Shelter-in-Place Orders and Public Health: Evidence from California During the COVID-19 Pandemic

Andrew I. Friedson
University of Colorado Denver
Center for Health Economics & Policy Studies
Email: andrew.friedson@ucdenver.edu

Drew McNichols
Center for Health Economics & Policy Studies (CHEPS)
University of San Diego-California & San Diego State University
Email: dmcnicho@uoregon.edu

Joseph J. Sabia
Center for Health Economics & Policy Studies (CHEPS)
San Diego State University & IZA
Email: jsabia@sdsu.edu

Dhaval Dave
Bentley University, IZA & NBER
Center for Health Economics & Policy Studies
Email: ddave@bentley.edu

Keywords: coronavirus, COVID-19, shelter in place order, synthetic control

JEL Codes: H75, I18

Abstract

A shelter-in-place order (SIPO) is one of the most restrictive non-pharmaceutical interventions designed to curb the spread of COVID-19. On March 19, 2020, California Governor Gavin Newsom issued the first statewide SIPO in the United States. The order closed non-essential businesses and required residents to shelter in place for all but essential activities such as grocery shopping, retrieving prescriptions from a pharmacy, or caring for relatives. This study is the first in the economics literature to estimate the effect of a statewide SIPO on public health. Using daily state-level coronavirus data and a synthetic control research design, we find that California's statewide SIPO reduced COVID-19 cases by 160.9 to 194.7 per 100,000 population by April 20, one month following the order. We further find that California's SIPO led to as many as 1,566 fewer COVID-19 deaths during this period. Back-of-the-envelope calculations suggest that there were about 649 to 703 job losses per life saved, and about 14 to16 job losses per case averted during this post-treatment period.

1. MOTIVATION

> *"I simply do not know if our aggressive actions early on ... have had the intended effect … I certainly am hoping and praying that that is the case. We still need the data to confirm that."*
>
> - Grant Colfax, San Francisco Director of Public Health, March 31, 2020

The 2020 U.S. coronavirus outbreak is one of the most serious public health challenges in American history. In the peak year of the polio epidemic (1952), 367 out of every million Americans contracted polio (Ochmann and Roser 2020). In the first 7 months of 2020, 462 out of every million Americans *died* from COVID-19.

In contrast to other nations, much of the authority used to combat public health threats in the United States rests not with the Federal government, but with state and local officials. The primary state and local policy strategy to prevent the spread of coronavirus during the early months of the pandemic was the enactment of shelter-in-place orders (SIPOs), sometimes called "stay at home" orders. SIPOs require residents to shelter in place for all but essential activities such as grocery shopping, retrieving prescriptions from a pharmacy, caring for relatives, or traveling to employment in sectors deemed essential.[1]

A SIPO is one of the most restrictive non-pharmaceutical interventions (NPIs) as it places strong limits on both individual and firm choice. Early studies in the U.S. have demonstrated that SIPOs increase social distancing behavior (as measured by cellphone mobility data), and generally find them to be more effective than other NPIs (Abouk and Heydari 2020; Cronin and

[1] For a deeper discussion of the composition of the "essential" workforce, see Blau, Koebe, and Meyerhofer (2020).

Evans 2020; Dave et al. 2020a,b; Goolsbee and Syverson 2020; Gupta et al. 2020; Sears et al. 2020).[2]

This study is the first in the economics literature to explore the impact of a SIPO on public health. We focus on the state of California, which was at the forefront of SIPO adoption as the COVID-19 crisis unfolded in a number of urban centers in the United States. On March 17 and 18, 2020, 12 California counties and the City of Berkeley adopted SIPOs.[3] Then, on the evening of March 19, Governor Gavin Newsom issued the first statewide SIPO, which ordered the closing of all non-essential businesses in the state and required all California residents to shelter in place for all but essential activities.[4]

In issuing the SIPO, Governor Newsom implored residents of California "to meet this moment and flatten the curve together" (Romero 2020). Thus, an important policy rationale was not simply to curb the pandemic's growth in California, but also to delay its peak, allowing the state additional time to obtain the necessary ventilators, hospital beds, and medical staff to meet the surge in demand for services among those who tested positive (Baker and Fink 2020; Greenstone and Nigam 2020; Ranney et al. 2020; Tsai et al. 2020; Hicks and Marsh 2020).

---

[2] This literature also documents a large increase in social distancing behavior in general for both SIPO and non-SIPO adopting locations, presumably due to information spread about the dangers of COVID-19 and associated changes in individual and firm behavior to adjust to the new risk environment.

[3] On March 16, Los Angeles Mayor Eric Garcetti implemented a non-essential business closure order. The seven counties that adopted a SIPO on March 17 were: Alameda, Contra Costa, Mendocino, San Francisco, San Mateo, Santa Clara, and Santa Cruz. The five counties that adopted a SIPO on March 18 were: Monterey, San Benito, Solano, Sonoma, and Ventura.

[4] While grocery stores, pharmacies, restaurants providing takeout or delivery service, and other essential businesses were permitted to remain open, most other non-essential businesses were ordered closed. California's statewide SIPO was soon followed by the closing of many public parks or beaches, driven in part by public outrage over the surge in beach parties and picnics immediately following the executive order (Kopetman 2020). In addition, residents were advised to continue to maintain a six-foot distance with non-household members with whom they come in contact and public gatherings of non-household members were strongly discouraged. Violations of the SIPO were subject to a $1,000 fine and up to 6 months of imprisonment (Allday 2020), though enforcement most often occurred through social pressure and warnings for first offenses.

Following the adoption of California's SIPO, 39 additional states and the District of Columbia enacted similar statewide SIPOs between March 20, 2020 and April 20, 2020.

There are a number of reasons to single out California to study the effects of SIPOs on COVID-19 infection and mortality. First, California's SIPO was enacted early relative to the spread of COVID-19 in the state. No other SIPO-implementing state in the upper quartile of the urbanicity distribution had an average daily coronavirus growth rate lower than California during this period (19 percent).[5] Thus, California serves as a "best case scenario" or reasonable upper bound for the benefits of early intervention. California may also provide a cleaner natural experiment in relation to later adopters, where policy adoption was more closely related to accelerating case growth.

Second, from an analytic perspective, California's position as the earliest mover in the U.S. is advantageous for the purpose of constructing a suitable counterfactual. We are able to draw from the largest possible pool of untreated donor states, limiting the potential for a poorly matched control group, and eliminating the potential for geographic policy spillovers into California from previously treated states.

California also has a number of features that make it an interesting case study. California ranks second out of 51 states (including the District of Columbia) in percent living in urbanized areas and urban clusters and eighth out of 51 states in population-weighted density.[6] The spread of coronavirus is exacerbated by increased population density, which generates greater opportunities for transmission among frequently interacting individuals (Centers for Disease

---

[5] As a comparison, New York, which had the highest per-capita case rate in the nation throughout much of the early outbreak, had an average daily coronavirus case growth rate of 40 percent in the four days prior to its enactment of a SIPO.

[6] In 2019, 95.0 percent of California residents lived in an urbanized area or urban cluster (Iowa Community Indicators Program 2020). The population-weighted density (population density counting only where people live) was 1,851.1 persons per square mile (calculated by the authors).

Control and Prevention 2020). Our findings, therefore, are most appropriately generalized to other highly urbanized states which are the places where COVID-19 (and other similarly transmitted diseases of the future) have the greatest potential to spread rapidly if left unchecked.

Following the release of Friedson et al. (2020), a fast-moving, emerging literature has studied short-run health effects of the "average" SIPO using location-by-day panel data and an event-study framework (Courtemanche et al. 2020; Dave et al. 2020a, b; Sears et al. 2020). Descriptive evidence points to health benefits of SIPOs as well as potentially important heterogeneity in policy impacts across adoption time and population density (Dave et al. 2020a, b). This finding, coupled with the insight that state SIPOs enacted early in the COVID-19 outbreak cycle are likely more plausibly exogenous than those enacted in response to accelerating COVID-19 case growth, underscores the importance of studying the California experience.

Using 40 days of state-level data on confirmed COVID-19 cases and mortality, we explore the early public health effects of California's first-in-the-nation statewide SIPO. Estimates from our preferred synthetic control models show that California's SIPO led to a 160.9 to 194.7 per 100,000 population reduction in COVID-19 cases by April 20, 2020, approximately one month following the SIPO's enactment. We also find a 3.6 to 3.9 per 100,000 population reduction in COVID-19-related deaths in the same time frame, although these estimates are not statistically significant at conventional levels across all methods. These findings are robust to the selection of observables (i.e. pre-treatment COVID-19 rates, population density, urbanicity, and other COVID-related policies) to generate the weights to construct our counterfactual. We find that the number of cases averted and lives saved were much larger in the second and third weeks following the SIPO's adoption, consistent with growing public health benefits over the period during which the outbreak was exponentially growing. Back-of-the-envelope calculations suggest that there were approximately 14 to 16 job losses per coronavirus case averted and 649 to 703 job losses per life saved during this short-run post-SIPO window in California.

2. DATA AND METHODS

2.1 Data

In order to examine the "first stage" compliance with the California SIPO, we utilize publicly available data from SafeGraph Inc.[7] For each state (and county) on each day SafeGraph provides a shelter-in-place index, based on the percent of individuals staying at home during the day. The metric is constructed from spatial data generated using anonymous cell phone pings. First, each cell phone is assigned a "home" (or 153m by 153m square) based on a common nighttime location over a baseline period.[8] SafeGraph then calculates the percent staying at home, i.e. the fraction of cell phones in a geographic unit (state, county, etc.) that do not leave the "home" for any given day.[9] The shelter-in-place index is the percentage point change in the number of cell phones staying at home relative to the baseline of February 6, 2020 through February 12, 2020.[10]

---

[7] Data and detailed descriptions of variable construction are available at: https://www.safegraph.com/dashboard/covid19-shelter-in-place

[8] We note certain inherent limitations with such measures. For instance, it does not capture whether an individual engages in social distancing while outside their home or if someone works at night. Furthermore, the definition of a "home" (a common location, within 153m by 153m square, that receives the most frequent GPS pings during the overnight hours of 6pm to 7am over a six-week period) does not adjust for differences between urban vs. suburban dwellings. Nevertheless, it is plausible to expect that having a higher fraction of the population "fully" sheltering in place would be positively correlated with rates of social distancing. Further, given that our focus is on changes within states over time, any measurement error introduced in these measures cross-spatially will not affect our results.

[9] SafeGraph makes adjustments for small geographic units which are not relevant for a state-level analysis.

[10] So, a value of 25 for the shelter in place index could represent an increase from 12 percent of phones staying at home at baseline to 37 percent of phones staying at home (37-12=25).

To examine the short-run public health effects of the statewide order, we utilize a panel of state-specific daily counts of COVID-19 cases and death reports from March 12, 2020 through April 20, 2020. These data are curated by *The New York Times* based on reports from state and local health agencies.[11] As of April 20, 2020, there were a total of 778,328 confirmed COVID-19 cases in the United States, 4.3 percent (33,862) of which were in California, and 37,372 coronavirus-related deaths, 3.2 percent (1,223) of which were in California.

Appendix Figure 1 (Panel a) shows state-specific trends in cumulative coronavirus rates per 100,000 population in each state. Daily cases can be calculated as the slope of this cumulative case distribution. Over the period under study, the average coronavirus case rate in California was 30.8 per 100,000 population, and its growth rate from March 12 to April 20 was the sixth lowest among the 50 states and the District of Columbia. In addition, California had the lowest case growth rate among the top 25th percentile of the most highly urbanized states, California also had the lowest case rate among the top 25th percentile of the most densely populated states. Panel (b) shows the trends in cumulative coronavirus-related death rates per 100,000 population. Between March 12 and April 20, 2020, the COVID-19 death rate in the United States grew by 8.6 deaths per 100,000 population, while it grew by less than one half as much (3.1 per 100,000 population) in California. New York, New Jersey, Washington, Louisiana, and Massachusetts had among the highest rates of coronavirus-related deaths at the end of the sample period. In terms of mortality, the growth in the COVID-19 death rate in California ranked lowest among the top 25th percentile of the most highly urbanized states.

[11] See data available here: https://www.nytimes.com/interactive/2020/us/coronavirus-us-cases.html. The date for a particular case or death is the day that the case was reported by state public health authorities.

2.2 Synthetic Control Design

We use the synthetic control method introduced by Abadie, Diamond and Hainmueller (2010) to infer the causal impact of a SIPO on the number of confirmed coronavirus cases and the number of COVID-19-related deaths per 100,000 population in California. This method relies on data from pre-treatment COVID-19 case (or mortality) rates and observable characteristics of states that may influence the spread of the virus (i.e. timing of community spread, pre-treatment social distancing, COVID-19 testing, population-weighted density, urbanicity, emergency decrees for a major disaster area, travel restrictions and school closures).[12] The synthetic control approach generates a counterfactual designed to capture how coronavirus cases would have evolved in California in the absence of its SIPO.

Our chief outcomes of interest, $Case\ Rate_{it}$ and $Death\ Rate_{it}$, measure the cumulative number of confirmed coronavirus cases and the number of coronavirus-related deaths per 100,000 population in state *i* at day *t*. We estimate the unobserved counterfactual ("synthetic California") as a weighted linear combination of states included in a donor pool. The weights are chosen so as to generate a synthetic state that is as similar as possible to California on key

[12] Population-weighted density captures the density where the average person lives and is computed by combining population density at the census block group (CBG) level weighted by the population of each CBG (see: https://www.census.gov/programs-surveys/metro-micro/data/tools/metro-micro-help/variables.html and https://www.census.gov/geographies.html). Urbanicity is measured as percentage of total population living in urban areas and is available from Iowa Community Indicators Program (see: https://www.icip.iastate.edu/tables/population/urban-pct-states). Emergency major disaster declarations are measured as a state with a disaster that exceeds the response capabilities of the state and local governments, and long-term recovery assistance is needed and are available from the Federal Emergency Management Agency (see: https://www.fema.gov/disasters). Travel restrictions are measured as states that restrict residents from traveling to other states and/or states that restrict residents of other states from entering the state. COVID-19 tests are measured as the natural log of total coronavirus tests reported by each state. These data are available from COVID Tracking Project (see: https://covidtracking.com). School closures are measured for states that ordered schools to close for the remainder of the 2019-2020 academic year; we obtain this information and the relevant dates from Courtemanche et al. (2020). California did not implement a travel ban; an emergency disaster declaration was issued in the state on March 22, 2020, and school closings were mandated starting on March 19, 2020. California issued its emergency declaration by about 9 days prior to the average state, and ordered school closings by about 1 day prior to the average state. Over the sample period, 16 states imposed travel restrictions, and all states issued an emergency declaration and mandated school closings.

observables. Given the importance of the selection (i) of states to be included in the donor pool, and (ii) observable characteristics on which to closely match California to its synthetic counterpart, it is incumbent on researchers to offer a theoretical defense of these choices and to explore the sensitivity of estimated policy impacts to these choices (Ferman, Pinto, and Possebaum 2020).

We begin our analyses with a donor pool comprised of 42 states and the District of Columbia: 10 states that had never enacted a SIPO during our sample period, and 32 states and the District of Columbia that adopted a SIPO at least 5 days after California did so. We select this five-day period because it is the median incubation period of COVID-19 (Lauer et al. 2020), and thus gives the case data from California sufficient time to reflect underlying changes in the transmission rate before any of the donor states implement their own SIPOs.[13] One limitation is that by including later-adopting SIPO states as potential donors, the synthetic control is contaminated on some post-treatment days. Thus, to the extent that later enacted SIPOs have taken effect, our estimated treatment effects can be construed as lower-bound estimates of the effect of the California SIPO.[14]

As California is more urbanized and densely populated than most other states, our preferred donor pool is further selected to exclude the least urban and least dense states (bottom quintile of states). We initially match on each of seven (7) days (March 12-18) of pre-treatment confirmed COVID-19 case rates per 100,000 population, which effectively requires case growth

[13] This decision rule eliminates the following states from our donor pool: Illinois, New Jersey, New York, Connecticut, Louisiana, Oregon, and Washington. Several of these states (in particular New Jersey and New York) appeared to be on very different pre-treatment case and mortality trends than the majority of states, including California (See Appendix Figure 1). We also re-estimate our synthetic control models without eliminating pre-March 24 SIPO-adopters from the donor pool, with a pattern of results that is unchanged.

[14] As one approach to address this concern, we select an alternate donor pool that includes states that had never adopted a SIPO or had adopted a SIPO but had 4 or fewer days of post-treatment data. The pattern of results remains similar.

rates to be identical. We estimate the unobserved counterfactual COVID-19 case rate for California on pre-treatment day t by $\sum_j w_j * Case\ Rate_{jt}$, where w_j is the weight assigned to donor state j. The analogous counterfactual death rate is $\sum_j w_j * Death\ Rate_{jt}$. The estimated weights w_j are chosen to minimize the absolute difference between $Case\ Rate_{i=CA,t}$ and $\sum_j w_j * Case\ Rate_{jt}$ and for all pre-treatment days, as well as the absolute difference between $Death\ Rate_{i=CA,t}$ and $\sum_j w_j * Death\ Rate_{jt}$. Then, the per-day treatment effect α_t is estimated as $\alpha_t = Y_{i=CA,t} - \sum_j w_j * Y_{jt}$ for t ∈ r [March 19, April 9], where $Y = [Case\ Rate, Death\ Rate]$. The average treatment effect is then the average over the post-treatment window.

While choosing a counterfactual based only on pre-treatment outcomes eliminates concerns of ‘p-hacking’ via selection of matching variables (Botosaru and Ferman, 2019), this approach effectively eliminates the role of factors that may legitimately and strongly influence the path of an outbreak such as timing of community spread, pre-treatment social distancing, urbanicity, weather, state travel restrictions, school closures, or a declaration of a major disaster.[15]

In light of this, we also generate our synthetic counterfactual by giving a larger role to the covariate predictors and drivers of the outbreak, and match on (i) pre-treatment social distancing to ensure that California’s SIPO is not an observable marker for voluntary distancing already underway, (ii) state population-weighted density and a state urbanicity index, factors that play an important role in the spread of infectious disease across communities due to increased crowding (Florida 2020), (iii) other COVID-19 policies (i.e. the number of days that an emergency major

[15] As shown by Kaul et.al (2018), matching on all periods of pre-treatment outcomes renders all covariates irrelevant in the prediction of the outcome.

disaster declaration was in effect for the state, if the state had imposed travel restrictions, and if school closures had been in effect), and (iv) COVID-19 testing rates, which may influence the number of confirmed cases.[16] When matching on these observable characteristics, we also match on the outcome variable for two days (March 12 and March 18) over the pre-treatment period. In addition, we explore the robustness of the results to restricting the set of donor states to those that had experienced community outbreak in COVID-19, including at least 10, 50, or 100 cases in the state.

