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The Dynamic and Heterogeneous Effects of COVID-19 Vaccination Mandates in the USA

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Abstract

Mandatory vaccination for COVID-19 has received intense political and ethical debates, while the literature on the causal effects of vaccination mandates on vaccination outcomes is very limited. In this study, we examine the effects of the announcement of vaccine mandates (VMs) for workers working in three sectors, including health, education, and state governments, on the uptake of first-dose and second-dose vaccination across 50 states in the United States of America. We show that VM announcements have heterogeneous effects; hence, standard two-way fixed effects and difference-in-differences estimators are biased. We present evidence for the heterogeneous treatment effects using recently developed estimators of de Chaisemartin and D'Haultfœuille (2020b) in single and two-treatment settings. In the setting of a single treatment, when treating all VM announcements equally, our results show that VM announcement was associated with an increase of 20.6% first-dose uptake from 1 July to 31 August 2021. In two-treatment settings, our results suggest that VM announcements for workers in health or state government sectors have significant causal effects on first-dose vaccination. Additionally, VM announcements do not have significant causal effects on second-dose uptake. Our results are robust to the choice of differing outcome variables and periods after controlling for state-level covariates, including COVID-19 death, unemployment, and cumulative two-dose vaccination.

Keywords: COVID-19; heterogeneous treatment effects; multiple treatment effects; Difference-in-Differences estimator

JEL Codes: I12, I15, I18, J18, J21, J23

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1 Introduction

High rates of COVID-19 vaccination are required for the achievement of sufficient immunization coverage to end the pandemic evolution (Cascini et al., 2021; Tregoning et al., 2021). Vaccine hesitancy and anti-vaccination are major barriers to this (Arce and Shana, 2021). By early December 2021, governments in more than 40 countries have implemented various forms of COVID-19 vaccination mandates (VMs) with the purpose of overcoming vaccine hesitancy and ultimately increasing vaccination rates (Reuters, 2021). However, mandates for COVID-19 vaccines are very controversial from political and ethical perspectives. Particularly in the United States of America (USA), several forms of mandates for COVID-19 vaccinations have been announced by state governments as early as 26 July 2021. As of 10 December 2021, mandates are in place in 23 states for health workers, 11 states for education workers, and 19 states for workers working in state governments (NASHP, 2021). At the same time, mandates for health and education workers are banned in six states, while mandates for state workers are banned in ten states. Interestingly, resistance to COVID-19 vaccination mandates was found to be higher than for other VMs (Hamel et al., 2021), possibly caused by political polarisation and the influence of propagation of vaccine scepticism on media by political leaders (Hamel et al., 2021; Moon et al., 2023). The lack of rigorous empirical evidence on the effects of mandates on vaccination rates is one of the main reasons for limited applications of vaccine mandates. In fact, studies presenting survey data or hypothetical analysis postulate that introducing compulsory vaccination can lower the inclination to accept a COVID-19 vaccine (de Figueiredo et al., 2021; Schmelz and Bowles, 2021). Therefore, there is a need to have more rigorous assessments of the effects of COVID-19 VMs on vaccination outcomes to inform policy interventions, especially for future health pandemics.

While measuring the causal effects of VMs on vaccination outcomes is not new for many types of vaccines in the literature (Abrevaya and Mulligan, 2011; Lawler, 2017), it is relatively rare for COVID-19 vaccines. Mills and Rüttenauer (2021) design a synthetic control model to estimate a counterfactual trend of what might have happened in similar circumstances if mandatory COVID-19 certificates, including vaccination passports, recent negative tests, or proof of recovery were not introduced in Denmark, Israel, Italy, France, Germany, and Switzerland for the period from April to August 2021. The authors report that these forms of COVID-19 certification could increase vaccinations 20 days before implementation, and the effect could last up to 40 days after. Also, this research finds no effect in countries that already had average uptake, such as Germany or an unclear effect when certificates were introduced during a period of limited vaccine supply, such as Denmark. Using the same synthetic control method and innovation diffusion theory, Oliu-Barton et al. (2022) attribute 13 percentage points of first-dose vaccine uptake for France, 6.2 percentage points for Germany and 9.7 percentage points for Italy to the COVID certificates. In fact, Oliu-Barton et al. (2022) confirm even stronger overall substantial effects of COVID certificates in these three countries than previously estimated in others (Karaivanov et al., 2022; Mills and Rüttenauer, 2021). One important interpretation of these cross-country studies is that the effects of mandatory COVID-19 certification also depend on the context of pre-existing levels of vaccine uptake, vaccine hesitancy, and the pandemic trajectory.

Focusing on within-country analysis for the USA, Howard-Williams et al. (2022) estimate 2-way fixed effects (state and time) linear regression models using state-level aggregated panel data to link the effect of state-issued COVID-19 vaccine mandates on the uptake of first-dose vaccination eight weeks before and eight weeks after VM announcements that went into effect before December 31, 2021. This study includes data from two groups of state-level jurisdictions: 13 jurisdictions with a vaccine mandate that did not allow recurring testing in lieu of vaccination as the mandate group, and 14 jurisdictions that allowed a test-out option and/or did not restrict vaccine requirements as the comparison group. The empirical results attribute 11.5 per cent of total first-dose vaccinations due to the mandate announcement. Also, in comparison with the referent comparison group with the coverage of 56.3 per cent, the first-dose uptake among the mandate jurisdictions was estimated to be higher after the mandate announcement by 0.20, 0.33, 0.39, 0.45, 0.49, and 0.59 percentage points starting from week 3 to week 8.

On the other hand, Karaivanov et al. (2022) quantifies the effects on COVID vaccine uptake caused by the announcement of vaccine passports in nine Canadian provinces. This paper evaluates the effect of the government-mandated proof of vaccination requirement for access to public venues and non-essential businesses on first-dose vaccination across Canadian provinces. Particularly, the first study uses two types of estimators in a difference-in-differences (DID) approach. The first estimator is a standard two-way fixed effect (TWFE), which considers the treatment effect constant across groups and over time. The second estimator of Sun and Abraham (2021) allows for heterogeneous effects due to the introduction of the single treatment, which is the announcement for vaccination passports. Overall results of Karaivanov et al. (2022) suggest that the vaccination passport announcement leads to a statistically significant and large increase in weekly first-dose vaccine uptake in Canada, and the magnitudes of the effect vary across provinces. Notably, Karaivanov et al. (2022) report insignificant differences in the results of the two estimators used, which differs from common observations that the standard TWFE estimates are typically not robust in the presence of heterogeneity in the effect of the treatment (de Chaisemartin and D'Haultfœuille, 2020a; Goodman-Bacon and Marcus, 2020; Sun and Abraham, 2021). In short, there are few recent empirical studies on the causal effects of COVID-19 vaccine mandates in several countries using appropriate econometric modelling approaches.

In the present paper, we first undertake the single treatment approach and estimate the overall effect of the VM announcement using the new heterogeneity-robust estimators of de Chaisemartin and D'Haultfœuille (2020a) that allow the heterogeneous effect of the treatment across states and over time. In addition to the heterogeneity in the treatment effect across states and time, state COVID vaccine mandates are also characterised by the presence of different types of VMs, which are announced and come into effect at differing times. For example, the governor of California was the first governor in the USA to announce VM for workers in health and state government sectors on 26 July 2021, which was quickly followed by similar announcements by New York's governor. On the other hand, Oregon announced the mandate for the health sector not until 4 August 2021. Considering variations in the timing of different VM mandates across states, we apply the multiple treatment DID estimator of (de Chaisemartin and D'Haultfœuille, 2020b) to examine the effects of these mandates and compare its results with those of the standard TWFE.

The TWFE estimators show a negative effect of the announcement of VM for state government workers but a positive effect for education workers. We detect the presence of heterogeneous and dynamic effects of the treatment; hence, the results of the standard TWFE estimators are not robust, which is in line with the recent literature (de Chaisemartin and D'Haultfœuille, 2020a; Goodman-Bacon and Marcus, 2020; Sun and Abraham, 2021). Our primary contributions rest on the empirical results originating from the estimates of the dynamic and heterogeneous effects of multiple VM announcements on vaccination outcomes. In the setting of a single treatment, states that announced VMs for workers in any of the three sectors are found to increase on average 20.6% first-dose uptake for the period from 1 July to 31 August 2021. In two-treatment settings, our results suggest that VM announcement for workers in health or state government sectors have stronger causal effects on first-dose vaccination outcomes. Notably, the announcement of VM in these three sectors does not have a statistically significant effect on the second-dose vaccination outcome.

The remainder of this paper is structured as below. Section 2 provides a background on COVID-19 vaccination and mandates. Section 3 presents a theoretical framework and empirical specification of the analysis. Section 4 presents data and descriptive analysis. Section 5 provides key empirical findings together with several robustness checks. Section 6 concludes the paper.

2 Background

As of 8 December 2021, there have been more than 266.5 million confirmed cases of COVID-19 and more than 5.28 million deaths around the globe (WHO 2021). In the USA alone, a total of nearly 49 million COVID-19 cases and 783.4 thousand deaths have been reported (CDC, 2021). Together with this enormous human strategy, the COVID-19 pandemic is resulting in severe shocks to global human livelihood with tremendous economic and social damage (Altig et al., 2020; Brodeur et al., 2021). Historically, vaccines have been the most effective way to combat disease outbreaks (Dhama et al., 2021). As of 8 December 2021, a total of nearly 8 billion vaccine doses have been administered globally, of which 4.28 billion persons vaccinated with at least one dose and nearly 3.37 billion persons fully vaccinated[1]. However, a growing number of studies show that sustained high level of hesitancy against COVID-19 vaccines poses a challenging barrier to the achievement of high vaccination rates, which are required for the achievement of sufficient immunization coverage, especially herd immunity, to end the COVID-19 pandemic evolution (Cascini et al., 2021; Arce and Shana, 2021).

Within the USA, Arce and Shana (2021) and Uslu et al. (2021) observe that most survey studies report a decreasing trend in willingness to vaccinate or an increasing trend in vaccine resistance over time, even in recent months of 2021. For example, in June 2020, 12% of 22,470 respondents indicated "extremely unlikely" responses if a vaccine was available to them. This proportion has increased to 17% during the survey of 25,640 people in February 2021. Focusing on the four largest metropolitan areas, El-Mohandes et al. (2021) show that more than 21% of 6,037 Americans surveyed during mid-April 2021 were unwilling to vaccinate, expressing concerns about vaccine efficiency and safety and questioning the disease's severity. Recently, Uslu et al.

