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INDOOR VACCINE MANDATES IN US CITIES, VACCINATION BEHAVIOR, AND COVID-19 OUTCOMES

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ABSTRACT

Many US cities implemented indoor vaccine mandates to incentivize citizens to get vaccinated and reduce the spread of COVID-19. Previous research finds that similar country-level mandates increased vaccine uptake substantially. However, with city-level mandates, unvaccinated individuals could easily travel to neighboring cities without the mandate, whereas it is difficult to travel across country. Thus, the effects of indoor vaccine mandates implemented in US cities on individuals' decisions to get vaccinated may differ from country-level mandates. This paper offers the first analysis of the effects of city-level indoor vaccine mandates. Using the synthetic difference-in-differences approach, we find that these mandates had no significant effect on COVID-19 cases, deaths, or vaccine uptake in any of the cities. We also compare our findings to the synthetic control and the difference-in-differences estimates and find the same result. Our findings put into question the efficacy of city-level vaccine mandates.

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DISCLAIMER

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1 Introduction

Various measures, campaigns, and behavioral nudges were implemented during the pandemic to incentivize citizens to get vaccinated and reduce the spread of COVID-19 (Acemoglu et al., 2021; Alvarez et al., 2021; Dai et al., 2021; Fang et al., 2020; Karaivanov et al., 2021). Indoor vaccine mandates were among these measures, and were arguably among the most restrictive and polarizing regulations enacted in the United States. Millions of people were prevented from entering restaurants, bars, gyms, theaters, sports arenas, and other public indoor areas without proof of COVID-19 vaccination. Many of the largest US cities—including New York City, San Francisco, Los Angeles, Seattle, Philadelphia, and Boston—implemented this mandate with the goal of increasing vaccine uptake, thereby reducing COVID-19 cases and deaths. The press release announcing Boston's indoor vaccine mandate stated that the decision was made to "increase vaccination rates among residents and slow the spread of COVID-19 and the new Omicron variant" (Boston Public Health Commission, 2021). In Philadelphia, the mayor claimed, "[Indoor vaccination mandates] are critical to slowing the spread of the Delta variant of COVID-19, which is more dangerous and transmissible than earlier forms of the virus. The science is clear: these measures will protect Philadelphians and save lives" (Philadelphia Board of Health, 2021). This paper offers the first analysis of the effects of indoor vaccine mandates on vaccine uptake, COVID-19 cases, and COVID-19 deaths in US cities.

Research has shown that country-level and province-level indoor vaccine mandates implemented in Europe and Canada led to an increase in vaccine uptake (Karaivanov et al., 2022; Mills & Rüttenauer, 2022). Yet, little is known about the effects of city-level vaccine mandates, which is the jurisdictional level at which indoor mandates were implemented in the United States. This distinction between country-level and city-level is important because the cost that a country-level

mandate imposes on unvaccinated residents is likely higher than a city-level mandate. Unvaccinated people who live or commute to cities that implement the mandate are prevented from going to indoor public facilities in the affected city, but they could still go to indoor facilities in neighboring cities without the mandate. By contrast, it is difficult for unvaccinated people in countries or provinces that implement similar mandates to go to places not affected by the mandate. Thus, the effect of city-level mandates on vaccine uptake, and consequently COVID-19 cases and deaths, may be smaller than the country-level mandates.

The relationship between COVID-19 vaccination rates and COVID-19 cases and deaths is complex and mediated by many factors, such as population density, demographics, and risk-mitigating behaviors. If vaccination reduces peoples' willingness to socially distance, wear masks, or adhere to hygiene guidelines, then the effects of vaccine uptake on COVID-19 cases and deaths would be smaller than anticipated. Andersson et al. (2021), for example, offer experimental evidence that vaccine availability can result in lower adherence to public health recommendations. Thus, it is important to analyze the effects of vaccine mandates on COVID-19 cases and deaths to understand the health impact of these policies.

We use the synthetic difference-in-differences method introduced by Arkhangelsky et al. (2021) to explore the effects of adopting indoor vaccine mandates in Boston, Chicago, Los Angeles, New Orleans, New York, Philadelphia, San Francisco, Seattle, and Washington DC.[1] The synthetic difference-in-differences estimator combines features of the widely used difference-in-differences and synthetic control methods, and it is argued by Arkhangelsky et al. (2021) to have desirable robustness properties, both theoretically and empirically, relative to conventional estimators. Similar to the difference-in-differences method, the synthetic difference-in-differences estimator is invariant to additive unit-level shifts, and, like the synthetic control method, the synthetic difference-in-differences estimator reweighs and matches pre-treatment trends to weaken the reliance on parallel trend type assumptions. Therefore, for each city that adopted an indoor vaccine mandate, we find a corresponding weighted average of non-adopting cities whose average outcome is approximately

[1]Minneapolis and St. Paul implemented an indoor vaccine mandate only for a couple of weeks, so we do not analyze or report the effect of the mandates in these cities.

parallel to that of the adopting city prior to the announcement of the mandate. This weighted average of non-adopting cities serves as a counterfactual measure to which the outcome in the corresponding adopting city is compared.

We find no evidence that the announcement or implementation of indoor vaccine mandate in the cities listed had any significant effect on vaccine uptake, COVID-19 cases, or COVID-19 deaths, and this is largely consistent for all US cities that implemented the mandate. Our findings are robust to the synthetic control and the difference-in-differences methods. These findings suggest that if the mandate had any effect at all, it is likely smaller or at least less statistically noticeable than the effects of country-level mandates previously studied by Karaivanov et al. (2022) and Mills and Rüttenauer (2022). Our findings also support the claim that city-level vaccine mandates are likely to have a smaller effect on vaccine uptake, and consequently a smaller effect on COVID-19 cases and deaths, than country-level and province-level mandates. Therefore, the effects of vaccine mandates in European countries are not likely to take place in US cities and should be treated as different policies.

Our findings put into question the efficacy of city-level vaccine mandates. Indoor vaccine mandates caused large disruptions for many individuals and businesses. New York City, for example, fired 1,430 city workers for failing to comply with its vaccine mandate (Fitzsimmons, 2022). A survey found that over 90% of NYC restaurants reported having customer-related challenges, such as losing customers who objected to the mandate, and 75% having staff-related challenges (New York State Restaurant Association, 2021). Those are just a small fraction of the disruptions caused by the mandates. Most supporters of the mandates claim that the associated increase in vaccination rates, and its implied reduction in the spread of COVID-19, outweigh the cost of the disruptions. However, we find that the effects of the mandates on their intended outcomes are not statistically noticeable in any of the cities they were implemented in all empirical strategies used.

2 Background and Literature Review

Governments at all levels responded to the pandemic with sweeping restrictions to curb COVID-19 morbidity and mortality, including shelter-in-place orders, mask mandates, closure of certain businesses, limits on large gatherings, and indoor vaccine mandates. A substantial empirical literature has explored the effects of these policies. Alfano and Ercolano (2020) and Fang et al. (2020) find that lockdown policies slowed COVID-19 transmission and reduced new cases, while a meta-analysis by Herby et al. (2022) concludes that lockdowns in spring 2020 had little to no effect on COVID-19 mortality. Based on risk modeling, Acemoglu et al. (2021) and Alvarez et al. (2021) suggest that adjustments to the structure, intensity, and duration of US lockdowns could have generated significant welfare gains. Exploiting temporal variation in the implementation of mask mandates in different jurisdictions, Joo et al. (2021), Karaivanov et al. (2021), and Krishnamachari et al. (2021) estimate that the mandates significantly reduced COVID-19 cases and hospitalizations. Similarly, Schnake-Mahl et al. (2021) find that closing indoor dining was associated with a sharp decline in new COVID-19 cases. The differences in compliance with pandemic-related rules and recommendations may explain some of the heterogeneity observed in the literature (Barrios et al., 2021; Cherry et al., 2021; Painter & Qiu, 2021; Wright et al., 2020).

Some papers have examined unintended health consequences of COVID-19 policies. Bullinger et al. (2021) suggest that Chicago's stay-at-home order may have led to an increase in domestic violence–related calls to the police. Di Novi et al. (2022) document a drastic reduction in drug adherence during the pandemic, especially among older and less-educated individuals. Serrano-Alarcón et al. (2022) offer evidence that lockdown measures worsened mental health. Melo (2022a) shows that isolation measures in nursing homes were associated with an increase in overall deaths. Melo (2022b) finds that decisions regarding nursing home isolation measures were largely affected by ownership structure, and that facilities with more restrictive isolation measures also had much higher rates of non-COVID deaths in the second year of the pandemic.

Since the start of the pandemic, public health experts viewed the development of COVID-19 vaccines as a decisive step toward controlling the virus and returning to normalcy. Yet, it soon

Table 1: Timing of Indoor Vaccine Mandates

City	Date Announced	Date Implemented	Date Repealed
New York	8/3/21	8/16/21	3/7/22
San Francisco	8/12/21	8/20/21	3/11/22
New Orleans	8/12/21	8/16/21	3/21/22
Seattle	9/16/21	10/25/21	3/1/22
Los Angeles	11/8/21	11/29/21	3/30/22
Philadelphia	12/13/21	1/3/22	2/16/22
Boston	12/20/21	1/1522	2/18/22
Chicago	12/21/21	1/3/22	2/28/22
Washington DC	12/22/21	1/15/22	2/15/22

Notes: We omit Minneapolis and St. Paul from our analysis because they implemented indoor vaccine mandates for only a couple of weeks in early 2022.

became clear that a substantial number of Americans were unwilling to be immunized. In February 2021, 30% of adults said they would "probably" or "definitely" not be vaccinated (Funk & Tyson, 2021). By early May 2021, despite experimental evidence that simple behavioral nudges could boost COVID-19 vaccination rates (Dai et al., 2021), just 57% of US adults had received at least one dose (Diesel et al., 2021)—far below the threshold needed to achieve robust herd immunity.

