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Did Covid-19-Healthcare Worker Vaccination Mandates Achieve Their Goal? Econometric Evidence from U.S. States

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ABSTRACT
This paper examines whether state COVID-19 vaccination mandates for healthcare workers reduced broader pandemic burdens in the United States. Using a monthly state panel from 2021–2022 (42 states; 1,008 state-month observations), we study log COVID-19 cases and log hospital admissions, controlling for unemployment and mask mandates, while absorbing time-invariant state factors and common monthly shocks. Difference-in-Differences estimates in a static two-way fixed effects framework do not show a robust post-adoption decline: once state and month fixed effects are included, the mandate coefficient becomes small and statistically indistinguishable from zero. Event Study estimates similarly show no immediate break at adoption and no stable, significant post-period decline for cases, while patterns for admissions suggest that treated states were already on worsening trajectories, which is consistent with endogenous policy timing and strong outcome persistence. Hence, a Dynamic Difference Generalized Method of Moments model was employed with lagged dependent variables and internal instruments that explicitly accounts for epidemic momentum. In preferred specifications, mandates are associated with statistically significant reductions in both outcomes: for cases, $\beta = -0.862$ (SE = 0.255, $p < 0.01$), and for hospital admissions, $\beta = -2.883$ (SE = 1.274, $p < 0.05$). The results support the policy relevance of healthcare worker mandates as a tool for lowering transmission and easing hospital strain once epidemic persistence is modeled. Methodologically, the paper underscores that in wave-driven settings where policy adoption responds to worsening conditions, static two-way fixed effects models may be best interpreted as diagnostics, and credible inference may require dynamic panel approaches that explicitly address persistence and endogenous timing.

KEYWORDS
Difference-in-Differences, Event Study, Difference Generalized Method of Moments, Panel data, Public health, Epidemiology, Regulation.

1. Introduction

Healthcare workers stand at the frontlines of every public health crisis, but during the COVID-19 pandemic, they also faced some of the highest risks of infection. Their role is paradoxical: while they treat patients and save lives, they can also become unwitting carriers of disease, spreading it within healthcare facilities and beyond. Vaccination mandates for healthcare workers (HCW vaccination mandates) became

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a central policy tool in the fight against COVID-19, designed to protect both these workers and the vulnerable populations they serve.

Early evidence by the Centers for Disease Control and Prevention (CDC) [5] showed that COVID-19 vaccines substantially reduced severe disease and transmission, and healthcare workers were identified as a priority group for vaccination due to their increased risk of exposure and their critical role in patient care. CDC [6] emphasized the importance of protecting healthcare workers to maintain healthcare system resilience and prevent nosocomial (hospital-acquired) infections. Following the Emergency Use Authorization of the Pfizer-BioNTech and Moderna vaccines in December 2020, amended by U.S. Food and Drug Administration [18], healthcare workers were among the first to receive vaccinations due to their high-risk exposure and critical role in patient care. However, vaccine hesitancy persisted among certain regions and demographic groups. By July 2021, only 67% of U.S. healthcare workers were fully vaccinated, leaving a significant portion of this essential workforce vulnerable to infection (see C. J. Peterson et at. [11]). The emergence of the highly transmissible Delta variant further exacerbated concerns, as it led to increased breakthrough infections and heightened the risk of outbreaks within healthcare facilities (see C. M. Brown [10]). In response to these challenges, HCW vaccination mandates were introduced as a targeted intervention to mitigate the spread of COVID-19 and protect both patients and staff in mid 2021.

The divergence in HCW vaccination state policies created a natural experiment for evaluating the impact of mandates: some states relied on voluntary vaccination campaigns, some enforced strict requirements with limited exemptions and some went in the opposite extreme, banning vaccination requirements. Our research addresses a critical question: How did state-level COVID-19 vaccination mandates for healthcare workers impact the general population's COVID-19 cases and hospitalizations?

2. Literature Review

Existing evidence on COVID-19 vaccine mandates is methodologically dominated by two-way fixed effects (TWFE) Difference-in-Differences and Event Study designs that exploit staggered policy adoption or discrete implementation dates. Across settings, the existing literature documents two recurring patterns: (1) mandates can increase vaccination uptake, and (2) in certain localized contexts they are associated with reductions in COVID-19 cases and deaths. However, the empirical scope is often sector-specific (e.g., nursing homes, colleges, municipal workforces) or geographically narrow (single-city or single-state), while most studies do not explicitly confront the challenges of policy endogeneity and outcome persistence that are central in statewide pandemic panels.