[1] https://COVID19.who.int/

(2021) using the survey of 16,996 people in the USA between 9 June 2021 to 6 July 2021, reported that 18% of respondents said that they are not getting the COVID-19 vaccine, much higher than 15% saying they are willing to get vaccinated. One can argue that hesitancy and anti-vaccination contribute to low vaccination rates in the USA even though there is no constraint in vaccine supply and a vaccine is freely available to the public. As of 8 December 2021, only 71.4% of the population received at least one dose, while only 60.4% were fully vaccinated. These levels are still below the need level for herd immunity (Randolph and Barreiro, 2020; Fernandes et al., 2021).

Mandatory vaccination has been introduced to increase vaccination rates, and hence, improve public health outcomes. Mandatory vaccination has a long history in the USA, going back to 1855 when Massachusetts passed the first law mandating vaccination for schoolchildren. In the absence of mandates, vaccination rates in the USA have generally been low for many vaccines (Haeder, 2021). Currently, all states in the USA require childhood vaccines as a condition of school entry, while adult vaccine mandates are less popular (Gostin et al., 2021). Given this history, it is not common for states not to mandate COVID-19 vaccination. Particularly, policymakers must determine to which populations and sectors mandates should apply as each has its own legal and ethical considerations (Gostin et al., 2021).

In the history of the USA, individual states have taken the leading role in regulating public health via vaccine mandates (Fernandes et al., 2021). In the context of COVID-19 vaccines, several states have implemented mandates for differing groups of adult populations, while other states have banned vaccine mandates. As of 10 December 2021, there are 23 states with at least one form mandate, 10 states have banned mandates, while the remaining states have no mandate. With respect to various categories, mandates are in place in 23 states for health workers, 11 states for education workers, and 19 states for state workers (NASHP, 2021). Among 23 states that require vaccination for healthcare employees, six states have taken a "vaccinate or terminate" approach, 13 states have taken "vaccination or testing" approach, while three states have taken "vaccination or testing and masking" approach. Mandates for health and education workers are banned in six states while mandates for state workers are banned in ten states (NASHP, 2021).[2]

From the perspective of legal interventions, Fernandes et al. (2021) report that as of the 5th of September 2021, 46 states proposed or enacted 148 legal interventions to facilitate or impede vaccination mandates. Among these interventions, there are 19 administrative or executive orders, and 88.5% of interventions are to impede mandates. One of the main arguments for those legal interventions to impede mandates is due to the lack of empirical evidence for the effect of VMs on vaccination rates. Our literature review shows no prior rigorous empirical analysis of the effect of VM announcements in the USA amid anecdotal evidence from media reports.

This paper aims to provide unbiased estimates of the effects of announcements for VMs

[2] Acharya and Dhakal (2021) note that VMs are not the only measures used to increase vaccination rates. Among these differing measures, lottery programs have received significantly increasing research attention Acharya and Dhakal (2021); Brehm et al. (2021); Dave et al. (2021). There is an increasing number of studies that examine the effects of lottery programs (see, for example Barber and West (2022)). Frankel and Kotti (2021) attempt to quantify the effect of vaccination rates on the death rate using the Biden-Trump vote in the 2020 election as an instrument. In this study, the authors estimated the first stage regression for many determinants of vaccination rates, but the VM announcement is not included in this regression.

across 50 states, given their complex setting characterised by the three notable features. First, in some states, there are multiple announcements for differing mandates applied in different sectors of employment. Second, the timing of these treatments varies across states. Third, the effects of these treatments are likely to be heterogeneous across states and over time. These features suggest careful attention to empirical research designs as well as underlying economic models.

3 Methods

3.1 Theoretical framework

We develop the following economic model to illustrate potential mechanisms through which VMs can affect vaccine uptake. This economic model assumes that individuals make vaccination decisions by evaluating benefits and costs. The benefits of getting vaccination can be evaluated through reduced risk of infection. Vaccinated individuals can continue to work without significant changes in their income level, hence economic benefits include avoided loss of income. At the community level, economic benefits could come in the form of benefits brought about by community immunity. On the other hand, costs encompass potential loss of income during days being sick due to vaccination side effects and direct costs of vaccination if vaccination is not free. By isolating these factors, we can better understand the motivations behind individual vaccination choices and identify the primary drivers influencing these decisions.

Our model builds upon existing literature in the field of vaccination behaviours (Agranov et al., 2021; Manski, 2023; Kitagawa and Wang, 2023), which primarily focus on modelling behavioural change as a binary choice: individuals decide to get vaccinated or not. This model allows individuals to choose a binary action while also maximizes welfare by optimizing their economic activities. There exists a trade-off between engaging in economic activities and the risk of infection. While individuals can earn more income by increasing their level of activity, doing so also heightens the risk of infection, ultimately reducing their overall payoff. For local social-planners, stricter vaccine mandates can help prevent infections but may lead to adverse economic outcomes. Thus, our model takes into account both individuals' vaccine behavior and policy making.

Consider a state indexed by $i \in N$, with a population J_i divided into vaccinated ($V_{i,t}$) and unvaccinated ($Q_{i,t}$) individuals at time t. Assuming no population change in the short term, $V_{i,t} + Q_{i,t} = J_i$ for all t where $V_{i,0} = 0, Q_{i,0} = J_i$. At any given time, an individual $j \in \{1, ..., J_i\}$ can allocate time to activities like work and leisure ($a_{j,t}$), constrained by $0 \leq a_{j,t} \leq \bar{a}$. For simplicity of notation, we temporarily omit the unit index i.

VMs reduce the activity level of the unvaccinated individual j to $\tau_j a_{j,t}$ where $0 \leq \tau_j < 1$ represents the impact of mandate stringency on individuals. In states with VMs, more stringent mandates correspond to lower τ_j. There are no activity restrictions for vaccinated individuals in treated states or anyone in untreated states. A period income loss can be measured by $(1 - \tau_j)a_{j,t}$.

In line with our below empirical study, we divide the working population into three sectors: health, education, and state governments, $Q_t = Q_{1,t} + Q_{2,t} + Q_{3,t}$. Suppose that the mandate

stringency is the same for each sector, which means $\tau_j = \tau_k$ for all j in sector k. Define

$$\mathcal{T}_{k,t} = \tau_k Q_{k,t}, k = 1, 2, 3$$

the impact of sector mandate stringency τ_k on unvaccinated workers $Q_{k,t}$. Let $A_{k,t} = \sum_{j=1}^{Q_{k,t}} a_{j,t}$ represent the total activities. In each sector $k = 1, 2, 3$,

$$\sum_{j=1}^{Q_{k,t}} a_{j,t} - \sum_{j=1}^{Q_{k,t}} a_{j,t}\tau_j = A_{k,t}(1 - \tau_k) = A_{k,t}(1 - \frac{\mathcal{T}_{k,t}}{Q_{k,t}})$$

qualifies the economic loss due to the sector mandate stringency on the sector k. Importantly, the higher the number of workers in the sector, the more stringent the sector mandate. Moreover, at time t, assume that an unvaccinated individual in sector k faces a probability of infection $p(\tau_k a_{j,t}, Q_t)$ which increases with individual activity and the total unvaccinated population. Our model provides valuable insights into the economic outcomes within each sector and the overall health outcomes across sectors, thereby offering a practical tool for policymakers and health professionals to understand the potential impact of VMs.

Unvaccinated payoff:

Denote $u(\tau_k a_{j,t})$ as the period net gain of a healthy unvaccinated individual in sector k from engaging in activity. Assuming that an infected individual cannot engage in any activity. Let u_0 represent a constant utility of being infected, which can encompass the net benefit from social subsidies or health insurance for workers and the associated costs of being infected. Therefore, the payoff for an unvaccinated individual under VMs is given by

$$w_{ju} = p(\tau_k a_{j,t}, Q_t)u_0 + (1 - p(\tau_k a_{j,t}, Q_t))u(\tau_k a_{j,t}).$$

Vaccinated payoff: Let $\delta_j \in [0, \bar{\delta}]$ represent the cost of being vaccinated. This cost depends on individual characteristics such as age, gender, or other health risk factors. Assuming that vaccination effectively protects individuals from infection, resulting in no activity restrictions from VMs, the period payoff for a vaccinated individual j is given by

$$w_{jv} = u(a_{j,t}) - \delta_j.$$

Given the VMs, let $\varphi_{j,t}$ represent the probability that an individual gets vaccinated

$$\varphi_{j,t} = \begin{cases} 0 \text{ if } w_{ju} > w_{jv} \\ 1 \text{ if } w_{ju} \leq w_{jv} \end{cases}$$

The payoff of the individual j in sector k is given by

$$\begin{aligned} w(a_{j,t}) &= \varphi_{j,t} w_{jv} + (1 - \varphi_{j,t}) w_{ju} \\ &= \varphi_{j,t}[u(a_{j,t}) - \delta_j] + (1 - \varphi_{j,t})[p(\tau_k a_{j,t}, Q_t)u_0 + (1 - p(\tau_k a_{j,t}, Q_t))u(\tau a_{j,t})]. \quad (1) \end{aligned}$$

The evolution of the vaccinated population in sector k is presented by

$$V_{k,t} = V_{k,t-1} + \sum_{j=1}^{Q_{k,0}} \varphi_{j,t} = V_{k,t-1} + Y_{k,t} \tag{2}$$

where

$$Y_{k,t} = \sum_{j=1}^{Q_{k,0}} \varphi_{j,t}$$

represents the number of unvaccinated individuals in sector k who changed their behaviors to get vaccinated at time t. Note that $Y_{i,t} = \sum_{k=1}^{3} Y_{k,t}$ and $Y_t = \sum_{i=1}^{N} Y_{i,t}$ are the vaccine uptakes at date t for state i and for the whole country, respectively. These are the outcomes of interest for our empirical tests. The equation (2) indicates that $V_{k,t}$ is the accumulated number of vaccinated people over period $[0, t]$.