In the spring and summer 2021, many jurisdictions began to implement measures to increase COVID-19 vaccine uptake. The federal government moved to force large employers to mandate vaccination or weekly testing; several states established lottery incentive programs to encourage vaccination (Acharya & Dhakal, 2021); and many cities required municipal workers to be vaccinated as a condition of employment. On August 3, 2021, amid concerns about the spread of the Delta variant, New York City became the first city in the United States to require proof of vaccination to enter restaurants, concerts, stadiums, gyms, and other venues (Fitzsimmons, 2022). Similar policies were adopted by other major US cities soon after, including San Francisco, New Orleans, Seattle, Los Angeles, Philadelphia, Boston, Chicago, and Washington DC. Table 1 shows the timing of the announcement, implementation, and repeal of these policies in each city.

The literature on vaccine mandates primarily focuses on their effects on vaccine uptake. Abre-

vaya and Mulligan (2011), Lawler (2017), and Carpenter and Lawler (2019) find that mandates requiring children to receive certain immunizations such as MMR and polio prior to childcare or school attendance, which were broadly adopted by states in the 1970s, boost vaccination rates by 10–30%. Pitts et al. (2014) report that implementing influenza vaccine mandates for healthcare personnel significantly increases uptake. Yet, the applicability of these results in predicting the impact of indoor vaccine mandates during the pandemic is uncertain. Attwell et al. (2018) argue that the effects of vaccine mandates on behavior depend crucially on their sociocultural context and the structure, exemptions, target populations, consequences, and enforcement of the specific policies. Furthermore, indoor vaccine mandates affect a different population (mainly adult members of the public vs. children or workers in the healthcare industry), establish different contingencies (engaging in indoor recreational activities vs. attending day care/school or remaining employed), and are implemented at a different geographic scale (cities vs. states or organizations) than most previously studied vaccine mandates.

Recent research has investigated the effects of COVID-19 vaccine mandates, primarily outside the United States. Karaivanov et al. (2022) estimate the effect of indoor vaccination mandates in Canada. The authors apply a difference-in-differences approach and compare provinces that implemented the mandate to provinces that did not. They also perform a time-series analysis to estimate the effects of the mandate in France, Italy, and Germany, and find that the announcement of the mandate was associated with a significant increase in vaccine uptake in those countries. Mills and Rüttenauer (2022) estimate the effects of mandatory COVID-19 certification (e.g., showing vaccination, recent negative test, or proof of recovery) on vaccine uptake in five European countries (Denmark, Italy, France, Germany, and Switzerland) and Israel, and find that the mandate significantly increased vaccine uptake. Oliu-Barton et al. (2022) estimate the effects of requiring COVID-19 certificate, recent negative test, or proof of vaccination on vaccine uptake in Germany, France, and Italy. The authors use counterfactuals that are constructed using innovation diffusion theory, and find that the requirement was associated with an increase in vaccine uptake in those countries. Kuznetsova et al. (2022) study the effects of various interventions, such as incentive

programs, introduction of fines, and COVID-19 certificates, on vaccine uptake in eight European countries, and find that the interventions were associated with an immediate and significant increase in vaccine uptake. Cohn et al. (2022) find that the use of an assortment of policies in New York City, including vaccine mandates, incentive payments, and proof of vaccine requirement, led to an increased vaccination uptake. We contribute to this literature by offering the first analysis of the effects of indoor vaccine mandates on COVID-19 vaccine uptake. Moreover, we extend the existing literature by offering the first analysis of the effects of city-level indoor vaccine mandates on COVID-19 cases and deaths.

3 Data

Data on daily county COVID-19 vaccinations, cases, and deaths come from the Centers for Disease Control and Prevention (CDC). Similar to Karaivanov et al. (2022), we focus on the number of first doses of COVID-19 vaccinations, because new vaccinations most directly reflect an individual's intent to be immunized following a proof of vaccination mandate.[2] We aggregate each of these variables to the Metro/Micropolitan Statistical Area (MSA) by week level, and then scale each measure by US Census population estimates (also aggregated to the MSA level). More specifically, our main outcome variables consist of weekly measures of administered first doses of COVID-19 vaccines, cases, and deaths per 100,000 residents. We aggregate the data to the MSA level, because MSAs consist of a city and surrounding areas that are linked by economic factors as established by the US Office of Management and Budget. One of the primary economic factors considered is the labor market. Thus, a vaccine mandate implemented in a city will affect people who live outside the city but work in the city.

Our sample period spans from the week of December 21, 2020, to the week of April 18, 2022, and initially our sample consisted of 916 MSAs. For reasons unknown to us, counties in 92 MSAs

[2]We also consider the total number of people fully vaccinated per 100,000 as an alternative vaccine uptake outcome variable. Similar to first doses, we find no statistically significant effect on full vaccine uptake following the announcement of a mandate in any of the cities that implemented a mandate.

reported either negative values of first doses several weeks in a row or negative values in a given week followed by a range of weeks with zero reported doses. So we dropped these 92 MSAs from the analysis. There were other MSAs that had reported negative first doses for one or two weeks over the sample period. But because these negative values only occurred once or twice over the sample period in these MSAs and accounted for only 0.56% of our sample (but about 27% of the MSAs in our sample), we replaced these negative values with an average number of first doses in the week preceding and the week following the negative value. Many counties also appeared to delay their reporting of first doses. In the first week of reporting, they reported the cumulative total of all first doses administered since the vaccines became available. We therefore omitted the first week that each MSA reported positive first dose numbers. Finally, we dropped three other MSAs from the analysis, because their reported number of vaccinations exceeded the number of people in the population. Consequently, our final data set consists of a balanced panel of 821 MSAs, with weekly values of first doses, COVID-19 cases, and COVID-19 deaths per 100,000—a total of 57,470 observations.

Table 2 shows descriptive statistics of outcome variables for all MSAs in our sample, broken down by treatment status. In an average week, over the sample period, there were about 817.47, 273.75, and 3.56 first doses, cases, and deaths per 100,000, respectively. Average first doses per 100,000 were higher among the treated MSAs compared to untreated MSAs (1,253.50 vs. 812.66), and both cases and deaths per 100,000 were on average relatively lower (247.47 and 2.03 vs. 274.04 and 3.58).

Table 2: Descriptive Statistics

	All MSAs			Treated MSAs			Untreated MSAs		
Variable	Mean	SD	Median	Mean	SD	Median	Mean	SD	Median
First Doses per 100,000	817.47	1,344.30	458.98	1,253.50	1,237.18	827.71	812.66	1,344.65	455.01
Cases per 100,000	273.75	373.61	147.73	247.47	394.75	121.95	274.04	373.37	148.16
Deaths per 100,000	3.56	5.87	1.90	2.03	2.31	1.17	3.58	5.90	1.91
Number of Observations		57,470			630			56,840	

Notes: The unit of observation is MSA week. Our sample consists of 821 MSAs, 9 of which are treated, and the period spans 70 weeks from December 21, 2020, to April 18, 2022.

4 Empirical Strategy

Our aim is to estimate the causal effects of indoor vaccine mandates on COVID-19 vaccine uptake, cases, and deaths in nine US cities. These policy changes were arguably not random and, absent exogenous variation, we rely on statistical models that connect observed data to unobserved counterfactuals. While many approaches have been developed for such settings, the difference-in-differences method has been widely used over the last three decades, and more recently the synthetic control method has emerged as an alternative for comparative case studies. Arkhangelsky et al. (2021) propose a new method: synthetic difference-in-differences (SDID), which combines attractive features of both the difference-in-differences and the synthetic control methods. The authors show that empirically, the SDID estimator is competitive with (or dominates) the difference-in-differences estimator, where the difference-in-differences method has been used in the past. Likewise, the SDID estimator is competitive with (or dominates) the synthetic control estimator in applications where the synthetic control method has been used in the past. Our main analysis, therefore, relies on the more robust SDID estimator, both theoretically and empirically.

To introduce the main ideas behind the SDID estimator, we closely follow the explanation and notation of Arkhangelsky et al. (2021). Consider a balanced panel with N units and T time periods, where the outcome for unit i in period t is denoted by Y_{it} and exposure to a binary treatment (the

implementation of an indoor vaccine mandate) is denoted by $W_{it} \in \{0,1\}$. Suppose further that the first N_{co} (control) units are never exposed to the treatment, whereas the last $N_{tr} = N - N_{co}$ (treated) units are exposed after time T_{pre}. We analyze the effects of the indoor vaccine mandates in each of the nine MSAs separately. Therefore, in our context, $N = 813$ MSAs, $N_{tr} = 1$ (the MSA that implemented the mandate), and $N_{co} = 812$ (the rest of the MSAs in our sample that did not implement the mandate). While T is the same in each of the nine analyses, T_{pre} will vary depending on when the treated MSA implemented the mandate.

