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How Partisan Policies Can Shape Health Behaviors: Executive Order Proof-of-Vaccine Mandate Bans Increased COVID-19 Vaccinations

Deena N. Brosi [1,*], Gregory Tung [2], Beth Mcmanus [2], Srinivas Parinandi [3] and Glen Mays [2]

[1] Department of Surgery, University of Colorado Anschutz Medical Campus | Aurora, Colorado
[2] Department of Health Systems, Management, and Policy, Colorado School of Public Health | Aurora, CO
[3] Political Science Department, University of Colorado, Boulder | Boulder, CO
* Correspondence: deenanicolebrosi@gmail.com

Abstract

Background/Objectives: COVID-19 vaccine resistance was detrimental to herd immunity and worsened COVID-19 morbidity and mortality during outbreaks. Despite more evidence showing reactionary behavior among residents exposed to vaccine mandates, little research has been conducted on the effects of state proof-of-vaccine (POV) mandate bans in the United States (US). We sought to investigate the causal effects of POV mandate bans, overall and stratified by policy passage via executive order or state legislature, on 1st-dose COVID-19 vaccinations. **Methods**: In the contiguous US, 21 states enacted POV mandate bans from 2/8/2021–10/25/2021. Using a geographic regression discontinuity design, we selected treatment and control counties within 150 miles of the POV mandate ban state border. The resulting sample was 4,612 county-observations and 2,466 unique counties. We conducted two-way fixed effects estimation to compare changes in bimonthly, 1st-dose COVID-19 vaccinations among individuals under 65 years old before and after POV mandate ban enactment between treatment and control counties. **Results**: Among executive order POV mandate ban counties, we saw a reduction in the decreasing 1st-dose COVID-19 trend following POV mandate ban enactment. This corresponded to an additional 32.6% increase in 1st-dose COVID-19 vaccinations in Weeks 1–2, 34.5% in Weeks 3–4, 35.0% in Weeks 5–6, and 36.9% in Weeks 7–8 post-POV mandate ban enactment when compared to control counties. **Conclusions**: These findings suggest that executive order POV mandate bans reversed reactance to vaccine mandates. Future public health efforts should consider potential reactance to mandatory policies and tailor efforts to community values.

Keywords: COVID-19 vaccination behavior; proof-of-vaccine mandate bans; psychological reactance theory; expressive function of law

1. Introduction

After a comparatively poor public health response to the COVID-19 pandemic in 2020, the United States (US) rolled out the first ever mRNA vaccine for COVID-19 in early 2021.[1] However, among some in the US the COVID-19 vaccine was met with intense skepticism. In March 2021, a US Department of Health and Human Services (HHS) survey found that 19% of survey respondents reported they were "definitely not" or "probably not" going to receive the COVID-19 vaccine.[2] Skepticism was highest among those with a high school-level education, conservative political ideologies, and persons under 65 years old.[2,3] In response, the White House and federal agencies began discussing the potential for that were eventually implemented several in the Summer and Fall of 2021. This included a Biden executive order requiring all health care workers to receive the COVID-19 vaccine and an Occupational Health and Safety Administration directive on employee vaccine mandates for large employers.[4,5]

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During those early White House discussions, news sources increased media content around the merits and consequences of vaccine mandates, further politicizing the topic.[1,6] The result was a flurry of policy activity directed at vaccine mandates and passports at the state and local level. States like New York and California passed vaccine mandates for healthcare workers and state government employees. However, conservative states reacted in opposition, with some adopting proof-of-vaccine (POV) mandate bans. These POV mandate bans prohibited mostly government organizations, and in some cases private businesses, from requiring COVID-19 vaccination status for employees or patrons. By September 2021, twenty-two states had adopted some variation of the POV mandate ban.[7]

Many researchers have tried to study the effects of vaccine mandates in the US, but no such research has been conducted on the effects of POV mandate bans on vaccination behavior.[5] Vaccine mandates, especially in other countries where the mandates extended to the general population, have been shown to be highly effective at increasing COVID-19 vaccinations.[8,9] In the US however, most analyses have shown little general-population effect, mostly because mandates only extended to certain populations (i.e. healthcare workers, state governments) and were implemented in states with already high COVID-19 vaccination rates.[5,10] Meanwhile, POV mandate bans were enacted in states with the highest unvaccinated populations and were seemingly intended to be more politically symbolic than an incentive for vaccinating. POV mandate ban language emphasized the rights of individual liberties and the importance of state protections for those constitutional rights, all under the context of potential national COVID-19 vaccine mandates.

Given that COVID-19 vaccination rates among states with POV mandate bans were among the lowest in the country, it is important to understand the potential causal effects of this policy on COVID-19 vaccination behavior.