This theoretical framework provides robust support for our empirical tests of two key results. First, as demonstrated in Proposition 1.A in Appendix, there exists a specific value of sector mandate stringency $\hat{\tau}$ and a corresponding value of the number of unvaccinated workers $\hat{Q}_t$. An individual will certainly choose to get vaccinated if the sector mandate stringency is below this cutoff ($\tau_k < \hat{\tau}$). Moreover, as $\hat{Q}_t$ increases, $\hat{\tau}$ decreases, suggesting a considerable influence on sectors with a high number of workers. This result underpins empirical tests for the causal effects of state VMs on vaccination uptakes ($Y_{i,t}$).

Second, in examining the dynamic effects of vaccination mandates on vaccination rates within a continuous time framework, we categorize individuals into timely vaccinators, delayers, or non-vaccinators. Focusing on the sub-population Q_t, which includes non-vaccinators and delayers, we assume that each delayer possesses a cumulative probability distribution, $F(t)$, representing their perceived likelihood of infection by or before a specific time t. In particular, assume that the delayer plans to get vaccinated at time T. We examine the dynamic effects of VMs on vaccination rates, focusing on delayers who weigh the payoff of getting sick before vaccination, with probability $F(T)$, against staying healthy until T, with probability $1 - F(T)$. We demonstrate in Proposition 1.B in Appendix that there exists a certain time T^* such that if the announcement date of VMs occurs after this threshold, unvaccinated individuals will choose not to get vaccinated because the benefits of delaying outweigh the benefits of vaccination. Conversely, if the announcement date of VMs is earlier, they will choose to get vaccinated. These behaviors influence the overall dynamics of vaccination uptake in the population and highlight the need for empirical tests on dynamic effects.

Econometric model: We construct an econometric framework aligned with canonical definitions of causal effects, as discussed in Angrist and Pischke (2008). This model elucidates how VMs policies are made by local governments. Specifically, it offers testable mechanisms for heterogeneous and dynamic effects resulting from single and multiple treatments. Social planner's decisions in introducing VMs are informed by economic welfare. Let $\mathbf{a}_t = (a_{j,t})_{j=1,\ldots,J_i}$ and $\boldsymbol{\tau}_k = (\tau_1, \tau_2, \tau_3)$. The period welfare of unit i is defined as the sum of the unweighted payoff over

sector populations $J_{i,k}(k = 1, 2, 3)$:

$$U_i(\mathbf{a}_t, \boldsymbol{\tau}_k) = \sum_{j=1}^{J_{i,k}} \sum_{k=1}^{3} w(a_{j,t}, \tau_k)$$

where $w(a_{j,t}, \tau_k)$ is defined as in equation 1. Let ρ the discount rate, the social planner's problem of the unit i is to maximize welfare

$$\begin{aligned} W_i(\boldsymbol{\tau}_k) \quad = \quad & \max_{\mathbf{a}_t} \mathbb{E} \int_{t=0}^{T} e^{-\rho t} U_i(\mathbf{a}_t, \boldsymbol{\tau}_k) dt \\ & \text{s.t.} \\ & 0 \leq a_{j,t} \leq \bar{a}, \\ & dQ_{i,t} = -(\sum_{j=1}^{J_i} \varphi_{j,t}) dt, \\ & Q_{i0} \text{ is given.} \end{aligned}$$

The value function which satisfies the Bellman equation:

$$\rho W_i(\boldsymbol{\tau}_k) = \max_{\mathbf{a}_t} \{U_i(\mathbf{a}_t, \boldsymbol{\tau}_k) + \frac{\mathbb{E}_t dW_i(\boldsymbol{\tau}_k)}{dt}\}$$

In case of no VM for sector k, we set $\tau_k = 1$.

Single and two-treatment settings: Suppose there exists only one sector, k_1, among the three sectors such that the sector mandate stringency is strictly less than 1 ($\tau_{k_1} < 1$). The following indicator function expresses the *single treatment* status:

$$D_{it}^{k_1} = \mathbb{1}[W_i(\tau_{k_1} < 1, \tau_{k_2} = \tau_{k_3} = 1) \geq W_i(\tau_{k_1} = \tau_{k_2} = \tau_{k_3} = 1)]$$

Similarly, if there exist k_1 and k_2 such that $\tau_{k_1} < 1$, $\tau_{k_2} < 1$, and $\tau_{k_3} = 1$, the *two treatment* status is defined as:

$$D_{it}^{k_1,k_2} = \mathbb{1}[W_i(\tau_{k_1} < 1, \tau_{k_2} < 1, \tau_{k_3} = 1) \geq W_i(\tau_{k_1} = \tau_{k_2} = \tau_{k_3} = 1)]$$

Our event study specification follows recent work (Sun and Abraham, 2021; Callaway and Anna, 2021; Borusyak et al., 2021; de Chaisemartin and D'Haultfoeuille, 2022) which emphasize the importance of allowing for flexibility in treatment effects across different treated units i and time t. The vaccination outcomes, represented as changes in the number of vaccinated people, Y_{it}, can be divided into N units and T periods. Denote the binary treatments $D_{it}^k, k = 1, 2, 3$, being mandates imposed on workers working in the state governments, health, and education sectors in unit i and at some date t. $D_{it}^k = 1$ implies that there exists a date t at which unit i has received a mandate. Date t can differs from each D_{it}^k. Suppose that treatment adoption is staggered and is an absorbing unit, e.g., if $D_{it}^k = 1$, then $D_{is}^k = 1$ for all $s > t$. Define D_{it} as the treatment status of *any mandate* where $D_{it} = 1$ if there exist t and k such that $D_{it}^k = 1$. Otherwise, $D_{it} = 0$.

We first consider the effect of a single treatment. Denote $Y_{it}(0)$ and $Y_{it}(1)$ the potential outcomes when $D_{it} = 1$ and $D_{it} = 0$, respectively. Under the stable unit treatment value assumption (SUTVA),the outcome in unit i is independent of the treatment status of other units, the observable outcome is given by $Y_{it} = D_{it}Y_{it}(1) + (1 - D_{it})Y_{it}(0)$.

The standard SUTVA assumption applied at the state level suggests that the potential outcome Y_{it} of state i is unrelated to the treatment status of other states. However, at the individual level, this assumption may be violated because the treatment status of one individuals could influence other individuals' treatment j, which is the probability of $\varphi_{j,t} = 1$ when the payoff $w_{jv} > w_{ju}$. According to the payoff function in 1, this treatment status depends on $Q_{i,t}$, representing the population of workers across all three sectors within unit i. Additionally, there is potential for spillover effects across states, where the treatment status of individuals from units other than i may influence the vaccination decisions of individuals in unit i. This is particularly relevant if we assume that p is a function of the total unvaccinated population in the entire country[3].

Denote X_{it} a vector of covariates. The mortality rate captures the disutility of infection. As VMs restrict the economic activity including working, we use unemployment rate as the second covariate. Moreover, as the number of vaccinated persons is accumulated, we use cumulative vaccination as the last covariate in our empirical specifications. We assume that the unobservable factors are included in unit fixed effect, α_i, and period fixed effect μ_t.

Denote G as the treated units whose treatment status has changed at least once: $G = \{i \mid \exists t^*$ such that $D_{it^*-1} = 0, D_{it^*} = 1\}$. Denote $\theta_{it} = Y_{it}(1) - Y_{it}(0)$, as the average treatment effect on the treated (ATT) :

$$ATT = \mathbb{E}\{\theta_{it} | D_{it} = 1, \forall i \in G, \forall t\}$$

Under parallel trends and no-anticipation assumptions, the parameter θ_{it} is identified. A common two ways fixed effect (TWFE) model for representing this situation is a panel model with dynamic policy

$$Y_{it} = \alpha_i + \mu_t + \sum_{s=-S}^{\bar{S}} \beta_{is} D_{it,s} + X'_{it}\gamma + \varepsilon_{it}$$

where scalar ε_{it} represents an unobserved shock that is not correlated with the mandate policy. With these dynamic effects, the outcome at time t can be directly affected by the value of the policy at most S periods before t and at most $\bar{S}$ periods after t. We are interested in $\theta_{it}(t^*) = \sum_{s \geq 0} \beta_{is} \mathbb{1}(t - t^* = s)$ with $i \in G$ and $D_{it^*-1} = 0, D_{it} = 1 \ \forall t \geq t^*$. In cases of several treatments, let us denote the binary treatments $D^1_{g,t}, D^2_{g,t}$ and $D^3_{g,t}$ of the state governments, health, and education mandates in group g, which consists of states that introduced a VM at the same date, and at period t. We are interested in the effects of VMs on the observed outcome

[3]We would like to thank the referee for pointing out the potential violation of the SUTVA at the individual level.

$Y_{g,t} = Y_{g,t}(D^1_{g,t}, D^2_{g,t}, D^3_{g,t})$.

3.2 Empirical specification

We undertook two approaches to designing the treatment. In the first approach, we consider the announcement of VMs in any sector to be the single treatment introduced in a staggered fashion (Design 1). In this design, we are keen on the overall effect of the general VM announcement and are not keen to explore potential differences in the effect of differing VMs for differing population sub-groups. In the second approach, we consider the announcements of VMs in differing sectors to be different treatments. With the second approach, we examine the effects of the first and subsequent treatments, which can provide useful information for public debates when governments have policy interventions with respect to differing population sub-groups. [4] In terms of the treatment variables, it is common to consider treatments to be binary variables as in recent literature (Karaivanov et al., 2022; Mills and Rüttenauer, 2021).

Data show that the VM announced for education workers happened only after mandates were announced for health and state government workers. Also, some states observed two VMs announced on the same day. Specifically, there is only one state with three mandates that were announced on separate days. Due to the insufficient number of observations with distinct dates for three treatments, we adopted two-treatment designs despite there being three mandates. In sum, we have three two-treatment designs (e.g. Designs 2 - 4). Design 2 considers the VM announcement for state government workers as the first treatment and the second treatment being the VM announcement for health or education workers. Design 3 has the VM announcement for health workers being the first treatment and the VM announcements for state government or education workers being the second treatment. Design 4 considers the first treatment an announcement for any VM for health or state workers, and the second treatment is the announcement of VM for education workers. Across Designs 3 and 4, the first treatment happens strictly before the second treatment.

4 Data

Outcome variable: Following Karaivanov et al. (2022); Mills and Rüttenauer (2021), we use the natural logarithm of fist-dose and second-dose vaccinations as the outcome variables in our main analysis. For robustness check, we also use first-dose (second-dose) vaccination numbers per one million population. To reduce the potential influence of day-of-the-week effects, anticipation effects, and reporting errors, we use a seven-day moving average.