With California being the first state in the nation to issue a statewide SIPO at a time when the COVID epidemic was still new, cases in the early periods by definition were low. The scale of the difference in the pre-policy periods would also by definition be low compared with latter periods as the epidemic was quickly expanding. Matching on these relatively small values of pre-treatment COVID-19 cases may not fully leverage the construction of a valid counterfactual and end up minimizing meaningful differences prior to policy adoption relative to post-treatment differences.[17] We undertake additional analyses to address concerns on this front. First, in lieu of the absolute case (and mortality) rate, we match on the natural log of the outcome. Relative changes, that is changes in the natural log of confirmed cases for instance, may provide better counterfactual tracking for infections that are growing at a non-linear exponential rate.[18] Second,

[16] One reason why we might see changes in cases is because testing resources have changed. As of March 13, only 15,000 tests had been conducted in the U.S. To address the initial low testing rate in the U.S., the Food and Drug Administration approved a new COVID-19 test from the pharmaceutical company Roche (Arnold 2020). In the following days, states including Delaware, New York, Massachusetts and Texas, began implementing drive-up testing sites, which made access to testing more available (Yancey-Bragg 2020). Despite these improvements in accessibility, many testing delays persisted due to laboratory capacity constraints (Brown and Court 2020). Coronavirus-related deaths are less likely to be affected by this selection into testing.

[17] For this reason, we opted not to extend the pre-treatment window beyond 7 days in our main analyses when we match on pre-treatment outcomes, which would have otherwise brought in even lower case counts. Nevertheless, our results are robust to extending the analysis further back (see Appendix Figure 2, where we expand the pre-policy window to the full two weeks of data predating the SIPO).

[18] The raw number of cases in California is 252 on 3/12, 320 on 3/13, 381 on 3/14, 478 on 3/15, 588 on 3/16, 732 on 3/17, and 893 on 3/18, which is the time period over which we are matching. Hence, while we are not in a situation

we limit matching on direct measures of the outbreak itself (such as confirmed cases or mortality) and allow a larger role in the matching process for factors that may legitimately and strongly influence the path of the COVID outbreak and the timing of community spread (urbanicity, population-weighted density, testing rates, travel restrictions, emergency declarations, school closures, stay-at-home behaviors). Third, we generate a counterfactual for California by matching on outpatient visits for influenza like illnesses (ILIs), derived from the CDC's influenza surveillance system. Volume of ILIs should be correlated with a large number of unobservable characteristics associated with population health and behaviors that are relevant to COVID-19. The general argument is that if a state is a good candidate for ILIs spreading then it is likely a good candidate for COVID-19 spreading. By constructing a counterfactual based on ILIs, we are also able to extend the pre-policy window to October of 2019 and match over a longer period of time, bypassing the need to rely on any direct COVID-19 related outcomes such as cases.[19]

A key strength of the synthetic control design is that, by explicitly netting out any common trends on social distancing (through matching on pre-treatment stay-at-home behaviors), the policy impact we estimate is the effect of the SIPO over and above the effect of any voluntary distancing. This is important given that trends in social distancing were positive even prior to policy adoption, reflecting expanding awareness or concern regarding COVID-19 as well as depressed economic activity. To the extent that the depressed economic activity

---

where everything in the pre-period is zero, the case rate is of orders of magnitude lower relative to the end of the sample period. However, the rate of growth is exponential with cases roughly doubling over this period every 4-5 days. By matching on trends in the natural log of the case rate, we are generating a counterfactual that tracked California identically in terms of the growth rate in cases over the early phase of the outbreak.

[19] Specifically, we bring in information on the percent of visits for influenza-like illness (ILI) reported by physicians to the U.S. ILI Surveillance Network (ILINet) as part of the CDC's flu surveillance system (see: https://www.cdc.gov/flu/weekly/overview.htm).

induced voluntary social distancing, we are identifying the additional effect of a SIPO on top of any economic slowdown, or voluntary distancing (either due to the outbreak or due to the slowdown).[20] We note, however, that the recession can be both a potential confounder as well as a mechanism through which the SIPO may be impacting social distancing and stay-at-home behaviors (and subsequently the COVID-19 caseload).[21]

Finally, for comparison, we also estimate a difference-in-differences model of the following form, drawing on the full and limited set of donor states discussed earlier as controls:

$$\ln (\textit{Case Rate}_{it}) = \beta_0 + \beta_1 SIPO_{it} + \beta_2 X_{it} + \gamma_s + \tau_t + \mu_{it} \quad (1)$$

In the specification above, $SIPO_{it}$ is an indicator for California having the shelter-in-place order in place, X_{it} is a set of state-level, day-varying controls that include indicators for whether the state enacted a statewide non-essential business closure order (that falls short of a shelter-in-place order), whether the state enacted a targeted shelter-in-place order that covers only older individuals over age 65 or those with underlying health conditions, whether the state had enacted travel-related restrictions, whether a major disaster declaration had been issued for the state, whether the state had mandated school closures for the remainder of the academic year, the

[20] In Appendix Figure 3, we plot the SafeGraph shelter-in-place index as well as Google Trends queries for "file for unemployment" in California and in the rest of the U.S. All three of these metrics begin moving well before March 19th, when California instituted its SIPO. This is not necessarily an issue for our identification strategy, as we partial out any common trends and attempt to isolate the effect of the policy change on the spread of the virus. Nevertheless, the Google Trends time series helps to reassure us, because the unemployment patterns appear to be very similar between CA and the rest of the country, suggesting that our strategy would be netting them out.

[21] When we treat Google Trends queries for "file for unemployment" (which is a proxy for unemployment filing) as an outcome, there is some indication that the statewide SIPO generated some economic costs in the form of job-loss, though the effects based on Google queries are noisy and imprecise. The latter effects are consistent with some recent working papers (for instance, Gupta et al. 2020; Baek et al. 2020; Rojas et al. 2020) which find that social distancing policies played some (though not a primary or large) role in moderating economic activity and increasing job-loss. We return to this issue in the concluding section.

average temperature (in degrees Celsius) at all weather stations in the state, and an indicator for whether any weather station in the state reported measurable precipitation. In addition, we control for state fixed effects (γ_s) and day fixed effects (τ_t), identifying our treatment effect (β_1) via within-state variation in the enactment of the SIPO. We also estimate equation (1) where we replace *Case Rate$_{it}$* with *Death Rate$_{it}$*, but given low counts of deaths during the earlier days of our analysis period, we alternately utilize Poisson models and inverse-hyperbolic sine transformations of the dependent variable. The difference-in-differences models do not rely on the synthetic weights or force any matching in the pre-treatment periods, and thus provide a check on the synthetic control estimates. They also allow us to more explicitly control for the timing of the other COVID-19 related policies.

For statistical inference on all point estimates, we conduct placebo tests following the method suggested by Abadie, Diamond and Hainmueller (2010) to generate permutation-based p-values. For the synthetic control estimates, we generate and report these p-values based on the ratio of the post-treatment to the pre-treatment mean squared prediction error (MSPE).[22] Additionally, for all point estimates we report confidence intervals, representing the 10th and the 90th percentiles of all treatment effects generated under randomization inference (Imbens and Rubin 2015).

[22] We compare the pre-treatment and post-treatment mean squared prediction error (MSPE) for each donor state being reclassified as pseudo-treatment state, calculating the MSPE ratio as follows:

$$MSPE\ ratio_j = \frac{\sum_{t=3/19}^{T}(Y_{jt} - Synth_{jt})^2}{\sum_{t=3/12}^{3/18}(Y_{jt} - Synth_{jt})^2}$$

The ranking of the treated states relative to the placebo states then provides a permutation-based p-value. The p-value relies directly on the number of states in the donor pool. With between 32 and 43 states in the donor pool, California's MSPE ratio must rank at least 1st or 2nd to receive a p-value below 0.05 and at least 3rd or 4th to receive a p-value below 0.1. A p-value of less than 0.01 is not attainable given the number of donor states. Our inferences are not materially affected if p-values are generated based on only the post-treatment MSPE. The rationale for the latter method is that the pre-treatment MSPE is close to zero and insignificant in all cases (reflecting that CA and its synthetic control tracked virtually identically over the validation window).

3. RESULTS

Our main findings on the effects of California's statewide SIPO can be found in Figures 1 through 3 and Tables 1 through 5.

3.1 "Stay at home" Behavior and California Statewide SIPO

Figure 1 presents trends in the shelter-in-place index for both California and its synthetic control. The index reflects the percent of the population in a given state that stays at home all day relative to a baseline, derived from anonymized cell phone geotagging. We assign weights to the synthetic control based on close matches in the shelter-in-place index for each of the 7 days prior to CA's statewide SIPO.[23] Trends in social distancing are expectedly positive over the entire analysis period as awareness of COVID-19 was proliferating and the benefits of social distancing were emphasized through public health advisories and guidelines. The synthetically-generated counterfactual tracks California nearly identically prior to the SIPO, with trends markedly diverging only after California issued its statewide order on March 19. Estimates of the average daily treatment effect indicate that the percent of individuals remaining at home throughout the day increased by 2.1 percentage-points (8.5 percent relative to the baseline mean) in California relative to its synthetic control, over the entire post-SIPO period. The effect is larger over the initial post-treatment window up to March 28, indicating a 3.3 percentage-point increase in the average daily rate (12 percent increase relative to the baseline mean) at which individuals shelter at home in California relative to the synthetic control, and suggestive of a rapid run-up in social distancing. Focusing on this window wherein CA experienced the largest

[23] As the baseline for the index is the same for all states (week ending February 12, 2020), and the pre-SIPO trends are nearly identical, the treatment effect can be interpreted as the increase in the percent of households in CA who are staying at home relative to its counterfactual in the post-SIPO period.

gains in social distancing, the effect is statistically significant with a one-sided permutation-based p-value of 0.023. As more donor states issued their own shelter-in-place orders, notably by the end of March, and gains for California decelerated, trends narrowed somewhat between California and its synthetic control.

Analysis of the SafeGraph mobility data underscores three points. First, it provides supporting evidence that individuals in California complied with the shelter-in-place order, and that the SIPO effectively and rapidly reduced social mobility in California above and beyond voluntary social distancing that was already underway. Second, the effect of the SIPO in increasing stay-at-home behavior in California, relative to its control, was generally sustained over the analysis period.[24] Third, the health effects that we estimate below capture the direct effect of California's SIPO on contagion by forcing certain forms of economic activity to stop, as well as the compounding effects of the SIPO accelerating social distancing behavior during the early period of the coronavirus outbreak cycle. The relationship between mobility and illness spread is likely non-linear, particularly when considered over time.

3.2 COVID-19 Confirmed Cases and California Statewide SIPO

Figure 2 shows trends in confirmed COVID-19 cumulative cases from March 12, 2020 through April 20, 2020 for California and its synthetic control.[25] Case rates in California rose fairly linearly over the period from March 12 through March 25 from .6 to 8 per 100,000 population before beginning more exponential growth, reaching 85.6 per 100,000 on April 20,

[24] There is some indication of catch-up in voluntary social distancing by those in control states, perhaps in response to widespread SIPO adoption in other states or general proliferation of awareness and concern regarding COVID-19, leading to some convergence in rates of staying at home by the end of the sample period.
[25] Appendix Table 1 lists the states receiving positive weights (and their corresponding weights) for the analyses underlying Figures 2 and 3.

2020. An important concern in synthetic control analysis is how robust findings are to the choice of observable controls and donor states. Consequently, we present estimates based on various matching strategies as discussed above to assess the sensitivity of our findings and to address specific empirical issues that arise in the estimation.

Our first synthetic control (Panel a) assigns weights based on close matches on all observed covariates and potential drivers of COVID-19 (stay-at-home behavior, testing rates, urbanicity and population-weighted density, and other COVID-19 related policies) in addition to matching on the outcome on two days (March 12 and March 18) over the pre-treatment period.[26] The synthetic control is drawn from a donor pool comprising 42 states plus D.C; "treated" states which adopted a stay-at-home order within temporal proximity to California are excluded. This estimated synthetic control serves as our counterfactual for coronavirus case trends that would have unfolded in California in the absence of the SIPO enactment. As shown in Panel (a), despite not forcing matches on the outcome across all pre-treatment days, COVID-19 case rate trends in California and synthetic California are virtually identical in the pre-March 19 period. During the first five days following treatment, which capture the coronavirus incubation period, coronavirus case rates remain quite similar in California and the synthetic control state.

[26] Based on the V matrix (Abadie, Diamond and Hainmueller, 2010), the most important predictors (other than the pre-treatment outcomes), from among these covariates, in generating the weights and the counterfactual are testing rates, disaster declarations, and stay-at-home behavior for the model presented in Panel (a), with the broader donor pool, and population-weighted density, urbanicity, and school closure for the model presented in Panel (b), with the limited donor pool. In supplementary analyses (available upon request), we generated the synthetic control by matching on the outcome on two days over the pre-treatment window and alternately matching on each of the covariates in turn, in order to gauge the quality of the match and the relative importance of each of the covariates in driving the match quality. The average treatment effect over the post-policy window ranges from -34.8 (matching on the two pre-treatment outcomes plus the travel restriction policy) to -78.1 (matching on the two pre-treatment outcomes plus stay-at-home behavior). In order to minimize reliance on low case counts in the matching strategy, we alternately matched on all of the covariate drivers of community spread of COVID-19 (urbanicity, population-weighted density, testing rates, travel restrictions, emergency declarations, school closings, and stay-at-home behaviors) and limited the amount of matching on pre-period outcomes to just the two days directly preceding the treatment. This yields a largely similar estimate of the policy impact as reported in Tables 1 and 2, on the order of a decline in the cumulative case rate of 74.7 cases (per 100,000 population) on average over the post-treatment period.

However, beginning on March 25 and accelerating soon thereafter, the rate of growth in California's coronavirus cases was substantially lower relative to the synthetic control.

In column (1) of Table 1, we show corresponding point estimates of the average treatment effect realized over the full post-SIPO period from March 19 through April 20 (Panel I), for the post-treatment period beginning March 23, which accounts for the median coronavirus incubation period (Panel II), and for the post-SIPO period beginning March 30, the 97th percentile of the coronavirus incubation period (Panel III). For the full post-treatment period, we estimate that the enactment of the SIPO is associated with a significant 54.2 decline in average coronavirus cases per 100,000 population. The daily decline in coronavirus cases is not constant over the post-treatment window. The estimated public health benefits accelerate in the days following enactment, consistent with an exponential growth in contagion that was averted from the SIPO's enactment. Following March 22, the enactment of the SIPO led to a 61.5 decline in average coronavirus cases per 100,000 population while in the post-March 30 period, the average cumulative case reduction that we attribute to California's SIPO is 78.7 per 100,000 population. This pattern of findings suggests that the public health benefits of the SIPO grew over the near-month long period of analysis. These public health benefits also capture any potential compounding effects due to California responding early during the initial phase of its outbreak cycle.[27] By the final day of case data in our analysis (April 20), we find that there were 160.94 fewer coronavirus cases per 100,000 population,[28] which translates to 63,663 total cases averted over the one month following the shelter-in-place mandate.

---

[27] If we employ the synthetic weights from the models in Table 1 to compute a weighted average date of SIPO enactment among the states in the control group and assign never adopters a pseudo-treatment date that is one day after the end of the analysis period, then, using this measure, California issued its SIPO on average 13 days prior to synthetic California.

[28] This estimate visually corresponds to the gap between California and synthetic California on April 20 in Figure 2 (Panel a). With a population base of 39.5 million, the averted decline in the case rate (160.94) by the end of the

In Panel (b) of Figure 2, we utilize a more limited donor pool, further excluding the least urban and least dense states (bottom quintile), in order to safeguard from California's synthetic control drawing on any states that are ex ante not comparable. Corresponding point estimates, which are presented in the second column in Table 1, indicate slightly larger reductions in confirmed cases. Over the full post-treatment period, the California SIPO led to a 62.9 per 100,000 population decline in average cumulative coronavirus cases. With 32 states forming the potential donor pool, the estimated effect for California ranks first among all of the placebo checks, making it statistically significant (p-value = 0.03).[29] By the end of the sample period, the cumulative number of coronavirus cases averted was approximately 76,997.[30]

Models (1) and (2) permit a larger role for covariates and predictors of COVID-19 in the matching process and limited matching on direct outcome measures of the spread. The final panel in Figure 2 (estimates in column 3 of Table 1) alternately assigns weights based on close matches in pre-SIPO case rates on each of the 7 pre-treatment days, thereby rendering the covariates irrelevant in the matching. The estimated treatment effect remains largely similar, indicating a decline in average cumulative cases of 58.9 (per 100,000 population) in California relative to its control over the post-policy period.[31]

---

sample period converts into 63,663 fewer total cases as a result of the SIPO. To take a more conservative approach, we also consider an alternate strategy where we force coronavirus cases during the period from March 19 through March 23 to be comparable to the donor states. This imposes a null effect of the SIPO on cases due to lagged effect from exposure to symptoms during the incubation period. This exercise yields similar results as our main model.

[29] One-sided p-values generated on only the post-treatment MSPE are somewhat larger, though most of the estimates remain statistically significant based on these p-values. The p-values are 0.068 for the point estimates in column (1), 0.091 for the point estimates in column (2), and 0.152 for those in column (3).

[30] It is important to recall that each of the above estimates is based on a synthetic control from a set of donor states that are partially treated in the latter part of the post-period, and as such are a lower bound for the true effect of the policy. In Appendix Table 2, we examine the sensitivity of estimated policy impacts to the use of never-adopting or later-adopting (after April 5) states as controls. We find that California's statewide SIPO reduced average cumulative case rates by about 183 cases per 100,000 relative to synthetic California. These results are expectedly somewhat larger in magnitude, but also less precise than the estimates when we allow partially treated states in the control group.