2. Materials and Methods

2.1. Conceptual Frameworks

We used the Social Ecological Model (SEM) to guide our analysis of POV mandate ban effects on COVID-19 vaccinations (Figure 1). The SEM details how perceptions go beyond individual characteristics and that behavior change is a function of various social structures. For example, the SEM could consider how an individual's political preference interacts with the community's public health values when measuring intent to receive the COVID-19 vaccine.[11] It is under this framework, and with several complementary theories, that we set the stage for examining POV mandate ban enactment on COVID-19 vaccination behaviors.

By Spring 2021, studies showed that politically conservative areas were questioning the severity of the COVID-19 pandemic and these perceptions were correlated with low COVID-19 vaccination rates.[3] Further, it was well documented during the COVID-19 pandemic that mandates, both mask and vaccine, produced behaviors described in *Psychological Reactance Theory* (PRT).[12,13] PRT posits that individuals will resist a behavior if they believe it poses a threat to their freedom to choose.[12,14] Studies in the US and Germany documented higher reactance to mandatory COVID-19 vaccine mandates, particularly among groups that were opposed to mandatory vaccine mandates prior to enactment. Further, studies among college students found that reactance was higher among students posed with a hypothetical vaccine mandate at another university vs. at their university (coined indirect threat).[15] We believe conservative populations were experiencing this indirect reactance by observing vaccine mandate enactment in liberal states and discussions of national vaccine mandates prior to POV mandate ban enactment.

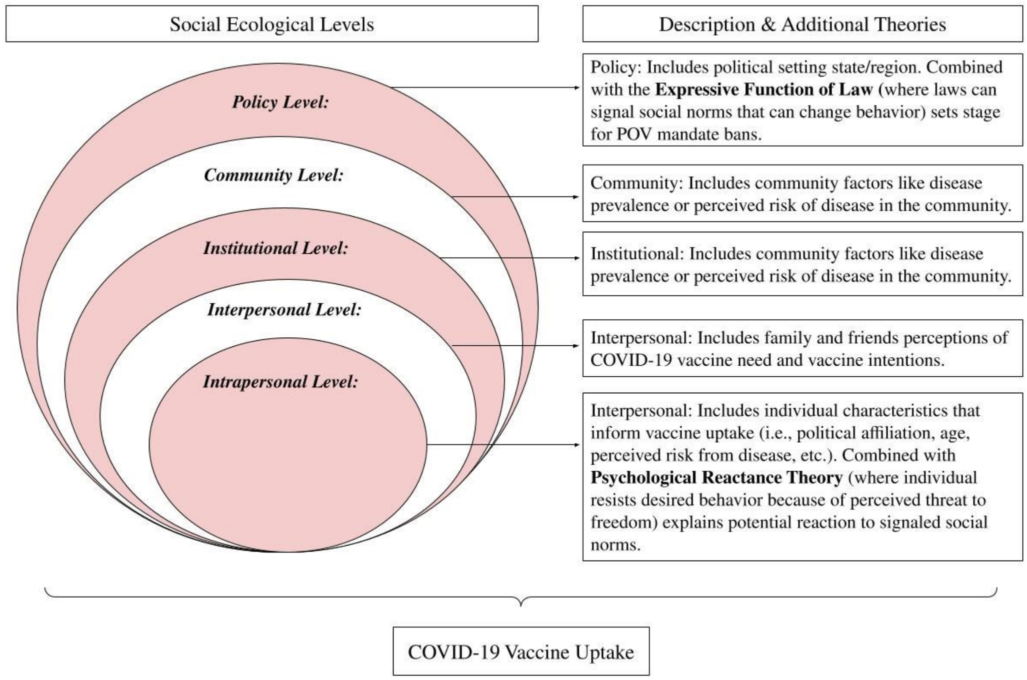

Figure 1. Full Social Ecological Model Diagram with Expressive Function of Law and Psychological Reactance Theory falling under the policy and intrapersonal levels, respectively.

This context is important when incorporating our next theory, the Expressive Function of Law (EFL). The EFL demonstrates the potential for policy, aside from the direct regulatory power and/or in the absence of enforcement, to change attitudes around a topic and motivate behavior change by signaling social norms.[16] For example, many localities do not enforce their laws around mandatory recycling, but these laws signal social expectations for residents, which can increase recycling behavior. The EFL also theorizes that norms signaled through laws are most effective at changing behavior when it complements existing community values.[16] Many characteristics of POV mandate bans, including that they were largely unenforced and signaled the strongly held value of individual liberties to conservative constituents, fit within the EFL framework.

Under the SEM, EFL, and PRT, we present two hypotheses for potential signaled social norms and subsequent COVID-19 vaccination behavior change. 1) POV mandate bans signaled that the COVID-19 pandemic was not as severe as federal public health institutions claim. We hypothesized that this signaled norm would lead to relative decreases in COVID-19 vaccinations (compared to controls), particularly among more politically moderate areas or cities within conservative states. 2) POV mandate bans signaled to a highly reactant population that an individual's choice to vaccinate is now protected under the POV mandate ban. While less is known about reversing reactance behavior during COVID-19, previous research shows reactance reversal and engagement in desired health behaviors when the perceived threat is removed.[14] Under this scenario of reactance reversal, we hypothesized a relative increase in COVID-19 vaccinations among more conservative and vaccine resistant populations as the POV mandate bans relieve perceived threats to freedom of choice in vaccinating.