Policy treatment variables: In our empirical models, announcements of VMs are the treatment variables. As noted earlier, we consider VM announcements for workers in the three sectors: state governments, health, and education. In the single treatment design (i.e. Design 1), the treatment variable takes a value of one if the state has any VM announcement, regardless of which sector the VM is for. In the two-treatment design (i.e. Designs 2 - 4), two binary variables are used to capture the first and second treatments. These treatment variables are defined as a

[4] Mills and Rüttenauer (2021) used the single treatment approach in their main analysis but provided secondary analysis using the multiple treatment approach in their cross-country analysis.

set of dummies (D_{ij}) that take the value of one when State i announces the VM j.

Covariates: As often done in DID analysis, we include state-fixed effects and date-fixed effects. As argued in our theoretical model, we include the death rate, cumulative two-dose vaccination numbers, and monthly unemployment rate. Following Karaivanov et al. (2022), we use the death rate per one million as this variable can inform a person's risk assessment or decision to get vaccinated.[5] This variable also captures the pandemic trajectory at the state level, which is considered to be an important factor affecting overall vaccination rates within the population (Mills and Rüttenauer, 2021). The cumulative vaccination variable is used to capture other unobserved factors that affect individuals' decisions to vaccinate through social interactions. The cumulative vaccination variable also captures pre-existing levels of vaccine uptake or hesitancy, which is considered to be important (Mills and Rüttenauer, 2021). Empirically, with these covariates, the assumption of conditional parallel trends becomes more plausible than an unconditional parallel trends assumption as discussed in Callaway and Anna (2021). In fact, Callaway and Anna (2021) argue that ignoring the presence of covariate-specific trends could result in biased estimates of the treatment effect.

Data sources: Data are from various publicly available sources. State-level COVID-19 death and vaccination data are from the Centre for Disease Prevention and Control. Data for unemployment are available from the US Bureau of Labor Statistics. Announcement dates of VMs are from The National Academy for State Health Policy website (NASHP, 2021). We also checked the accuracy of these VM announcements on the official websites of each state government.

Time periods and samples: Data cover the period of 62 days starting from 1^{st} July to 31^{th} August 2021. This start date is 26 days prior to the first VM announcement. We also extend the start date to 1 May 2021 in our robustness analysis. We believe that the supply of vaccines has no constraints during these periods. As shown in Figure 1, this period exhibits a down-turn trend several weeks after first-dose vaccination reached its peak around mid-April 2021. The decline of vaccination rates reached a trough in early June 2021 and increased from mid-July 2021, when the first VM was introduced. Thus, we focus on the period one month before and after the first vaccine mandates to estimate their effects. Extending the control period (e.g., to March 2021) will create a downward bias on the effects of VMs because the control period covers the peak vaccination period. Due to the possibility of contamination from state mandates, we purposely did not consider dates after the announcement of VM by President Biden. Also, apart from the original sample, which we used for Designs 1 and 4, we generated Sample 2 for Design 2 and Sample 3 for Design 3. Sample 2 excluded observations with state employee VMs were first introduced while Sample 3 excluded observations with healthcare VMs were first introduced. Descriptive statistics for key variables are in Table 1.

Figure 2 presents an overview of the timing of the treatment across 50 states in the USA. This figure shows variations in the date of VM announcements across states. Also, there are a large number of states without any vaccination mandate.

[5]Reported cases or hospitalisation can be used. However, these two variables are very highly correlated with the death rate, so only the death rate is used to avoid multicollinearity.

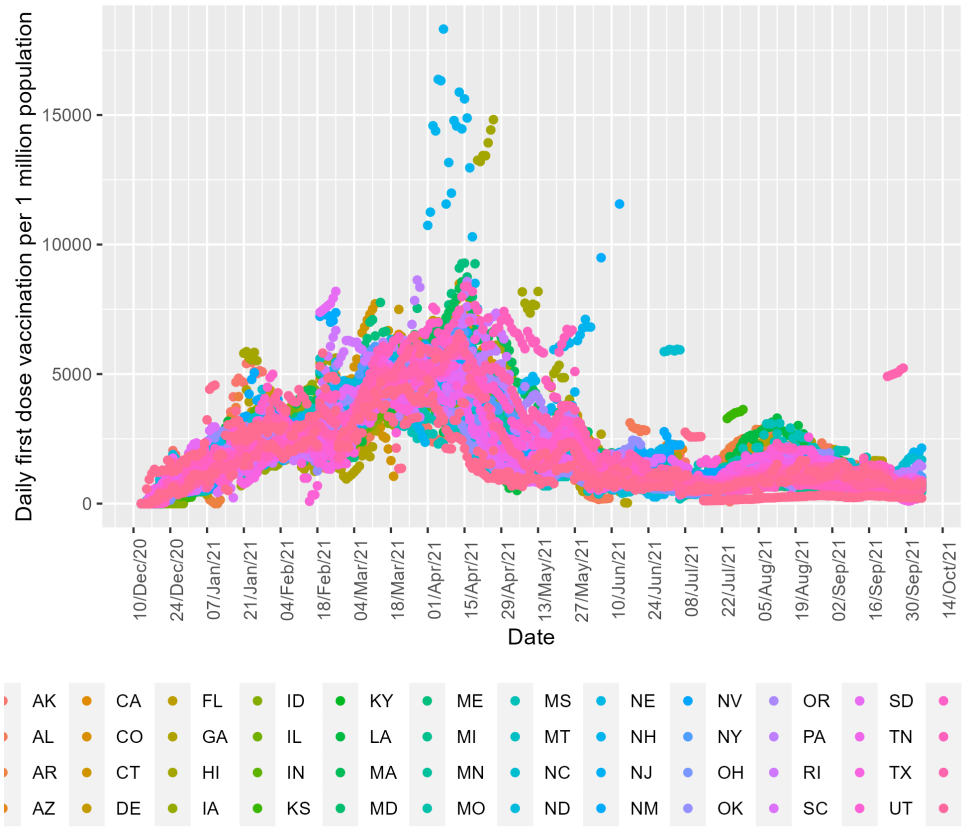


Figure 1. First-dose vaccination uptakes by dates and states

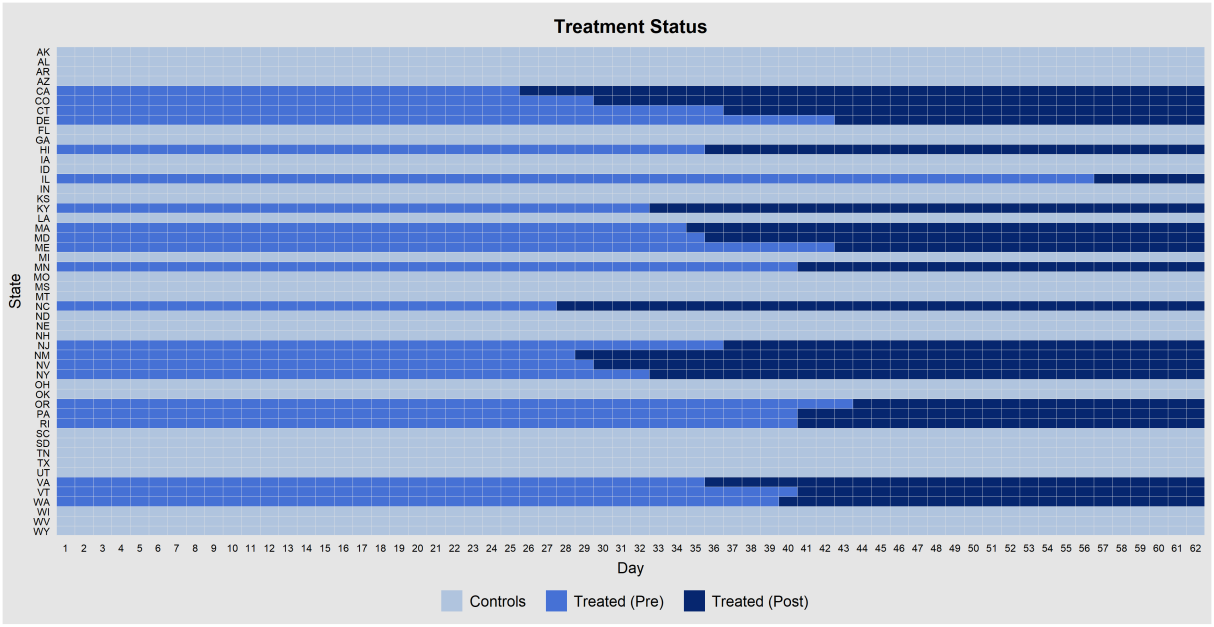


Figure 2. Treatment status (any VM announcement) across 50 US states

Descriptive analysis: Table 1 presents descriptive statistics characterising attributes of the two groups of states: control and treatment. The control group includes all states that had no VM during the surveyed period, while the treatment group includes all states that have at least one VM announcement regardless of which sector among the three sectors.

Not only do these two groups of states exhibit differences in the key variables used in our empirical models, also they differ in many other social, economic, and political dimensions including macroeconomic indicator such as unemployment rate. As discussed in the theoretical framework, we use the percentage of workers in each sector (i.e., government, education, health) as a proxy for the VM stringency. It can be seen that health workers have the largest share (from 4.05% to 4.75%). In addition, health workers face frequent contact with high-risk individuals (i.e., COVID patients). Thus, VMs affecting health workers incur the highest costs if failing to comply. Between the two remaining sectors, education workers have a slightly larger share (from 2.6% to 2.9% vs from 2.1% to 2.3%) than government workers. Also, education workers have more frequent contact with large number of individuals (i.e., students and their parents), hence failing to comply with the vaccine mandate for this group will also incur a huge cost to the society.

