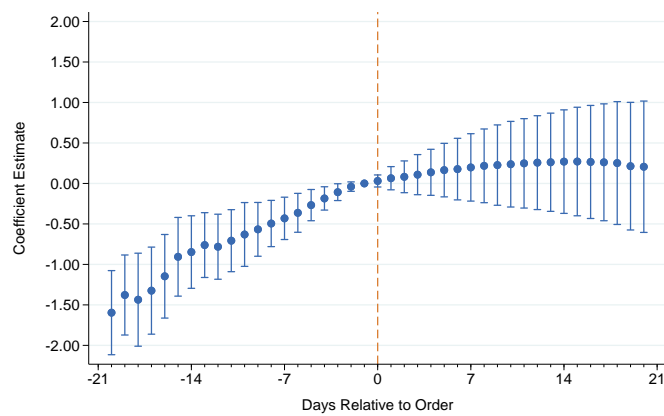
Panel A: 7-day Preperiod as in Dave et al. (2020)
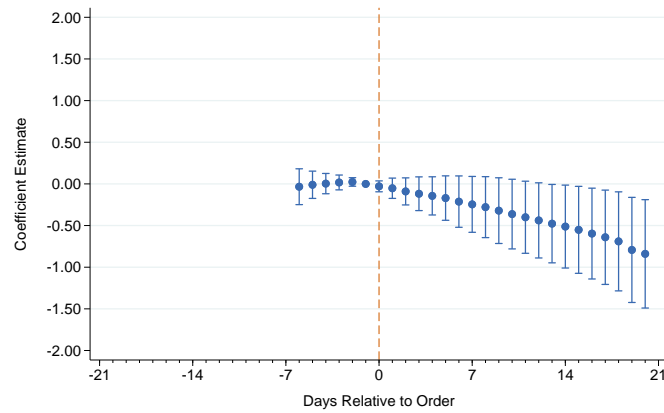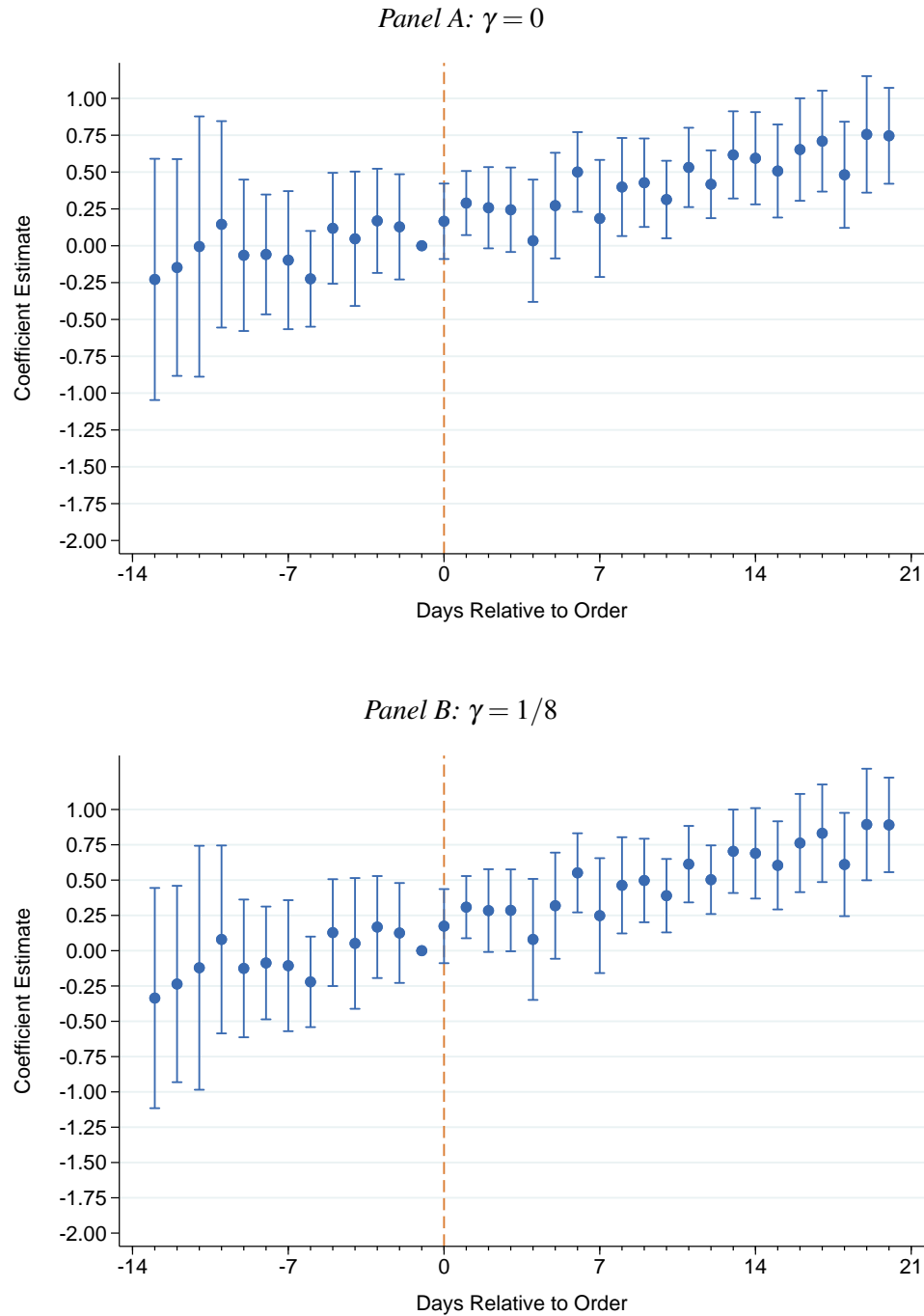