A first strand focuses on the relationship between health-care-sector mandates and vaccination rate, typically using individual- or facility-level data and staggered policy timing. Using repeated cross-sectional biweekly microdata from the U.S. Household Pulse Survey (May 26-Oct 11, 2021), Y. Wang et al. [19] estimate staggered DID event studies (Sun & Abraham) by comparing mandate vs non-mandate states before and after mandate announcements, with robustness checks against TWFE and Callaway-Sant'Anna estimators. They find that state HCW mandates are associated with short-run increases in vaccination uptake, with effects concentrated in more stringent regimes (no test-out option) and among younger HCWs. Complementing this uptake-focused evidence at the facility level, B. E. McGarry et al. [3] use a facility-week panel from the National Healthcare Safety Network nursing home module (June-Nov 2021) and estimate Event Study models with nursing home and calendar-week fixed effects, distinguishing mandates with versus without test-out options. They document increases in staff vaccination coverage, especially under stricter mandates, without detectable increases in reported staffing shortages, and show heterogeneity by county political leaning. Taken together, these studies provide strong evidence on the "first-stage" of HCW-oriented mandates (vaccination uptake), but they largely stop short of measuring downstream, population-level COVID-19 burdens at the state level.

A second strand evaluates mandates in other high-contact institutions, showing that vaccination requirements can generate measurable public health benefits in local communities. Riley K. Acton et al. [14] link college vaccine mandates to surrounding county-level outcomes using an Event Study DID centered on semester start dates and a county-week panel of public health outcomes, with extensive controls for regional shocks, baseline vaccination patterns, and political environment, plus placebo tests. Their estimates imply reductions in cases and deaths during the early fall 2021 semester window. In a related workplace-policy context, B. L. Rubenstein et al. [4] study NYC municipal employees using administrative vaccination records matched to workforce data (May-Dec 2021) and compare trends against other NYC residents using phase-based slope comparisons in a Poisson regression framework; they find that vaccination increases are most pronounced after the test-out option is removed. Finally, R. Juarez et al. [15]analyze a county-level vaccine-or-test "passport" policy in Hawaii (Honolulu and Maui) using DID models on county/day and business/day data, linking the policy to changes in cases per 100,000, vaccination increases, and economic activity (foot traffic), illustrating both public health gains and short-run economic tradeoffs in a single-state setting.

Despite these contributions, two gaps remain for understanding HCW mandates as a nationwide, state-level policy regime. First, the evidence on reductions in cases/deaths is strongest in contexts where the treated population and spillovers are geographically localized (college counties, a single city workforce, or a single state), whereas HCW mandates operate at the state level with heterogeneous implementation, exemptions, and enforcement that may interact with statewide epidemiological dynamics. Second, the dominant DID/Event Study approach, while valuable for diagnostics and transparent counterfactual comparisons, does not resolve the identification challenges posed by high persistence in COVID outcomes and policy endogeneity when mandate adoption responds to contemporaneous or anticipated waves.

3. Data

This study merges three data sources to construct a monthly state-level panel spanning January 2021-December 2022, covering the pre-mandate period, the concentrated adoption wave of HCW mandates in mid-to-late 2021, and the subsequent post-period. Policy exposure is drawn from the CDC's state-level vaccine mandate and mandate-restriction datasets ([7, 8]), which identify the timing and scope of healthcare worker (HCW) mandates and prohibition policies. Health outcomes are measured using monthly state COVID-19 reported cases published by USAFacts [16] and hospital admissions published by U.S Government Department of Health and Human Services [17], and are analyzed in logs. To account for concurrent economic and policy variation, the panel includes monthly unemployment rates from the Bureau of Labor Statistics [2] and statewide mask mandates from the COVID-19 U.S. State Policy

Database [9]. All datasets are harmonized and merged at the state–month level to form a balanced panel.

Policy coding and sample construction follow standard practice in the mandate-evaluation literature. We code HCW mandates using the first enforceable compliance date (rather than announcement/signing) and treat hospital-based personnel and long-term-care staff as a unified HCW policy target for comparability across heterogeneous state orders. Because the federal CMS vaccination rule created a nationwide "floor" in late 2021/early 2022, it is absorbed by month fixed effects; identification therefore relies on cross-state variation in state-level mandate timing and stringency around the 2021 adoption wave. To address heterogeneity in enforcement, the baseline treatment indicator includes both strict vaccination requirements and vaccine-or-test policies, and robustness checks add an intensity flag for test-out provisions. Prohibition policies are incorporated as secondary controls/robustness checks given their mutual exclusivity with mandates. Finally, to avoid leverage from atypical timing (late adoption) or early rescissions that do not align with the main adoption window, we exclude mandate states with substantial timing mismatches, yielding a final sample of 42 states [1] over 24 months (1,008 state-month observations).

Table 1. Summary Statistics by Treatment Status

	No Mandate (Control)					Mandate (Treatment)				
Variables	N	Mean	SD	Min	Max	N	Mean	SD	Min	Max
Log(Admissions)	816	7.369	1.324	3.045	11.07	192	7.896	1.329	4.454	11.02
Log(Cases)	816	13.33	1.145	9.390	15.89	192	13.80	1.348	10.58	16.20

Table 1 reports summary statistics by mandate status. Averaged across state-month observations, outcomes are higher in the mandate group than in the non-mandate group (log(cases): 13.80 vs. 13.33; log(admissions): 7.896 vs. 7.369), but these differences should not be interpreted as evidence of mandate effects.