Table 1. Descriptive Statistics

Selected variables Mean (SD)	Control (N=1674)	Treatment (N=1426)	Total (N=3100)	p-value
First dose				
(Moving average)	7115.2 (10272.2)	8345.8 (10881.0)	7681.0 (10572.71)	0.01
First dose (daily)	7167.0 (12451.4)	8138.65 (10994.62)	7614.0 (11811.7)	< 0.001
Cumulative Two-Dose				
Vaccination	2506918.6 (2939616.3)	4056654.6 (4351135.3)	3219797.2 (3737299.6)	< 0.001
Death rate (%)	2.3 (2.8)	1.1 (1.2)	1.72 (2.28)	< 0.001
Unemployment				
rate (%)	4.3 (1.21)	5.8 (1.4)	4.98 (1.48)	< 0.001
Percentage of state government				
workers				
Min	2.14	2.34	2.14	
Max	5.96	4.56	5.96	
Mean	3.24 (0.88)	3.14 (0.61)	3.2 (0.58)	0.672
Percentage of health workers				
Min	4.74	4.05	4.05	
Max	7.98	8.62	8.62	
Mean	6.08 (0.81)	6.5 (1.12)	6.27 (0.98)	0.134
Percentage of education workers				
Min	2.91	2.64	2.64	
Max	4.84	5.55	5.55	
Mean	3.82 (0.54)	4.02 (0.65)	3.91 (0.78)	0.217
Percentage of workers in 3 sectors				
Min	10.84	9.26	9.26	
Max	16.71	16.85	16.85	
Mean	13.13 (1.65)	13.66 (1.66)	13.38 (1.66)	0.263

5 Results and Discussion

5.1 Two-Way Fixed Effects Estimators

We first estimate the average effect of any mandate (Design 1) on the logarithm of the number of vaccinations with the following regression specification:

$$log(Y_{it}) = \lambda_i + \gamma_t + \beta D_{it} + \alpha X_{it} + \epsilon_{it} \tag{3}$$

where Y_{it} is the vaccination rate in state i at date t, D_{it} is a dummy for whether the state had a VM announcement, X_{it} is a set of control variables, λ_i is state fixed effects and γ_t is time fixed effects.

The coefficient of interest, β, measures the difference in the average outcome between states with and without VMs (henceforth, treated and untreated states). Using the logarithm of vaccination rates, the estimated parameters can be interpreted as the percentage changes in vaccination, on average, after the introduction of VMs, under several key assumptions (i.e., parallel trend, no anticipation, homogeneous effects) discussed earlier.

Table 2 presents the results from the standard TWFE estimators for the four treatment designs. We also estimated the pooled model in which all three VM announcements were included individually in the form of dummies. Designs 2 and 3 have restricted samples (i.e. Sample 2 and 3), which are smaller than Sample 1 used for the pooled model and Design 4.

Table 2. Standard Two-Way Fixed Effect Estimates

Covariates	Pooled	Design 1	Design2	Design 3	Design 4
VM on health sector	0.036			0.058*	
	(0.026)			(0.030)	
VM on state government sector	−0.122***		−0.097*		
	(0.030)		(0.052)		
VM on education sector	0.126***				0.093***
	(0.037)				(0.035)
Any VM announcement		−0.025			
		(0.020)			
VM on education or health sector			0.057		
			(0.054)		
VM on education or state government sector				−0.136***	
				(0.034)	
VM on state government or health sector					−0.041*
					(0.021)
Log of cumulative completed doses	2.170***	2.065***	2.459***	2.191***	2.114***
	(0.417)	(0.418)	(0.455)	(0.435)	(0.418)
Death rate	0.019***	0.019***	0.015***	0.019***	0.019***
	(0.004)	(0.004)	(0.005)	(0.005)	(0.004)
Unemployment rate	−0.227**	−0.180**	−0.176*	−0.287***	−0.144*
	(0.088)	(0.084)	(0.101)	(0.097)	(0.085)
Observations	3100	3100	2418	2790	3100
Adjusted R^2	0.418	0.414	0.407	0.396	0.415
F Statistic	34.894***	35.416***	26.721***	29.365***	35.055***
State FEs	x	x	x	x	x
Date FEs	x	x	x	x	x
Note:	Standard errors are in parentheses, *p<0.1; **p<0.05; ***p<0.01				

In the pooled model, the announcements of VMs for state government and education workers are significant at 1% but not the VM for health workers. Notably, these results suggest a significant negative relationship between the announcement of VM for state government workers and the logarithm of first-dose vaccination while a positive relationship between VM announcement for education workers and the outcome variable.

Design 1 shows a negative but statistically insignificant relationship between the announcement

of any VM and the logarithm of first-dose vaccination. In Design 2, results show a statistically significant and negative effect of the first treatment, while the second treatment is statistically insignificant. Designs 3 and 4, however, present opposite results: the first treatment has a positive and the second treatment has a negative effect on the outcome, but both parameters are statistically significant. Across all designs, results from standard TWFE estimators suggest inconsistent relationships between the VM announcements and the vaccination outcome.

With respect to the three covariates, standard TWFE results show consistent associations with the outcome across all designs. A positive relationship between the cumulative two-dose vaccination and the first-dose vaccination is reasonable, suggesting that first-dose uptake during the observed period increases with the overall rates of two-dose vaccination of the state-level population. The number of deaths shows a statistically positive relationship with the vaccination outcome in all treatment designs, while the unemployment variable has a negative coefficient but no statistical significance. So, if using these TWFE results, one might report the insignificance and the negative effects of VM announcements. Recent literature shows that the standard TWFE estimates might be biased and not robust in the presence of heterogeneous treatment effects or the presence of the control group, including units that already received treatments or become treated (i.e., switch the treatment status)(de Chaisemartin and D'Haultfœuille, 2020b; Borusyak et al., 2021; Goodman-Bacon and Marcus, 2020; Sun and Abraham, 2021). The next section examines if these issues are present in our empirical TWFE estimators.

5.2 Test for heterogeneous treatment effects

5.2.1 Single treatment

Recent literature has introduced several diagnostic methods for detecting heterogeneity in treatment effects when treatment timing is staggered. In particular, we employed Jakiela's diagnostic tests (Jakiela, 2021) to assess whether the assumption of homogeneous treatment effects holds. This approach examines the mathematical relationship between the residuals of the outcome variable and the residuals of the treatment variable. The test leverages the Frisch-Waugh-Lovell theorem, which states that an OLS estimate of β is the weighted sum of the outcome values, with weights being the ratios of the treatment residuals and the weighted sum of their squares. The treatment residuals are derived from regressing the treatment indicator on location (e.g., states) fixed effects and time fixed effects.

If treatment effects are homogeneous across time and locations, the residualized outcome (adjusted for location and time fixed effects) should have a linear relationship with the residualized treatment (adjusted for location and time fixed effects). Essentially, if there is no difference in the regression slopes for treated and untreated populations, indicating a linear relationship across these groups, there is no evidence of heterogeneity. However, as illustrated in Figure 3, the regression lines for treated and untreated groups diverge — the slope is negative for untreated observations but positive for treated ones.[6] The difference in slopes between treated

[6]Following Jakiela (2021), we estimated the treatment effect on the outcome using a two-way fixed effects model. In this approach, the OLS estimate of β is derived from the residuals, which represent deviations from the average treatment value that are not explained by fixed effects. To conduct the test, we regressed the outcome variable on time and location effects, with the remaining variation considered as the residualized outcome. Similarly, we calculated the residualized treatment by removing the effects of location and time.

and untreated observations in a regression of residualized outcome on residualized treatment provides evidence of heterogeneity. This suggests a violation of the treatment effect homogeneity assumption.

5.2.2 Multiple treatments

The estimated parameter β in a two-treatment setting is the average of two weight components: 1) outcome changes in the two periods where the first treatment started ($D^1_{g,t} = 1$) and the second treatment was at the observed value; and 2) outcome changes in the two periods where the second treatment started ($D^2_{g,t} = 1$) while keeping the first treatment at 0 (de Chaisemartin and D'Haultfœuille, 2020b). The weights of the first component sum to one, and that of the second component sums to zero. If the treatment effects are homogeneous over time, the second component would be zero. The second component would differ from zero if the treatment effects are heterogeneous.

Table 3 presents the decomposition of the coefficients estimated from the standard TWFE estimators into the effects with the positive and negative weights, estimated using the *twowayfeweights* STATA package by de Chaisemartin and D'Haultfœuille (2020b). Note that decompositions for the second treatment are only available in Designs 2 - 4. The first (second) treatment refers to any announcement of VMs observed first (second) as described earlier in the research design section.

With respect to the two-treatment model, there exist negative weights for the first and the second treatments, and the number of negative weights varies across research designs. For the first treatment, there were only 17 effects with negative weights (nearly 6% of the number of effects) in Design 2. These percentage numbers are higher in Design 3 (84 effects or 19%) and Design 4 (54 effects or 9%). The mean values of the positive and negative weights in the first treatment statistically differ from zero in all designs. There are slightly more numbers of negative weights observed in the second treatment.For example, 9.5% of the effects of the second treatment are negative in Design 2, while around 34% of the effects are negative in Design 3 and 49% in Design 4. Also note that weights on the effects of the second treatment sum to zero because our research design represents a special case of two treatments. See de Chaisemartin and D'Haultfœuille (2020a) for more details on this.

5.3 Heterogeneous Treatment Effects Estimators

The previous results show that there is likely heterogeneity in treatment effects (de Chaisemartin and D'Haultfœuille, 2020b). Therefore, the estimates from the TWFE estimators are not robust. In this section, we present the results of de Chaisemartin and D'Haultfœuille (2020a)'s estimator for the single treatment setting (Design 1) and de Chaisemartin and D'Haultfœuille (2020b)'s estimator for multiple treatment settings (Designs 2 - 4).

5.3.1 Single-Treatment Settings

Main results: As shown in Table 4, in the setting of a single treatment (i.e. Design 1), the average treatment effect is positive and statistically significant at 1%. This new result is opposite to the result of the standard TWFE estimator. The literature indicates that the standard TWFE estimator is not reliable in the context of staggered treatment designs and

Table 3. Decomposition of weights in TWFE treatment effects

Compositions	Design 1	Design 2	Design 3	Design 4
First component				
Sum of positive weights	1	1.1516	1.0843	1.0286
Sum of negative weights	0	-0.1516	-0.0843	-0.0286
Mean positive weights	0.0018***	0.0042***	0.003***	0.002***
Mean negative weights	-	–0.0089***	-0.001***	-0.0005***
ATTs receiving effects	569	294	443	569
ATTs receiving positive effects	569	277	359	515
ATTs receiving negative effects	0	17	84	54
Second component				
Sum of positive weights	-	0.355	0.1536	0.0286
Sum of negative weights	-	-0.355	-0.1536	-0.0286
ATTs receiving effects	-	253	272	110
ATTs receiving positive effects	-	229	180	56
ATTs receiving negative effects	-	24	92	54
Observations	3100	2418	2790	3100
Note:				*p<0.1; **p<0.05; ***p<0.01

in the presence of heterogeneity in the treatment effects (de Chaisemartin and D'Haultfœuille, 2020a; Goodman-Bacon and Marcus, 2020; Sun and Abraham, 2021) while the new estimator of de Chaisemartin and D'Haultfoeuille (2022) provides more robust and less biased estimates. Based on these, we believe that the results reported here provide more accurate estimates of the effect of the VM announcement in the USA.