[31] In Appendix Table 3 we further implemented the various matching choices recommended in Ferman, Pinto, and Possebaum (2020), including: (i) matching on first ¾ of pre-treatment outcomes; (ii) matching on the first ½ of pre-

It is possible that our donor states may be too different from California in experiencing community spread of the coronavirus prior to policy implementation. Different states experienced coronavirus outbreak at different times. In the pre-treatment period, California had experienced nearly 252 cases of COVID-19 on March 12, which suggests that outbreak and community spread was underway. To ensure that the donor pool for synthetic California had also experienced community spread, we limit our donor pool to states that had experienced at least 10 (column 1), 50 (column 2), or 100 cases (column 3) of coronavirus in Appendix Table 4 (the full synthetic control results are also plotted in Appendix Figure 4). The results continue to show strong evidence that California's SIPO was associated with a reduction in COVID-19 cases.

One concern with generating a counterfactual, during a validation period when the epidemic is still new but expanding rapidly, is that we are necessarily matching on low values of the outcome. These pre-policy case rates, and any differences between the treatment and control, are expectedly on a different order of magnitude than the levels and differences towards the end of our analysis period; the estimated treatment effect then will largely reflect the post-treatment difference between California and its counterfactual.[32] Hence, it is paramount that the counterfactual be credible and track California on observed and unobserved drivers of the transmission of COVID-19.

treatment outcomes; (iii) matching on odd pre-treatment outcome periods; (iv) matching on even pre-treatment outcome periods; (v) matching on the pre-treatment outcome mean; and (vi) matching on three pre-treatment outcome values: first, middle, and last periods. It is validating that our effects are not sensitive to which functions of the pre-treatment periods are included in the matching choice; the average treatment effect over the post-policy period ranges from -42.0 to -63.5.

[32] In other words, the role typically played by the pre-treatment difference between the treated and control units in netting out unobservables across these units will be largely moot because this difference is so small relative to the post-treatment difference.

We take a number of tacks to address this issue. In Table 2 (columns 1-3), we replicate our main analyses (from Table 1), but in lieu of matching on the low case counts, we match on their natural logarithm. Even if the early case counts were relatively small, they were growing exponentially. And, by balancing on the trend in the log of the case rate, we generate a synthetic control that tracks California in terms of the exponential rate of growth in cases over the early phase of the outbreak (Appendix Figure 5). The results confirm previous findings and patterns. California's statewide SIPO is predicted to have reduced its cumulative cases on average by between 54.1 to 65.9 percent, relative to the control group, with this effect becoming progressively stronger over time.[33]

Next, we generate estimates of the policy impact where we match only on the drivers of the community spread, and do not rely on any direct matching on the pre-treatment outcomes themselves. Specifically, in columns 4-7 we match on outpatient visits for influenza-like illnesses (ILI), derived from the CDC's flu surveillance system, in addition to matching on the other observed covariates. By matching on flu-like illnesses, a proxy for state factors that promote transmission of infectious respiratory diseases such as COVID-19, we are also able to extend the pre-SIPO window back to October of 2019. Columns (4) and (5) of Table 2 utilize the broader donor pool, alternately matching on the mean of the ILI outpatient visits over the pre-treatment window vs. matching on these visits for every week over this window;[34] the final two columns report parallel estimates for the limited donor pool that excludes the least urban and dense states. Two points are worthy of note. First, despite not forcing matches on the outcome across any of the pre-treatment periods, California and its synthetic control trend virtually

[33] Given that the outcome is the natural log of the caseload, the percent change implied by $\hat{\beta}_1$ from an estimation of model 1 is $[e^{\widehat{\beta_1}} - 1] * 100 = [e^{-0.778} - 1] * 100 = -54.1$ percent.
[34] ILI outpatient visits are available from the CDC ILI Network at the weekly level.

identically with respect to confirmed cases prior to SIPO adoption (Appendix Figures 6A and 6B).[35] Second, the magnitude of the policy effect across these models remains very similar to those discussed earlier. That the estimated effects remain consistent across alternate donor pools, matching algorithms, and variation in the donors and weights used to form the counterfactual set instills a degree of confidence to our results.

While we match on the average testing rate over the sample period in all models, to further ensure that coronavirus testing is not biasing estimates of the effect of California's SIPO on confirmed cases, we alternately select weights for donor states by also matching on the average rate of testing separately over each week of the analysis period, spanning both the pre-treatment and post-treatment windows (see Appendix Figure 7). After forcing testing rates to be similar between California and its synthetic control throughout the sample timeframe, we continue to find that the California SIPO led to an average reduction of 52.5 COVID-19 cases per 100,000 population. By April 20, we estimate 57,484 COVID-19 cases averted.[36]

Table 3 presents standard difference-in-differences estimates of the effect of California's SIPO on confirmed COVID-19 cases (columns 1 and 2) and the natural log of confirmed cases (columns 3 and 4), based on equation (1). We present estimates capitalizing on both the broader

---

[35] The pre-treatment window extends to October 2019; the figures are truncated since COVID-19 case counts were low in early March, and zero prior to that. It is validating that a counterfactual derived from ILI visits over a longer lookback period tracked California so well with respect to the increase in COVID cases.

[36] In Appendix Figure 8 and Appendix Table 5, we also directly assess whether the statewide SIPO is associated with reduced testing in California relative to its counterfactual. There is some indication that testing for the coronavirus may have increased in the short-term following the SIPO, suggesting that estimated case reductions may be biased toward zero during this period, though, after about two weeks following CA's SIPO, testing rates in the control group increased more rapidly relative to CA. None of the point estimates (Appendix Table 5) are statistically distinguishable from zero. We note that testing may also be a potential mechanism as well; SIPOs may affect testing because infected individuals who are unaware of their infection may choose to stay at home rather than seek out testing or other medical care, possibly because of fears of contagion at medical facilities or because of a desire to adhere to a "civic duty" to shelter in place. Nevertheless, that we continue to find significant declines in the case rate in California (Appendix Figure 7), even after matching on testing rates over the full analysis period, suggests that the treatment effect is not driven by differential testing rates.

donor pool (columns 1 and 3) and the limited donor pool (columns 2 and 4) as alternate controls. Our findings provide consistent evidence that the California SIPO had substantial negative effects on coronavirus cases. Panel I presents the average treatment effect over the post-policy period, reflecting about a 64.3 to 66.1 percent decline in the coronavirus case rate (based on the log models) or about a reduction of 60.7 cases per 100,000 population (based on model 1).

Consistent with the synthetic control estimates, we find that the effectiveness of the SIPO grows larger following the virus's incubation period and the period from time until first symptom until respiratory failure. This is reflected in Panel II, where we decompose the average treatment effect based on the approximate interquartile range of the incubation window following enactment of the SIPO.[37]

Together, the findings in Tables 1 through 3 provide compelling evidence that California's first-in-the-nation SIPO generated important public health benefits in preventing the spread of the coronavirus during the first three weeks of enactment.[38] Next, we explore whether these case declines generated improvements in mortality rates.

---

[37] As a robustness check and to more aggressively adjust for selection on observables, we also implemented a propensity score-weighted difference-in-differences, assigning larger weights to control states that are ex ante similar to California at baseline. The predicted propensity score for each of the control states, based on a probit model, is a function of the outcome (confirmed COVID case rate or death rate) and all observable covariates over the pre-treatment period. We then estimate the difference-in-differences models, giving CA a weight of one and each donor state the inverse probability treatment weight of (1 / (1 – propensity)) (Thoemmes and Ong 2016). Alternately, we applied normalized weights, wherein each control state is given a weight that is proportional to the probability of their being similar to California (propensity score) relative to the probability of their being a control state (# of control states / 1 + # of control states)) (Stuart et al. 2014). We find similar point estimates of the policy impact (63.4 to 66.7 percent decline in cumulative cases on average over the post-policy window), though the effects are imprecisely estimated (p-value of 0.132 – 0.154, based on randomization inference). When we decompose the average treatment effect into policy lags, we find a similar pattern of results as in the synthetic control and conventional difference-in-differences models, with effects becoming progressively larger with the length of time from the adoption of the policy. See Appendix Table 6.

[38] In Appendix Figure 9, we show synthetic control results when we use daily case rates rather than cumulative case rates. This isolates a different local average treatment effect; that is, the effect of a SIPO on the rate of change in the change in cumulative coronavirus cases. Furthermore, this exercise explicitly matches on the daily growth in COVID-19 cases in all of the pre-SIPO periods. By doing so, we are also implicitly matching on the second derivative of the cumulative case rate function, which is the growth in the daily change or the acceleration in the growth in daily case rates. Daily case rate data are noisier, but the results continue to provide evidence that California's SIPO reduced coronavirus-related infections.

3.3 COVID-19 Deaths and California Statewide SIPO

In Figure 3, we show estimates of the effect of California's SIPO on coronavirus-related mortality. The figure shows the exponential rise in cumulative COVID-19-related deaths in California, from 0.01 per 100,000 population (4 deaths) on March 12 to 3.09 per 100,000 population (1,223 deaths) on April 20. Each of the three panels parallels our three main matching strategies for confirmed cases (from Table 1). In Panel (a), when we generate a synthetic control based on the broader donor pool, matching on all observed covariates in conjunction with matching on the mortality rate on two days from the pre-treatment validation window, we find strong evidence of a pre-March 19 common trend for California and synthetic California. On March 25, 6 days following enactment of the SIPO, we find that the rate of increase in mortality begins falling in California as compared to the estimated counterfactual, with a gap that widens exponentially over time, getting particularly large nearly two weeks after the policy's adoption on March 31, 2020.

This longer lagged effect on mortality relative to cases is to be expected given the incubation period from exposure to symptoms and time from first symptoms to acute respiratory distress syndrome (ARDS), the latter of which may take up to 8 days (Wang et al. 2020). We note that there are channels through which mortality may be affected by SIPOs that are not directly affected by exposure to contagious infected patients. For instance, SIPOs may affect the likelihood that previously infected patients seek medical care due to beliefs about contagion at medical facilities. SIPOs may also impact the availability of resources for medical care, including testing, as public resources are used to enforce SIPOs.

The remaining panels in Figure 3 visually show the estimated effect of California's SIPO when we restrict the donor pool, further excluding the least urbanized and dense states (Panel b), and shift the matching from covariates to matching on all pre-treatment outcomes (Panel c). Corresponding point estimates with permutation-based inferential statistics are reported in Table 4. Column (2) shows that California's SIPO led to an average decline in the coronavirus death rate of approximately 1.4 per 100,000. By April 20, there were 1,566 fewer COVID-19 deaths. Combined with the estimated case effect shown in column (1) of Table 4, our estimated mortality effect implies a coronavirus-related mortality rate of 2.2 percent, somewhat high, but within the range of mortality estimates suggested by the WHO and CDC (World Health Organization 2020; Wilson et al. 2020). Of course, there are a number of caveats to this back-of-the-envelope assessment to judge the credibility of our estimates. First, deaths occur with a lag with respect to confirmed cases. Second, during the earlier stages of the outbreak, there is evidence that state medical resource constraints permitted testing of only those with more severe symptoms and a higher probability of succumbing to their illness (Baker and Fink 2020). Third, this implied mortality rate is based on the margin of cases averted due to the SIPO, which may be different than the average infected case.[39]

While our estimated mortality decline is substantial in magnitude, permutation-based p-values are insufficiently small to conclude definitively that there was a decline in COVID-19 deaths due to California's SIPO. As expected, the size of the death reduction induced by the state SIPO grows much larger when the incubation period and time from symptoms until possible death are excluded from the analysis. The estimated treatment effect grows later in the

[39] In other words, the implied mortality rate is based on (Δ Death Rate / Δ Case Rate) and may be different than the average mortality rate (Total Death Rate / Total Case Rate). The average treatment effects from the other columns imply a marginal coronavirus-related mortality rate of 2.21 percent to 2.53 percent.

treatment window and is largest following March 30, when we expect lagged effects on mortality due to reaching the sum of the median incubation period and median time until ARDS. Specifically, we estimate effect sizes that range from –1.77 to (Table 4, column 1, Panel III) to –2.17 (Table 4, column 3, Panel III) per 100,000 population, realized on average 20+ days after the adoption of the shelter-in-place mandate.

The potential concerns that arise with matching on low outcome counts during the early phase of the outbreak cycle are more pronounced for mortality.[40] In Appendix Figures 10A and 10B, we examine whether the estimated mortality effects we observe are sensitive to alternately matching on only observed covariates and influenza-like illness outpatient visits over a longer pre-treatment window (through October 2019). And, in Appendix Figure 11, we assess sensitivity to requiring donor states to have experienced community spread. The findings suggest that we obtain comparable estimated lives saved as those reported in Table 4. Specifically, we estimate that the adoption of a SIPO is associated with 636 to 1,556 fewer deaths across these specifications, with a median estimate of around 1,436 lives saved.

Finally, in Table 5, we present difference-in-differences estimates of the effect of California's SIPO on COVID-19-related mortality. Given low counts of deaths and zero counts for a few states during the early periods, we present estimates from Poisson models (columns 1 and 2) and from an inverse hyperbolic sine transformation of the mortality rate (columns 3 and 4). The latter approximates the natural log, is interpreted in a similar manner, but has the advantage of retaining observations with zero death counts (Bellemare and Wichman 2020). Panel I shows our findings on the average treatment effect in the post-treatment period, while

[40] Because death rates during the early phase of the outbreak were low for all states, and 0 for a few states during the first few days, we also generated synthetic control estimates for deaths by matching on COVID-19 cases on all pre-treatment days. Estimated effects for mortality rates are not sensitive to matching on case rates, and continue to indicate a marked reduction in deaths in CA relative to the control group.

Panel II decomposes the treatment effect into the early period when death effects should be relatively small (time from incubation until ARDS) and the latter period accounting for this lag. We find robust evidence across all specifications that California's SIPO results in a significant decline in mortality, on the order of 75.0 percent to 78.8 percent on average over the post-treatment period.[41] The mortality declines from the implementation of the SIPO are generally larger in the period following March 30, as expected. Over this post-incubation-until-ARDS period (Panel IV), we find consistent evidence that the SIPO was associated with an 81.6 to 91.6 percent decline in deaths.

Taken together, the results from Tables 4 and 5 provide strong evidence that California's SIPO generated substantial short-run public health benefits via reduced coronavirus-related mortality.

4. CONCLUSIONS

In this study we rigorously examine the short-run impact of California's SIPO on public health outcomes. Specifically, our preferred synthetic control estimates imply that the SIPO led to between a 160.9 to 194.7 per 100,000 population reduction in COVID-19 cases and a 3.6 to 3.9 per 100,000 population reduction in COVID-19-related deaths per 100,000 as of one month following the SIPO's enactment. These findings are robust to a wide array of choices with regards to the donor pool as well as the observables with which we weight potential donor states.

[41] Given the possibility of over-dispersion in deaths, we also estimated the count models via negative binomial regression. While computationally more intensive, we find estimated SIPO effects that are qualitatively similar. For example, in column (1) of Panel III, we find that that the SIPO was associated with an 81.7 percent decline in deaths. This compares to an identical decline using a negative binomial model. Appendix Table 6 presents propensity score-weighted difference-in-differences estimates, which imply a 49.0 percent to 51.9 percent decline in the death rate on average over the 30 days following the enactment of the SIPO.

Moreover, lagged case and mortality effects are consistent with the median incubation period of COVID-19 as well as estimates of time from first symptoms until ARDS.

These findings have several policy implications for pandemic response. Most important is the top line result that SIPOs were effective as slowing the early spread of COVID-19, and thus can be an effective tool for subsequent waves of COVID-19 or for future pandemics with similar means of transmission, so long as the population responds to future SIPOs in a similar manner. This study also underscores the importance of California's early action, and serves as a plausible upper bound for the benefits of a SIPO. When compared to estimates from Dave et al. (2020a), who find that the average state-level SIPO decreases cumulative cases by 3,073 after almost a month, which translates to approximately 47.9 cases per 100,000 population for the average state, the benefits of the California SIPO are considerably larger.[42]

To the extent that SIPOs' public health returns are driven by changes in mobility, it appears that small changes in mobility can yield large changes in public health. This study, as well as others have found modest, but significant decreases in mobility due to SIPOs (for example Cronin and Evans (2020) find that SIPOs decrease foot traffic at businesses by between 4 and 21 percent, depending on the type of business). These small changes in transmission behaviors can have compounding effects (as disease spread can be exponential if left unchecked in a population), leading to much larger changes in transmission and mortality outcomes. Taken together with the larger literature, this study serves as an important point of caution for policymakers and analysts that small estimates for the effect of NPIs on mobility metrics should not be disregarded.

[42] Some of this difference may also be due to California being more densely populated than the average state.

One should also note that SIPOs are far from costless. Baek et al. (2020) estimate that SIPOs accounted for approximately 23.5 percent of new unemployment insurance claims between March 14th and April 4th 2020. Similarly, Beland, Brodeur and Wright (2020) estimate that SIPOs increased the unemployment rate by nearly 4 percentage points. Back-of-the-envelope calculations from our own synthetic control analyses suggest that there were approximately 14 to 16 job losses per coronavirus case averted and 649 to 703 job losses per life saved during this short-run post-SIPO window in California.[43] These relatively steep costs need to be appropriately balanced against the public health benefits of SIPOs (as well as the benefits and costs of other relevant policies that could replace a SIPO) in order to make sound policy decisions.

Finally, there are many questions about the long-run dynamics of SIPOs and of the COVID-19 pandemic that are as of yet unresolved. If some of the deaths or illnesses averted by the SIPO were merely postponed to the future when the SIPO is lifted, the intervention's net health benefits will be smaller. Likewise, whether the job losses accumulated during the SIPO are short or long lived has large implications for the economic cost of the policy. The findings in this study (and in the growing literature) establish the short-run effects of the policy – the long-run impact will likely not be known for years.