Table 2. Treatment/Control by Pre/Post-Treatment Summary for Outcome Variables

Group	Period	Log(Cases)	Log(Admissions)
No Mandate (Control)	Before Mandate	12.774	7.382
	After Mandate	13.659	7.361
Mandate (Treatment)	Before Mandate	13.180	7.846
	After Mandate	14.166	7.925

In a closer look, Table 2 presents a two-by-two comparison of treatment and control states before and after the implementation of healthcare worker (HCW) vaccination mandates. Specifically, mandate states exhibit higher average levels of both log cases and log hospital admissions than control states in the pre-treatment period. This reflects systematic differences in baseline pandemic intensity across states, consistent with the fact that mandates were more likely to be adopted in states experiencing higher COVID-19 burdens. Both treatment and control states experienced increases in log case counts from the pre- to post-treatment period. However, the increase appears larger in mandate states than in control states in raw means. A similar pattern is observed for hospital admissions in mandate states.

[1] U.S. has 50 states.

4. Methodology

To estimate the causal impact of healthcare worker (HCW) COVID-19 vaccination mandates on state-level COVID-19 outcomes, we first employ a difference-in-differences (DiD) framework complemented by an Event Study specification. The DiD model captures the average treatment effect of mandate adoption by comparing within-state changes in COVID-19 outcomes before and after the mandate to contemporaneous changes in states without such policies. The Event Study design extends this analysis by exploiting temporal variation to examine the dynamic effects of mandates over time. To address concerns around serial correlation and persistence over time, we then estimate dynamic panel models using Generalized Method of Moments (GMM), which explicitly model outcome persistence by including lagged dependent variables while using internal instruments to mitigate bias from serial correlation and unobserved time-varying shocks.

4.1. *Difference-in-Differences*

The Difference-in-Differences (DiD) approach estimates the average treatment effect of HCW vaccination mandates by comparing changes in outcomes over time between states that implemented mandates (treatment group) and states that did not (control group). This approach assumes that, in the absence of the mandates, the trends in COVID-19 outcomes would have been parallel between the two groups. The DiD model specification for is as follows:

$$\text{COVIDMeasure}_{s,t} = \beta_0 + \beta_1 \text{Mandate}_{s,t} + \beta_2 \text{Unemployment}_{s,t} + \beta_3 \text{Mask}_{s,t} + \gamma_s + \lambda_t + \varepsilon_{s,t}. \tag{1}$$

In Equation 1, $\text{COVIDMeasure}_{s,t}$ represents the COVID-19 outcome of interest, either the log of cases or the log of hospital admissions, in state s and month t. The variable $\text{Mandate}_{s,t}$ is a binary indicator equal to one if a healthcare worker vaccination mandate is in effect in state s at time t, and zero otherwise. The controls $\text{Unemployment}_{s,t}$ and $\text{Mask}_{s,t}$ account for state-month unemployment rates and statewide mask mandates, respectively. The term γ_s represents state fixed effects, which control for time-invariant state characteristics such as baseline healthcare infrastructure or population density. The term λ_t represents time fixed effects, which account for shocks common to all states in a given month, such as national COVID-19 surges. The error term $\varepsilon_{s,t}$ captures unobserved factors that vary across states and time. The coefficient β_1 is the key parameter of interest and represents the average effect of healthcare worker vaccination mandates on the outcome variable. A negative and statistically significant β_1 would indicate that mandates are associated with reductions in COVID-19 cases or hospital admissions.

4.2. *Event Study*

To complement the DiD analysis, an event study model is used to capture the dynamic effects of the mandates over time. While DiD provides the average treatment effect, the event study framework allows for a more nuanced understanding of when the effects of the mandates emerged and whether they persisted over time. The event study model includes a series of binary indicators that represent the number of periods which, in this case, months before and after the implementation of the mandates. The specification is as follows:

$$\begin{aligned} \text{COVIDMeasure}_{s,t} = \beta_0 + &\sum_{k=-10}^{13} \beta_k \mathbb{I}(t - t_s^* = k) \times \text{Mandate}_s \\ &+ \beta_2 \text{Unemployment}_{s,t} + \beta_3 \text{Mask}_{s,t} + \gamma_s + \lambda_t + \varepsilon_{s,t}, \qquad (k \neq -1). \end{aligned} \tag{2}$$

In Equation 2, $\mathbb{I}(t - t_s^* = k)$ are indicator variables for the event time k, measuring the time relative to the implementation of the vaccination mandate t_s^* in state s. The event window ranges from $k = -10$ (ten months before mandate implementation) to $k = 13$ (thirteen months after). $k = -1$ is the reference period, representing the month before the mandate's implementation. This choice avoids capturing anticipatory effects close to the policy rollout while serving as a clean baseline for comparison.