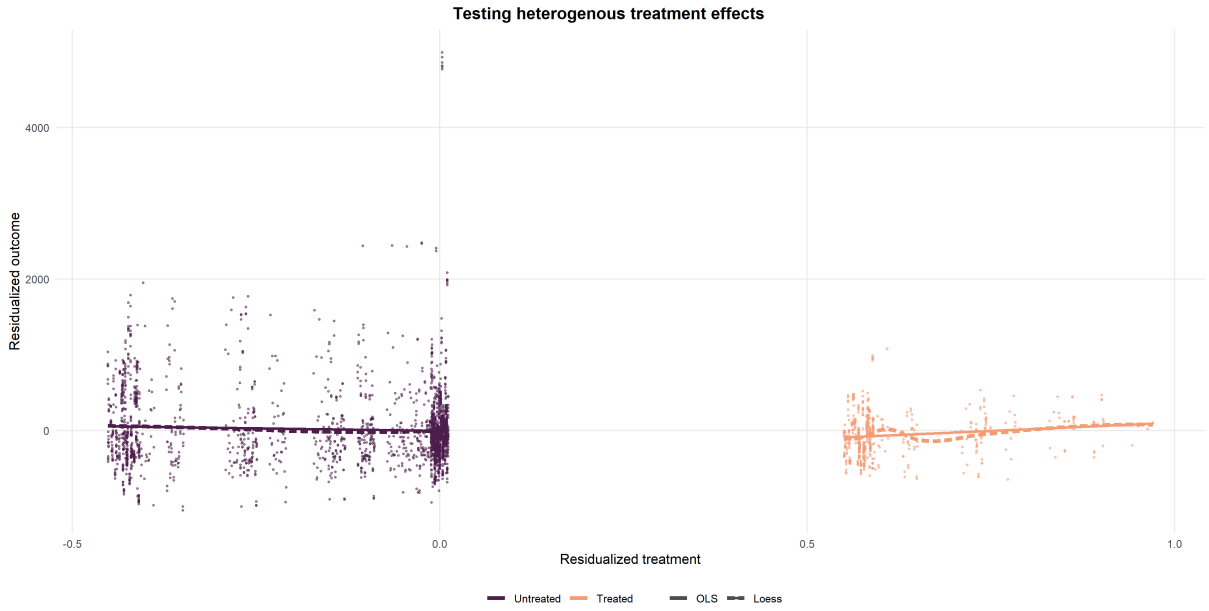
With the estimated value of the average treatment effect of 0.187, we interpret that, on average, the VM announcement leads to an average increase of 18.7 log points or 20.6%[7] in seven-day-average first-dose vaccinations. This magnitude of the average effect is smaller than an average increase of nearly 60% reported for Canada in Karaivanov et al. (2022), probably because of much stronger and broader coverage of the vaccination passports in Canada. The dynamics of this single treatment effect are presented in Figure 4, which shows that the effect remains positive many days after the treatment.

Robustness Analysis: We conducted robustness analysis by estimating three alternative specifications. The first specification extends the starting data from 1 July to 1 May 2021. The second specification uses the level values of the weekly first-dose vaccination as the outcome variable. The third specification extends the second specification with the starting data from 1 May 2021. Results of these three specifications are in Table 4. Consistently, the estimated average treatment effects in these alternative specifications are positive and statistically significant.

In comparison with our benchmark model, the first alternative specification reports a bigger magnitude of the average treatment effect. Specifically, in this robustness specification, it was estimated that the VM announcement leads to an average increase of 32.7 log points or 38.7% in first-dose vaccinations. The results estimated from the second alternative specification suggest that the announcement of the VM in any of the three sectors is found to be associated with an average increase of more than 363 extra first-dose vaccination per one million of the population.

[7]Since the outcome variable is measured in the unit of natural log, the estimated parameter β is interpreted as $(e^{\beta} - 1)\%$ changes in outcome

Figure 3. Different direction in treated and untreated groups



This average effect increases to more than 763.5 first-dose vaccinations per one million in the third specification.

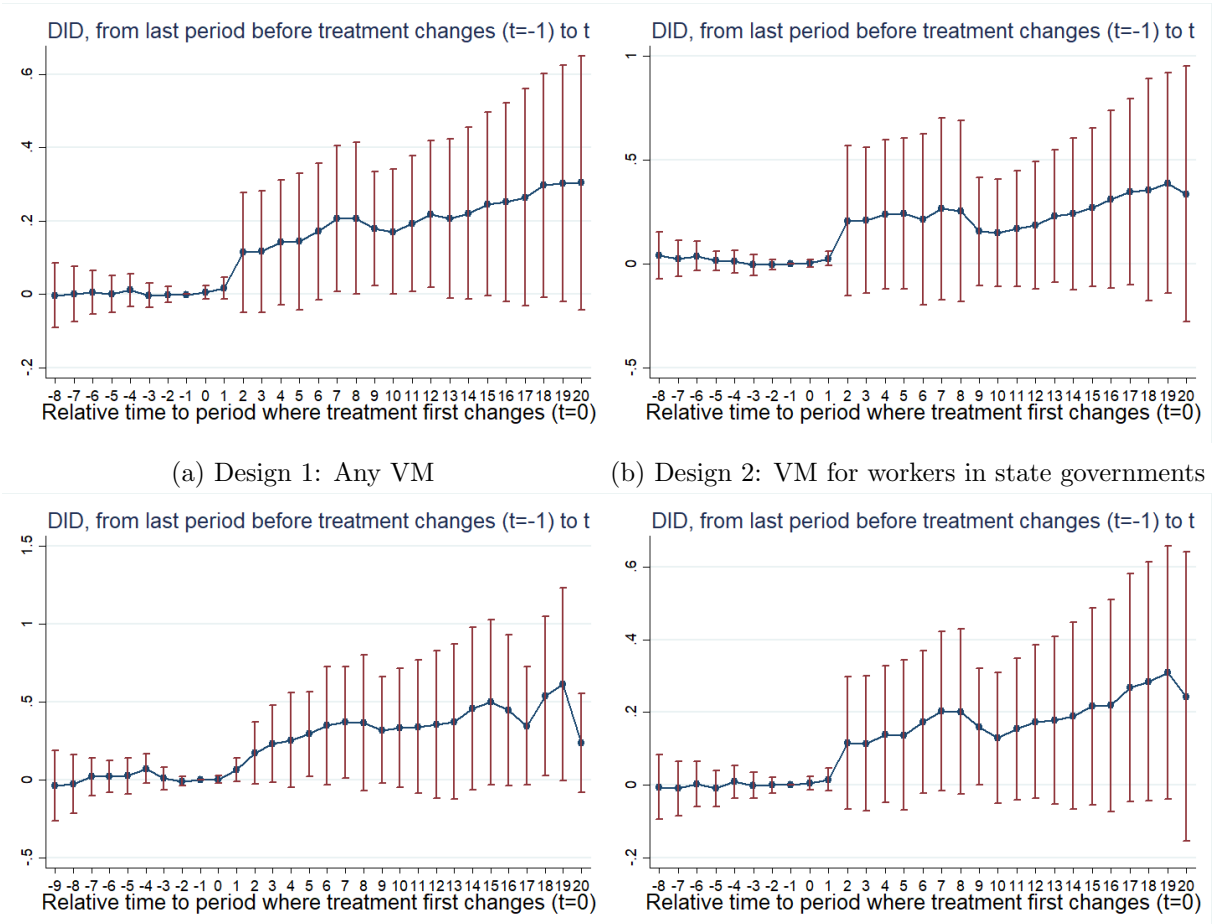
Table 4. Average treatment effect in a single-treatment setting (Design 1)

Average Treatment Effects	Coef.	Std.Err
Main result	0.187***	0.094
Robustness test 1: Extended sample to 1^{st} May 2021	0.327***	0.104
Robustness test 2: Use the level of first-dose vaccination as outcome	363.328***	145.2
Robustness test 3: Test 2 with extended sample to 1^{st} May 2021	763.518***	193.235
Note:	*p<0.1; **p<0.05; ***p<0.01	

5.3.2 Two-Treatment Settings

Main results: Designs 2 - 4 have two treatments, and estimated results of these treatment designs are in Table 5. Design 2's results show that the second treatment (i.e., VM for either health or education workers) significantly increases the first-dose vaccination by an amount of 8.6 log points or nearly 9%. Design 3 implies that the first treatment (i.e., VMs for health workers) could increase first-dose vaccinations by 26 log points or 29.7%. Design 4 shows that the first treatment (i.e., VMs in state government or health sectors) could increase the first-dose vaccination by 16.6 log points or 18.05%. All of these average treatment effects are statistically significant. As shown in Figure 5, the dynamics of the second treatment confirm that only Design 2 shows positive and significant effects of the second treatment (VM in the health or education sector), but the effect diminished after nine days.[8]Taking all these together, the results suggested

[8]Note that the confidence interval for Design 2 in Figure 5a was not visible because the values of standard errors were so small to the extent that the lower and upper 95% confidence interval values were not differentiable at three decimal places. We estimated the effects of the second treatment by using a sub-sample of states where the first treatment was already implemented. For Design 2, this is limited to only two observations per period, and hence, the estimated standard errors using bootstraps were too small.