2.2. Data and Measures

We obtained state-level POV mandate ban legislation and date of enactment from the National Academy for State Health Policy (NASHP) website. By the end of 2021, 21 states in the contiguous US passed a POV mandate ban.[7] A list of state POV mandate bans, enactment dates, and enactment mechanism (via executive order or through a legislative bill) are presented in Table 1.

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Table 1. Details on policy mechanism (passed via executive order or through house and senate bills), date of enactment, and state proof-of-vaccine (POV) mandate bans for the first POV mandate bans during 2021 calendar year.[7].

State	*Enactment Date*	*Policy Mechanism*
Utah	3/16/2021	HB[a] 308
Florida	4/2/2021	EO[b] 2021-81
Texas	4/5/2021	EO GA-35
Idaho	4/7/2021	EO 2021-04
Montana	4/13/2021	EO 7-2021
Arizona	4/19/2021	EO 2021-09
South Dakota	4/20/2021	EO 2021-08
Alaska[c]	4/26/2021	AO[d] 321
Indiana	4/29/2021	HB 1405
Arkansas	4/30/2021	SB[e] 615
North Dakota	5/7/2021	HB 1465
Wyoming	5/7/2021	EO
South Carolina	5/11/2021	EO 2021-23
Alabama	5/17/2021	SB 267
Iowa	5/20/2021	HF[e] 889
Georgia	5/25/2021	EO 5.25.21.01
Kansas	5/26/2021	SB 159
Tennessee	5/26/2021	SB 858
Oklahoma	6/1/2021	EO 2021-16
Missouri	6/15/2021	HB 271
New Hampshire	7/23/2021	HB 220
Michigan	9/29/2021	SB 82

[a] HB (House Bill).
[b] EO (Executive Order).
[c] AK was removed since it is not part of the contiguous 48 and would likely skew results if we were to include "comparable control" counties based on distance.
[d] AO (Administrative Order).
[e] SB (Senate Bill).
[f] HF (House File).

The outcome of interest was county-level, weekly, 1st-dose COVID-19 vaccinations among persons under 65 years old, as those 65 and older were less likely to report vaccine resistance.[2,3] We obtained data from the Centers for Disease Control and Prevention's (CDC) publicly available COVID-19 data repository and supplemented missing data with state health department data.[17] We calculated weekly COVID-19 vaccinations by summing daily 1st-dose COVID-19 vaccinations over a Monday–Sunday period, with week of enactment considered "Week 0" and included in the pre-period. However, even aggregated weekly COVID-19 vaccinations seemed to suffer from lags in data collection and reporting. To smooth out weekly values, our primary analysis estimates bimonthly changes in weekly, 1st-dose vaccinations following POV mandate ban enactment. We also conducted sensitivity analysis on weekly and monthly effects post POV mandate ban (**Appendix**).

Data for other time-varying covariates, including other state vaccine policies and COVID-19 deaths, was collected from the CDC's COVID-19 data repository and the NASHP website.[7,18] County and city vaccine policies were collected from the HHS and data for sensitivity testing was collected from US Census and CDC Hesitancy Data.[19–21]

2.3. Study Sample

We used a geographic regression discontinuity (GRD) design to determine our study sample, which included treatment and control counties within 150-miles of the POV mandate ban state border. The GRD design is sharp, and the running variable is county centroid distance, in miles, from the state border. GRD design is a quasi-experimental method that exploits "local randomization" that occurs when an arbitrary cut-off point, such as a state border, determines treatment/control samples.

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The design speculates that counties close to the POV mandate ban state border will be similar in factors related to COVID-19 vaccination, which is supported by studies investigating spatial proximity on vaccine uptake.[22] This selection mechanism has been used in other COVID-19 policy analyses and was also empirically tested in our sample (**Appendix**).[23,24]

This sample selection excluded many counties (and the whole state of Alaska) that do not share a state border with a POV mandate ban state, are too far away from the state border to be considered comparable controls, and/or had no US border counties (i.e counties along the Canadian border, counties along the Gulf of Mexico). This also resulted in some counties acting as multiple controls. This selection mechanism has been used in other COVID-19 policy analyses and was shown through sensitivity testing to be a good mechanism for selecting comparable treatment and control counties.[25,26] We conducted additional sensitivity testing that includes all counties within selected states and that removed multiple county controls (**Appendix**).

In addition to the GRD design, we also used a stacked data structure, which removed early vs. late adopter county comparisons and can be problematic in two-way fixed effects estimation (TWFEE).[27] We also removed time periods for states that enacted multiple POV mandate bans during the study period, which included Florida, Montana, Arizona, North Dakota, South Carolina, and Georgia. Both the stacked structure and removal of secondary POV mandate bans led to an unbalanced panel data set. Both the stacked structure and removal of secondary POV mandate bans led to an unbalanced panel data set.