[43] Specifically, we find that the SIPO adoption in California resulted in an increase in 714,773 to 1,001,372 jobs lost (as proxied by UI claims), which combined with our estimates of 61,669 to 71,519 total cases averted (from our synthetic control estimates), implies 14 to 16 job losses per case averted. Similarly, the estimated increase in UI claims combined with the mortality effects (1,424 to 1,623 deaths averted) imply 649 to 703 job losses per life saved.

References

Abadie, A., Diamond, A., & Hainmueller, J. (2010). Synthetic Control Methods for Comparative Case Studies: Estimating the Effect of California's Tobacco Control Program. Journal of the American Statistical Association 105 (490): 493–505.

Abouk, R., & Heydari. B. (2020). The Immediate Effect of COVID-19 Policies on Social Distancing Behavior in the United States. Available at SSRN: https://ssrn.com/abstract=3571421

Allday, E. (2020). Bay Area Orders 'Shelter in Place,' Only Essential Businesses Open in 6 Counties. San Francisco Chronicle, March 16. Available at: https://www.sfchronicle.com/local-politics/article/Bay-Area-must-shelter-in-place-Only-15135014.php

Arnold, C. (2020). U.S. Coronavirus Testing Gets A Potential Breakthrough. NPR, March 13. Available at https://www.npr.org/2020/03/13/815522836/u-s-coronavirus-testing-gets-a-breakthrough

Baek, C., McCrory, P.B., Messer, T., & Mui, P. (2020). Unemployment effects of stay-at-home orders: Evidence from high frequency claims data. Institute for Research on Labor and Employment Working Paper 101-20.

Baker, M. & Fink. S. (2020). At the Top of the Covid-19 Curve, How Do Hospitals Decide Who Gets Treatment? The New York Times, March 31. Available at: https://www.nytimes.com/2020/03/31/us/coronavirus-covid-triage-rationing-ventilators.html

Beland, L.P., Brodeur, A., & Wright, T., (2020). COVID-19, stay-at-home orders and employment: Evidence from CPS data. IZA Discussion Paper No. 13282.

Bellemare, M.F., & Wichman, C.J. (2020). Elasticities and the inverse hyperbolic sine transformation. Oxford Bulletin of Economics and Statistics, 82(1): 50-61.

Blau, F.D., Koebe, J., & Meyerhofer, P. (2020). Essential and Frontline Workers in the COVID-19 Crisis. Econofact, Available at: https://econofact.org/essential-and-frontline-workers-in-the-covid-19-crisis

Botosaru, I., & Ferman, B. (2019). On the Role of Covariates in the Synthetic Control Method. The Econometrics Journal 22(2): 117-130.

Brown, K.V. & Court, E. (2020). U.S. Labs Face Crisis After Crisis Despite Improvements in Testing. Bloomberg, April 7, 2020. Available at: https://www.bloomberg.com/news/articles/2020-04-07/coronavirus-testing-accuracy-and-availability-shortages-remain

Centers for Disease Control and Prevention. (2020). Social Distancing, Quarantine, and Isolation. Available at: https://www.cdc.gov/coronavirus/2019-ncov/prevent-getting-sick/social-distancing.html

Courtemanche, C., Garuccio, J., Le, A., Pinkston, J.C. & Yelowitz, A. (2020). Strong Social Distancing Measures In The United States Reduced The COVID-19 Growth Rate. Health Affairs, 39(7): 1237-1246.

Dave, D.M., Friedson, A.I., Matsuzawa, K. & Sabia, J.J., (2020a). When Do Shelter-In-Place Orders Fight COVID-19 Best? Policy Heterogeneity Across States And Adoption Time. Economic Inquiry, forthcoming.

Dave, D.M., Friedson, A.I., Matsuzawa, K. Sabia, J.J., & Safford, S. (2020b). Were Urban Cowboys Enough to Control COVID-19? Local Shelter-In-Place Orders and Coronavirus Case Growth. NBER Working Paper No. 27229.

Florida, R. (2020). The Geography of Coronavirus. Available at: https://www.citylab.com/equity/2020/04/coronavirus-spread-map-city-urban-density-suburbs-rural-data/609394/

Friedson, A.I., McNichols, D., Sabia, J.J. & Dave, D. (2020). Did California's shelter-in-place order work? early coronavirus-related public health effects. NBER Working Paper No. 26992.

Ferman, B., Pinto, C., & Possebom, V. (2020). Cherry picking with synthetic controls. Journal of Policy Analysis and Management, 39(2): 510-532.

Goolsbee, A., & Syverson, C. (2020). Fear, lockdown, and diversion: Comparing drivers of pandemic economic decline 2020. NBER Working Paper No. 27432

Gupta, S., Nguyen, T.D., Rojas, F.L., Raman, S., Lee, B., Bento, A., Simon, K.I. & Wing, C. (2020). Tracking Public and Private Responses to the COVID-19 Epidemic: Evidence from State and Local Government Actions. NBER Working Paper No. 27027..

Greenstone, M., and Nigam, V. (2020). Does Social Distancing Matter? Becker Friedman Institute for Economics Working Paper, 2020-26. Available at SSRN: https://ssrn.com/abstract=3561244

Hicks, N., and Marsh, J. 2020. Coronavirus in NY: City Has Fewer Than 400 Free Intensive Care Beds. New York Post, March 31. Available at: https://nypost.com/2020/03/31/coronavirus-in-ny-nyc-has-fewer-than-400-free-hospital-beds/

Iowa Community Indicators Program. (2020). Urban Percentage of the Population for States, Historical. Available at: https://www.icip.iastate.edu/tables/population/urban-pct-states

Imbens, G.W., and Rubin, D.B. (2015). Causal inference in statistics, social, and biomedical sciences. Cambridge University Press.

Kaul, A., Klößner, S., Pfeifer, G., & Schieler, M. (2015). Synthetic Control Methods: Never Use All Pre-Intervention Outcomes Together with Covariates. Working Paper, Saarland University, Saarbrucken, Saarland, Germany

Kopetman, R. (2020). Santa Ana Freezes Rent Increases; Adds to Closure List. The Orange County Register. April 20. Available at: https://www.ocregister.com/2020/04/10/coronavirus-santa-ana-freezes-rent-increases-adds-to-its-closure-list/

Ochmann, S., & Max R. (2020). Polio. Available at: https://ourworldindata.org/polio

Ranney, M.L., Griffeth, V., & Jha, A.K., (2020). Critical Supply Shortages – The Need for Ventilators and Personal Protective Equipment during the Covid-19 Pandemic. The New England Journal of Medicine, 382:e41.

Rojas, F.L., Jiang, X., Montenovo, L., Simon, K.I., Weinberg, B.A., & Wing, C. (2020). Is the cure worse than the problem itself? immediate labor market effects of covid-19 case rates and school closures in the us. NBER Workiing Paper No. w27127.

Romero, D. (2020). What are the Limits of Stay-at-Home Orders? NBC News, March 20. Available at: https://www.nbcnews.com/news/us-news/what-are-limits-stay-home-orders-n1165391

Sears, J. & Villas-Boas, J. M, Villas-Boas, V. & Villas-Boas, S. (2020). Are We #StayingHome to Flatten the Curve? Department of Agricultural and Resource Economics. CUDARE Working Papers. Available at SSRN: https://ssrn.com/abstract=3569791.

Stuart, E.A., Huskamp, H.A., Duckworth, K., Simmons, J., Song, Z., Chernew, M.E. & Barry, C.L. (2014). Using propensity scores in difference-in-differences models to estimate the effects of a policy change. Health Services and Outcomes Research Methodology, 14(4), pp.166-182.

Thoemmes, F. and Ong, A.D., (2016). A primer on inverse probability of treatment weighting and marginal structural models. Emerging Adulthood, 4(1), pp.40-59.

Tsai, Thomas C., Jacobson, B.H., & Jha, A.K. (2020). American Hospital Capacity and Projected Need for COVID-19 Patient Care. Health Affairs, March 17. Available at: https://www.healthaffairs.org/do/10.1377/hblog20200317.457910/full/

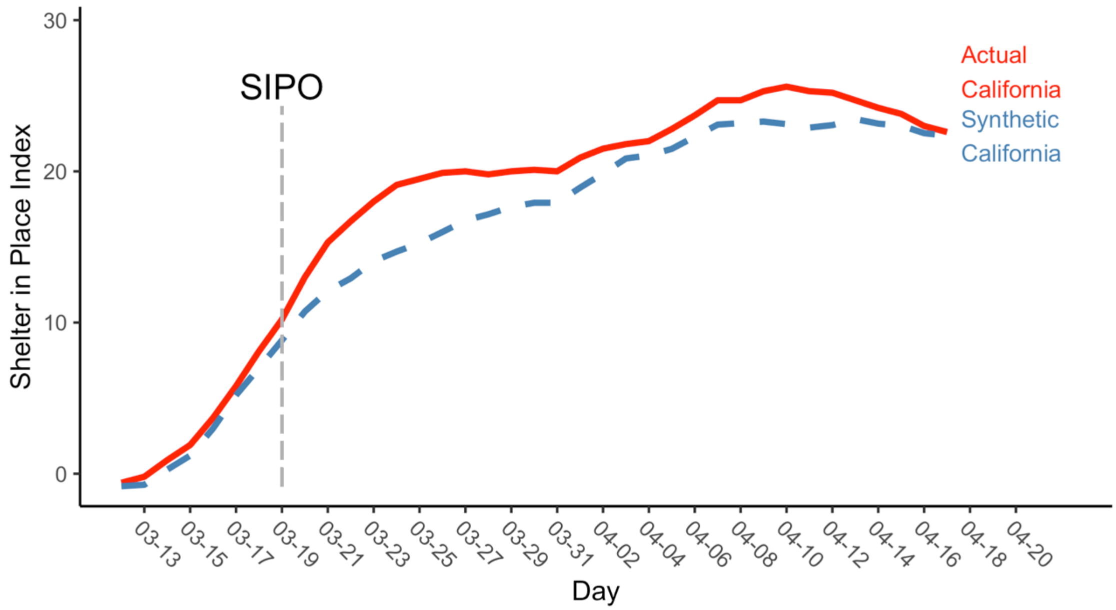
Wang, D., Hu, B., Hu, C., Zhu, F., Liu, X., Zhang, J., Wang, B., Xiang, H., Cheng, Z., Xiong, Y., Zhao, Y., Li, Y., Wang, X., & Peng. Z., (2020). Clinical Characteristics of 138 Hospitalized Patients With 2019 Novel Coronavirus–Infected Pneumonia in Wuhan, China. JAMA.

Wilson, N., Kvalsvig, A., Barnard, L.T. & Baker, M.G. (2020). Case-fatality risk estimates for COVID-19 calculated by using a lag time for fatality. Emerging infectious diseases 26(6):1339-1441.

World Health Organization. (2020). Coronavirus Disease 2019 (COVID-19) Situation Report – 46. Available at: https://www.who.int/docs/default-source/coronaviruse/situation-reports/20200306-sitrep-46-covid-19.pdf?sfvrsn=96b04adf

Yancey-Bragg, N. (2020). Going to a drive-thru COVID-19 Testing Site? Here's a Step-By-Step Look at What to Expect. USA Today, March 20. Available at: https://www.usatoday.com/story/news/health/2020/03/20/drive-thru-coronavirus-testing-step-step-look-what-expect/287332400

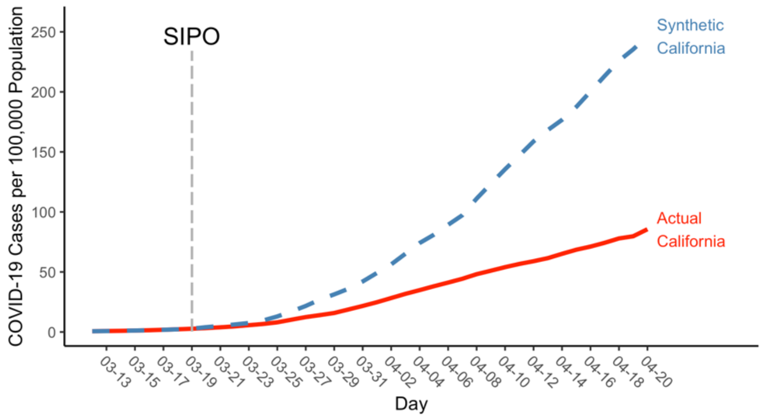
Figure 1: Synthetic Control Estimates for Shelter-in-Place Index



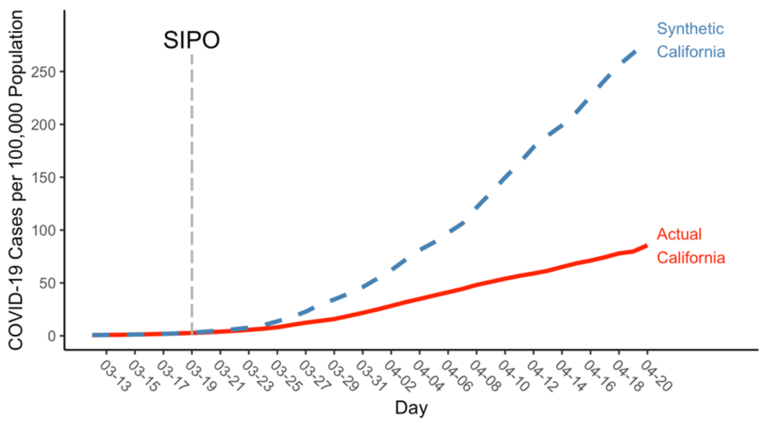
Notes: Estimate is generated using synthetic control methods. The matching was based on seven days of the pre-SIPO shelter-in-place index. Synthetic California is comprised of DC (.666) and MA (.333). P-values are computed using permutation test. Two-sided p-value is .95 and one-sided p-value is .53. However, the p-value generated from a one-sided test of an examination of the March 19 through March 28 is .02.

Figure 2: Synthetic Control Estimates for COVID-19 Cases Per 100,000 Population

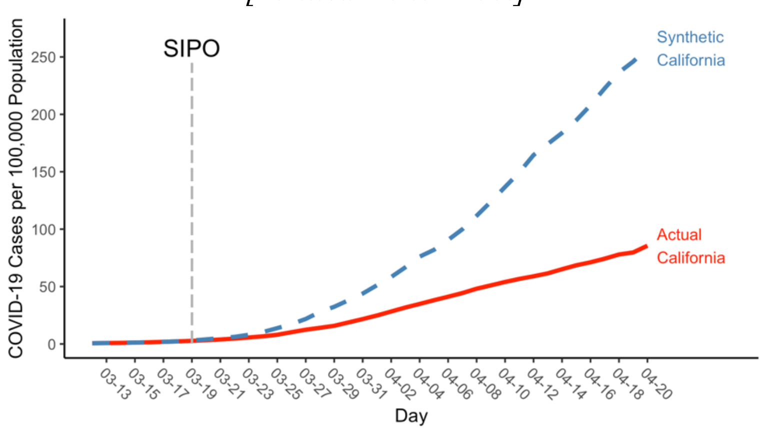
Panel (a): Matching on 2 Days of Pre-Treatment Days & Observables [Full Donor Pool]



Panel (b): Matching on 2 Days of Pre-Treatment Days & Observables [Limited Donor Pool]



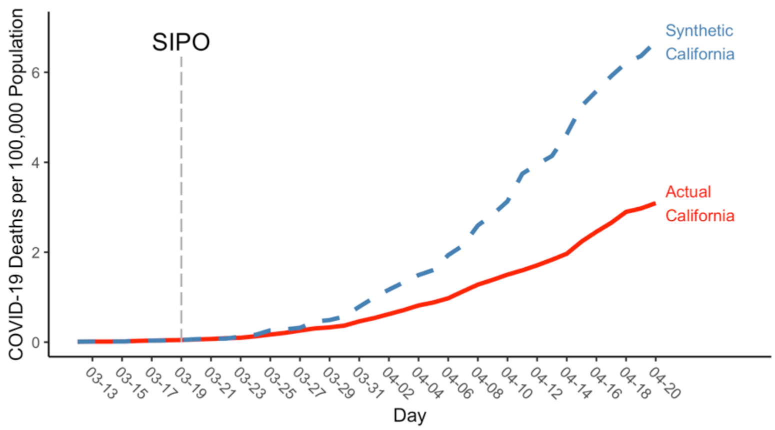
Panel (c): Matching on 7 Pre-Treatment Days [Limited Donor Pool]



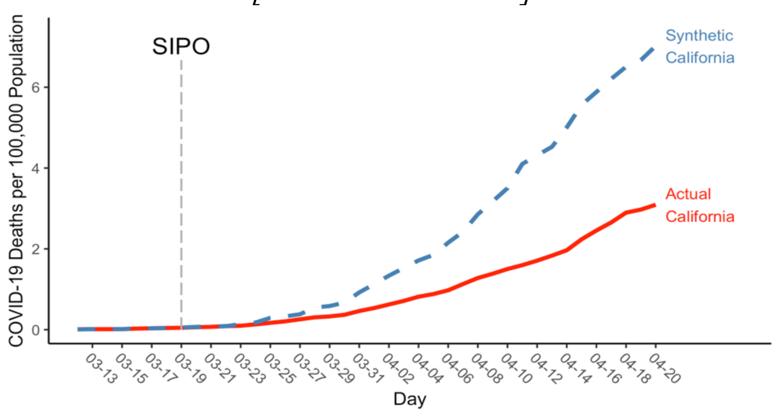
Notes: Observable controls include urbanicity, population-weighted density, the mean COVID-19 testing rate, number of days a disaster emergency declaration was in place, a pretreatment shelter in place index, the number of days the state had a travel ban, and the number of days state public schools were closed. The list of states that received positive weights are listed in Appendix Table 1. The full donor pool is comprised of states that had not implemented a statewide SIPO by March 22. The limited donor pool further limits donor states to the upper 80^{th} percentile in population-weighted density and urbanicity.