Similar to the synthetic control method, the SDID starts by finding unit weights for all control units, denoted by $\hat{\omega}^{sdid}$, such that the outcome for the treated unit in the pre-treatment period is approximately parallel to the weighted average outcome for control units over the same period. The SDID also searches for time weights, denoted by $\hat{\lambda}^{sdid}$, that balance pre-treatment time periods with post-treatment time periods. These weights are then used in a two-way fixed effects regression to estimate the average causal effect of exposure to the treatment, denoted by τ.

$$\left(\hat{\tau}^{sdid}, \hat{\mu}, \hat{\alpha}, \hat{\beta}\right) = \underset{\tau,\mu,\alpha,\beta}{\arg\min} \left\{ \sum_{i=1}^{N} \sum_{t=1}^{T} \left(Y_{it} - \mu - \alpha_i - \beta_t - W_{it}\tau\right)^2 \hat{\omega}_i^{sdid} \hat{\lambda}_t^{sdid} \right\} \tag{1}$$

By way of comparison, the difference-in-differences method often estimates the effect of a treatment by solving the same two-way fixed effects regression, but without either unit or time weights.

$$\left(\hat{\tau}^{did}, \hat{\mu}, \hat{\alpha}, \hat{\beta}\right) = \underset{\tau,\mu,\alpha,\beta}{\arg\min} \left\{ \sum_{i=1}^{N} \sum_{t=1}^{T} \left(Y_{it} - \mu - \alpha_i - \beta_t - W_{it}\tau\right)^2 \right\} \tag{2}$$

The synthetic control estimator can be expressed in a similar way by omitting the unit fixed effects and the time weights from the two-way fixed effects regression.

$$\left(\hat{\tau}^{sc}, \hat{\mu}, \hat{\beta}\right) = \underset{\tau,\mu,\beta}{\arg\min} \left\{ \sum_{i=1}^{N} \sum_{t=1}^{T} \left(Y_{it} - \mu - \beta_t - W_{it}\tau\right)^2 \hat{\omega}_i^{sc} \right\} \tag{3}$$

The use of unit weights in the SDID and the synthetic control methods is intuitively appealing in that more weight is given to control units that are similar (in terms of pre-treatment characteristics)

to the treated unit. If estimating the effect of the indoor vaccine mandate in New York City, for example, it makes sense to emphasize MSAs that are similar to the MSA containing New York City, especially prior to the mandate. Moreover, the use of unit weights in the SDID and the synthetic control methods weakens the reliance on parallel-trends type assumptions inherent in the difference-in-differences method. This is because the weights by design are chosen such that the weighted average outcome of the control group matches (in the case of synthetic control) or is parallel to (in the case of the SDID) the trend in the outcome of the treated unit. The inclusion of the unit fixed effects in the SDID estimator is what facilitates this added flexibility relative to the synthetic control estimator. Arkhangelsky et al. (2021) argue that, in general, the inclusion of weights and unit fixed effects in the SDID estimator makes it more flexible and robust, in addition to potentially being more precise, relative to the conventional difference-in-differences and synthetic control estimators.

4.1 Synthetic Difference-in-Differences Weights

Arkhangelsky et al. (2021) provide an R package—*synthdid*—for implementing the methods developed in their paper. We use this package to find the unit and time weights as well as to run the weighted two-way fixed effects regression in equation (1). The unit weights, $\hat{\omega}^{sdid}$, are chosen by solving the optimization problem

$$\left(\hat{\omega}_0, \hat{\omega}^{sdid}\right) = \underset{\omega_0 \in \mathbb{R}, \omega \in \Omega}{\arg\min}\ \ell_{unit}\left(\omega_0, \omega\right), \tag{4}$$

where

$$\ell_{unit}\left(\omega_0, \omega\right) = \sum_{t=1}^{T_{pre}} \left(\omega_0 + \sum_{i=1}^{N_{co}} \omega_i Y_{it} - \frac{1}{N_{tr}} \sum_{i=N_{co}+1}^{N} Y_{it} \right)^2 + \zeta^2 T_{pre} \|\omega\|_2^2,$$

$$\Omega = \left\{ \omega \in \mathbb{R}_+^N : \sum_{i=1}^{N_{co}} \omega_i = 1, \omega_i = N_{tr}^{-1} \ \text{ for all } \ i = N_{co}+1, \ldots, N \right\},$$

and $\mathbb{R}_+$ denotes the positive real line.

The regularization parameter ζ is defined as

$$\zeta = (N_{tr}T_{post})^{1/4}\,\hat{\sigma} \qquad \text{with} \qquad \hat{\sigma}^2 = \frac{1}{N_{co}(T_{pre}-1)}\sum_{i=1}^{N_{co}}\sum_{t=1}^{T_{pre}-1}(\Delta_{it}-\bar{\Delta})^2, \tag{5}$$

where

$$\Delta_{it} = Y_{i(t+1)} - Y_{it} \qquad \text{and} \qquad \bar{\Delta} = \frac{1}{N_{co}(T_{pre}-1)}\sum_{i=1}^{N_{co}}\sum_{t=1}^{T_{pre}-1}\Delta_{it}.$$

Arkhangelsky et al. (2021) provide a more in depth explanation for and justification of this approach. The basic idea is that the unit weights are chosen to find a convex combination of potential control states whose treatment trend in the outcome variable of interest is most parallel to that of the treated state. The inclusion of the intercept term ω_0 (made possible because of the inclusion of the unit fixed effects) is one way in which the SDID unit weights differ from those of the synthetic control weights. Instead of the weights needing to make the pre-trend control unit perfectly match that of the treated unit, as is the case with the synthetic control estimator, allowing for this intercept makes it sufficient for the weights to just make the trends parallel.

The other main way that the two sets of weights differ is that the SDID incorporates the regularization parameter ζ, which is chosen to match the size of a typical one-period change in the outcome variable (Δ_{it}) for control states in the pre-period, multiplied by a theoretically motivated scaling $(N_{tr}T_{post})^{1/4}$. This is included to ensure the uniqueness, and to increase the dispersion, of the weights.

Similarly, the time weights, $\hat{\lambda}^{sdid}$, are chosen by solving the optimization problem

$$\left(\hat{\lambda}_0, \hat{\lambda}^{sdid}\right) = \underset{\lambda_0\in\mathbb{R},\lambda\in\Lambda}{\arg\min}\ \ell_{time}(\lambda_0,\lambda), \tag{6}$$

where

$$\ell_{time}(\lambda_0,\lambda) = \sum_{i=1}^{N_{co}}\left(\lambda_0 + \sum_{t=1}^{T_{pre}}\lambda_t Y_{it} - \frac{1}{T_{post}}\sum_{t=T_{pre}+1}^{T} Y_{it}\right)^2 \quad \text{and}$$

$$\Lambda = \left\{\lambda\in\mathbb{R}_+^T : \sum_{t=1}^{T_{pre}}\lambda_t = 1, \lambda_t = T_{post}^{-1} \ \text{ for all } \ t = T_{pre}+1,\ldots,T\right\}.$$

The main difference between (4) and (6) is that regularization is used for the former but not the latter, as explained and justified in Arkhangelsky et al. (2021).

To summarize, the procedure for estimating $\hat{\tau}^{sdid}$ separately for each of the nine treated MSAs consists of computing the regularization parameter ζ using (5); calculating the unit and time weights $\hat{\omega}^{sdid}$ and $\hat{\lambda}^{sdid}$ via (4) and (6); and then estimating the weighted two-way fixed effects with difference-in-differences regression in (1).

4.2 Inference

We implement the placebo variance estimation approach used in Arkhangelsky et al. (2021) to conduct statistical inference. This approach is similar to the placebo evaluations often used in the synthetic control applications, where there is only one treated unit. The main idea of this approach is to consider the behavior of the SDID estimator when replacing the MSAs that were treated with MSAs that were not treated. Because these MSAs did not actually experience the treatment, "placebo" estimates of $\hat{\tau}^{sdid}$ provide a way of estimating the noise level, which can then be used to estimate the asymptotic variance.

Specifically, we randomly select B of the N_{co} untreated MSAs, and for each of the randomly selected untreated MSAs (indexed by b), we compute a placebo estimate of $\hat{\tau}^{sdid}$, denoted by $\hat{\tau}^{(b)}$, using the same procedure outlined in the previous section.[3] We then use this placebo distribution to estimate the asymptotic variance $\hat{V}_\tau$ by finding the average sum of squared deviations from the mean. Thus,

$$\hat{V}_\tau = \frac{1}{B}\sum_{b=1}^{B}\left(\hat{\tau}^{(b)} - \frac{1}{B}\sum_{b=1}^{B}\hat{\tau}^{(b)}\right)^2. \tag{7}$$

With $\hat{V}_\tau$, we report confidence intervals defined conventionally by $\tau \in \hat{\tau} \pm z_{\alpha/2}\sqrt{\hat{V}_\tau}$. The validity of this approach relies crucially on homoscedasticity across MSAs, because if the treated and untreated MSAs have different noise distributions, then there is no way to estimate $\hat{V}_\tau$ from only the untreated MSAs.

[3] We use the *synthdid* R package to implement this procedure, where the default value of B is 200.

5 Results

Indoor COVID-19 vaccine mandates were announced and implemented in nine major US cities between August 2021 and March 2022 with the intention of increasing vaccine uptake, thereby reducing COVID-19 cases and deaths. Previous studies have shown that country-level and province-level indoor vaccine mandates implemented in Europe and Canada were effective in increasing first-dose vaccine uptake (Karaivanov et al., 2022; Mills & Rüttenauer, 2022). The descriptive statistics in table 2 reveal that, in an average week over the sample period, first doses of the vaccines per 100,000 in the treated MSAs were considerably higher than in the untreated MSAs, and cases and deaths per 100,00 were relatively lower. These statistics are consistent with past findings suggesting that the indoor vaccine mandates in these US cities have been effective in fulfilling their intended objective. However, it is possible—although not likely, given the politicization of the COVID-19 vaccines and the political orientation of these cities at the time—that individuals in the treated MSAs were more likely to be vaccinated than those in the untreated MSAs, independent of the mandates.