The coefficients β_k measure the difference in outcomes between treatment and control states at each event time k, relative to the reference period $k = -1$. The pre-treatment period ($k = -10$ to $k = -1$) tests the parallel trends assumption. If the pre-treatment coefficients (β_k, $-9 \leq k < 0$) are close to zero and not statistically significant, this supports the parallel trends assumption. The post-treatment period ($k = 0$ to $k = +13$) captures the dynamic effects of mandates, showing how COVID-19 case and hospitalization rates evolve after the policy is implemented. The post-treatment coefficients (β_k, $0 \leq k \leq 13$) reveal the impact of healthcare worker vaccination mandates over time. Positive or negative values indicate the direction and magnitude of the policy's effect on COVID-19 outcomes.

4.3. *Difference Generalized Method of Moments*

To address the limitations of the static Two-Way Fixed Effects framework, which assumes that current health outcomes are determined solely by contemporaneous factors, this study transitions to a dynamic panel approach using the Difference Generalized Method of Moments (GMM). GMM is particularly advantageous in this context because it relies on "population moment conditions" derived from the underlying theory rather than requiring a fully specified probability distribution, thereby making the estimator robust to misspecification regarding the exact distributional form of viral transmission (see P. Zsohar [13] and A. R. Hall [1]). Furthermore, analyzing the impact of mandates within a longitudinal data involves complex correlations between time-dependent covariates and health outcomes (E. Vazquez-Arreola et al. [12]). Following the framework described by Vazquez-Arreola et al., GMM facilitates the inclusion of lagged dependent variables to control for state dependence while simultaneously handling time-dependent covariates to parse out "immediate" versus "delayed" policy effects. This approach effectively instruments for endogeneity using the data's own history, correcting for the bias that arises when including lagged outcomes in standard fixed-effects models.

We now employ the following revised model, given in Equation 3:

$$\begin{aligned}\text{COVIDMeasure}_{s,t} = {} & \beta_0 + \alpha_1 \text{COVIDMeasure}_{s,t-1} + \beta_1 \text{Mandate}_{s,t} + \alpha_2 \text{Unemployment}_{s,t-1} \\ & + \beta_3 \text{Mask}_{s,t} + \lambda_t + \varepsilon_{s,t}.\end{aligned} \tag{3}$$

Here, we add the lagged dependent variable as a right-hand side variable which not only absorbs the state fixed effects but also helps deal with endogeneity concerns and potential reverse causality between COVID measures and covariates. Additionally, we use lagged unemployment rate as it's much more plausible that last month's unemployment affects this month's cases/admissions, while being less directly tied to this month's unobserved COVID shock. As such, current unemployment can plausibly move with the same shocks that move COVID outcomes (e.g., a surge increases absenteeism, local restrictions, behavioral changes, reporting disruptions). That makes contemporaneous unemployment a risky control because it can be correlated with the current error term. A high and significant coefficient on lagged variables indicate the presumed high persistence.

5. Results

This section reports results in three steps that move from a static policy-evaluation design to a dynamic panel framework. We begin with two-way fixed effects Difference-in-Differences estimates for both outcomes, presenting alternative specifications that vary the set of controls and the definition of mandate exposure. We then turn to an Event Study design to assess the pre-trends (parallel trends) assumption underlying DiD and to trace the evolution of outcomes in event time following mandate enforcement. Finally, we present our main estimates from Difference GMM, which explicitly models outcome persistence and mitigates bias from endogenous policy timing through internal instruments.

5.1. *Difference-in-Differences*

COVID-19 Cases

Table 3 presents the estimates from the Difference-in-Differences (DiD) specifications evaluating the impact of state-level healthcare worker (HCW) vaccine mandates on log of COVID-19 cases. We start with a simple panel regression that relates outcomes to the HCW mandate indicator without any fixed effects (Column 1). We then move to our main DiD-style specifications that include state fixed effects and month fixed effects, with standard errors clustered at the state level (Column 2-4). This progression is helpful because it shows how much of the raw mandate–outcome relationship is driven by baseline differences across states and common time shocks, rather than a within-state change after adoption.

In the baseline specification (column 1), the mandate indicator is positive and highly significant (≈ 0.984 log points); that is, $(e^{0.984} - 1) \times 100\% \approx 168\%$ increase in cases. However, this is not a causal estimate as it likely reflects selection into treatment while suffering from omitted variable bias. In other words, states that adopted mandates look like they had higher case levels around the time they adopted, which is consistent

Table 3. DiD Estimates for Log of COVID-19 Cases

VARIABLES	(1) Log(Cases)	(2) Log(Cases)	(3) Log(Cases)	(4) Log(Cases)[4]
Vaccine Mandate	0.984***	0.101	0.0381	–
	(0.0693)	(0.0711)	(0.0908)	
Stricter Vaccine Mandate				0.0311
				(0.0928)
Vaccine Ban				-0.0266
				(0.0331)
Mask			-0.111***	-0.112***
			(0.0399)	(0.0406)
Unemployment Rate			-0.106*	-0.102*
			(0.0606)	(0.0602)
Constant	13.30***	12.61***	13.28***	13.27***
	(0.170)	(0.0354)	(0.347)	(0.345)
Time FE	No	Yes	Yes	Yes
State FE	No	Yes	Yes	Yes
Observations	1,008	1,008	1,008	1,008
R-squared		0.936	0.947	0.947
Number of Groups	42	42	42	42