Figure 3: Synthetic Control Estimates for COVID-19 Deaths Per 100,000 Population

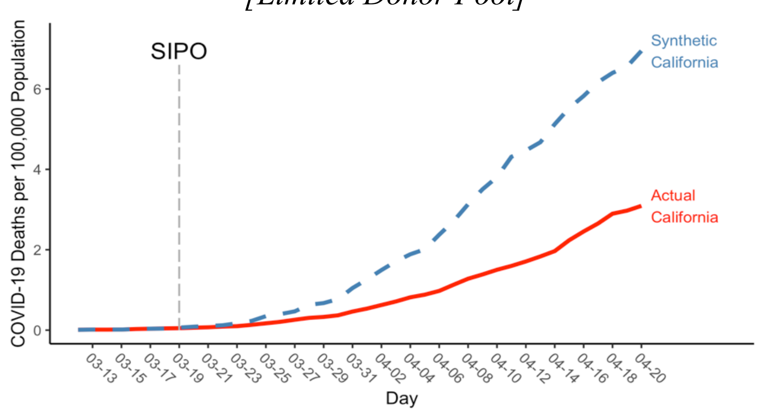
Panel (a): Matching on 2 Days of Pre-Treatment Days & Observables
[Full Donor Pool]



Panel (b): Matching on 2 Days of Pre-Treatment Days & Observables
[Limited Donor Pool]



Panel (c): Matching on 7 Pre-Treatment Days
[Limited Donor Pool]



Notes: Observable controls include urbanicity, population-weighted density, the mean COVID-19 testing rate, number of days a disaster emergency declaration was in place, a pretreatment shelter in place index, the number of days the state had a travel ban, and the number of days state public schools were closed. The list of states that received positive weights are listed in Appendix Table 4. The full donor pool is comprised of states that had not implemented a statewide SIPO by March 22. The limited donor pool further limits donor states to the upper 80th percentile in population-weighted density and urbanicity.

Table 1: Synthetic Control Estimates of Effect of SIPO on COVID-19 Cases per 100,000 Population

	(1)	(2)	(3)
	Panel I: Post-Treatment Window -- March 19 to April 20		
SIPO	-54.193*	-62.870**	-58.869*
P-Value	[0.091]	[0.030]	[0.182]
One Sided P-Value	[0.068]	[0.030]	[0.091]
90% Confidence Interval	(-108.39,-26.51)	(-173.77,-35.19)	(-15.63,-174.10)
	Panel II: Post-Treatment Window -- March 23 to April 20		
SIPO	-61.541*	-71.412**	-66.856*
P-Value	[0.091]	[0.030]	[0.182]
One Sided P-Value	[0.068]	[0.030]	[0.091]
90% Confidence Interval	(-123.08,-30.10)	(-196.93,-39.97)	(-197.28,-17.82)
	Panel III: Post-Treatment Window -- March 30 to April 20		
SIPO	-78.709*	-91.286*	-85.464*
P-Value	[0.114]	[0.061]	[0.182]
One Sided P-Value	[0.091]	[0.061]	[0.091]
90% Confidence Interval	(-157.42,-37.68)	(-249.07,-50.26)	(-251.41,-23.48)
Donor Pool and Matching Variables			
Size of Donor Pool	43	32	32
Pre-SIPO Covid-19 Matching Days	2	2	7
Match on All Observable Controls	Yes	Yes	No

* Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level

Notes: Estimates are generated using synthetic control methods. The matching was constructed using pre-SIPO COVID-19 cases per 100,000 and variables listed under each column. All observable controls include state urbanicity rate, population-weighted density, mean COVID-19 testing rate, number of days under disaster emergency declaration, pre-SIPO shelter in place index, number of days of travel ban, and number of days of school closings. The permutation-based p-values are included in brackets below each point estimate. Confidence intervals, generated using Fisher's permutation test, are reported in parentheses.

Table 2: Sensitivity of Synthetic Control Estimates to Functional Form and Pre-Treatment Influenza Matches

	Log (COVID-19 Cases per 100,000 Pop)			*COVID-19 Cases per 100,000 Population*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Panel I: Post-Treatment Window -- March 19 to April 20			
SIPO	-0.778**	-0.969*	-1.075	-63.317**	-67.846**	-79.207**	-53.387**
P-Value	[0.045]	[0.061]	[0.242]	[0.023]	[0.023]	[0.031]	[0.031]
One Sided P-Value	[0.045]	[0.061]	[0.121]	[0.023]	[0.023]	[0.031]	[0.031]
90% Confidence Interval	(-1.57,-0.04)	(-1.90,-0.30)	[-2.19,-0.63)	(-126.63,-36.51)	[(135.69,-30.53)	(-172.53,-56.19)	(-146.69,-16.07)
				Panel II: Post-Treatment Window -- March 23 to April 20			
SIPO	-0.853**	-1.072*	-1.175	-71.924**	-77.073**	-90.014**	-60.631**
P-Value	[0.045]	[0.061]	[0.242]	[0.023]	[0.023]	[0.031]	[0.031]
One Sided P-Value	[0.045]	[0.061]	[0.121]	[0.023]	[0.023]	[0.031]	[0.031]
90% Confidence Interval	(-1.71,-0.09)	[-2.13,-0.34]	[-2.43,-0.68]	(-143.85,-41.41)	(-154.15,-34.65)	(-195.58,-63.93)	(-166.16,-18.21)
				Panel III: Post-Treatment Window -- March 30 to April 20			
SIPO	-0.934**	-1.209*	-1.277	-92.158**	-98.529**	-115.453**	-77.390**
P-Value	[0.045]	[0.061]	[0.242]	[0.023]	[0.023]	[0.031]	[0.031]
One Sided P-Value	[0.045]	[0.061]	[0.121]	[0.023]	[0.023]	[0.031]	[0.031]
90% Confidence Interval	(-1.87,-0.13)	(-2.42,-0.36)	(-2.67,-0.70)	(-184.32,-51.76)	(-197.06,-44.35)	(-247.68,-81.69)	(-209.52,-23.21)
Donor Pool and Matching Variables							
Size of Donor Pool	43	32	32	43	43	32	32
Pre-SIPO Covid-19 Matching Days	2	2	7	0	0	0	0
Match on All Observable Controls	Yes	Yes	No	Yes	Yes	Yes	Yes
Match on Mean Pre-Treatment ILI	No	No	No	Yes	No	Yes	No
Match on Weekly Pre-Treatment ILI	No	No	No	No	Yes	No	Yes

* Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level

Notes: Estimates are generated using synthetic control methods. The matching was constructed using pre-SIPO COVID-19 cases per 100,000 and variables listed under each column. All observable controls include state urbanicity rate, population-weighted density, mean COVID-19 testing rate, number of days under disaster emergency declaration, pre-SIPO shelter in place index, number of days of travel ban, and number of days of school closings. The permutation-based p-values are included in brackets below each point estimate. Confidence intervals, generated using Fisher's permutation test, are reported in parentheses.

Table 3: Difference-in-Differences Estimates of the Effect of California's SIPO on COVID-19 Cases

	Cases Per 100,000		Log (Cases Per 100,000)	
	(1)	(2)	(3)	(4)
	Panel I: Post-Treatment Effect			
SIPO	-62.35**	-44.11	-0.97*	-0.889*
P-value	[0.023]	[0.121]	[0.068]	[0.061]
90% Confidence Interval	(-89.20, 22.11)	(-92.18, 46.84)	(-1.25, 0.14)	(-1.77, 0.15)
N	1804	1353	1796	1351
	Panel II: Lagged Effects			
March 19-22	11.51	19.84	-0.322	-0.329
P-value	[0.954]	[0.939]	[0.182]	[0.121]
90% Confidence Interval	(6.11, 22.81)	(12.74, 39.69)	(-0.80, 0.17)	(-0.69, 00.03)
March 23-29	-18.22**	-0.53	-0.814*	-0.786*
P-value	[0.023]	[0.636]	[0.068]	[0.061]
90% Confidence Interval	(-30.02, -0.84)	(-15.23, 21.02)	(-1.62, -0.11)	(-1.57, 0.11)
March 30+	-92.23***	-64.49	-1.265**	-1.218**
P-value	[0.023]	[0.151]	[0.023]	[0.030]
90% Confidence Interval	(-163.79, 27.01)	(-138.88, 78.50)	(-2.39, 0.06)	(-2.43, 0.05)
N	1804	1353	1796	1351
States?	Full	Limited	Full	Limited

* Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level

Notes: All estimates include the following controls: COVID-19 testing rate, an indicator for a non-essential business closure order or a targeted SIPO in a given state-day, an indicator for whether a state has issued a travel ban, an indicator for whether a state declared a disaster emergency, an indicator for whether precipitation fell in the state, average temperature, an indicator for whether schools were closed, the COVID-19 testing rate, and state and day fixed effects. P-values, generated using permutation tests, are reported in brackets. Confidence intervals, generated using Fisher's permutation test, are reported in parentheses.

Table 4: Synthetic Control Estimates of Effect of SIPO on COVID-19 Deaths per 100,000 Population

	(1)	(2)	(3)
	Panel I: Post-Treatment Window -- March 19 to April 20		
SIPO	-1.199	-1.392	-1.49
P-Value	[0.744]	[0.758]	[0.848]
One Sided P-Value	[0.419]	[0.455]	[0.242]
90% Confidence Interval	(-3.74,0.79)	(-5.10,1.17)	(-5.74,1.25)
	Panel II: Post-Treatment Window -- March 23 to April 20		
SIPO	-1.365	-1.583	-1.691
P-Value	[0.744]	[0.788]	[0.848]
One Sided P-Value	[0.419]	[0.485]	[0.242]
90% Confidence Interval	(-4.25,0.90)	(-5.80,1.33)	(-6.53,1.42)
	Panel III: Post-Treatment Window -- March 30 to April 20		
SIPO	-1.772	-2.044	-2.165
P-Value	[0.767]	[0.818]	[0.848]
One Sided P-Value	[0.419]	[0.485]	[0.242]
90% Confidence Interval	(-5.47, 1.12)	(-7.50,1.75)	(-8.42,1.85)
Donor Pool and Matching Variables			
Size of Donor Pool	43	32	32
Pre-SIPO Covid-19 Matching Days	2	2	7
Match on All Observable Controls	Yes	Yes	No

* Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level

Notes: Estimates are generated using synthetic control methods. The matching was constructed using pre-SIPO COVID-19 deaths per 100,000 and variables listed under each column. All observable controls include state urbanicity rate, population-weighted density, mean COVID-19 testing rate, number of days under disaster emergency declaration, pre-SIPO shelter in place index, number of days of travel ban, and number of days of school closings. The permutation-based p-values are included in brackets below each point estimate. Confidence intervals, generated using Fisher's permutation test, are reported in parentheses.

Table 5: Difference-in-Differences Estimates of the Effect of California's SIPO on COVID-19 Deaths

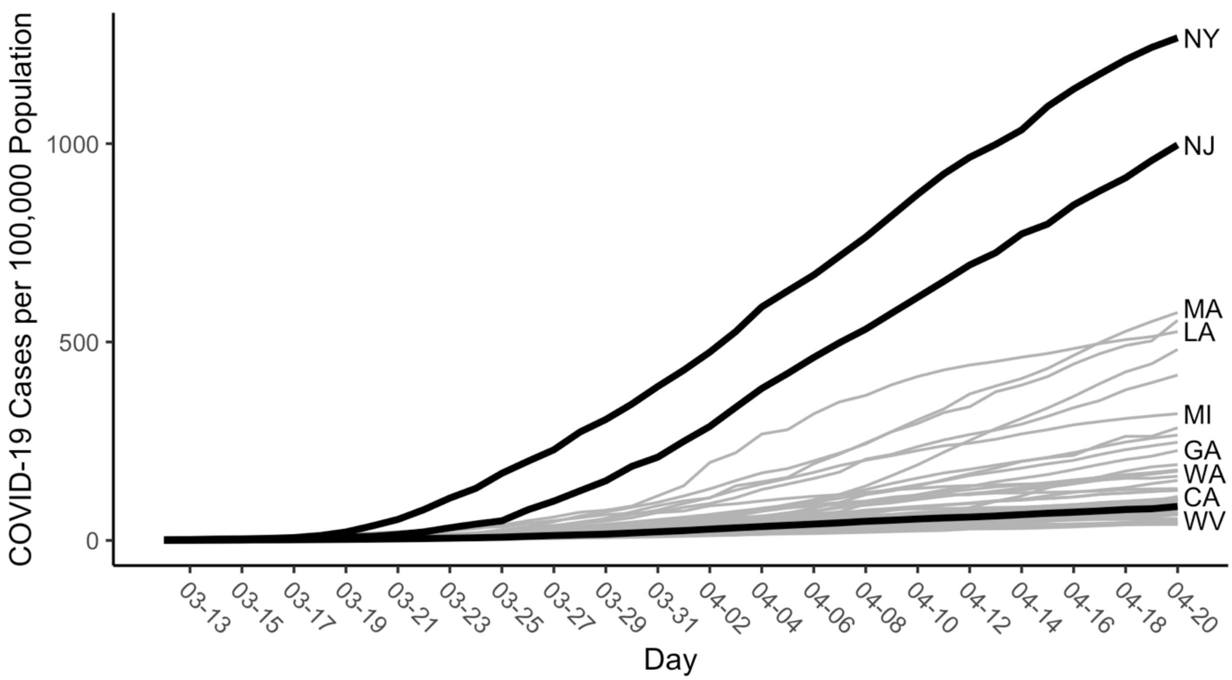
	Poisson Model of Deaths		OLS Model of Inverse Hyperbolic Sine of Deaths	
	(1)	(2)	(3)	(4)
	Panel I: Post-Treatment Effect			
SIPO	-1.54**	-1.55**	-1.687*	-1.386*
P-value	[0.045]	[0.030]	[0.068]	[0.061]
90% Confidence Interval	(-3.01, -1.08)	(-2.58, -0.99)	(-3.37, -0.41)	(-2.77, 0.15)
N	1804	1353	1804	1353
	Panel II: Lagged Effects			
March 19-22	-0.477	-0.433	-0.149	-0.307
P-value	[0.182]	[0.212]	[0.478]	[0.212]
90% Confidence Interval	(-1.25, -0.01)	(-0.81, 0.07)	(-0.68, 0.80)	(-1.02, 0.70)
March 23-29	-1.141*	-1.108*	-1.088	-0.78*
P-value	[0.068]	[0.061]	[0.136]	[0.091]
90% Confidence Interval	(-2.28, -0.59)	(-1.78, -0.72)	(-2.74, 0.31)	(-1.62, -0.002)
March 30+	-1.832**	-1.773**	-2.474*	-1.693**
P-value	[0.045]	[0.030]	[0.068]	[0.030]
90% Confidence Interval	(-3.06, -1.57)	(-1.98, -1.43)	(-4.95, -1.16)	(-2.74, -1.28)
N	1804	1353	1804	1353
Donor Pool?	Full	Limited	Full	Limited

* Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level

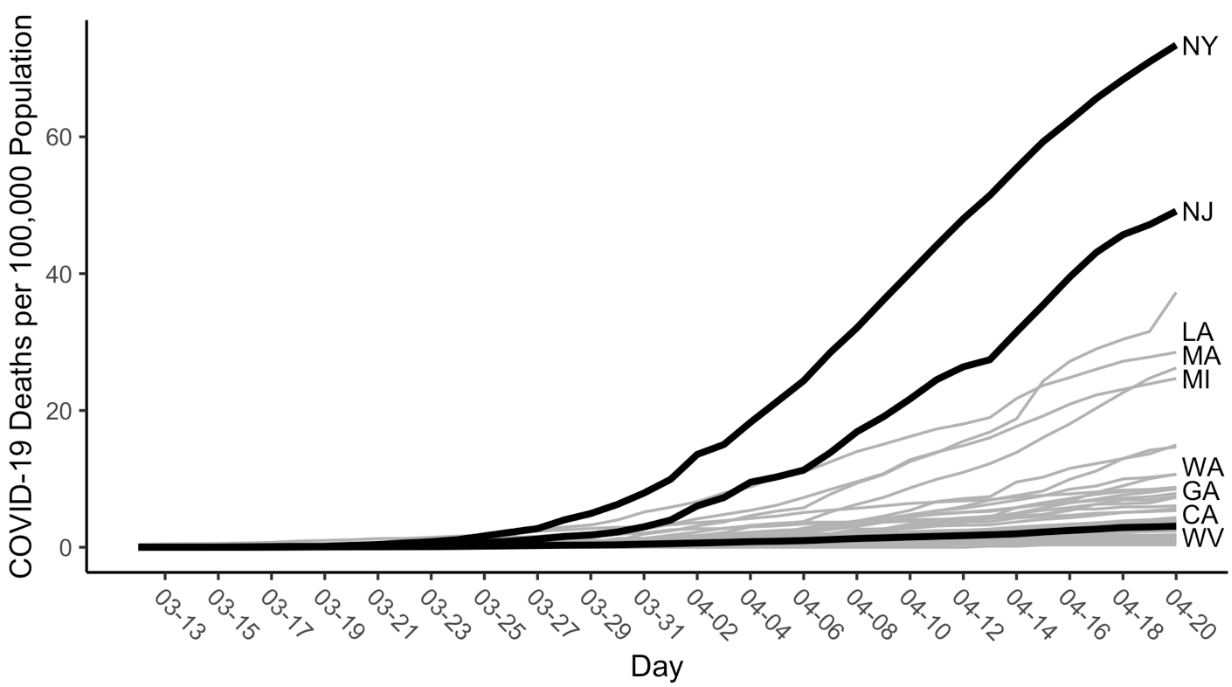
Notes: All estimates include the following controls: an indicator for whether a state has issued a travel ban, an indicator for whether a state declared a disaster emergency, an indicator for whether precipitation fell in the state, average temperature, an indicator for whether schools were closed, the COVID-19 testing rate, and state and day fixed effects. P-values, generated using permutation tests, are reported in brackets. Confidence intervals, generated using Fisher's permutation test, are reported in parentheses.