As it relates to the previous studies on country-level and province-level mandates in Europe and Canada, it is worth noting that the city-level mandates in the United States were implemented toward the end of 2021 and beginning of 2022, after the majority of mandates were implemented in other countries and a large proportion of the population in each adopting city had already received the vaccine. Moreover, we expect the US city-level mandates, if they had any effect at all, to be relatively less effective than country-level and province-level mandates because these mandates increase the cost of not being vaccinated more than city-level mandates. For example, an unvaccinated person in France would have to travel to another country to legally eat at an indoor restaurant, whereas the same unvaccinated person in New York City could simply drive to New Jersey to do so.

Our results overwhelmingly support the conclusion that the city-level indoor vaccine mandates in the United States had a statistically negligible effect on vaccine uptake, cases, and deaths, and they were likely less effective when compared to country-level and province-level mandates pre-

viously studied. Tables 3, 4, and 5 report the average estimated effects of the announcement of each city-level mandate on weekly first doses, cases, and deaths per 100,000, respectively. The SDID estimates, $\hat{\tau}^{sdid}$ from equation (1), are in column (3) of each table. For comparison, columns (1) and (2) show estimates using the difference-in-differences and the synthetic control estimators ($\hat{\tau}^{did}$ and $\hat{\tau}^{sc}$ from equations (2) and (3)).

Table 3: Announcement of Indoor COVID-19 Vaccine Mandates and First-Dose Vaccine Uptake

	Dependent Variable : *Weekly First Doses per* 100,000		
	Difference-in-Differences (1)	Synthetic Control (2)	SDID (3)
Panel A. **Boston**			
Average Effect ($\hat{\tau}$)	319.04	–140.04	72.69
95% Confidence Interval	(–1047.25, 1685.33)	(–1211.06, 930.99)	(–1160.96, 1306.33)
Panel B. **Chicago**			
Average Effect ($\hat{\tau}$)	–39.95	–28.4	–172.34
95% Confidence Interval	(–1197.23, 1117.34)	(–894.70, 837.91)	(–1188.18, 843.50)
Panel C. **Los Angeles**			
Average Effect ($\hat{\tau}$)	–143.09	–242.61	–185.31
95% Confidence Interval	(–1142.48, 856.31)	(–740.82, 255.59)	(–966.80, 596.19)
Panel D. **New Orleans**			
Average Effect ($\hat{\tau}$)	–341.38	–219.38	–209.07
95% Confidence Interval	(–1642.73, 959.97)	(–724.08, 285.32)	(–721.84, 303.70)
Panel E. **New York**			
Average Effect ($\hat{\tau}$)	–575.97	123.77	–82.59
95% Confidence Interval	(–1907.72, 755.79)	(–398.14, 645.68)	(–605.48, 440.30)
Panel F. **Philadelphia**			
Average Effect ($\hat{\tau}$)	104.16	–295.41	–303.02
95% Confidence Interval	(–1148.25, 1356.57)	(–1252.58, 661.76)	(–1401.35, 795.31)
Panel G. **San Francisco**			
Average Effect ($\hat{\tau}$)	−1197.67*	–42.89	–195.37
95% Confidence Interval	(–2504.92, 109.58)	(–566.19, 480.41)	(–726.44, 335.71)
Panel H. **Seattle**			
Average Effect ($\hat{\tau}$)	–736.58	–97.14	–207.02
95% Confidence Interval	(–1978.53, 505.38)	(–688.32, 494.03)	(–840.35, 426.32)
Panel I. **Washington DC**			
Average Effect ($\hat{\tau}$)	–253.99	18.77	–76.53
95% Confidence Interval	(–1620.28, 1112.31)	(–1059.12, 1096.67)	(–1309.86, 1156.80)

Notes: This table reports the average estimated effects of announcing an indoor COVID-19 vaccine mandate on first-dose vaccine uptake as measured by weekly first doses per 100,000 residents using the difference-in-differences, the synthetic control, and the SDID estimators ($\hat{\tau}$ from equations (2), (3), and (1)). Also reported are 95% confidence intervals using the placebo variance estimation approach outlined in section 4.2. Significance levels are reported as *** p<0.01, ** p<0.05, and * p<0.1.

Table 4: Announcement of Indoor COVID-19 Vaccine Mandates and COVID-19 Cases

	Dependent Variable : *Weekly COVID*-19 Cases per 100,000		
	Difference-in-Differences (1)	Synthetic Control (2)	SDID (3)
Panel A. **Boston**			
Average Effect ($\hat{\tau}$)	274.32	240.05	224.57
95% Confidence Interval	(–252.03, 800.67)	(–272.99, 753.09)	(–267.13, 716.27)
Panel B. **Chicago**			
Average Effect ($\hat{\tau}$)	139.6	184.48	121.14
95% Confidence Interval	(–299.65, 578.84)	(–245.53, 614.49)	(–289.63, 531.91)
Panel C. **Los Angeles**			
Average Effect ($\hat{\tau}$)	202.06	340.28***	176.49
95% Confidence Interval	(–74.98, 479.09)	(97.34, 583.22)	(–58.08, 411.05)
Panel D. **New Orleans**			
Average Effect ($\hat{\tau}$)	–22.81	6.15	–27.28
95% Confidence Interval	(–216.80, 171.19)	(–182.99, 195.28)	(–217.93, 163.36)
Panel E. **New York**			
Average Effect ($\hat{\tau}$)	–53.04	7.02	4.62
95% Confidence Interval	(–251.54, 145.46)	(–186.94, 200.98)	(–190.87, 200.12)
Panel F. **Philadelphia**			
Average Effect ($\hat{\tau}$)	110.41	290.62	114.41
95% Confidence Interval	(–368.76, 589.58)	(–180.84, 762.08)	(–329.83, 558.66)
Panel G. **San Francisco**			
Average Effect ($\hat{\tau}$)	–107.37	65.71	–95.48
95% Confidence Interval	(–311.98, 97.24)	(–135.80, 267.22)	(–297.46, 106.50)
Panel H. **Seattle**			
Average Effect ($\hat{\tau}$)	19.85	20.72	–16.99
95% Confidence Interval	(–239.41, 279.12)	(–202.52, 243.95)	(–247.34, 213.36)
Panel I. **Washington DC**			
Average Effect ($\hat{\tau}$)	149.7	600.71	190.26
95% Confidence Interval	(–376.66, 676.05)	(86.71, 1114.72)	(–301.70, 682.22)

Notes: This table reports the average estimated effects of announcing an indoor COVID-19 vaccine mandate on the number of weekly COVID-19 cases per 100,000 residents using the difference-in-differences, the synthetic control, and the SDID estimators ($\hat{\tau}$ from equations (2), (3), and (1)). Also reported are 95% confidence intervals using the placebo variance estimation approach outlined in section 4.2. Significance levels are reported as *** p<0.01, ** p<0.05, and * p<0.1.