Finally, we excluded counties for dates that had no data on 1st-dose COVID-19 vaccinations for persons under 65 years old (8.4% of sample). This missing data occurred throughout the study sample, with 88.9% occurring in Nebraska, South Dakota, Texas, New Mexico and Ohio. Our final sample included 4,612 observations (with duplicate controls) and 2,466 unique counties. GIF S1 in the **Supplementary Files** depicts how our sample changed over time.

2.4. Study Period

The first POV mandate bans for each state were enacted between 3/16/2021–9/29/2021. Based on previous COVID-19 vaccination studies, we used up to eight weeks pre POV mandate ban enactment date and 12 weeks post POV mandate ban and conducted sensitivity analysis on varying date inclusion.[8,9] We also conducted sensitivity analysis of varying inclusion criteria for weeks before POV mandate ban enactment date with similar results (**Appendix**). With pre- and post-period designations included our study time frame spans from 2/8/2021–10/25/2021.

2.5. Sample Stratification

Our sample includes all first POV mandate bans passed in 2021, but we do not believe that POV mandate bans were a monolith. Instead, POV mandate bans enacted through executive order versus the legislative process had some crucial differences that we believe signaled different social norms to the population through the EFL. Among these are potential differences in content and language for a governor passing a relatively quick executive order vs. state congress members that needed a majority to approve the legislation.[28] In addition to analyzing the full sample of POV mandate bans, we also stratified our analysis by executive order and legislative POV mandate bans (termed full sample, executive order sample, and legislative samples in the **Results**/**Discussion**). Ten states passed POV mandate bans through executive orders and 11 passed POV mandate bans through the legislative process.[7]

2.6. Statistical Analysis

We estimated the effect of POV mandate ban enactment on the log of weekly, 1st-dose COVID-19 vaccinations using a stacked TWFEE framework and a GRD design to select treatment/control groups. The TWFEE is a type of difference-in-differences (DiD) analysis and allowed us to study county trends before and after POV mandate ban enactment against controls that did not experience

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POV mandate ban enactment during the same time. The combination of GRD along with the TWFEE allowed us to select comparable treatment and control counties and eliminate state-border confounding (i.e. different health-care systems) and secular trends. Below is the equation for our model:

$$Y_{ct} = \alpha_c + \alpha_t + \beta_1 D_c + \beta_2 T_{post=1,tp} + \sum_{p=1}^{6} \delta_p \left(T_{post=1,tp} * D_c\right) + X_{ct}\beta' + \epsilon_{ct} \quad (1)$$

Subscripts were c = county, t = time in weeks. Y_{ct}=log of weekly, 1st-dose COVID-19 vaccinations, α_t = time fixed effects (using dummy-variables for date), α_c = county fixed effects, and $X_{ct}\beta'$ = vector of time-variant covariates. The time-varying covariates included county centroid distance to the POV mandate ban state border, vaccine-specific state, county, or city legislation, one-week COVID-19 death rate lag, and a one-week lag for the percent of weekly-averaged, cumulative 1st-dose vaccinations among persons under 65 years old. Variable $T_{post=1,tp}$ represents the weeks following POV mandate ban enactment at time t, aggregated bi-monthly (p), and D_c is a dummy variable for treatment/control counties. The coefficients of interest were the set of interaction terms, δ_p, which we interpreted as the additional percent change in p bimonthly, 1st-dose COVID-19 vaccinations among treatment counties, before and after POV mandate ban enactment when compared control counties. In our **Results** we present bimonthly period coefficients (δ_{1-6}), standard errors (s.e.) for those coefficients, and p-values. All analyses were performed in Stata/SE 19.5 and standard errors were clustered at the state level.

3. Results

To explore changes in COVID-19 vaccinations during POV mandate ban enactments, we first looked at unadjusted weekly, 1st-dose COVID-19 vaccinations (Figure 2). We saw an increase in COVID-19 vaccinations among treatment countries following POV mandate ban enactment for all three samples and no increase in control counties. The full sample spike in COVID-19 vaccinations was likely due to the large increase seen in the executive order sample, as the legislative sample had only a small increase. Of note, there was an increase in unadjusted COVID-19 vaccinations around four weeks before the POV mandate ban enactment among the executive order sample. This is due to the introduction of Texas counties, which did not have data on 1st-dose COVID-19 vaccinations among those < 65 years old before March 2, 2021.