Standard errors in parentheses
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

[4] In column 4, I code policy exposure as a three-category regime variable: 0 = neither mandate nor ban (reference), 1 = mandate, and 2 = ban. This specification reflects the fact that mandate and ban regimes are mutually exclusive. As a result, the "Stricter Vaccine Mandate" coefficient in column 4 is identified relative to states/months with no policy (neither mandate nor ban), whereas the "Vaccine Mandate" coefficient in columns (1)–(3) compares mandate observations to all non-mandate observations, which include both ban-regime and no-policy states/months.

with the idea that mandates were implemented in response to worsening conditions. At the same time, other controls for demographic, health, and employment are omitted, which the estimate is not taking into account.

Once we include state and month fixed effects in column 2, the estimated mandate effect becomes small and statistically indistinguishable from zero (≈ 0.10, $p \approx 0.16$). This suggests that much of the baseline positive relationship was coming from time-invariant state differences and shared national waves, not a within-state treatment effect. Complementing this idea, when we add time-varying controls (mask policy and unemployment) in column 3, the mandate estimate remains close to zero (≈ 0.04, $p \approx 0.68$). Notably, in this model, the mask policy is negative and statistically significant, while unemployment is negative and only borderline significant. To address the fact that mandate and ban are mutually exclusive, we use a multi-valued regime variable (none / mandate / ban) in column 4. In the cases regression with fixed effects and controls, neither the mandate-regime nor the ban-regime coefficient is statistically different from zero.

After all, when we compare within-state changes over time and absorb common month shocks, we do not see a clear DiD-style decline in log cases attributable to the mandate. The results displayed for cases suggest that mandates appear where cases are worse, but the within-state FE DiD does not show a robust post-adoption reduction in cases, which further motivates us to look at timing and dynamics rather than a single average effect.

COVID-19 Hospital Admissions

Table 4. DiD Estimates for Log of COVID-19 Admissions

	(1)	(2)	(3)	(4)
VARIABLES	Log(Admissions)	Log(Admissions)	Log(Admissions)	Log(Admissions)
Vaccine Mandate	0.0963	0.0999	0.0488	–
	(0.107)	(0.164)	(0.169)	
Stricter Vaccine Mandate				0.0595
				(0.169)
Vaccine Ban				0.0411
				(0.0685)
Mask			0.0160	0.0186
			(0.0855)	(0.0879)
Unemployment Rate			-0.131*	-0.136*
			(0.0761)	(0.0771)
Constant	7.458***	8.301***	9.008***	9.033***
	(0.177)	(0.0729)	(0.451)	(0.454)
Time FE	No	Yes	Yes	Yes
State FE	No	Yes	Yes	Yes
Observations	1,008	1,008	1,008	1,008
R-squared		0.677	0.682	0.682
Number of Groups	42	42	42	42

Standard errors in parentheses
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Consistent with the specifications for cases, Table 4 reports the corresponding

Difference-in-Differences estimates for log COVID-19 hospital admissions.

The baseline model (column 1) shows a small, statistically insignificant association between mandate status and admissions ($p \approx 0.37$). After adding state and month fixed effects (column 2) and controls (column 3), the mandate effect remains statistically indistinguishable from zero. Lastly, in the regime specification (none/mandate/ban) (column 4), both the mandate-regime and ban-regime estimates remain imprecise and insignificant.

Taken together, the DiD results suggest two things that naturally motivate our next steps. First, our policy timing looks endogenous, evidenced by the case baseline estimate being strongly positive but collapsing once fixed effects are added. This is consistent with mandates being adopted in higher-burden periods, implying potential reverse causality and endogeneity issues. Second, dynamics and persistence are plausible. COVID outcomes are highly persistent and adjust over time, so an event study is the right next check: we can test for pre-trends and see whether any effects appear with lags (or only during certain months/waves). If the event study shows delayed responses, persistence, or evidence that treated states were already on different trajectories, that creates a clean justification for moving to a dynamic panel approach (Difference GMM) to address lag dependence and remaining endogeneity.

5.2. *Event Study*

The figure below plots the estimated coefficients (β_k) for the log of cases (A) and the log of hospital admissions (B), with 95% confidence intervals. The reference period is set to $k = -1$, the month prior to mandate enforcement (September 2021). The full coefficient table is reported in the appendix (we include the graphs in the main text and the event-time estimates in the appendix.)