Table 5: Announcement of Indoor COVID-19 Vaccine Mandates and COVID-19 Deaths

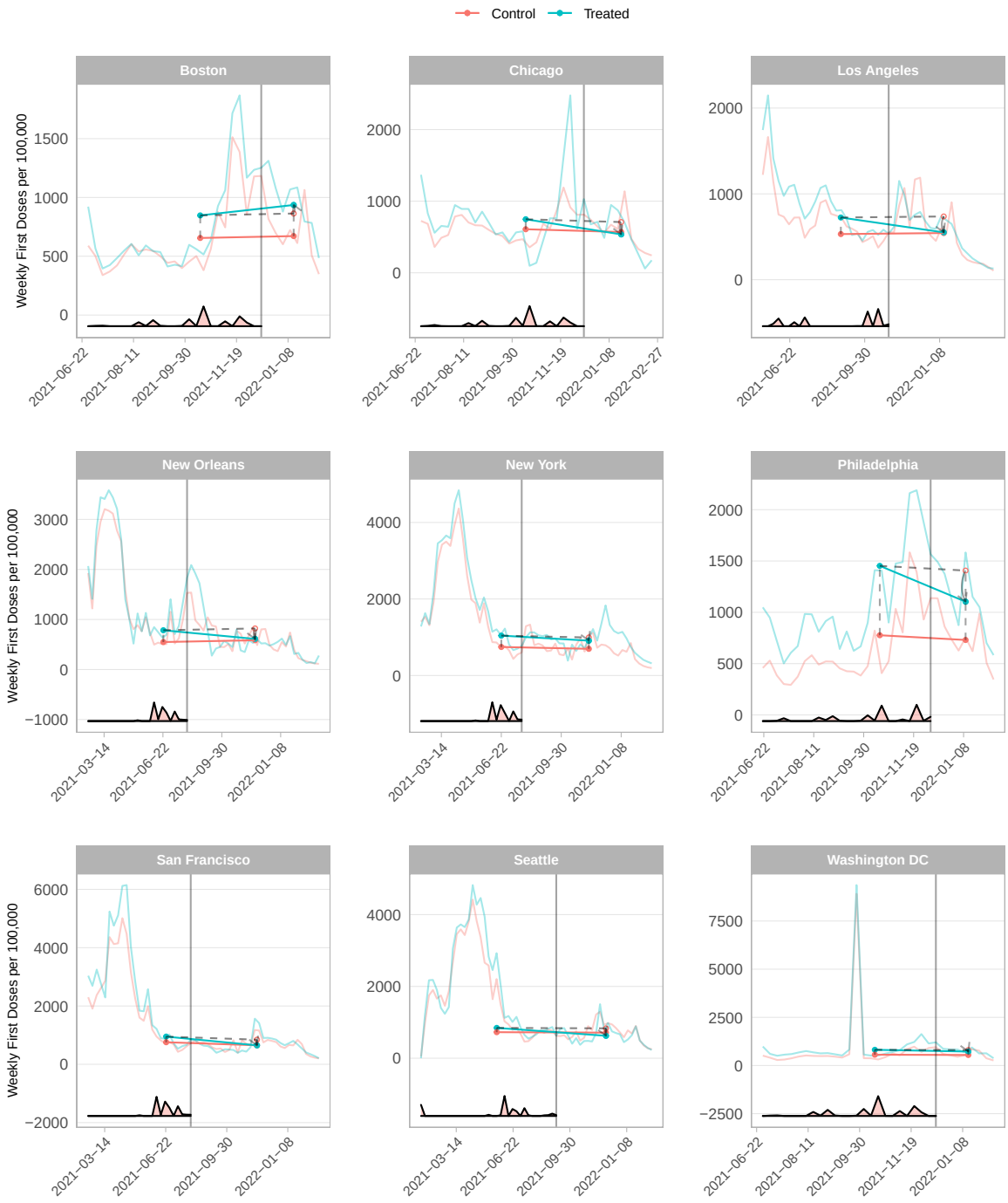
	Dependent Variable : *Weekly COVID*-19 Deaths per 100,000		
	Difference-in-Differences (1)	Synthetic Control (2)	SDID (3)
Panel A. **Boston**			
Average Effect ($\hat{\tau}$)	2.32	1.65	1.38
95% Confidence Interval	(–4.75, 9.39)	(–5.97, 9.28)	(–4.76, 7.53)
Panel B. **Chicago**			
Average Effect ($\hat{\tau}$)	1.94	1.46	1.39
95% Confidence Interval	(–4.21, 8.09)	(–5.10, 8.03)	(–4.06, 6.84)
Panel C. **Los Angeles**			
Average Effect ($\hat{\tau}$)	–0.2	0.67	–0.3
95% Confidence Interval	(–5.26, 4.86)	(–3.85, 5.19)	(–4.52, 3.92)
Panel D. **New Orleans**			
Average Effect ($\hat{\tau}$)	–0.65	–2.5	–1.37
95% Confidence Interval	(–4.48, 3.18)	(–6.07, 1.07)	(–4.96, 2.22)
Panel E. **New York**			
Average Effect ($\hat{\tau}$)	–2.37	–2.66	–1.91
95% Confidence Interval	(–6.16, 1.43)	(–6.09, 0.76)	(–5.42, 1.60)
Panel F. **Philadelphia**			
Average Effect ($\hat{\tau}$)	2.76	–2.16	2.21
95% Confidence Interval	(–4.11, 9.63)	(–9.35, 5.02)	(–3.69, 8.11)
Panel G. **San Francisco**			
Average Effect ($\hat{\tau}$)	–2.19	-4.72^{**}	–2.66
95% Confidence Interval	(–6.14, 1.76)	(–8.51, –0.93)	(–6.36, 1.04)
Panel H. **Seattle**			
Average Effect ($\hat{\tau}$)	–1.07	-1.08	–1.43
95% Confidence Interval	(–5.04, 2.91)	(–4.63, 2.47)	(–5.09, 2.22)
Panel I. **Washington DC**			
Average Effect ($\hat{\tau}$)	0.46	–0.92	0.2
95% Confidence Interval	(–6.61, 7.53)	(–8.55, 6.70)	(–5.95, 6.35)

Notes: This table reports the average estimated effects of announcing an indoor COVID-19 vaccine mandate on the number of weekly COVID-19 deaths per 100,000 residents using the difference-in-differences, the synthetic control, and the SDID estimators ($\hat{\tau}$ from equations (2), (3), and (1)). Also reported are 95% confidence intervals using the placebo variance estimation approach outlined in section 4.2. Significance levels are reported as *** p<0.01, ** p<0.05, and * p<0.1.

Analogous to tables 3, 4, and 5, tables A1, A2, and A3 in the appendix report the average estimated effects of the implementation of each city-level mandate (as opposed to the announcement of the mandates). Regardless of the outcome, with few exceptions, and regardless of the estimator used, we do not find any statistically significant evidence that the announcement or implementation of an indoor vaccine mandate affected vaccine uptake, cases, or deaths within these cities.

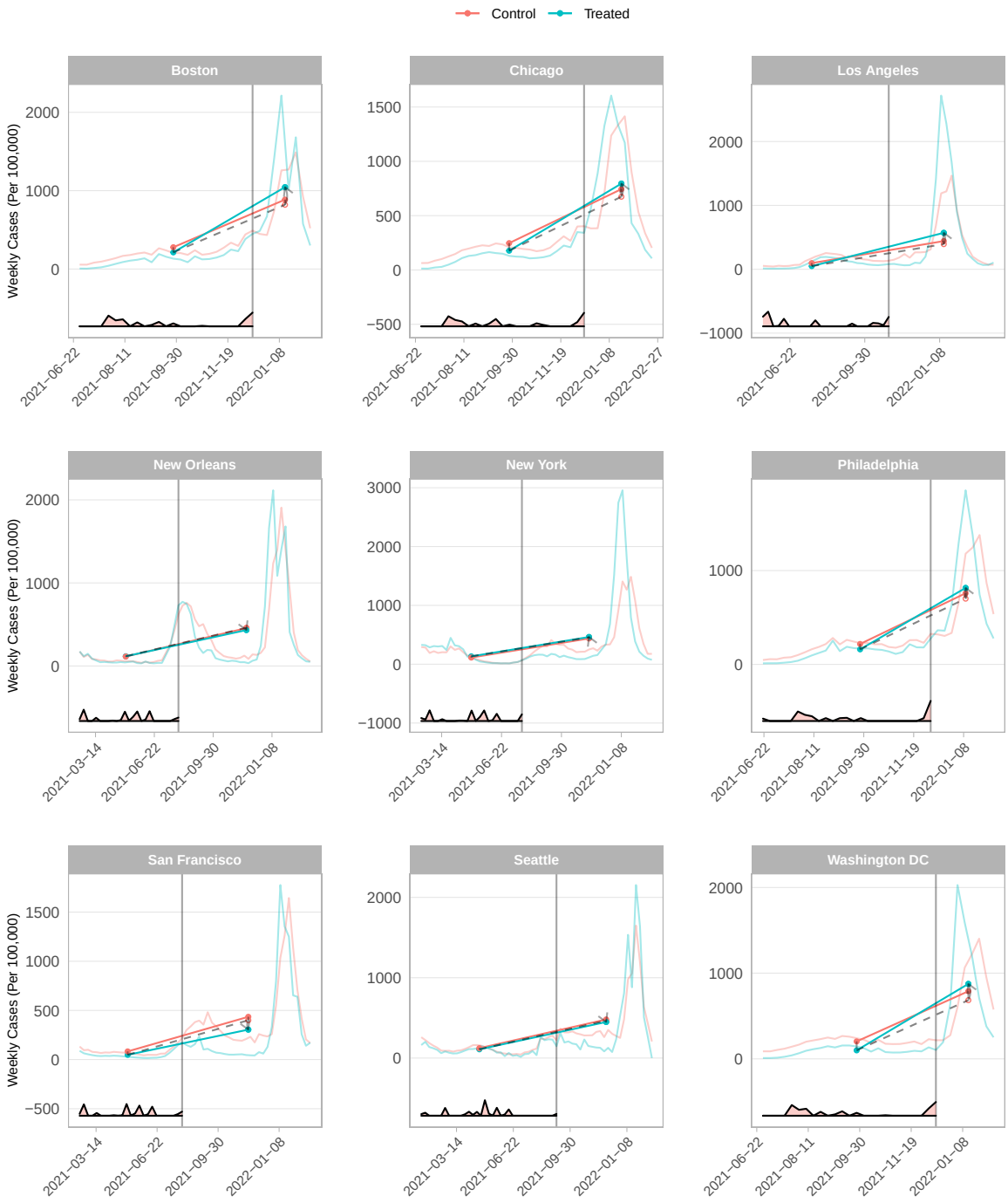
The plots in figures 1, 2, and 3 show trends in weekly first doses, cases, and deaths per 100,000, respectively, in each of the treated MSAs and their corresponding SDID synthetic controls. As noted earlier, the SDID synthetic controls are weighted averages of all untreated MSAs, with the unit and time weights chosen to solve the optimization problems in equations (4) and (6). Therefore, they provide a counterfactual to which the corresponding treated MSA is compared. The plots in the figures shed light on where the SDID point estimates in column (3) of tables 3, 4, and 5 are coming from. In the traditional difference-in-differences fashion, the curved arrow in the plots show the magnitude of the difference between the average outcome in each treated MSA and their synthetic control, both before and after the announcement of the mandate (indicated by the vertical line).