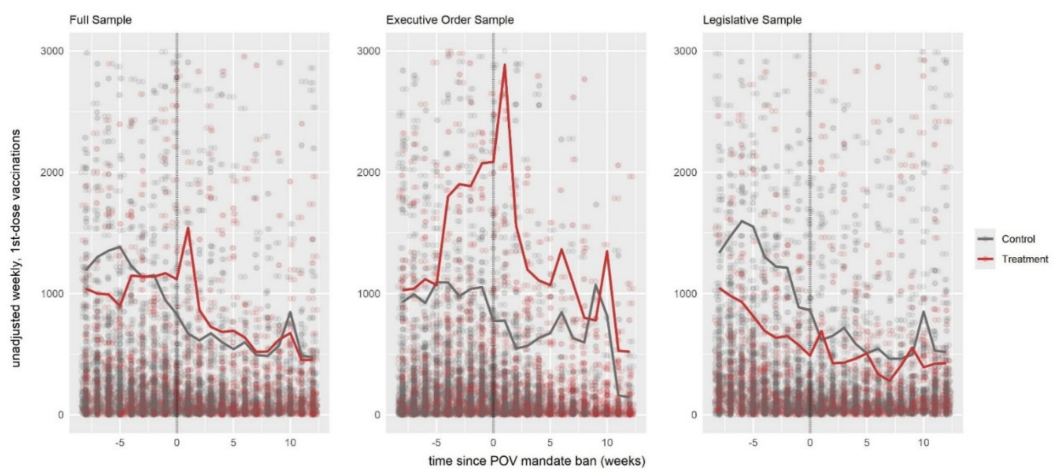

Figure 2. Unadjusted weekly, 1st-dose COVID-19 vaccinations for persons under 65 years old before and after POV mandate ban enactment for full sample, executive order, and legislative samples, respectively.

For our adjusted model outcomes, we found no significant effect on the percent change in weekly, 1st-dose COVID-19 vaccinations for the full POV mandate ban sample (Table 2). However, when we stratified the sample, we found a significant, positive effect in the first several bimonthly

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time periods among the executive order sample. We saw an additional 32.6% increase in Weeks 1–2, 34.5% in Weeks 3–4, 35.0% in Weeks 5–6, and 36.9% in Weeks 7–8 post enactment. Among the legislative sample, no bimonthly time periods had significant percent changes in weekly, 1st-dose COVID-19 vaccinations when compared to control counties.

Table 2. Two-way fixed effects estimation (TWFEE) coefficients and standard errors (se) for percent (%) change in bimonthly, 1st-dose COVID-19 vaccinations post-proof-of-vaccine (POV) mandate ban enactment across full, executive order, and legislative samples.

	Full Sample		Executive Order Sample		Legislative Sample	
	Coefficient (se)	p-value	Coefficient (se)	p-value	Coefficient (se)	p-value
Variables						
Treatment Counties * Post POV Mandate Ban (δ_p)						
Pre-period (ref)	--	--	--	--		
Weeks 1–2	9.60% (11.7%)	0.42	32.6% (9.84%)	0.002**	-5.29% (16.8%)	0.76
Weeks 3–4	15.3% (11.5%)	0.19	34.5% (11.3%)	0.005**	3.88% (17.8%)	0.83
Weeks 5–6	15.3% (15.3%)	0.32	35.0% (14.5%)	0.022*	7.92% (22.1%)	0.72
Weeks 7–8	13.3% (17.7%)	0.46	36.9% (15.6%)	0.025*	3.93% (24.5%)	0.87
Weeks 9–10	22.1% (20.4%)	0.28	26.8% (16.6%)	0.12	20.0% (26.4%)	0.45
Weeks 11–12	19.0% (22.2%)	0.40	28.1% (13.9%)	0.05	19.1% (28.6%)	0.51

* P-value significant at the 0.05 level, ** 0.01 level, and *** <0.001 level.

Figure 3 shows estimated bimonthly, 1st-dose COVID-19 vaccinations before and after POV mandate ban enactment for both executive order and legislative samples. We see that within the executive order sample that treatment counties had a significantly larger estimated increase in bimonthly, 1st-dose COVID-19 vaccinations for the first several time periods following POV mandate ban enactment. Parallel trends testing was satisfied for both the executive order and legislative samples.