A. Effect on Log COVID Cases **B. Effect on Log COVID Admissions**

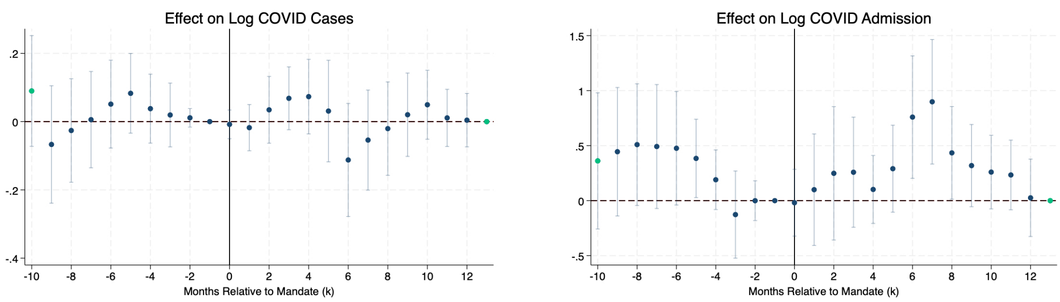

Figure 1. Event Study on Log of COVID-19 Cases and Hospital Admissions

Validation of Parallel Trends

Under the identifying assumption, the pre-treatment coefficients (left hand side of the vertical bar) should be statistically indistinguishable from zero, indicating that outcomes in treated and control states were evolving similarly prior to the policy.

For log cases, the pre-period coefficients (the "leads") are all close to zero and not statistically different from zero. For example, the coefficients from lead10 through lead2 are small and imprecise (e.g., lead10 $= 0.090$, $p = 0.271$; lead5 $= 0.083$, $p = 0.159$; lead2 $= 0.011$, $p = 0.414$). This pattern supports the parallel-trends assumption for cases: we do not see strong evidence that treated states were already on a different cases trajectory before adoption. This supports the plausibility of the DiD comparison for cases and suggests that, at least in the pre-period, mandate and non-mandate states were not on sharply different trajectories after accounting for fixed effects and controls.

For log admissions, the parallel-trends check is notably weaker. Several pre-period coefficients are positive and borderline significant, which suggests treated states may have had rising admissions before the mandate took effect (e.g., lead8 = 0.509, $p = 0.070$; lead7 = 0.492, $p = 0.085$; lead6 = 0.476, $p = 0.069$; lead5 = 0.384, $p = 0.035$). Because these "lead" estimates are not flat, we treat the admissions event-study results more cautiously: they are consistent with policy adoption during worsening hospital conditions, which is consistent with endogenous timing. This pattern complicates causal interpretation of the post-period coefficients for admissions in a static event-study/DiD setup. In particular, when pre-trends are already trending upward, post-adoption estimates may partly reflect continuation of pre-existing momentum rather than a policy-induced change. Accordingly, we interpret the admissions event-study results more cautiously and treat them primarily as diagnostic evidence that hospitalization dynamics are highly persistent and that policy adoption may be responding to contemporaneous strain.

Dynamic Effects on Log of Cases

For log cases, there isn't a sharp break at the time of adoption. The estimate at ($k = 0$) is very close to zero and not significant (lag0 = -0.008, $p = 0.699$). Looking across the post period, the coefficients fluctuate slightly around zero and remain statistically insignificant (for example, lag6 = -0.112, $p = 0.178$; lag10 = 0.049, $p = 0.329$; lag12 = 0.004, $p = 0.913$). The event study for cases is broadly consistent with the DiD results: once we net out common month shocks and compare within states, we do not find a clear, sustained post-mandate decline in statewide case counts. At the same time, the pattern still supports our motivation for the dynamic extension: cases move in waves and show persistence, and a static event-study/DiD design may miss effects that depend on lagged outcomes or policy timing.

Dynamic Effects on Log of Hospital Admissions

For log admissions, there is also no immediate "jump" at adoption: the estimate at ($k = 0$) is near zero and not significant (lag0 = -0.019, $p = 0.901$). However, the post-period pattern is very different from cases. Several months after adoption, the estimates turn positive and statistically significant, especially around months 6–8 (lag6 = 0.759, $p = 0.009$; lag7 = 0.899, $p = 0.003$; lag8 = 0.434, $p = 0.043$). This should not be interpreted as evidence that mandates increase admissions. With the pre-trend evidence, admissions appear to be rising before adoption in treated states, and that upward momentum can carry forward after adoption, which validates our concerns about policy endogeneity and outcome persistence.

5.3. ***Difference Generalized Method of Moments***

For each outcome, the preferred Difference GMM specification is chosen based on standard diagnostics (AR(2) non-rejection, Hansen non-rejection) and instrument parsimony (collapsed instruments; low instrument count relative to 42 states). We report one close alternative lag-window as sensitivity and a regime specification to account for mutually exclusive mandate/ban environments.

COVID-19 Cases

Table 5 below shows Difference GMM estimates for Log of COVID-19 Cases. We include a monthly trend rather than a full set of month indicators as month dummies can soak up almost all of the wave variation that dominates COVID cases.