Figure 1: Trends in Weekly First Doses per 100,000 in Treated MSAs and Their Respective Synthetic Controls



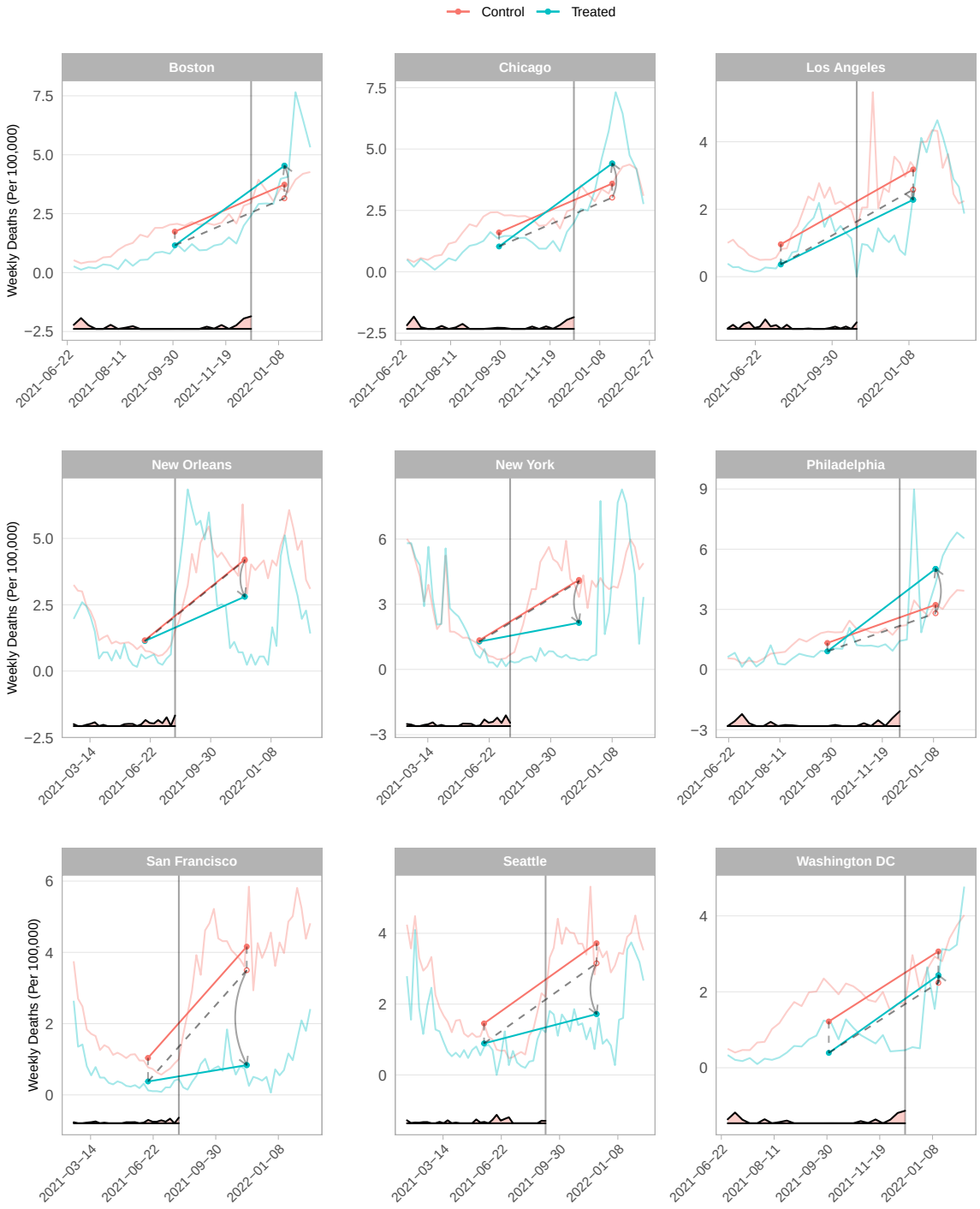
Notes: Each plot shows trends in weekly first doses of COVID-19 vaccinations per 100,000 residents for each MSA that adopted an indoor vaccine mandate and for their corresponding synthetic control. The weights used to average pre-treatment time periods are shown at the bottom of the plots. The curved arrows indicate the estimated average treatment effect ($\hat{\tau}$ from equation (1)) and the vertical lines represent the week each MSA announced their vaccine mandate.

Figure 2: Trends in Weekly COVID-19 Cases per 100,000 in Treated MSAs and Their Respective Synthetic Controls



Notes: Each plot shows trends in weekly COVID-19 cases per 100,000 residents for each MSA that adopted an indoor vaccine mandate and for their corresponding synthetic control. The weights used to average pre-treatment time periods are shown at the bottom of the plots. The curved arrows indicate the estimated average treatment effect ($\hat{\tau}$ from equation (1)) and the vertical lines represent the week each MSA announced their vaccine mandate.

Figure 3: Trends in Weekly COVID-19 Deaths per 100,000 in Treated MSAs and Their Respective Synthetic Controls



Notes: Each plot shows trends in weekly COVID-19 deaths per 100,000 residents for each MSA that adopted an indoor vaccine mandate and for their corresponding synthetic control. The weights used to average pre-treatment time periods are shown at the bottom of the plots. The curved arrows indicate the estimated average treatment effect ($\hat{\tau}$ from equation (1)) and the vertical lines represent the week each MSA announced their vaccine mandate.

In most instances, the estimated effect is quite small; even in instances where the estimated effect appears large (e.g., San Francisco in figure 3), our placebo variance analysis suggests that these estimates are not large when compared to the distribution of placebo estimates. This can be most easily seen in figures A1, A2, and A3 in the appendix, which show the distribution of all placebo estimates for each outcome and treated MSA, and where the corresponding actual estimated effects for each treated MSA (denoted by the vertical lines) fall within each distribution. Regardless of the outcome and MSA in consideration, these placebo estimates appear normally distributed around zero, and in most cases the actual estimated effects do not approach the tails of these distributions. Even for cases like San Francisco in figure A3, where the magnitude of the estimated effect is relatively large, the corresponding distribution of placebo estimates suggest that a nontrivial amount of control MSAs—which did not actually have an indoor vaccine mandate in place—saw a relatively larger change in the outcome when compared to their SDID counterfactual.

The plots in figures 1, 2, and 3 also allow for a visual assessment of pre-trends. In each instance, the chosen SDID weights perform well at finding a weighted average outcome of control MSAs that is approximately parallel to that of the treated MSA in the pre-treatment period. This instills confidence that the SDID synthetic controls provide valid counterfactual trajectories of the treated MSAs throughout each treatment period.

While we cannot claim, based on our results, that the indoor vaccine mandates in these US cities were not effective at all, our results do suggest that if they were effective, the effect was likely smaller or at least less statistically noticeable than the effects of country-level and province-level mandates studied previously.

6 Conclusion

Many of the largest cities in the United States introduced COVID-19 indoor vaccine mandates with the goal of increasing vaccine uptake and thereby reducing COVID-19 cases and deaths. These mandates were among the most stringent policies ever implemented in US cities, and they neg-

atively affected thousands of citizens and businesses. This paper explores the efficacy of these mandates. Using the synthetic difference-in-differences method, we find that indoor vaccine mandates had no significant impact on COVID-19 vaccine uptake, cases, or deaths across all nine cities that implemented the policy. We also find that our results are robust to the synthetic control and the difference-in-differences methods.

Our findings are important for at least two reasons. First, they highlight that policies implemented at different jurisdictional levels have different outcomes. Karaivanov et al. (2022) and Mills and Rüttenauer (2022) show that indoor vaccine mandates in European countries and Canadian provinces significantly increased vaccine uptake. However, we find that in all US cities that implemented the mandate, the effects are not statistically noticeable. If they had any effect on vaccine uptake, it was likely smaller than the mandates previously studied. Second, our findings bring to question the efficacy of city-level indoor vaccine mandates. These mandates imposed severe restrictions on the lives of many citizens and business owners. Yet, we find no evidence that the mandates were effective in their intended goals of reducing COVID-19 cases and deaths.

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A Appendix

Table A1: Implementation of Indoor COVID-19 Vaccine Mandates and First-Dose Vaccine Uptake

	Dependent Variable : *Weekly First Doses per* 100,000		
	Difference-in-Differences (1)	Synthetic Control (2)	SDID (3)
Panel A. **Boston**			
Average Effect ($\hat{\tau}$)	104.6	5.54	6.54
95% Confidence Interval	(–1921.55, 2130.74)	(–1553.05, 1564.13)	(–1829.43, 1842.52)
Panel B. **Chicago**			
Average Effect ($\hat{\tau}$)	–89.83	–23.26	–24.5
95% Confidence Interval	(–1448.17, 1268.51)	(–1060.20, 1013.67)	(–1257.66, 1208.66)
Panel C. **Los Angeles**			
Average Effect ($\hat{\tau}$)	–175.27	–92.06	–74.89
95% Confidence Interval	(–1141.31, 790.78)	(–636.63, 452.51)	(–738.64, 588.86)
Panel D. **New Orleans**			
Average Effect ($\hat{\tau}$)	–402.42	–232.38	–225.67
95% Confidence Interval	(–1707.03, 902.20)	(–754.23, 289.47)	(–751.98, 300.64)
Panel E. **New York**			
Average Effect ($\hat{\tau}$)	–506.26	112.43	–57.01
95% Confidence Interval	(–1813.78, 801.26)	(–422.51, 647.37)	(–604.47, 490.46)
Panel F. **Philadelphia**			
Average Effect ($\hat{\tau}$)	–32.32	–229.56	–283.49
95% Confidence Interval	(–1762.13, 1697.49)	(–1610.46, 1151.34)	(–1891.17, 1324.18)
Panel G. **San Francisco**			
Average Effect ($\hat{\tau}$)	-1135.58^{*}	–41.51	–197.08
95% Confidence Interval	(–2447.93, 176.78)	(–582.63, 499.61)	(–743.45, 349.28)
Panel H. **Seattle**			
Average Effect ($\hat{\tau}$)	–592.98	–95.88	–72.6
95% Confidence Interval	(–1897.90, 711.94)	(–793.90, 602.14)	(–829.01, 683.80)
Panel I. **Washington DC**			
Average Effect ($\hat{\tau}$)	–382.43	–157.05	–215.53
95% Confidence Interval	(–2408.57, 1643.72)	(–1721.07, 1406.97)	(–2050.79, 1619.73)

Notes: This table reports the average estimated effects of implementing an indoor COVID-19 vaccine mandate on first-dose vaccine uptake as measured by weekly first doses per 100,000 residents using the difference-in-differences, the synthetic control, and the SDID estimators ($\hat{\tau}$ from equations (2), (3), and (1)). Also reported are 95% confidence intervals using the placebo variance estimation approach outlined in section 4.2. Significance levels are reported as *** p<0.01, ** p<0.05, and * p<0.1.