Appendix Figure 1. COVID-19 Trends by State, March 12-April 20, 2020

Panel (a): COVID-19 Cases Per 100,000



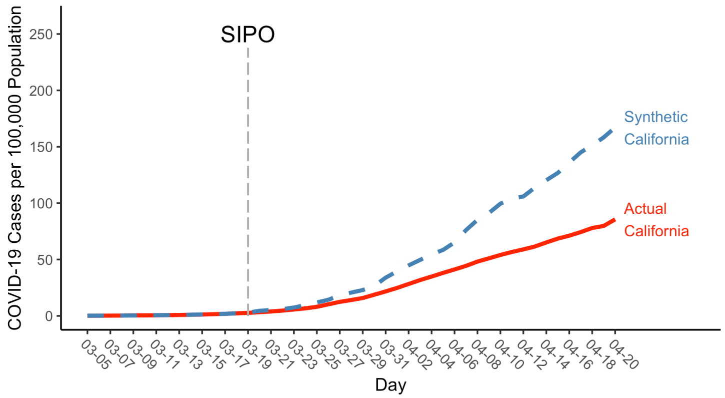
Panel (b): COVID-19 Deaths Per 100,000



Appendix Figure 2. Sensitivity of Synthetic Control Estimates to Two-Week Pre-Treatment Period

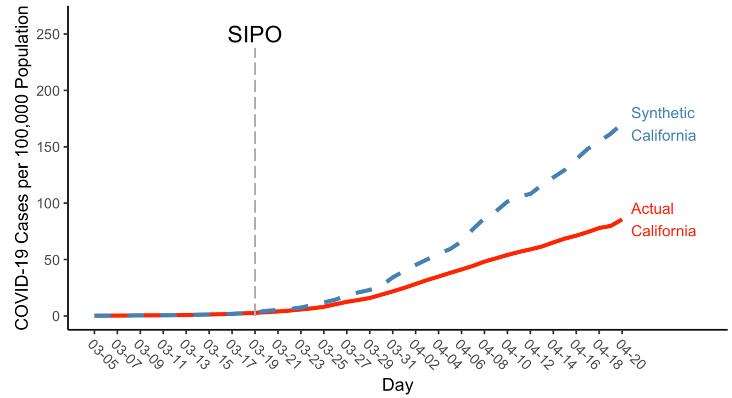
[All Models Match on Two Pre-Treatment Covid-19 Days and the Full Set of Observable Characteristics]

Panel (a):<u>COVID-19 Cases</u> – Full Donor Pool



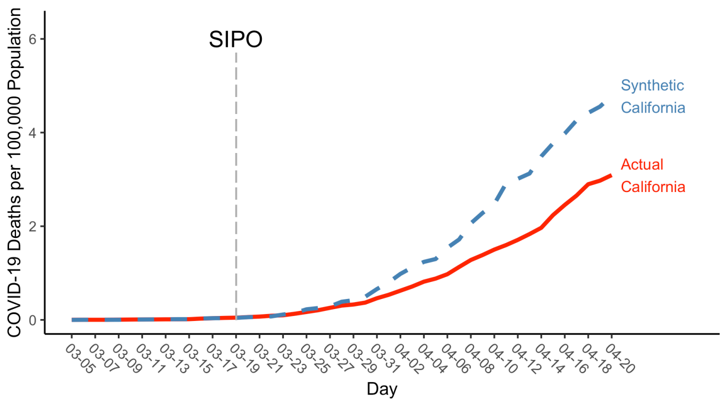
Note: Synthetic CA is comprised of GA (.566), NE (.245), DC (.075) CO (.063), IA (.034), & TX (.016).

Panel (b): <u>COVID-19 Cases</u> – Limited Donor Pool



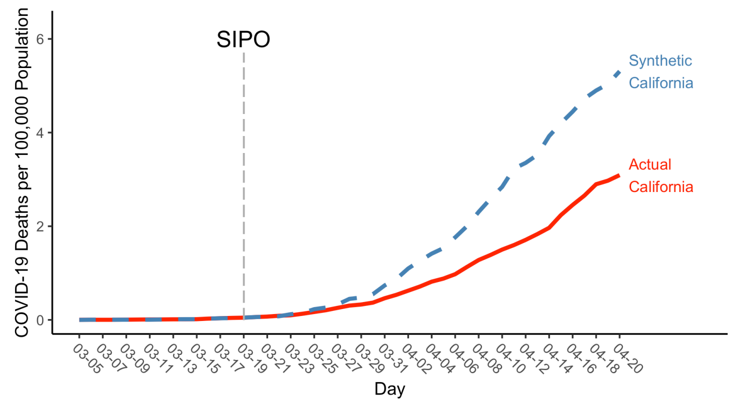
Note: Synthetic CA is comprised of GA (.508), NE (.226), TX (.10), DC (.098), CO (.066), & MD (.001)

Panel (c): <u>COVID-19 Deaths</u> – Full Donor Pool



Note: Synthetic CA is comprised of NV (.413), FL (.261), VA (.154), CO (.116), SD (.052).

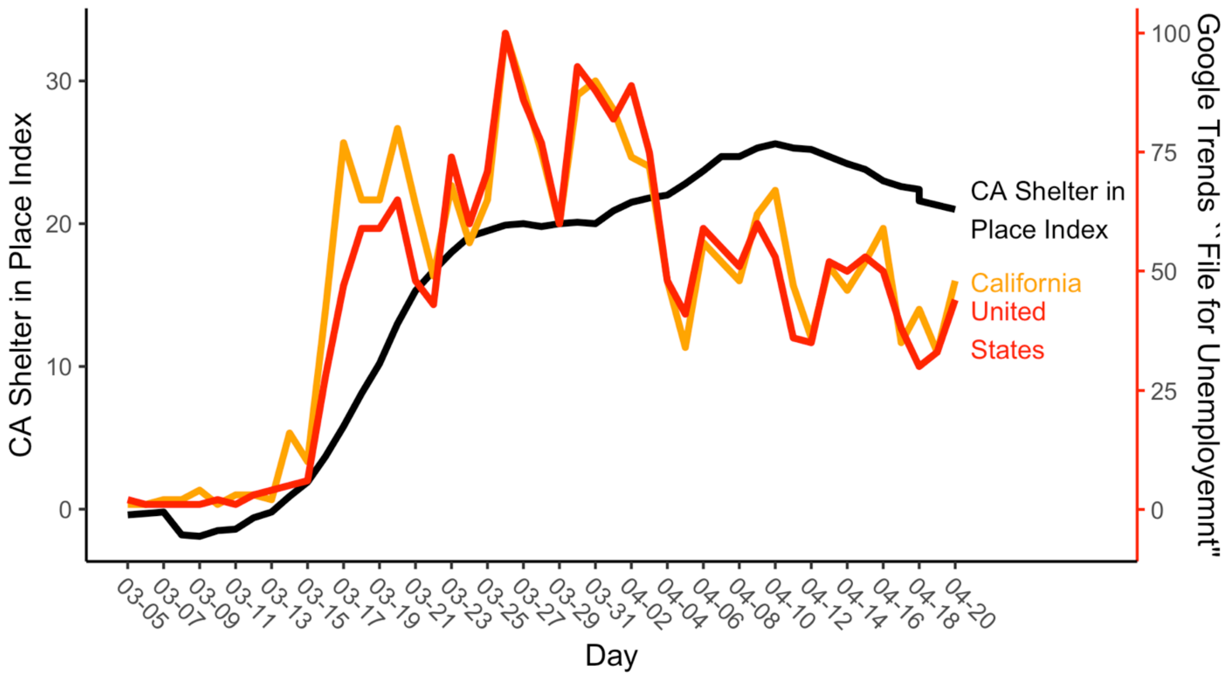
Panel (d): <u>COVID-19 Deaths</u> – Limited Donor Pool



Note: Synthetic CA is comprised of FL (.458), NV (.289), CO (.253).

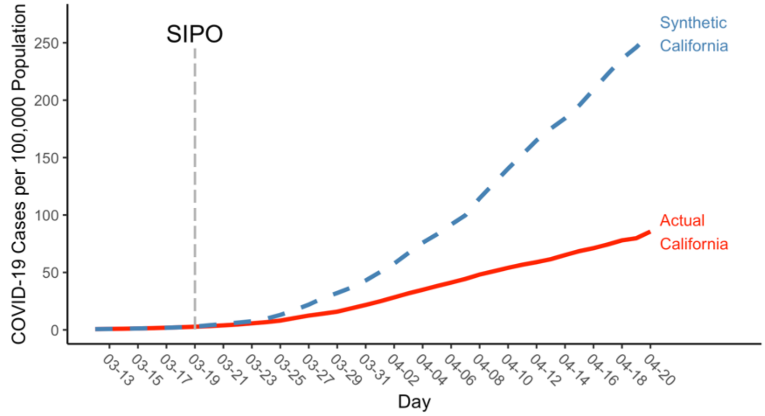
Notes: Estimate is generated using synthetic control methods. All observable controls include urbanicity, population-weighted density, testing rate, disaster emergency declaration, shelter in place index, and school closing. The full donor pool is comprised of states that had not implemented a statewide SIPO by March 19 and March 23. The limited donor pool further limits donor states to the upper 80th percentile in population-weighted density and urbanicity.

Appendix Figure 3. Trends in Shelter in Place Index and "File for Unemployment" in CA and the U.S.

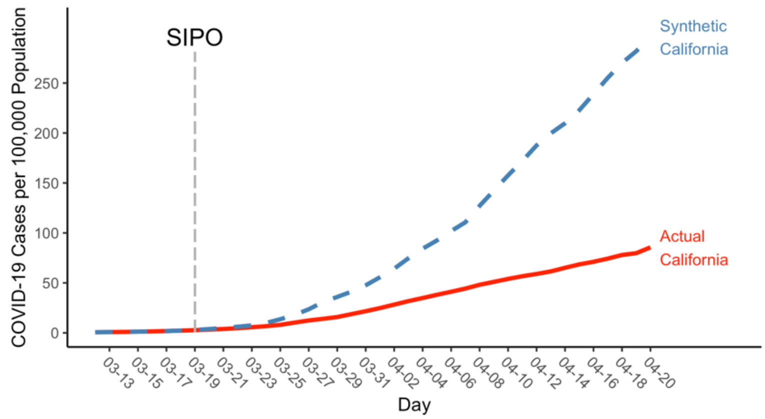


Appendix Figure 4. Sensitivity of Synthetic Control Estimates of COVID-19 Cases to Community Spread

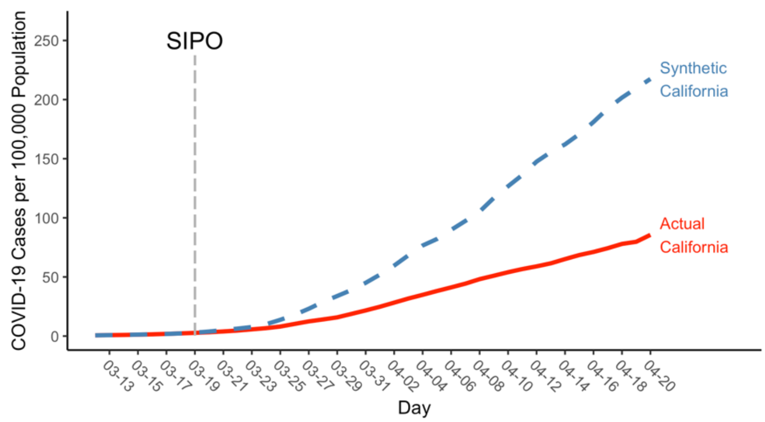
Panel (a): Donors Restricted to States with at Least 10 COVID-19 Cases



Panel (b): Donors Restricted to States with at Least 50 COVID-19 Cases



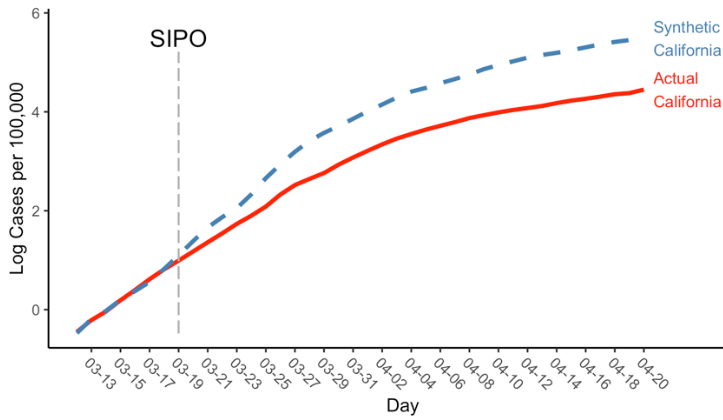
Panel (c): Donors Restricted to States with at Least 100 COVID-19 Cases



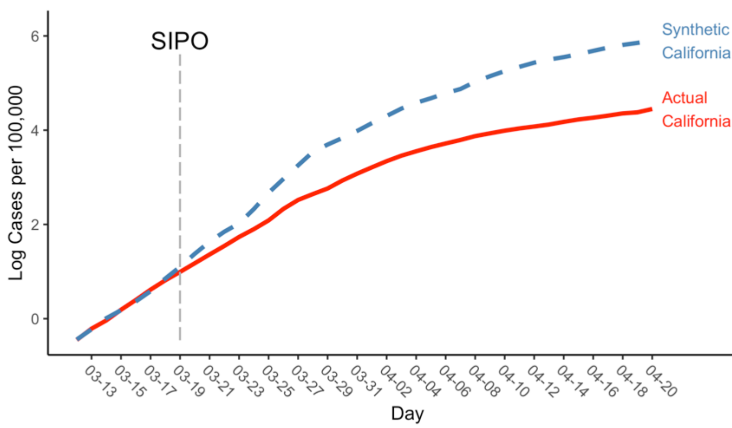
Notes: Estimate is generated using synthetic control methods. All observable controls include urbanicity, population-weighted density, testing rate, disaster emergency declaration, shelter in place index, and school closing. The full donor pool is comprised of states that had not implemented a statewide SIPO by March 19 and March 23.

Appendix Figure 5. Synthetic Control Estimates of Log(COVID-19 Cases)

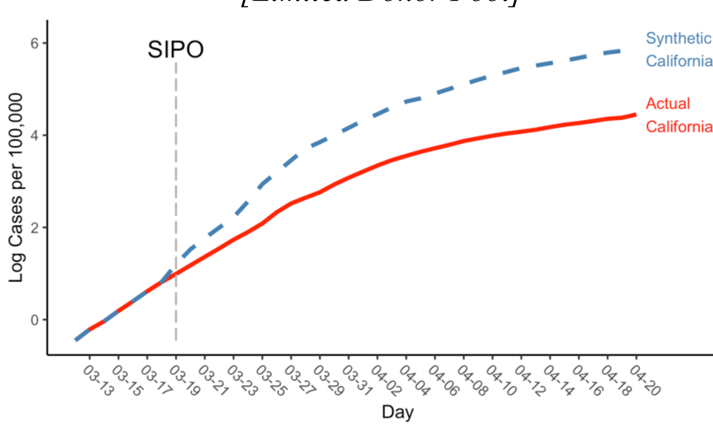
Panel (a): Matching on 2 Days of Pre-Treatment Days & Observables
[Full Donor Pool]



Panel (b): Matching on 2 Days of Pre-Treatment Days & Observables
[Limited Donor Pool]



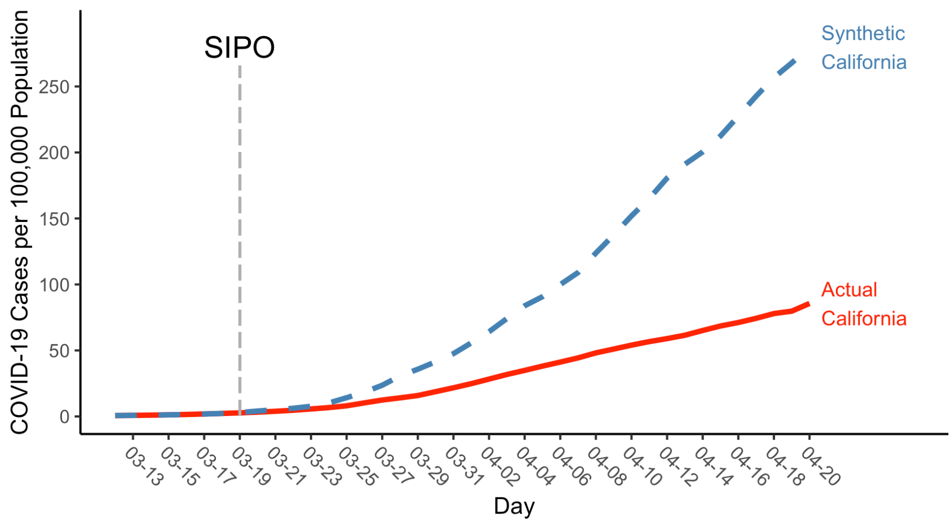
Panel (c): Matching on 7 Pre-Treatment Days
[Limited Donor Pool]



Notes: Observable controls include urbanicity, population-weighted density, the mean COVID-19 testing rate, number of days a disaster emergency declaration was in place, a pretreatment shelter in place index, the number of days the state had a travel ban, and the number of days state public schools were closed. The full donor pool is comprised of states that had not implemented a statewide SIPO by March 22. The limited donor pool further limits donor states to the upper 80th percentile in population-weighted density and urbanicity.