Table 5. GMM Estimates for COVID-19 Log of Cases

VARIABLES	(1) Log(Cases)	(2) Log(Cases)	(3) Log(Cases)
Vaccine Mandate	-0.862***	-6.942	–
	(0.255)	(5.226)	
Stricter Vaccine Mandate			-0.673***
			(0.207)
Vaccine Ban			0.016
			(0.045)
Log(Cases)_{t-1}	0.241	-1.958	0.299
	(0.185)	(2.146)	(0.217)
$\text{Unemployment}_{t-1}$	-0.341***	-1.872	-0.292***
	(0.071)	(1.360)	(0.084)
Mask	0.142***	1.139	0.136**
	(0.046)	(0.931)	(0.051)
Monthly Trend	0.022***	0.096	0.020**
	(0.007)	(0.083)	(0.008)
Observations	924	924	924
Number of Groups	42	42	42
AR(1)	0.876	0.742	0.952
AR(2)	0.190	0.295	0.152
Hansen	0.502	0.902	0.225
Number of Instruments	9	9	11

Standard errors in parentheses
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Column 1 instruments the dynamic terms using a lag window of (2 3), which keeps the instrument set small and avoids relying on very distant lags. In this specification, the HCW vaccine mandate is negative and statistically significant ($\beta = -0.862$, SE $= 0.255$, $p < 0.01$), suggesting that once we account for the strong persistence in cases, mandates are associated with lower case counts. Interpreting the coefficient in log terms, it is approximately $(e^{-0.862} - 1) \times 100\% \approx -57.8\%$, i.e., about a 58% decrease in cases. For the covariates, unemployment coefficient is negative and precisely estimated, while mask mandate coefficient is positive and significant. Nevertheless, the mask coefficient should not be read as causal as it likely reflects policy timing during surges (mask policies often appear when cases are already rising), so it functions more as a control for the broader policy environment than as an effect estimate. Furthermore, the specification tests also look reassuring: the AR(2) test does not reject ($p = 0.190$), and the Hansen test is not rejected ($p = 0.502$). With only 9 instruments for 42 states, these diagnostics are less likely to be distorted by instrument proliferation.

Column 2 uses the same basic model but switches to a deeper lag window of (3 4). In practice, this version becomes much noisier: the mandate estimate remains negative, but it is imprecise ($\beta = -6.942$, SE = 5.226). Hence, the main takeaway from Column 2 is not the point estimate itself, but the fact that pushing identification further into the past (using deeper lags) can weaken the usable signal in a setting where cases are dominated by national waves and state-level policy changes are relatively infrequent.

Mirroring our DiD coding, instead of estimating separate "mandate" and "ban" indicators in the same equation, we use regime indicators relative to the omitted "neither" category in column 3. Under this coding, the stricter mandate regime remains negative and significant ($\beta = -0.673$, SE = 0.207, $p < 0.01$), while the ban regime is close to zero and not significant, which strengthens the negative mandate association in Column 1. Besides, comparing with the vaccine mandate coefficient in column 1, this stricter vaccine mandate seems to have a smaller reduction on log of cases. The AR(2) and Hansen tests remain acceptable (AR(2) $p = 0.152$; Hansen $p = 0.225$).

Overall, Table 5 suggests that once case persistence is modeled explicitly, HCW vaccine mandates are associated with reductions in log cases in the preferred specification (Column 1) and are further supported by an even stricter regime (Column 3). The deeper-lag variant (Column 2) shows how sensitive precision can be to lag depth. Taken together, these GMM results complement the DiD/event-study section by showing that accounting for momentum in COVID outcomes can reveal mandate-associated declines, while still leaving room for caution given policy timing and specification sensitivity.

COVID-19 Hospital Admissions

Having shown for cases that accounting for persistence can meaningfully change what the data suggest about mandate effects, we apply the same dynamic framework to admissions in Table 6. The structure mirrors the cases table, but we maintain monthly controls for admissions because hospitalization dynamics are especially shaped by sharp month-to-month national shocks, such as variant surges, changes in clinical practice, and capacity strain, that are not well approximated by a smooth trend.

Columns 1 and 2 present our main admissions specifications. Across both, the estimated mandate coefficient is negative. In Column 1, the mandate estimate is -3.761 (SE = 2.099) and is statistically significant at the 10% level; in Column 2, it remains negative and becomes more precise at -2.883 (SE = 1.274), significant at the 5% level. That is a reduction of 97.67% and 94.4% in hospital admissions, respectively. We treat these as evidence that, once we condition on the strong dynamic structure of admissions and common monthly shocks, mandates are associated with reductions in admissions, while noting that the magnitudes are large and should be interpreted cautiously.