Panel B: 14-day Preperiod

Panel C: 21-day Preperiod

Note: Figure plots estimated treatment effects ω_k from the event-study specification outlined in equation (16) using the log of cases in a state for a given day and including state-specific linear trends. Panel A restricts the preperiod to 7 days. Panels B and C are the same as Panel A, except they use a 14- and 21-day preperiod. States are not balanced across the event window. States are weighted by population in the regression. Data is restricted to dates on or before April 20, 2020. Standard errors are clustered at the state level.

Appendix Figure A12: Effect of Stay-at-Home Orders on Change in Log New Deaths

Panel A: $\gamma = 0$

Panel B: $\gamma = 1/8$

Note: Figure plots estimated treatment effects ω_k from the event-study specification outlined in equation (15) using the log of new deaths in a state for a given day. Panel A only includes date fixed effects δ_t and sets $\gamma = 0$ which implies $\log(I_{it}) = \log(C_{it})$. Panel B is the same as Panel A, except that it sets $\gamma = 1/8$. States are weighted by population in the regression. Standard errors are clustered at the state level.

Appendix Table A1: Sources of Non-pharmaceutical Interventions

	Stay-at-home		Business closures	
	Unweighted	Weighted	Unweighted	Weighted
Inherited from State	0.897	0.464	0.923	0.628
NYT	0.895	0.638	0.000	0.000
Keystone Strategy	0.016	0.078	0.949	0.822
Crowdsourced	0.038	0.215	0.033	0.139
Hikma Health	0.018	0.057	0.016	0.038
Manual Entry	0.034	0.012	0.000	0.000

Note: Table summarizes the source of county stay-at-home and business closure policies. We report shares of county policies both unweighted and weighted by county population. The first row indiciates the share of county policies that are inheritied from the state; that is, counties did not enact the corresponding policy before the state took action. The remaing columns indicate the share of county policies coming from each of the our sources. The manual entry source is reserved for corrections to state policies which we hand checked. We only had to recode the Tennessee state policy.

Appendix Table A2: Decomposing Changes in Distancing, Economic, and Health Outcomes

		(1)	(2)	(3)
Panel A: Stay-at-Home Orders (Counties)				
		$k = 1$	$k = 20$	Pre-Trend
	Total Δ		Policy-Induced Δ	
POI Visits	-0.649	-0.106	-0.220	-0.095
		(0.009)	(0.027)	(0.029)
Homebase Wages	-0.611	-0.083	-0.109	0.050
		(0.033)	(0.062)	(0.065)
Homebase Employment	-0.581	-0.067	-0.175	-0.041
		(0.025)	(0.053)	(0.054)
Facteus Debit Transactions	-0.035	-0.057	-0.078	-0.028
		(0.007)	(0.023)	(0.024)
Facteus Total Spending	0.190	-0.071	-0.044	-0.015
		(0.010)	(0.031)	(0.033)

		(1)	(2)	(3)
Panel B: Business Closure Orders (Counties)				
		$k = 1$	$k = 20$	Pre-Trend
	Total Δ		Policy-Induced Δ	
POI Visits	-0.648	-0.070	-0.216	-0.167
		(0.012)	(0.060)	(0.061)
Homebase Wages	-0.610	-0.145	-0.303	-0.383
		(0.042)	(0.101)	(0.107)
Homebase Employment	-0.580	-0.108	-0.324	-0.418
		(0.028)	(0.064)	(0.066)
Facteus Debit Transactions	-0.035	-0.022	-0.037	-0.036
		(0.009)	(0.048)	(0.048)
Facteus Total Spending	0.191	-0.016	0.004	-0.025
		(0.011)	(0.048)	(0.049)

Note: Table reports the total and policy-induced changes in various outcomes as outlined in Section 6 using alternative estimators of the treatment effect. In Panel A, we consider the policy-induced changes of stay-at-home orders. In Panel B, we consider the policy-induced changes of business closure orders. For $k = 1$ and $k = 20$, we use the treatment effects ω_k with corresponding value k. The 'Pre-Trend' estimator use the trend in estimates in the two weeks leading up to treatment to adjust the treatment effect ω_k for $k = 20$.