Table A2: Implementation of Indoor COVID-19 Vaccine Mandates and COVID-19 Cases

	Dependent Variable : *Weekly COVID-19 Cases per* 100,000		
	Difference-in-Differences (1)	Synthetic Control (2)	SDID (3)
Panel A. **Boston**			
Average Effect ($\hat{\tau}$)	30.18	108.81	99.49
95% Confidence Interval	(–654.89, 715.25)	(–500.91, 718.53)	(–510.82, 709.80)
Panel B. **Chicago**			
Average Effect ($\hat{\tau}$)	–9.85	19.38	–13.13
95% Confidence Interval	(–521.82, 502.12)	(–450.31, 489.07)	(–487.93, 461.68)
Panel C. **Los Angeles**			
Average Effect ($\hat{\tau}$)	251.14*	422.93***	260.33*
95% Confidence Interval	(–35.55, 537.83)	(157.88, 687.97)	(–3.42, 524.08)
Panel D. **New Orleans**			
Average Effect ($\hat{\tau}$)	–57.99	3.7	–24.09
95% Confidence Interval	(–257.51, 141.54)	(–188.31, 195.70)	(–218.26, 170.08)
Panel E. **New York**			
Average Effect ($\hat{\tau}$)	–30.69	6.57	13.55
95% Confidence Interval	(–240.62, 179.24)	(–194.28, 207.42)	(–191.10, 218.20)
Panel F. **Philadelphia**			
Average Effect ($\hat{\tau}$)	–6.79	115.69	23.6
95% Confidence Interval	(–656.31, 642.74)	(–478.57, 709.95)	(–573.61, 620.80)
Panel G. **San Francisco**			
Average Effect ($\hat{\tau}$)	–105.8	34.63	–86.23
95% Confidence Interval	(–316.27, 104.66)	(–170.50, 239.76)	(–291.99, 119.54)
Panel H. **Seattle**			
Average Effect ($\hat{\tau}$)	70.29	79.29	74.47
95% Confidence Interval	(–222.24, 362.82)	(–161.65, 320.22)	(–179.05, 327.98)
Panel I. **Washington DC**			
Average Effect ($\hat{\tau}$)	–270.02	15.23	–260.99
95% Confidence Interval	(–955.10, 415.05)	(–594.82, 625.29)	(–871.55, 349.58)

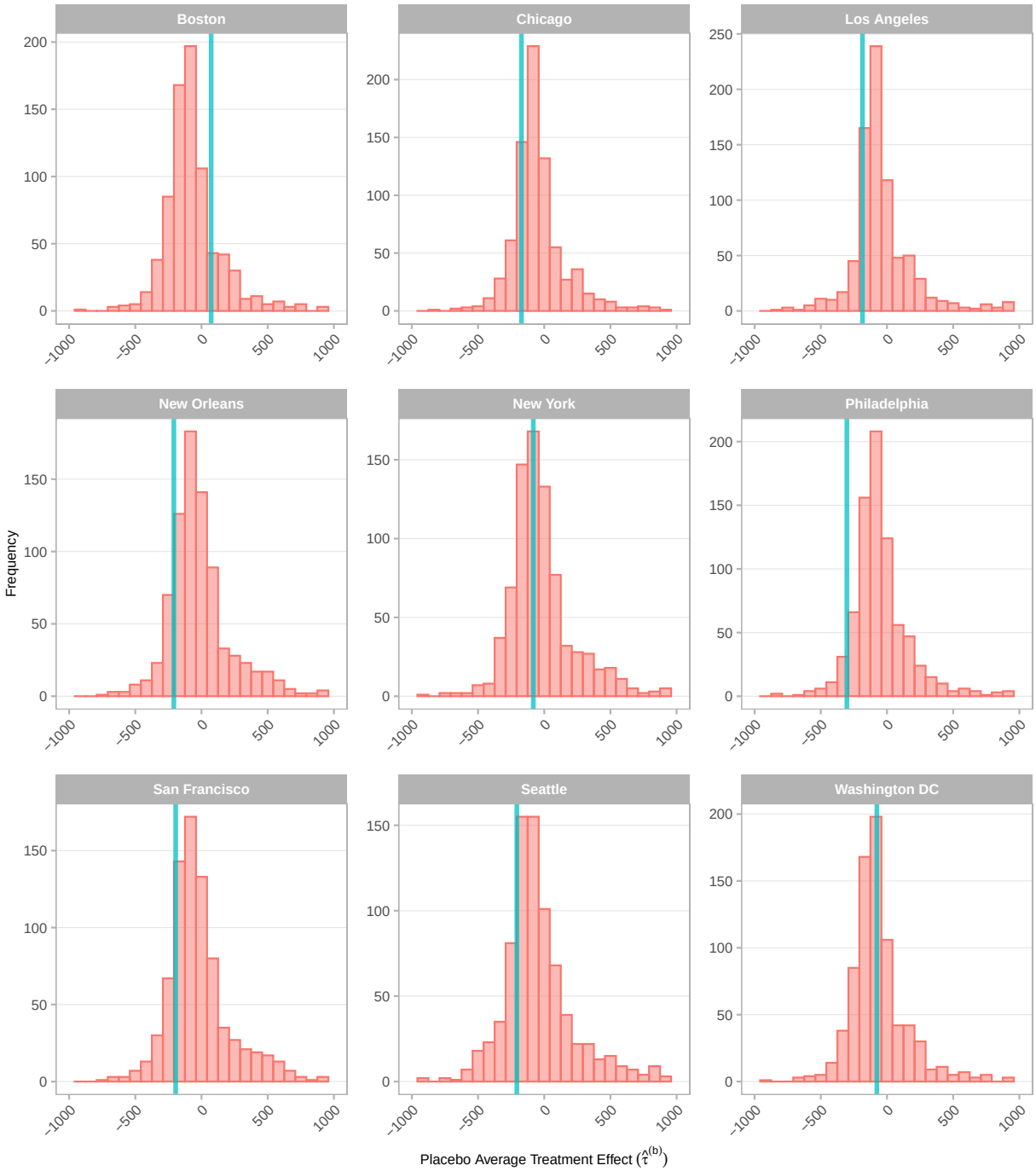
Notes: This table reports the average estimated effects of implementing an indoor COVID-19 vaccine mandate on the number of weekly COVID-19 cases per 100,000 residents using the difference-in-differences, the synthetic control, and the SDID estimators ($\hat{\tau}$ from equations (2), (3), and (1)). Also reported are 95% confidence intervals using the placebo variance estimation approach outlined in section 4.2. Significance levels are reported as *** p<0.01, ** p<0.05, and * p<0.1.

Table A3: Implementation of Indoor COVID-19 Vaccine Mandates and COVID-19 Deaths

	Dependent Variable : *Weekly COVID-19 Deaths per* 100,000		
	Difference-in-Differences (1)	Synthetic Control (2)	SDID (3)
Panel A. **Boston**			
Average Effect ($\hat{\tau}$)	2.9	3.39	2.21
95% Confidence Interval	(–2.76, 8.57)	(–1.41, 8.19)	(–2.76, 7.18)
Panel B. **Chicago**			
Average Effect ($\hat{\tau}$)	2.28	3.13	1.88
95% Confidence Interval	(–2.74, 7.31)	(–1.63, 7.88)	(–2.72, 6.48)
Panel C. **Los Angeles**			
Average Effect ($\hat{\tau}$)	0.2	1.38	0.09
95% Confidence Interval	(–4.90, 5.30)	(–3.41, 6.17)	(–4.47, 4.66)
Panel D. **New Orleans**			
Average Effect ($\hat{\tau}$)	–0.89	–2.13	–1.63
95% Confidence Interval	(–4.72, 2.94)	(–5.74, 1.48)	(–5.21, 1.95)
Panel E. **New York**			
Average Effect ($\hat{\tau}$)	–2.36	–2.73	–1.86
95% Confidence Interval	(–6.20, 1.48)	(–6.40, 0.94)	(–5.43, 1.70)
Panel F. **Philadelphia**			
Average Effect ($\hat{\tau}$)	2.78	2.77	1.66
95% Confidence Interval	(–2.88, 8.44)	(–2.09, 7.64)	(–3.31, 6.64)
Panel G. **San Francisco**			
Average Effect ($\hat{\tau}$)	–2.26	–4.63**	–2.62
95% Confidence Interval	(–6.22, 1.70)	(–8.50, –0.76)	(–6.33, 1.09)
Panel H. **Seattle**			
Average Effect ($\hat{\tau}$)	–0.57	–0.04	–0.94
95% Confidence Interval	(–5.71, 4.56)	(–3.95, 3.86)	(–5.00, 3.12)
Panel I. **Washington DC**			
Average Effect ($\hat{\tau}$)	0.95	1.88	0.69
95% Confidence Interval	(–4.72, 6.61)	(–2.92, 6.67)	(–4.28, 5.67)