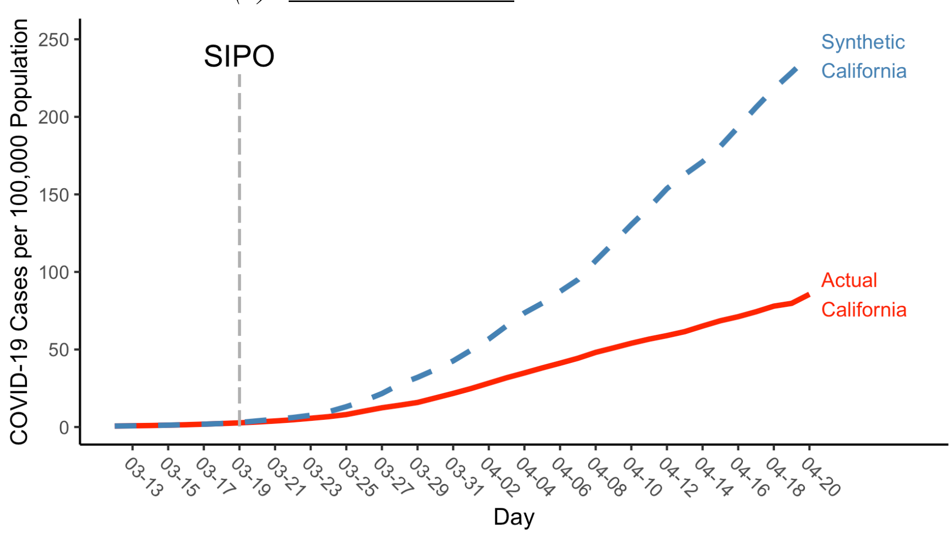
Appendix Figure 6A. Sensitivity of Synthetic Control Estimates of COVID-19 Cases to Pre-Treatment Weekly Influenza Match

Panel (a):<u>COVID-19 Cases</u> – Full Donor Pool



Note: Synthetic CA is comprised of MA (.314), HI (.299), MD (.274), NV (.062), & DC (.039)

Panel (b): <u>COVID-19 Cases</u> – Limited Donor Pool

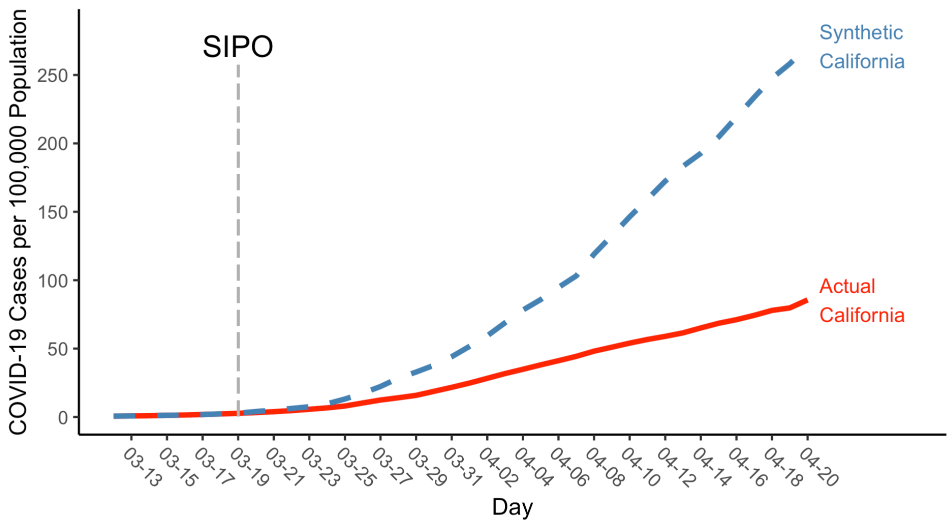


Note: Synthetic CA is comprised of MD (.228), HI (.225), MA (.216), IA (.136), CO (.095), NV (.062), & DC (.039).

Notes: Estimate is generated using synthetic control methods. All observable controls include urbanicity, population-weighted density, testing rate, disaster emergency declaration, shelter in place index, and school closing. The full donor pool is comprised of states that had not implemented a statewide SIPO by March 19 and March 23. The limited donor pool further limits donor states to the upper 80th percentile in population-weighted density and urbanicity.

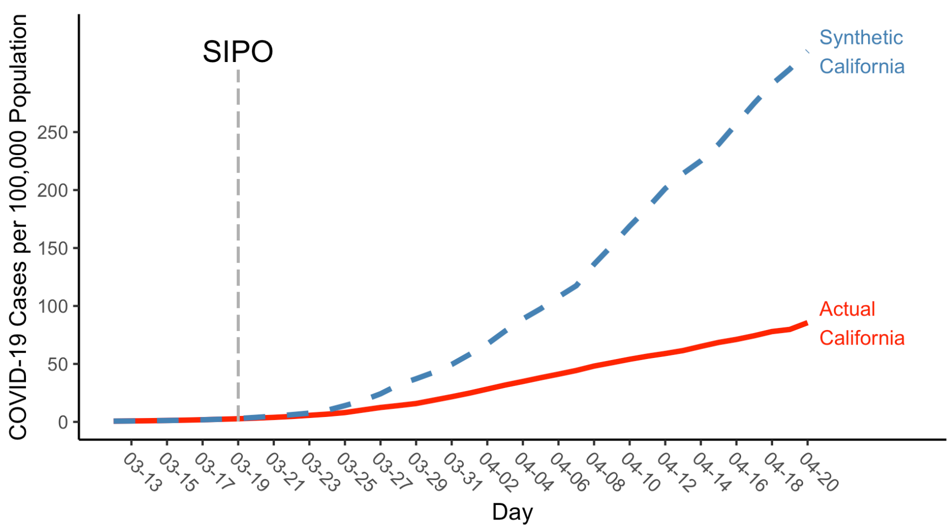
Appendix Figure 6B. Sensitivity of Synthetic Control Estimates of COVID-19 Cases to Pre-Treatment Mean Influenza Match

Panel (a):<u>COVID-19 Cases</u> – Full Donor Pool



Note: Synthetic CA is comprised of MD (.409), TX (.196), MA (.19), CO (.126), & DC (.79).

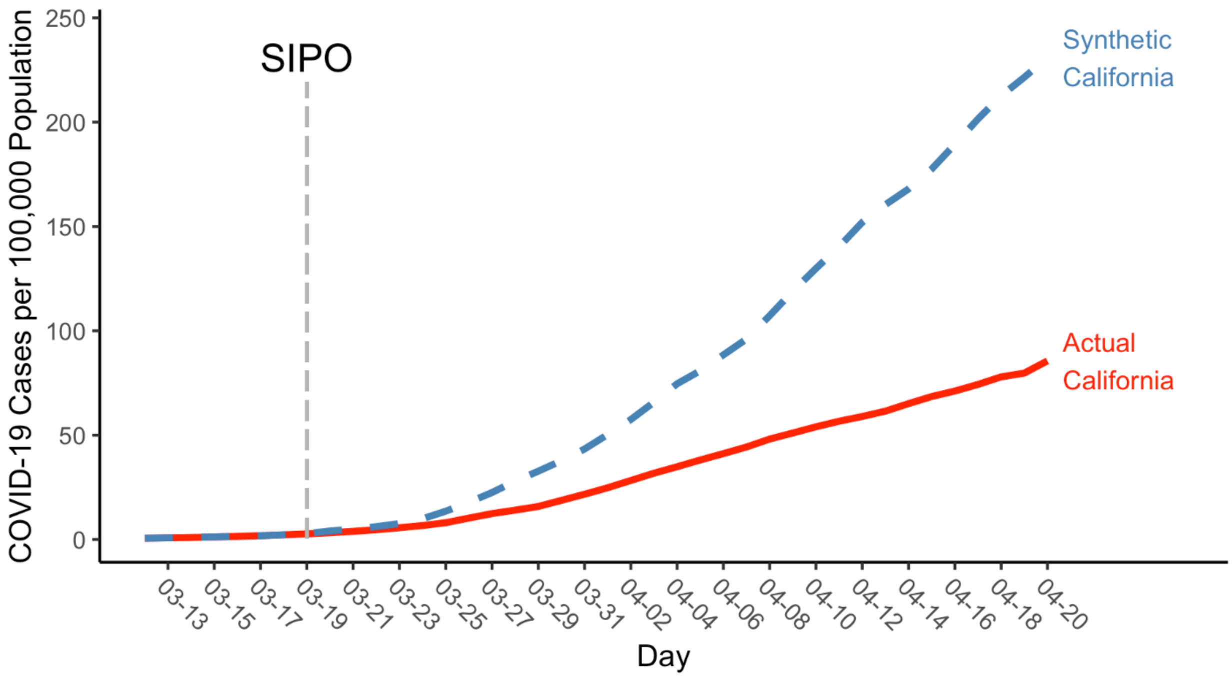
Panel (b): <u>COVID-19 Cases</u> – Limited Donor Pool



Note: Synthetic CA is comprised of MD (.525), MA (.303), UT (.144), & DC (.028).

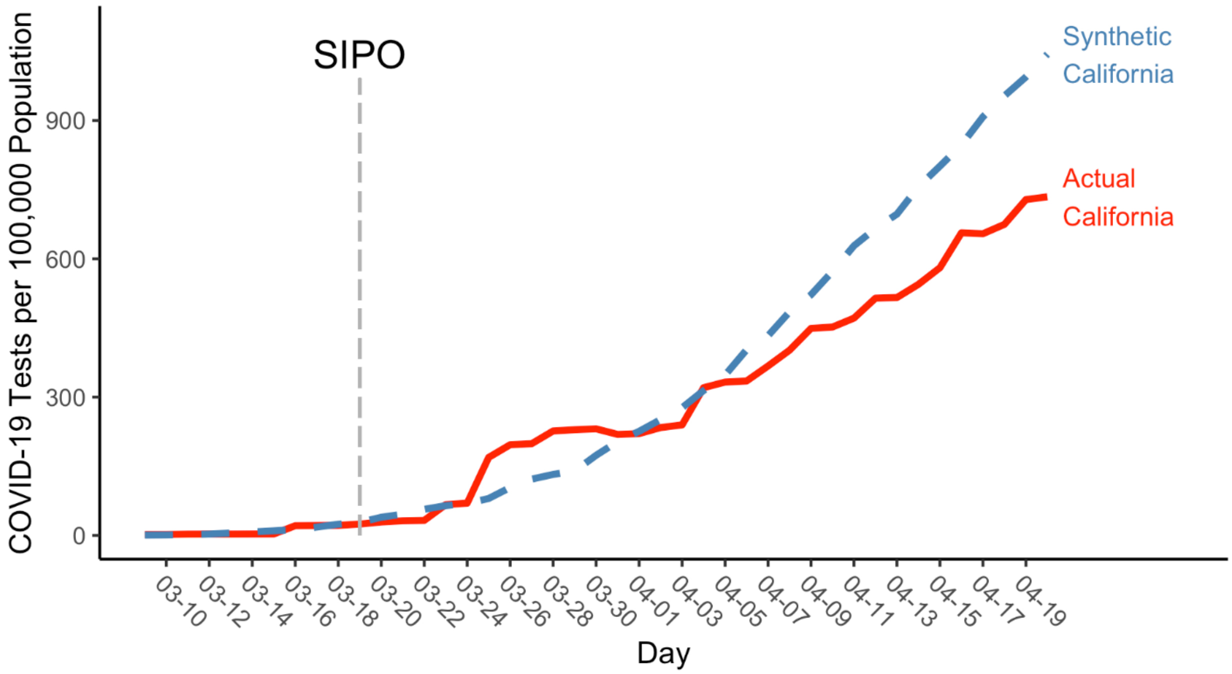
Notes: Estimate is generated using synthetic control methods. All observable controls include urbanicity, population-weighted density, testing rate, disaster emergency declaration, shelter in place index, and school closing. The full donor pool is comprised of states that had not implemented a statewide SIPO by March 19 and March 23. The limited donor pool further limits donor states to the upper 80th percentile in population-weighted density and urbanicity.

Appendix Figure 7. Sensitivity of Synthetic Control Estimates of COVID-19 Cases to Full Period Testing Match



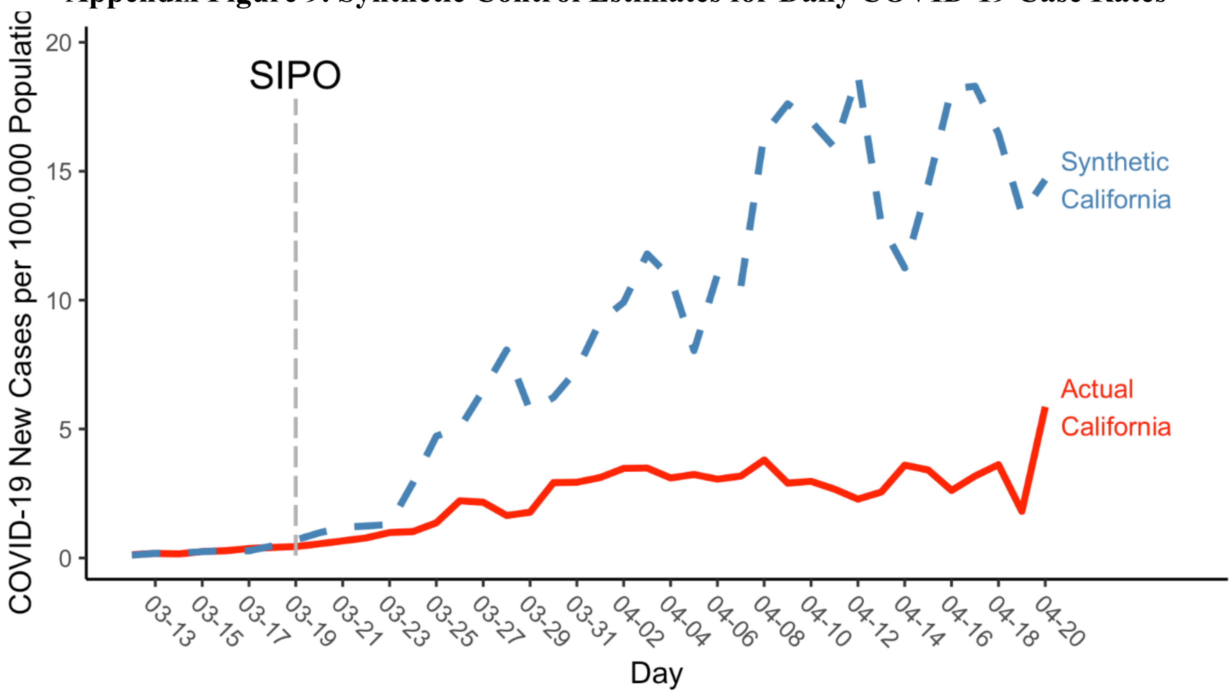
Notes: Estimate is generated using synthetic control methods. All observable controls include urbanicity, population-weighted density, testing rate, disaster emergency declaration, shelter in place index, and school closing. The full donor pool is comprised of states that had not implemented a statewide SIPO by March 19 and March 23. The donor pool further limits donor states to the upper 80th percentile in population-weighted density and urbanicity. Synthetic CA is comprised of TX (.255), MA (.202), CO (.124), NV (.117), MD (.107), AZ (.098), DC (.061), & UT (.035).

Appendix Figure 8. Synthetic Control Estimates for COVID-19 Testing Rates



Notes: Estimate is generated using synthetic control methods. All observable controls include urbanicity, population-weighted density, testing rate, disaster emergency declaration, shelter in place index, and school closing. The donor pool is comprised of states that had not implemented a statewide SIPO by March 19 and March 23, and fall in the upper 80th percentile in population-weighted density and urbanicity. Synthetic CA is comprised of TX (.741), MD (.122), RI (.089), & DC (.048).

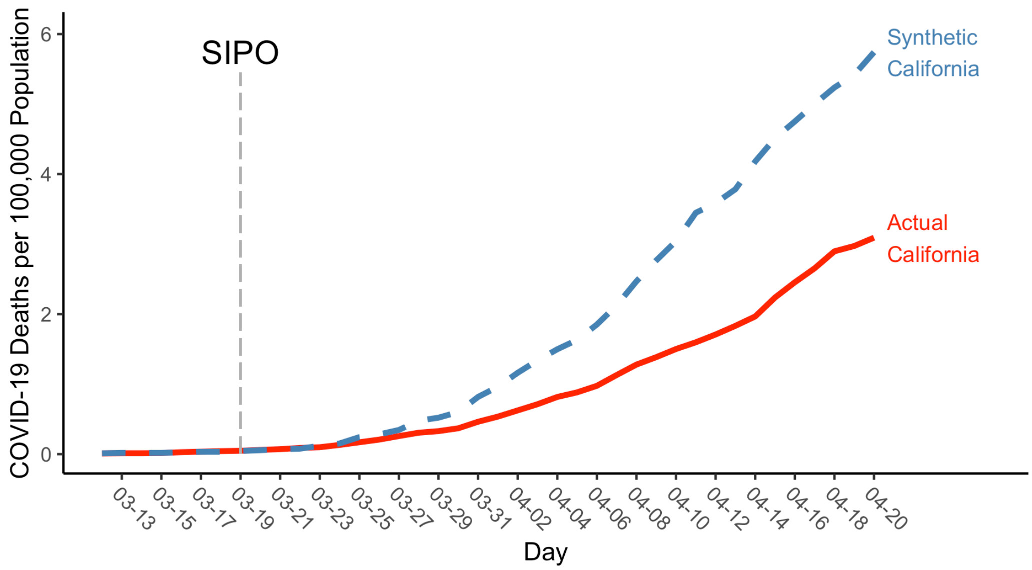
Appendix Figure 9. Synthetic Control Estimates for Daily COVID-19 Case Rates



Notes: Estimate is generated using synthetic control methods. All observable controls include urbanicity, population-weighted density, testing rate, disaster emergency declaration, shelter in place index, and school closing. The donor pool is comprised of states that had not implemented a statewide SIPO by March 19 and March 23, and fall in the upper 80th percentile in population-weighted density and urbanicity. Synthetic CA is comprised of MA (.386), MD (.334), TX (.168), & CO (.106).

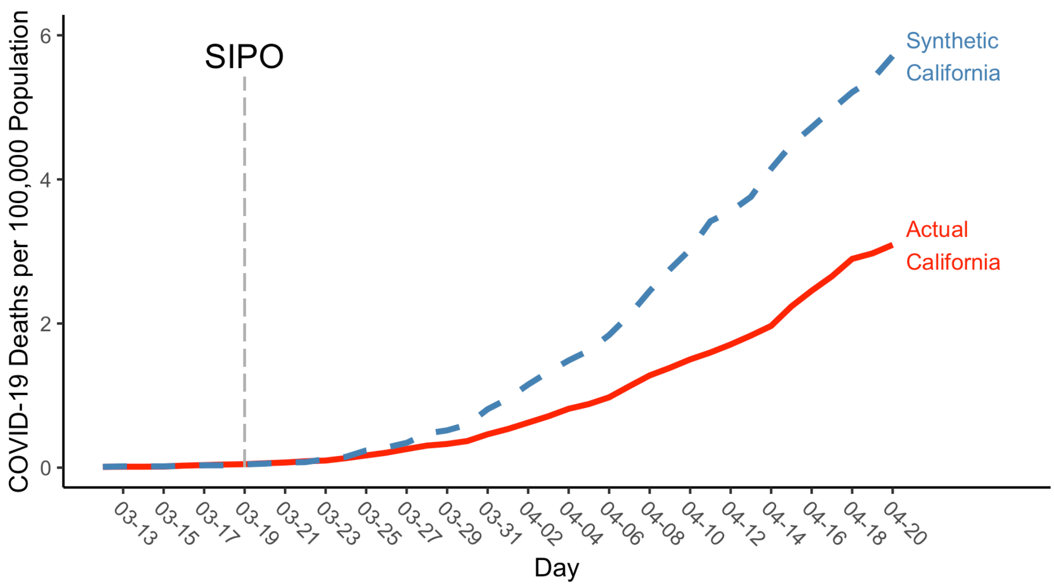
Appendix Figure 10A. Sensitivity of Synthetic Control Estimates of COVID-19 Deaths to Pre-Treatment Weekly Influenza Match

Panel (a): <u>COVID-19 Deaths</u> – Full Donor Pool



Note: Synthetic CA is comprised of CO (.361), NV (.259), OH (.216), & KS (.152).