A key difference from the cases results is that admissions display an even more pronounced dynamic structure: the first lag of log admissions is large and highly significant in both specifications (0.910 and 0.808), and the second lag is negative and significant (-0.627 and -0.569). This pattern shows that admissions tend to change slowly from month to month, but they also move back toward normal after surges rise and then fall. In other words, the admissions process is highly persistent, and the dynamic controls employed by GMM are doing substantial work. Furthermore, the diagnostic tests support these two baseline admissions specifications. In both columns, the AR(2) test does not reject ($p = 0.688$ and $p = 0.666$), and the Hansen test is not rejected. The instrument count (30 for 42 states) is higher than in the parsimonious cases baseline, but remains below the number of groups, which helps limit concerns about instrument proliferation.

Column 3 places the admissions results in the same policy regime context emphasized in the cases table by replacing the single mandate indicator with mutually exclusive regime indicators, Stricter Vaccine Mandate and Vaccine Ban. Here, the stricter mandate coefficient remains negative (-1.561) and the ban coefficient is positive (1.130), but neither is statistically distinguishable from zero.

In general, Difference GMM estimates confirm that COVID outcomes are highly persistent, while showing evidence that mandates are associated with fewer cases and admissions. Appendix robustness checks that add intensity measures and alter between time control/trend are generally imprecise and, in some variants, fail over-identification tests, reinforcing the need for cautious interpretation. Overall, the dynamic-panel evidence is consistent with mandates reducing COVID burdens after accounting for strong persistence, but results remain sensitive to policy-regime coding and specification choices.

Table 6. GMM Estimates for COVID-19 Log of Admissions

	(1)	(2)	(3)
VARIABLES	Log(Admissions)	Log(Admissions)	Log(Admissions)
Vaccine Mandate	-3.761*	-2.883**	–
	(2.099)	(1.274)	
Stricter Vaccine Mandate			-1.561
			(1.365)
Vaccine Ban			1.130
			(0.789)
$\text{Log(Admissions)}_{t-1}$	0.910***	0.808***	0.618***
	(0.100)	(0.159)	(0.227)
$\text{Log(Admissions)}_{t-2}$	-0.627***	-0.569***	-0.446**
	(0.107)	(0.127)	(0.184)
$\text{Unemployment}_{t-1}$	-0.530*	-0.444	-0.375
	(0.282)	(0.264)	(0.326)
Mask	-0.116	-0.166	-0.196
	(0.150)	(0.228)	(0.284)
Monthly Control	Yes	Yes	Yes
Observations	882	882	882
Number of Groups	42	42	42
AR(1)	0.00340	0.00896	0.219
AR(2)	0.688	0.666	0.435
Hansen	0.659	0.800	0.668
Number of Instruments	30	30	32

Standard errors in parentheses
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

6. Conclusion

In this research, we evaluate whether state-level healthcare worker (HCW) vaccination mandates reduced broader COVID-19 burdens. Rather than treating a standard DiD as the final causal test, we use our DiD and Event Study results primarily as diagnostic evidence about the data-generating process. Across both outcomes, log cases and log hospital admissions, the DiD patterns and the event-time profiles show that COVID outcomes are strongly persistent, dominated by waves, and closely tied to policy timing. In that setting, a static DiD design is not well suited to isolate a stable average treatment effect, because policy adoption is likely responding to underlying conditions and because today's outcomes depend heavily on recent outcomes.

Consequently, we extend the analysis using Difference GMM, which explicitly models persistence through lagged outcomes and uses internal instruments to reduce bias from dynamic feedback and policy endogeneity. In these preferred GMM specifications, we find consistent evidence that mandates are associated with reductions in COVID burdens once persistence is accounted for. For cases, the mandate effect is negative and statistically meaningful in the parsimonious model, and a mutually exclusive regime specification points in the same direction. For admissions, the mandate coefficients are again negative across close lag-window choices, and the specifications satisfy standard diagnostic checks. Overall, once we account for epidemic momentum and endogenous policy timing, mandates are linked to lower cases and lower hospital admissions.

At the same time, the DiD and Event Study evidence suggests that statewide outcomes are heavily shaped by variant waves and other common shocks, so a sharp effect at the time of adoption is not expected in a static design. Instead, the main takeaway is that HCW mandates appear to contribute to reductions in COVID burdens through dynamics. In other words, effects emerge once we model persistence directly and compare states on a more comparable footing over time.

In policy terms, these findings support the view that HCW vaccination mandates did achieve their intended public-health direction, not necessarily by producing an immediate, easily detectable drop in a static DiD, but by shifting outcomes once the underlying persistence of the pandemic is properly taken into account. More broadly, this research highlights a lesson for evaluating COVID policies: when outcomes are wavedriven and policy timing is responsive, dynamic methods are often required to recover the policy signal.

Furture extensions would include examining the relationship between healthcare worker vaccination mandates and work absenteeism, which has been documented as a key tradeoff of these policies. We would also move beyond a binary policy indicator by measuring mandate intensity, allowing for variation in restrictions, compliance requirements, and enforcement across states, which reflect the unique U.S. policy landscape. Finally, extending the study window through 2023 would allow a more complete assessment of the later pandemic period, when additional variants emerged and policy responses continued to evolve.

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