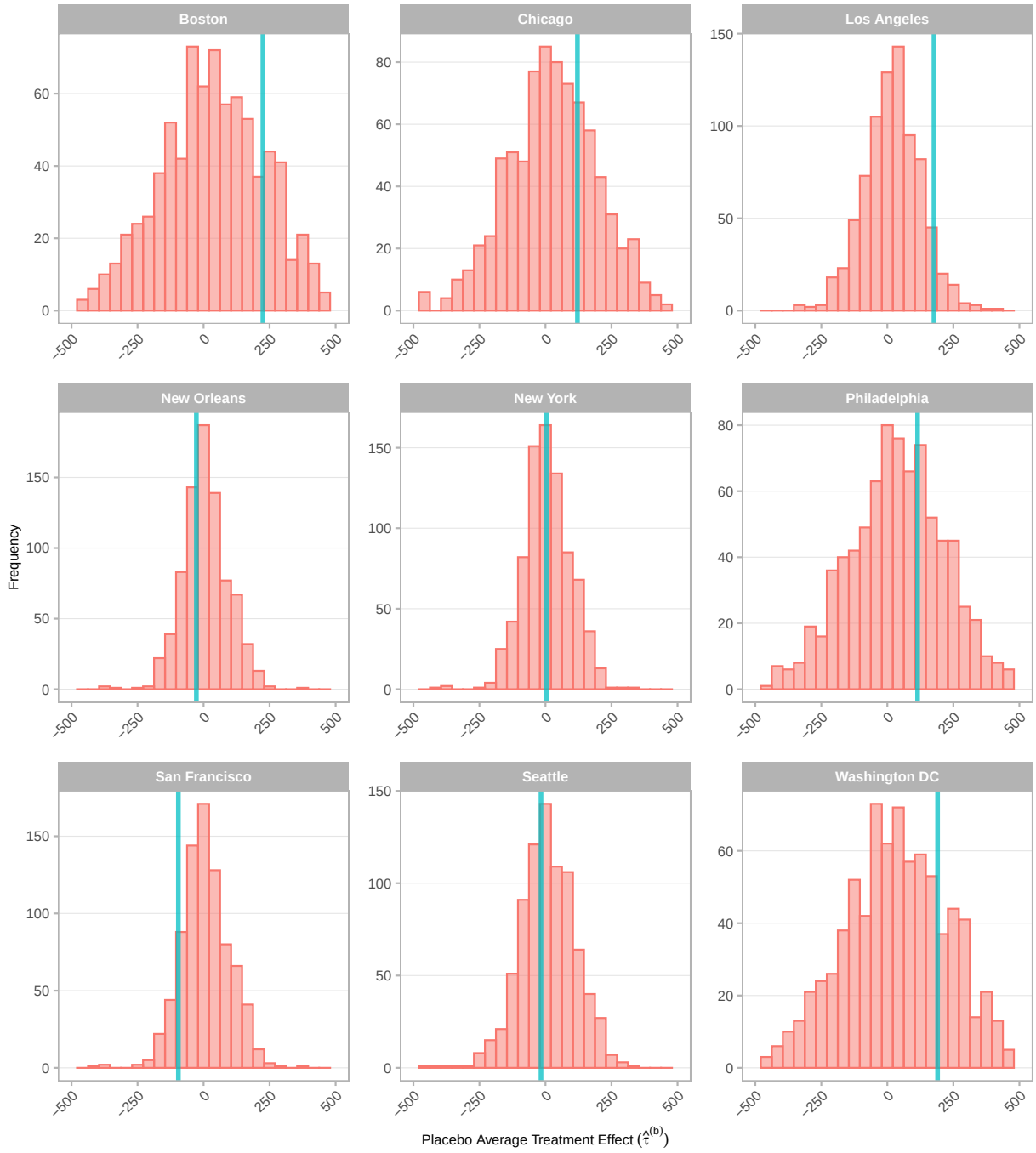
Notes: This table reports the average estimated effects of implementing an indoor COVID-19 vaccine mandate on the number of weekly COVID-19 deaths per 100,000 residents using the difference-in-differences, the synthetic control, and the SDID estimators ($\hat{\tau}$ from equations (2), (3), and (1)). Also reported are 95% confidence intervals using the placebo variance estimation approach outlined in section 4.2. Significance levels are reported as *** p<0.01, ** p<0.05, and * p<0.1.

Figure A1: Distribution of Placebo Estimates for
Weekly COVID-19 Vaccine First Doses per 100,000



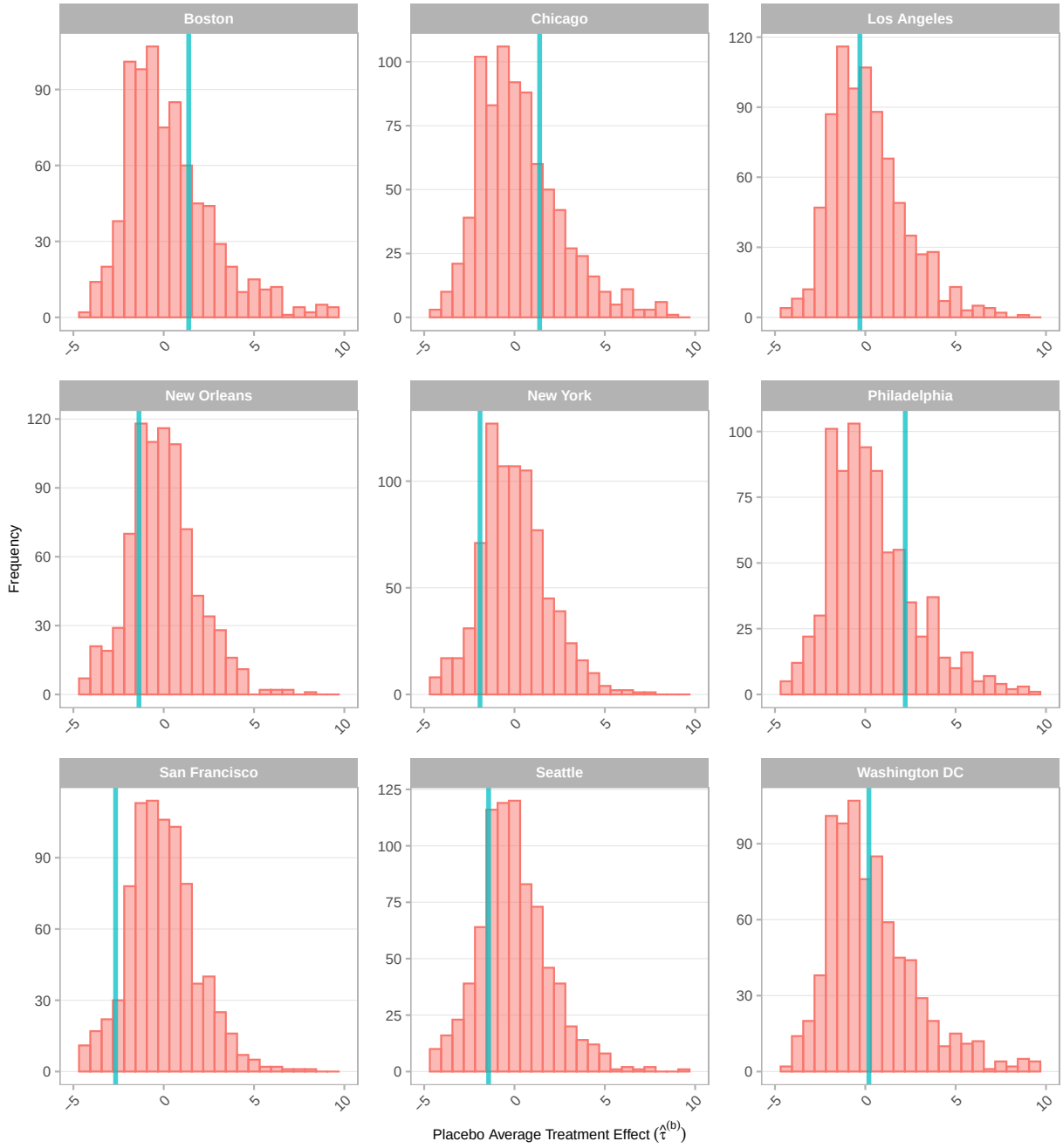
Notes: Each plot shows the distribution of all placebo estimates of announcing an indoor COVID-19 vaccine mandate on the number of weekly COVID-19 first doses per 100,000 residents (denoted by $\hat{\tau}^{(b)}$ and discussed in section 4.2).

Figure A2: Distribution of Placebo Estimates for
Weekly COVID-19 Cases per 100,000



Notes: Each plot shows the distribution of all placebo estimates of announcing an indoor COVID-19 vaccine mandate on the number of weekly COVID-19 cases per 100,000 residents (denoted by $\hat{\tau}^{(b)}$ and discussed in section 4.2).

Figure A3: Distribution of Placebo Estimates for
Weekly COVID-19 Deaths per 100,000



Notes: Each plot shows the distribution of all placebo estimates of announcing an indoor COVID-19 vaccine mandate on the number of weekly COVID-19 deaths per 100,000 residents (denoted by $\hat{\tau}^{(b)}$ and discussed in section 4.2).