Panel (b): <u>COVID-19 Deaths</u> – Limited Donor Pool

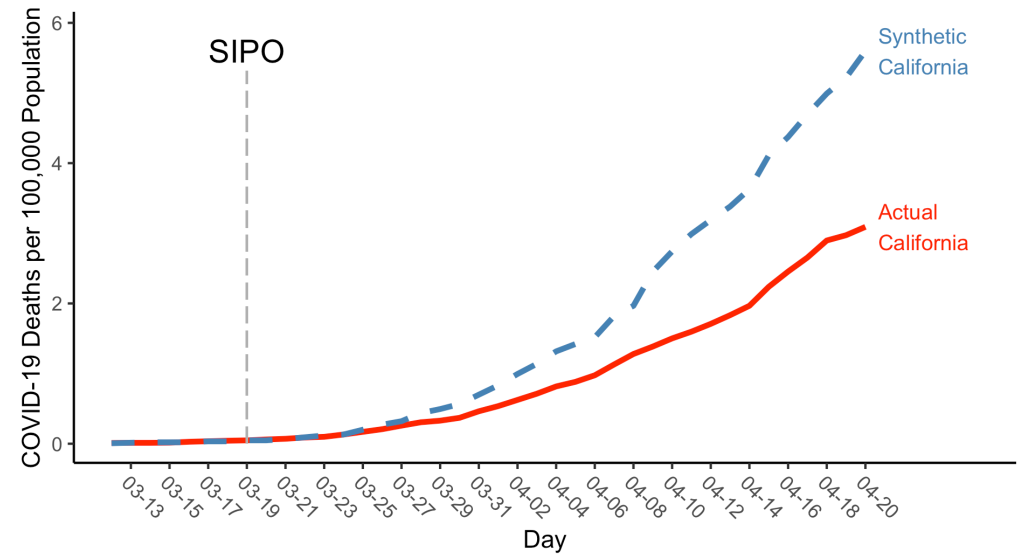


Note: Synthetic CA is comprised of CO (.364), NV (.241), OH (.229), & KS (.141).

Notes: Estimate is generated using synthetic control methods. All observable controls include urbanicity, population-weighted density, testing rate, disaster emergency declaration, shelter in place index, and school closing. The full donor pool is comprised of states that had not implemented a statewide SIPO by March 19 and March 23. The limited donor pool further limits donor states to the upper 80th percentile in population-weighted density and urbanicity.

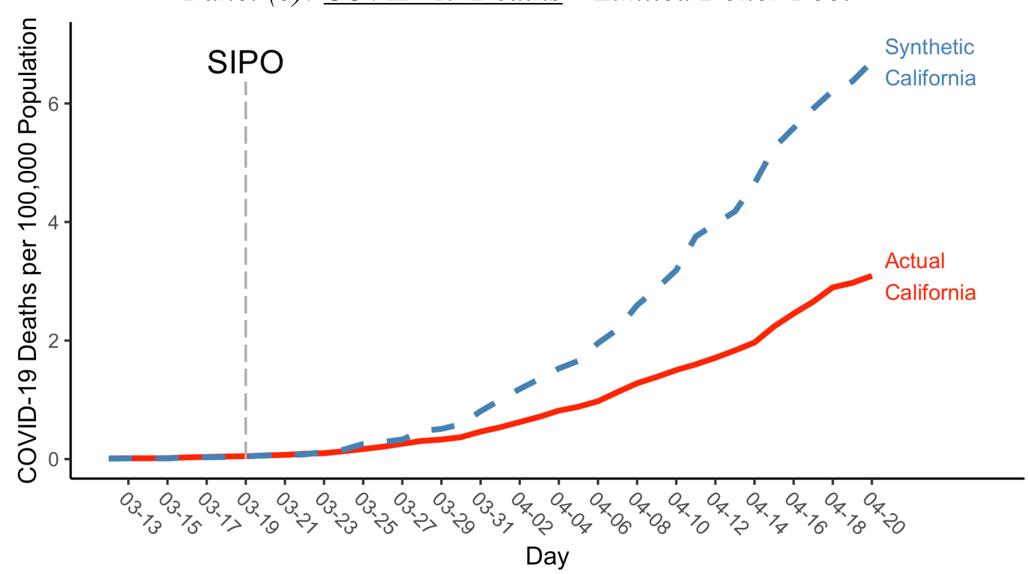
Appendix Figure 10B. Sensitivity of Synthetic Control Estimates of COVID-19 Deaths to Pre-Treatment Mean Influenza Match

Panel (a): <u>COVID-19 Deaths</u> – Full Donor Pool



Note: Synthetic CA is comprised of VA (.5442), CO (.40), DC (.028), & TX (.021).

Panel (b): <u>COVID-19 Deaths</u> – Limited Donor Pool

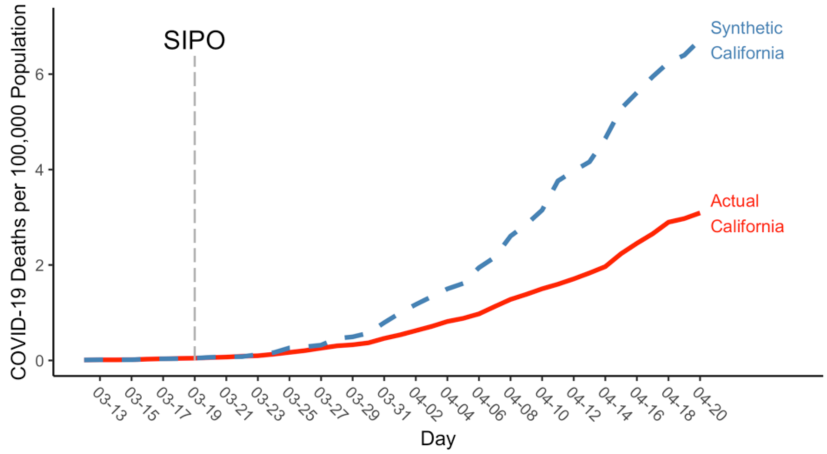


Note: Synthetic CA is comprised of NV (.402), CO (.317), MD (.195), TX (.069) & AZ (.017).

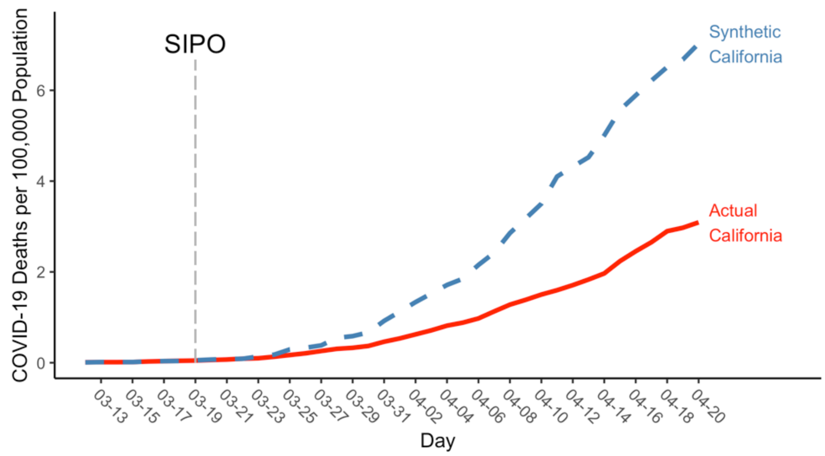
Notes: Estimate is generated using synthetic control methods. All observable controls include urbanicity, population-weighted density, testing rate, disaster emergency declaration, shelter in place index, and school closing. The full donor pool is comprised of states that had not implemented a statewide SIPO by March 19 and March 23. The limited donor pool further limits donor states to the upper 80th percentile in population-weighted density and urbanicity.

Appendix Figure 11. Sensitivity of Synthetic Control Estimates of COVID-19 Deaths to Community Spread

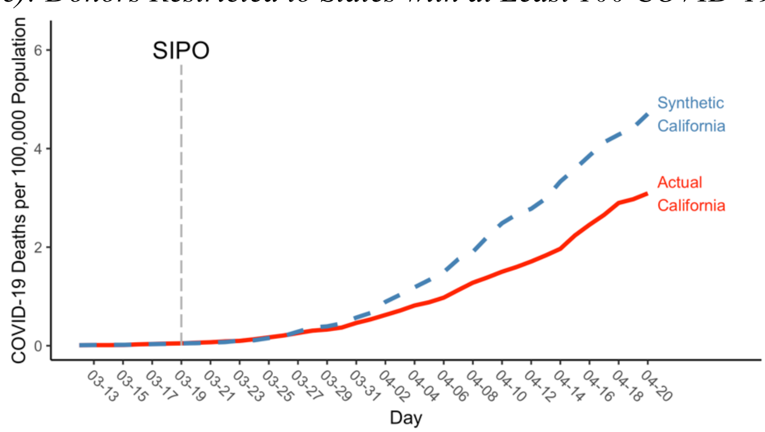
Panel (a): Donors Restricted to States with at Least 10 COVID-19 Cases



Panel (b): Donors Restricted to States with at Least 50 COVID-19 Cases



Panel (c): Donors Restricted to States with at Least 100 COVID-19 Cases



Notes: Estimate is generated using synthetic control methods. All observable controls include urbanicity, population-weighted density, testing rate, disaster emergency declaration, shelter in place index, and school closing. The donor pool is comprised of states that had not implemented a statewide SIPO by March 19 and March 23.

Appendix Table 1: List of Donor States that Received Positive Weights

	(1)	(2)	(3)
	Panel I: Positively Weighted States for Figure 2		
	TX (.347) MD (.238) MA (.158) CO (.146) DC (.112)	MD (.339) HI (.26) MA (.258) CO (.084) DC (.031) NV (.017)	HI (.484) MA (.279) DE (.084) RI (.073) DC (.033)
	Panel II: Positively Weighted States for Figure 3		
	NV (.479) CO (.254) MD (.215) SD (.052)	NV (.441) CO (.414) MD (.145)	NV (.39) CO (.365) GA (.173) IN (.073)
Donor Pool and Observable Matches			
Size of Donor Pool	43	32	32
Pre-SIPO covid-19 Days	2	2	7
Match on All Observable Controls	Yes	Yes	No

* Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level

Notes: Observable controls include urbanicity, population-weighted density, the mean COVID-19 testing rate, number of days a disaster emergency declaration was in place, a pretreatment shelter in place index, the number of days the state had a travel ban, and the number of days state public schools were closed. The list of states that received positive weights are listed in Appendix Table 4. The full donor pool is comprised of states that had not implemented a statewide SIPO by March 22. The limited donor pool further limits donor states to the upper 80th percentile in population-weighted density and urbanicity.

Appendix Table 2. Sensitivity of Synthetic Control Estimates to the use of Never-adopting or Later-adopting States

	(1)	(2)
	Panel I: Post-Treatment March 19 to April 20	
SIPO	-65.228	-65.826
P-Value	[0.231]	[0.182]
One Sided P-Value	[0.231]	[0.182]
	Panel II: Post-Treatment March 23 to April 20	
SIPO	-74.107	-74.792
P-Value	[0.308]	[0.182]
One Sided P-Value	[0.308]	[0.182]
	Panel III: Post-Treatment March 30 to April 20	
SIPO	-94.428	-95.323
P-Value	[0.308]	[0.182]
One Sided P-Value	[0.308]	[0.182]
Donor Pool	Late Adopting SIPO States & Never Adopting SIPO States	Never Adopting SIPO States

* Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level

Notes: Notes: Estimate is generated using synthetic control methods. The number of donor states are 43. The matching was constructed using 2 pre-SIPO Covid-19 cases per 100,000, urbanicity, population-weighted density, the mean COVID-19 testing rate, number of days a disaster emergency declaration was in place, a pretreatment shelter in place index, the number of days the state had a travel ban, and the number of days state public schools were closed. The permutation-based p-values are included in brackets below each point estimate. We define states that adopted SIPO on April 5 or later as Late Adopting SIPO States, and states that never adopted a SIPO as Never Adopting SIPO States

Appendix Table 3: Sensitivity of Synthetic Control Estimates to Pre-Treatment Day COVID-19 Case Match

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel I: Post-Treatment March 19 to April 20					
SIPO	-63.478**	-47.413**	-53.514**	-57.520**	-42.045	-51.436**
P-Value	[0.023]	[0.045]	[0.023]	[0.023]	[0.182]	[0.045]
One Sided P-Value	[0.023]	[0.045]	[0.023]	[0.023]	[0.159]	[0.045]
	Panel II: Post-Treatment March 23 to April 20					
SIPO	-72.055**	-53.830*	-60.774**	-65.324**	-47.668	-58.412**
P-Value	[0.023]	[0.068]	[0.023]	[0.045]	[0.227]	[0.045]
One Sided P-Value	[0.023]	[0.068]	[0.023]	[0.045]	[0.159]	[0.045]
	Panel III: Post-Treatment March 30 to April 20					
SIPO	-91.665**	-68.598*	-77.489**	-83.299**	-60.722	-74.451**
P-Value	[0.045]	[0.068]	[0.023]	[0.045]	[0.250]	[0.045]
One Sided P-Value	[0.045]	[0.068]	[0.023]	[0.045]	[0.159]	[0.045]
Observable used to construct the weights						
Number of Days of Pre-Treat Match	¾	½	Odd	Even	All	3
Match on Pre-Treat Mean of Outcome	No	No	No	No	Yes	No

* Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level

Notes: Estimate is generated using synthetic control methods. The number of donor states are 43. The matching was constructed using pre-SIPO Covid-19 cases per 100,000. The permutation-based p-values are included in brackets below each point estimate.

Appendix Table 4: Sensitivity of Synthetic Control Estimates of COVID-19 Cases to Community Spread

	(1)	(2)	(3)
	Panel I: Post-Treatment March 19 to April 20		
SIPO	-59.180**	-71.641*	-50.384
P-Value	[0.049]	[0.053]	[0.125]
One Sided P-Value	[0.049]	[0.053]	[0.125]
	Panel II: Post-Treatment March 23 to April 20		
SIPO	-67.218**	-81.409*	-57.207
P-Value	[0.073]	[0.053]	[0.125]
One Sided P-Value	[0.049]	[0.053]	[0.125]
	Panel III: Post-Treatment March 30 to April 20		
SIPO	-86.054**	-104.281*	-72.573
P-Value	[0.073]	[0.105]	[0.250]
One Sided P-Value	[0.049]	[0.053]	[0.250]
Case Threshold for Community Outbreak	10	50	100

* Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level

Notes: Estimate is generated using synthetic control methods. The number of donor states are 43. The matching was constructed using pre-SIPO Covid-19 cases per 100,000. The permutation-based p-values are included in brackets below each point estimate.

Appendix Table 5: Synthetic Control Estimates of SIPO on COVID-19 Testing Rate

	(1)
	Panel I: Post-Treatment March 19 to April 20
SIPO	-67.645
P-Value	[0.909]
One Sided P-Value	[0.318]
	Panel II: Post-Treatment March 23 to April 20
SIPO	-75.173
P-Value	[0.932]
One Sided P-Value	[0.318]
	Panel III: Post-Treatment March 30 to April 20
SIPO	-119.343
P-Value	[0.932]
One Sided P-Value	[0.318]

* Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level

Notes: Estimates are generated using synthetic control methods. The number of donor states are 43. The matching was constructed using 2 pre-SIPO Covid-19 cases per 100,000, urbanicity, population-weighted density, the mean COVID-19 testing rate, number of days a disaster emergency declaration was in place, a pretreatment shelter in place index, the number of days the state had a travel ban, and the number of days state public schools were closed. The permutation-based p-values are included in brackets below each point estimate. The donor pool is comprised of states that had not implemented a statewide SIPO by March 22 and that fall in the upper 80th percentile in population-weighted density and urbanicity.

Appendix Table 6: Matched Difference-in-Difference Estimates of Effect of California SIPO on COVID-19 Cases and Mortality

	Log(COVID-19 Cases)		Log(COVID-19 Deaths)	
	(1)	(2)	(3)	(4)
	Panel I: Estimated Effect on COVID-19 Case: Post-Treatment -- March 19 to April 20			
SIPO	-1.101	-1.006	-0.674	-0.732
P-Value	[0.154]	[0.132]	[0.365]	[0.230]
N	2042	1755	1637	1367
	Panel II: Estimated Effect on COVID-19 Case: Lagged Effect, Post-Incubation and ARDS			
March 19-22	-0.664*	-0.750*	0.186	-0.383
P-value	[0.090]	[0.099]	[0.333]	[0.251]
March 23-29	-1.049	-1.059	-0.300	-0.453
P-value	[0.125]	[0.148]	[0.346]	[0.283]
March 30+	-1.216	-1.318	-0.987	-1.241
	[0.199]	[0.172]	[0.396]	[0.227]
N	2042	1755	1637	1367
Weights	Inverse Probability Treatment	Proportional to the probability of being similar to CA relative to the probability of being a donor state.	Inverse Probability Treatment	Proportional to the probability of being similar to CA relative to the probability of being a donor state.

* Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level

Notes: All estimates include the following controls: an indicator for whether a state has issued a travel ban, an indicator for whether a state declared a disaster emergency, an indicator for whether precipitation fell in the state, average temperature, an indicator for whether schools were closed, the COVID-19 testing rate, and state and day fixed effects. P-values, generated using permutation tests, are reported in brackets.