(a) Design 1: Any VM

(b) Design 2: VM for workers in state governments

(c) Design 3: VM for health workers

(d) Design 4: VM for workers in state governments or healthcare facilities

Figure 4. Dynamics of the first treatment

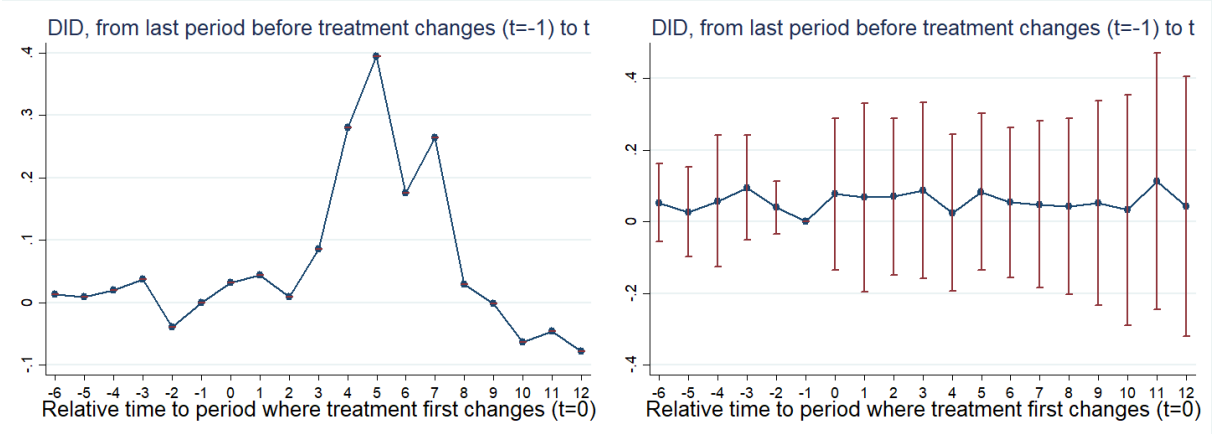
that the announcement of VM for workers in health or state government sectors had statistically significant causal impacts on the first-dose vaccination. This finding is consistent with our Proposition because healthcare workers represent the largest share of government workforce and they have frequent contact with high-risk individual (i.e., vaccine mandate for healthcare workers have the highest stringent index).

Direct comparisons between Design 1 (single-treatment) and Designs 2 or 3 (two-treatment) are not possible due to differing data samples. It is more reasonable to compare Design 1 with Design 4. While Design 1 reports an average treatment effect of 20.7%, there is no further information on the relative importance of the announcement of VM in differing sectors. In contrast, the results of Design 4 are in favours of the arguments that the announcement of VM for workers in state governments or in the health sector in fact have causal effects on increased first-dose vaccinations as the second treatment (VM announcement for workers in the education sector) is not statistically significant.

The positive effects of multiple vaccine mandates suggest that with the increase in stringent

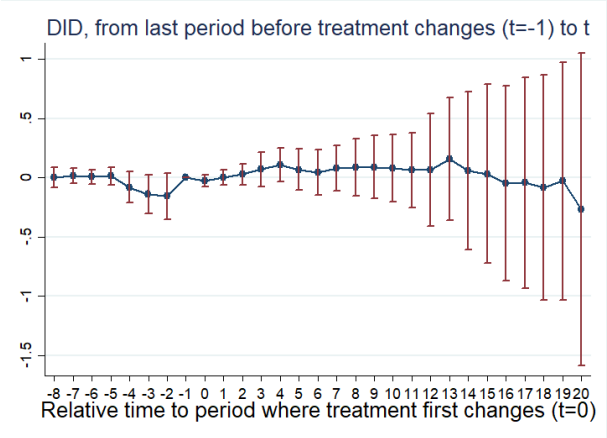
Table 5. Average treatment effects in two-treatment settings

Average Treatment Effects	First treatment		Second treatment	
	Coef.	Std.Err	Coef.	Std.Err
Design 2: State, then Health or Education	0.206	0.153	0.086***	<0.001
Design 3: Health, then State or Education	0.260*	0.142	0.061	0.118
Design 4: State or Health, then Education	0.166*	0.098	0.048	0.13
Note:				*p<0.1; **p<0.05; ***p<0.01



(a) Design 2: VM for workers in health or education sector

(b) Design 3: VM for workers in state governments or education



(c) Design 4: VM for education workers

Figure 5. Dynamics of the second treatment

Robustness Analysis: Table 6 presents the results of the three alternative specifications as mentioned earlier for the purpose of robustness check. When we extended the data to the starting date of 1 May 2021, Design 2 reported statistically significant and positive effects of both the first and second treatments. The magnitudes of these two average treatments in this first alternative specification were also larger. Similarly, Design 4 reports a larger average treatment effect for the two treatments. Results in Design 3 show no statistical significance.

In the remaining two alternative specifications, the results show consistent positive effects of VMs across three differing treatment designs (Designs 2 - 4). In the second alternative specification using the level value in the vaccination outcome, the first treatment is statistically significant in Designs 3 and 4 with a positive value of 383.916 and 349.763 for the average

treatment effect, respectively. It suggests that the VM announcement for state government workers leads to an increase of nearly 350 first-dose vaccinations in one million people, while the VM announcement for the health sector, on average, leads to an additional 384 first-dose vaccinations per million. In the third alternative specification, the first treatment is statistically significant in all designs, while the second treatment is significant in Design 2. Using Design 2's results, one can interpret that the announcement of VM for state government workers leads to nearly 659 first-dose vaccinations per one million, while additionally, VM in the health or education sector increased further by an amount of more than 136 first-dose per one million population.

Table 6. Average treatment effect in two-treatment settings: robustness analysis

Average Treatment Effects	First treatment		Second treatment	
	Coef.	Std.Err	Coef.	Std.Err
Specification 1: Extended sample starting from 1st May 2021				
Design 2: State, then Health or Education	0.266***	0.103	0.088***	0.023
Design 3: Health, then State or Education	0.160	0.106	-0.007	0.09
Design 4: State or Health, then Education	0.301***	0.111	0.088	0.138
Specification 2: Using the level of first-dose vaccination as outcome				
Design 2: State, then Health or Education	323.57	222.302	145.697***	0.000
Design 3: Health, then State or Education	383.916*	212.492	86.262	147.863
	349.763***	156.325	33.192	141.015
Specification 3: Spec 2 and extended sample starting from 1st May 2021				
Design 2: State, then Health or Education	658.66***	201.118	136.432***	20.3
Design 3: Health, then State or Education	337.120*	170.496	-23.121	101.272
Design 4: State or Health, then Education	726.802***	214.549	71.103	132.502

Note: *p<0.1; **p<0.05; ***p<0.01

5.4 Second-Dose Vaccinations

Table 7 provides results for the average treatment effect on the second-dose vaccination outcomes.[9] The benchmark model uses the logarithm of weekly second-dose vaccination as the outcome variable for the sample starting from 1 May to 31 August 2021. Three alternative specifications vary in the time period as well as the outcome variable, as mentioned earlier.

Overall results show minimal statistical significance of the average treatment effect on the second-dose vaccination outcome, except for the second treatment in Design 2. Using our benchmark specification, the announcement of VM for workers in health or education sectors after the VM has been announced for state government workers is found to increase the second dose vaccination by 37.5 log points, which is equivalent to nearly 46%. Across alternative specifications for robustness check, the estimated impact of this treatment is consistent: being positive and statistically significant.

5.5 The plausibility of assumptions

There are three main assumptions for the single treatment setting: SUTAV, no-anticipation, and parallel trend. In the setting of multiple treatments, additional assumptions, as in de Chaisemartin and D'Haultfœuille (2020b), are required. Specific to our research designs, due to the two treatments, the announcements of VMs at the state level, assumptions regarding the

[9]We included one additional covariate: the first-dose vaccination three weeks earlier.

Table 7. Second-dose treatment effects

Average Treatment Effects	First treatment		Second treatment	
	Coef.	Std.Err	Coef.	Std.Err
Main specification: Logarithm of weekly two-dose vaccination as outcome.				
Design 1: Single Treatment with any VM	-0.083	0.076		
Design 2: State, then Health or Education	-0.164	0.142	0.375***	0.05
Design 3: Health, then State or Education	0.078	0.106	0.040	0.13
Design 4: State or Health, then Education	-0.042	0.073	0.013	0.072
Alternative 1: Main Spec and extended sample starting from 1st May.				
Design 1: Single Treatment with any VM	-0.1***	0.046		
Design 2: State, then Health or Education	-0.086	0.071	0.375***	0.05
Design 3: Health, then State or Education	-0.020	0.073	0.040	0.13
Design 4: State or Health, then Education	-0.078	0.044	0.013	0.072
Alternative 2: Using two-dose vaccination as outcome.				
Design 1: Single Treatment with any VM	64.938	77.204		
Design 2: State, then Health or Education	-73.937	83.972	323.688*	190.672
Design 3: Health, then State or Education	193.592*	118.049	24.489	121.641
Design 4: State or Health, then Education	111.348	86.535	31.792	63.74
Alternative 3: Alternative 2 and extended sample starting from 1st May.				
Design 1: Single Treatment with any VM	22.338	47.306		
Design 2: State, then Health or Education	61.514	48.424	329.955***	42.132
Design 3: Health, then State or Education	97.675	106.212	23.941	129
Design 4: State or Health, then Education	33.368	52.423	-8.979	75.246

Note: *p<0.1; **p<0.05; ***p<0.01

independent groups, balanced panel group, and sharp design are satisfied. Also, we impose a strong exogeneity assumption which means that, for any state, the mean of vaccination evolution is independent of the state's treatments. [10]

Generally, the SUTVA assumption implies that the outcome of one unit is not affected by the treatment status of any other unit. Applied to our research designs, this assumption negates the spillover effects at the state level (i.e. spillover effects from one state to another). We believe this assumption is reasonable because of the nature of the treatment design in our empirical studies. Our treatments include state VMs announced for health and education sectors and state governments and the majority of workers in these sectors are located within the treated states. It is possible that there could be a very small number of people working in these sectors but residing in neighbouring untreated states, which leads to the small and negligible possibility of state-level spill-over effects. However, we acknowledge the presence of spillover effects at the individual level. People may get vaccinated due to social interactions, for example, through the two channels of herding and social norms discussed in Agranov et al. (2021).[11]. We captured this issue using covariates as discussed in the Data and Variables section. Huber and Steinmayr (2021) also provided discussions on relaxing this assumption.

In the presence of the anticipation effect, individuals in the sector affected by the vaccine mandate might get vaccinated just before the mandate was introduced because they had anticipated the introduction of VMs. The data used in our empirical work as shown in Figures 4 and 5 indicate no sudden increases in vaccination outcomes just prior to the introduction of VMs in all designs, indicating no evidence of the anticipation effects.