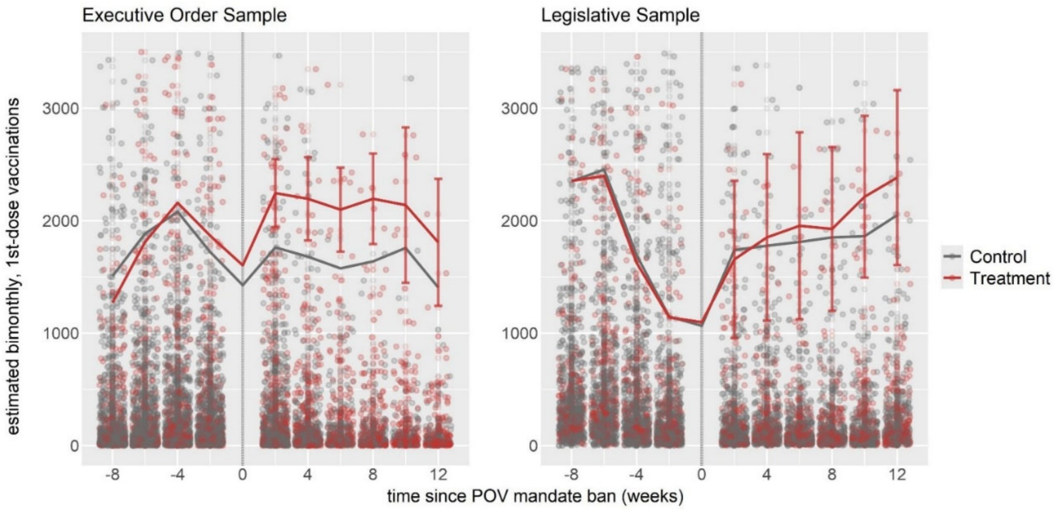

Figure 3. Predicted TWFEE values for bimonthly, 1st-dose COVID-19 vaccinations before and after POV mandate ban enactment for executive order and legislative samples, respectively.

We also conducted additional model specifications that included varying inclusion distances to the POV mandate ban state border and removed spillover counties (**Supporting Information**). These sensitivity tests confirmed the results of our primary analysis. Further, we tested the main model specification with different assumptions including non-normal distributions, Sun & Abrahams TWFEE corrections, TWFEE with Poisson distributions, and unspecified nonparametric model fixed effects.[29] While estimates and standard errors differed, all models showed an additional percent increase in bimonthly 1st-dose COVID-19 vaccinations for the first several bimonthly time periods post POV mandate ban when compared to control counties for the executive order sample.

4. Discussion

Among the executive order sample, we found an additional increase in the percent change of bimonthly, 1st-dose COVID-19 vaccinations post POV mandate ban when compared with controls and no effect among the legislative sample. We believe that the EFL was the primary mechanism for behavior change, which signaled a protection of individual liberties for residents experiencing reactance behavior to vaccine mandates. If true, the effect would occur amongst areas with higher populations of documented COVID-19 resistance and strongly held beliefs around individual liberties.[2] To check this, we further sectioned our samples (at the sample mean) for the following variables associated with COVID-19 vaccine resistance; percent of the county voting for Trump in 2020 (Republican), percent of the county that reported strong COVID-19 hesitancy in Spring 2021 (estimated by the CDC using the Census Household Survey), percent of the county with a college education, and percent of the county that is uninsured.[20] We found that the increased effect among the executive order sample was concentrated among communities with more Trump voters, higher COVID-19 vaccine hesitancy, less college degrees, and more uninsured (Table 3).[19,20]

Table 3. Two-way fixed effects estimation (TWFEE) coefficients and standard errors (se) for percent (%) change in bimonthly, 1st-dose COVID-19 vaccinations post- proof-of-vaccine (POV) mandate ban enactment for the executive order sample stratified at the sample mean across % of county voting for Trump in 2020, % of the county with a college degree, % of the county that is uninsured, and % of the county estimated to be strongly COVID-19 vaccine hesitant.

	% of county voting for Trump 2020			
	≤ 69.6%		> 69.6%	
	Coefficient (se)	p-value	Coefficient (se)	p-value
Treatment Counties * Post POV Mandate Ban (δ_p)				
Pre-period (ref)	--	--	--	--
Weeks 1–2	30.2% (15.1%)	0.06	37.3% (9.4%)	<0.001***
Weeks 3–4	36.0% (13.5%)	0.012*	38.0% (11.5%)	0.003**
Weeks 5–6	35.1% (15.7%)	0.033*	40.3% (13.7%)	0.007**
Weeks 7–8	35.0% (19.0%)	0.08	42.2% (13.3%)	0.004**
Weeks 9–10	44.4% (24.2%)	0.08	19.1% (9.96%)	0.07
Weeks 11–12	32.9% (20.1%)	0.11	26.4% (11.1%)	0.024*
	% of county with college degree			
	≤ 18.4%		> 18.4%	
	Coefficient (se)	p-value	Coefficient (se)	p-value
Treatment Counties * Post POV Mandate Ban (δ_p)				
Pre-period (ref)	--	--	--	--
Weeks 1–2	25.7% (8.89%)	0.007**	42.0% (12.7%)	0.003**
Weeks 3–4	38.0% (10.6%)	0.001**	26.0% (14.1%)	0.08
Weeks 5–6	44.6% (14.1%)	0.004**	20.4% (16.7%)	0.23
Weeks 7–8	43.2% (14.5%)	0.006**	24.5% (18.8%)	0.20
Weeks 9–10	32.6% (16.7%)	0.06	17.5% (20.3%)	0.40
Weeks 11–12	36.7% (16.2%)	0.031*	15.2% (13.7%)	0.28
	% of county uninsured			
	≤ 13.7%		> 13.7%	
	Coefficient (se)	p-value	Coefficient (se)	p-value
Treatment Counties * Post POV Mandate Ban (δ_p)				
Pre-period (ref)	--	--	--	--
Weeks 1–2	14.9% (11.9%)	0.22	43.7% (11.4%)	0.001**
Weeks 3–4	22.2% (16.0%)	0.18	46.9% (12.4%)	0.001**
Weeks 5–6	18.7% (13.3%)	0.17	50.8% (14.5%)	0.002**
Weeks 7–8	15.5% (20.6%)	0.46	55.3% (14.3%)	0.001**
Weeks 9–10	17.2% (22.1%)	0.44	24.3% (12.0%)	0.06
Weeks 11–12	4.0% (15.9%)	0.80	50.0% (16.6%)	0.006**
	% of county strongly hesitant of COVID-19 vaccines			
	≤ 9.36%		> 9.36%	
	Coefficient (se)	p-value	Coefficient (se)	p-value
Treatment Counties * Post POV Mandate Ban (δ_p)				
Pre-period (ref)	--	--	--	--
Weeks 1–2	24.2% (11.7%)	0.05	24.6% (13.0%)	0.016*
Weeks 3–4	13.7% (11.5%)	0.25	45.0% (15.0%)	0.008**
Weeks 5–6	-9.22% (12.1%)	0.45	82.4% (10.5%)	<0.001***
Weeks 7–8	-2.43% (16.0%)	0.88	63.9% (18.2%)	0.003**
Weeks 9–10	10.7% (20.7%)	0.61	48.8% (15.6%)	0.006**
Weeks 11–12	9.15% (16.7%)	0.59	33.7% (15.8%)	0.046*
*P-value significant at the 0.05 level, ** 0.01 level, and *** <0.001 level.				