The parallel trends assumption intuitively states that the average outcome for the treated and untreated populations would have evolved in parallel if treatments had not occurred. Thus, the existence of any treatment trend in outcomes suggests that a trend should be controlled for in the analysis. Figures 4 and 5 clearly show that vaccination outcomes in the pre-treatment periods are not significantly different from zero; hence, there is no clear pre-treatment trend. Additionally, according to Roth (2022), the absence of a pre-treatment trend is not necessarily a powerful indicator for no violation of the parallel trend assumption. The author proposed a diagnosis tool to examine the potential bias. A key indicator of this tool is the Bayes factor, which converges to unity if the pretend test has no power to distinguish between biased and unbiased designs. A Bayes factor close to zero or power of at least 80%, as a minimum requirement in power analysis (Cohen, 2013), indicates slight bias in the pre-trend test. Our results show that the Bayes factors ware close to zero in most of our designs. A couple of exceptions are the first treatment in Design 2 (Bayes factor=0.23) and the Second treatment in Design 3 (Bayes factor = 0.22).

[10] Strong exogeneity assumption: For all $g \in \{1, ..., G\}$, $E(Y_{g,t}(\mathbf{0}) - Y_{g,t-1}(\mathbf{0})|D_{g,1}, ..., D_{g,T}) = E(Y_{g,t}(\mathbf{0}) - Y_{g,t-1}(\mathbf{0}))$where $\mathbf{0} = (0, 0, 0)$.

[11] Agranov et al. (2021) discuss three channels through which vaccination decisions by individuals are affected by the behaviours of others: herding (if others vaccinate, it is probably because it is safe), social norms (this is what we do as a society to protect others, and free-riding (widespread vaccination might reduce individual incentives to vaccine). Using experimental data from 1,500 USA citizens, the authors conclude that social norms are a key driver of vaccination behaviour.

6 Conclusion

Mandates for COVID-19 vaccines have received significant public debates across the USA during the COVID-19 pandemic from both political and ethical perspectives. Rigorous evidence of the effects of VMs on vaccination rates would assist those public debates, especially to prepare for future pandemics. Given the lack of rigorous research for the USA, the present paper aims to provide one of robust and unbiased estimates of the causal effects of VM announcements for workers working in health, education, and state governments on vaccination uptake across 50 states.

We show that the VM announcements have heterogeneous effects; hence, estimates from standard two-way fixed effects (TWFE) difference-in-differences estimators are not robust in both single-treatment and multiple-treatment approaches. Using the recently developed TWFE estimators of de Chaisemartin and D'Haultfœuille (2020a,b); de Chaisemartin and D'Haultfoeuille (2022), which account for multiple treatments with heterogeneous and dynamic effects, our empirical analysis delivers several important new findings.

We undertook two approaches in treatment designs. In the setting of a single treatment, states that announced VMs for workers in any of the three sectors are found to increase on average 20.6% first-dose uptake for the period from 1 July to 31 August 2021. Extending our data to an earlier starting date of 1 May, results show larger effects, being 38.7%. When using the level of vaccination as the outcome variable, our results show that the VM announcement has caused more than 763.5 extra people to vaccinate their first dose per 1 million state population during the period from 1 May to 31 August. All these results are statistically significant.

In two-treatment settings, our results give more insights into what announcement of VM has more effects on vaccination outcomes. Consistently across Designs 2 - 4, our results suggest that the announcement of VM for workers in health or state government sectors had statistically significant causal effects on the first-dose vaccination. We also investigated the effects of VM announcements on the second-dose vaccination outcomes measured by the weekly second-dose vaccination or the logarithm of weekly two-dose vaccination in four differing designs. Consistently, the results across different specifications in Design 2 showed a statistically significant causal effect of the second VM announcement for health or education sectors after the first VM announced for state workers. The effect of the first VM was only statistically significant in Design 3 using the outcome measured by the two-dose vaccination. As expected, our results are sensitive to the choice of time periods. The magnitudes of estimated average treatment effects increase when data is extended to 1 May 2021, which could be due to vaccination reductions, which increase over time in states that do not make any VM announcements.

This paper is subject to several limitations that require caution in interpreting our empirical results but also poses opportunities for future research. First, data at the county level could be collected and analysed to provide more disaggregated analysis. Second, our analysis considers VMs as binary treatment variables, while one could consider VMs as ordinal or continuous data. Third, our research does not address the separation of individual-level treatment effects and potential spillover effects within states as discussed in the literature (Manski, 2017; Huber and

Steinmayr, 2021).

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APPENDIX

Proposition 1.A

There exists a specific value of sector mandate stringency and a corresponding value of the accumulated number of unvaccinated individuals such that individuals will certainly choose to get vaccinated if the sector mandate stringency is below this cutoff.

Proof

This proposition builds upon the epidemiological ISI model (Kermack and McKendrick, 1927). The population (N_t) is divided in two classes: susceptible, healthy and who can catch the disease (S_t) and infective, those infected and capable of transmitting the disease (I_t). with $S_t + I_t = N_t$.

$$\begin{aligned} dS_t/dt &= -dS_t - \alpha S_t I_t/N_t \\ dI_t/dt &= \alpha S_t I_t/N_t - \gamma I_t \\ & S_t, I_t, N_t \geq 0, \forall t; \\ & S_0, I_0, N_0 > 0 \text{ with } N_0 = S_0 + I_0. \end{aligned}$$

The key epidemiology variables are the contact rate, α, i.e. the average number of adequate contacts of a person to catch the disease per unit time and γ, the recovery rate from the disease.

For any unit and any sector k, because the infective population I_t in the ISI model is a fraction of Q_t, it is reasonable to assume that the probability of infection is

$$p(\tau_k a_{j,t}, Q_t) = \frac{\alpha \tau_k a_{j,t} Q_t}{J}.$$

We omit the individual index j and sector index k for simplicity. Suppose that the net gain $u(a) = \theta a$ where $\theta > 0$ is the utility gain of exercising one more unit of activity. By assumption, the vaccinated individual can fully exercise activity then

$$w_v = \theta \bar{a} - \delta.$$

With this specification, at period t, the payoff of an individual is given by

$$\begin{aligned} w(a_t) &= \varphi_t(\theta \bar{a} - \delta) + (1 - \varphi_t)[p(\tau a_t, Q_t)u_0 + (1 - p(\tau a_t, Q_t))u(\tau a_t)] \\ &= \varphi_t A + B \end{aligned}$$

where

$$\begin{aligned} A &= u(\bar{a}) - \delta - p(\tau a_t, Q_t)u_0 + (1 - p(\tau a_t, Q_t))u(\tau a_t) \\ B &= p(\tau a_t, Q_t)u_0 + (1 - p(\tau a_t, Q_t))u(\tau a_t). \end{aligned} \tag{4}$$

The individual maximizes the payoff over activity a_t. Let $w'(a_t) = 0$ we get

$$\frac{\partial p(\tau a_t, Q_t)u_0}{\partial a_t} + (1 - p(\tau a_t, Q_t))u'(\tau a_t) - \frac{\partial p(\tau a_t, Q_t)}{\partial a_t}u(\tau a_t) = 0 \tag{5}$$

Note that

$$\frac{\partial p(\tau a_t, Q_t)}{\partial a_t}a_t = p(\tau a_t, Q_t). \tag{6}$$

Substituting (5) and (6) into (4) we get

$$A = u(\bar{a}) - \delta - p\theta\tau a_t^2.$$

If $\tau < \left[\frac{(u(\bar{a})-\delta)J}{a_t^2\alpha\theta Q_t}\right]^{\frac{1}{2}} := \hat{\tau}$, then $A > 0$, leading to $\varphi_t = 1$ as individuals maximize their payoff and $w(a_t)$ increases with φ_t. This establishes a threshold such that $Y_{k,t}$ increases when sector mandate stringency is below $\hat{\tau}$. Note that $\hat{\tau}$ is small when Q is large, indicating a significant impact on the sector with a large number of workers.

Proposition 1.B

There exists a certain time such that if the announcement date of VMs occurs after this threshold, the delayer will opt not to get vaccinated because the benefits of delaying outweigh those of vaccination. Conversely, if the announcement date of VMs is earlier, they will choose to get vaccinated.

Proof

Assume that the delayer plans to get vaccinated at time T who weigh the payoff of getting sick before vaccination, with probability $F(T)$, against staying healthy until T, with probability $1 - F(T)$. Let ρ the discount rate, the expected present utility of that delayer is given by

$$\begin{aligned} w_d(T) &= \int_0^T e^{-\rho t}w_u(\tau a_t)dF(t) + (1 - F(T)\int_T^\infty e^{-\rho t}u_0 dt \\ &= \int_0^T e^{-\rho t}w_u(\tau a_t)dF(t) + (1 - F(T))\frac{u_0}{\rho e^{\rho T}}. \end{aligned}$$

Delayers decide to get vaccinated at time T if

$$w_d(T) \leq w_v.$$

We will derive a solution for a time threshold and establish the condition for delayers to choose vaccination or not. Suppose that $F(t)$ follows an exponential distribution, where the cumulative distribution function (CDF) is given by $F(t) = 1 - e^{-\lambda t}$ where λ is the infection rate awareness parameter. Additionally, assume that within the short period $[0, T]$, the optimal activity and infection rate of one individual approximately constant over time. Therefore, the

expected present payoff $w_d(T)$ is given by:

$$w_d(T) = D - De^{-(\rho+\lambda)T} + \frac{u_0 e^{-(\rho+\lambda)T}}{\rho}$$

where $D = pu_0 + (1-p)u(\tau a)$.

To find T, we set $w_d(T) \leq w_v = \theta\bar{a} - \delta$:

$$\frac{D - (\theta\bar{a} - \delta)}{D - \frac{u_0}{\rho}} \leq e^{-(\rho+\lambda)T.}$$

Taking the natural logarithm on both sides:

$$\ln\left(\frac{D - (\theta\bar{a} - \delta)}{D - \frac{u_0}{\rho}}\right) \leq -(\rho+\lambda)T$$

Thus, the optimal delay time before an individual decides to get vaccinated, is given by:

$$T \leq \frac{1}{\rho+\lambda} \ln\left(\frac{D - \frac{u_0}{\rho}}{D - (\theta\bar{a} - \delta)}\right) := T^*$$

Therefore, if the announcement date of VM occurs after the threshold T^*, unvaccinated individuals will opt not to get vaccinated because the benefits of delaying outweigh those of vaccination. Conversely, if the announcement date of VM is earlier, they will choose to get vaccinated.