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However, this does not explain the more robust signal among the executive order sample. When reviewing the bills we found two distinctions: 1) on average executive order POV mandate bans included more public and private sector organizations and were more focused on COVID-19 vaccinations and 2) the language among executive orders was more explicit about individual liberties and government overreach. Our descriptive review found that 60% of executive orders also included private businesses with and without public funding as opposed to only 18% of legislative POV mandate bans. Further, 45% of legislative POV mandate bans were components of other legislative periodic bills, such as omnibus spending bills, and were not their own legislative bill.[7] This would result in a diluted signal as compared to executive orders which were solely focused on the POV mandate ban.

Regarding the bill's language, Table 4 provides some quotes from executive order and legislative POV mandate bans. Executive orders tended to explicitly address the importance of individual liberties, that vaccine mandates/passports threaten those liberties, and the need to protect those liberties through POV mandate bans. Further, the language used in executive orders mirrors conservative political rhetoric at the time and this connection could result in a stronger signal to constituents experiencing reactionary behavior caused by the same political rhetoric.[30] Conversely, legislative POV mandate bans mostly just detailed the functions of the POV mandate ban. In cases where legislative POV mandate bans did use language around individual liberties to cite reasons for enactment, the intensity was muted compared to executive orders.

Table 4. Examples of language used in executive order and legislative proof-of-vaccine (POV) mandate bans.

State	*POV Mandate Ban Quotes*	*Legislation Type*
Arkansas	"(a) As used in this section, 'vaccine passport' means documentation that an individual has been vaccinated against coronavirus 2019 (COVID-19)". "(b) The state, a state agency or entity, a political subdivision of the state, or a state or local official shall not require an individual to use a vaccine passport in this state for any purpose."	SB[a] 615
Florida	"WHEREAS, requiring so-called COVID-19 vaccine passports for taking part in everyday life – such as attending a sporting event, patronizing a restaurant, or going to a movie theater – would create two **classes of citizens based on vaccination**…" "WHEREAS, so-called COVID-19 vaccine passports **reduce individual freedom** and **will harm patient privacy**…" "WHEREAS, it is necessary to **protect the fundamental rights and privacies** of Floridians and the **free flow of commerce** within the state."	EO[b] 2021-81
Georgia	"WHEREAS: I have determined that the following actions are necessary and appropriate to protect the individual liberty of Georgia's residents…"	EO 5.25.21.01
Kansas	"…no state agency named…shall expend any moneys appropriated…to (1) Issue a COVID-19 vaccination passport to any individual without the individual's consent; (2) require an individual to use a COVID-19 vaccination passport within this state for any purpose…"	SB 159
North Dakota	"A private business located in this state may not require a patron or customer to provide any documentation certifying vaccination…to gain access to, entry upon, or services from the business."	HB[c] 1465

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	"This section may not be construed to interfere with an **individual's rights** to access that individual's own personal health information…".	
South Dakota	"Whereas, The vaccines have led to discussions of 'vaccine passports'…used to 'allow' **certain exercises of freedom** that Americans already possess …". "Whereas, Any rationale for imposing public health restrictions that **limit freedoms should be tailored to mitigate a verifiable, scientific risk**…". "Whereas, It is improper to adopt an official government policy – a mandate – requiring widespread use of vaccine passports **when such a mandate is overreaching, morally objectionable**…".	EO 2021-08

[a] SB (Senate Bill).
[b] EO (Executive Order).
[c] HB (House Bill).

4.1. Limitations

There were limitations with our analysis. With any DiD specification, there is the potential for other events to affect the relationship between the treatment and outcome. We were able to control for some city and state-level vaccine policies, however it is possible that these data were not exhaustive.[21] Further, roughly 8% of the full sample was missing, which reduces the generalizability of the effect we found among the executive order sample. Finally, because we could not measure person-level perceptions around COVID-19 vaccine mandates and POV mandate bans during our study period, we are unable to know for certain which persons were motivated by reversal of reactance. However, our study design leveraged multiple quasi-experimental approaches, performed several sensitivity analyses on various model specifications and sub-group populations, and reviewed the content and language within the POV mandate bans. The diversity of testing and sensitivity checks performed gives us more confidence in our conclusions.

5. Conclusions

POV mandate bans, specifically executive orders, signaled social norms that connected with populations that were highly resistant to COVID-19 vaccinations. This resulted in an additional increase in bimonthly, 1st-dose COVID-19 vaccinations following POV mandate ban enactment when compared to control counties. These results indicate that reactance behavior to COVID-19 vaccines was reversable when POV mandate bans remove the perceived threat of forced vaccination and signaled existing community values around individual liberties.

While it may seem that POV mandate bans were innocuous or even beneficial for vaccination rates, POV mandate bans set a dangerous precedent for both public health and policy. POV mandate bans were masked preemption policies, with some targeting the scope of both local and federal governments to enforce vaccinations.[28] This scope extends to areas, such as hospitals and schools, where vaccinations requirements have historically been supported and are considered standard practice.[7,31] Further, the downstream effects of vaccine reactance go beyond POV mandate bans. Research has shown that subsequent voluntary uptake of COVID-19 boosters was less among states with vaccine mandates than states with POV mandate bans, making them more susceptible to future COVID-19 strains.[32]

For future steps, public health officials should consider the wide-spread implications of vaccine mandates. Indeed, recent research has shown that among states with vaccine mandates that subsequent voluntary uptake of COVID-19 boosters was less than in states with POV mandate bans.[32] Further, we are now experiencing the large-scale implications of vaccine reactance with elected officials at HHS removing experts on immunizations and changing mandatory vaccine

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schedules.[33] We believe that vaccine mandates are necessary and powerful public health measure, but future efforts should consider these unintended consequences when messaging and designing mandated public health policies. Public health mitigation efforts should incorporate community values and social norms to prevent future reactance behavior and increase adherence to recommended public health behaviors, including vaccinations.

Supplementary Files: The following supplementary files can be downloaded at: https://www.mdpi.com/article/doi/s1, GIF S1. US map depicting changes in the stacked, panel sample of POV mandate ban treatment and control counties over time from March 16, 2021 – October 25, 2021.

Author Contributions: Deena Brosi - Conceptualization; Formal analysis; Investigation; Methodology; Visualization; Writing – original draft; Writing – review & editing. Gregory Tung (gregory.tung@cuanschutz.edu) - Project administration; Resources; Supervision; Writing – review & editing. Beth Mcmanus (beth.mcmanus@cuanschutz.edu) - Methodology; Resources; Validation; Writing – review & editing. Srinivas Parinandi (srinivas.parinandi@colorado.edu) - Methodology; Resources; Validation; Writing – review & editing. Glen P. Mays (glen.mays@cuanschutz.edu) - Project administration; Methodology; Resources; Supervision; Writing – review & editing.

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Abbreviations

The following abbreviations are used in this manuscript:

POV	Proof-of-vaccine
SEM	Social Ecological Model
EFL	Expressive Function of Law
PRT	Psychological Reactance Theory
GRD	Geographic-regression discontinuity
TWFEE	Two-way fixed effects
DiD	Difference-in-differences
HHS	The US Department of Health and Human Services
NASHP	National Academy for State Health Policy
EO	Executive order
SB	Senate Bill
HB	House Bill

Appendix A

Appendix A.1. Sample Selection

Our main analysis study sample was selected through a geographic regression discontinuity (GRD) design, which identifies treatment and control counties based on distance from the POV mandate ban state's border. Based on previous research, we selected a 150-mile buffer from the POV

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mandate ban state border.[23] GIF S1 in the **Supplementary Files** shows how the treatment and control sample changes over the study period. We also wanted to test the validity of the GRD design to appropriately select treatment and control counties. Figure S1 shows linear estimates for the running variable, county centroid distance (in miles) to POV mandate ban state border, across several potential confounding variables within the executive order sample. The graphs were smooth across the threshold for all tested variables, which is an indication of a valid GRD design. These GRD checks gave us more confidence that our treatment and control counties were similar across factors that have been shown to be related to COVID-19 vaccine acceptance.[2]

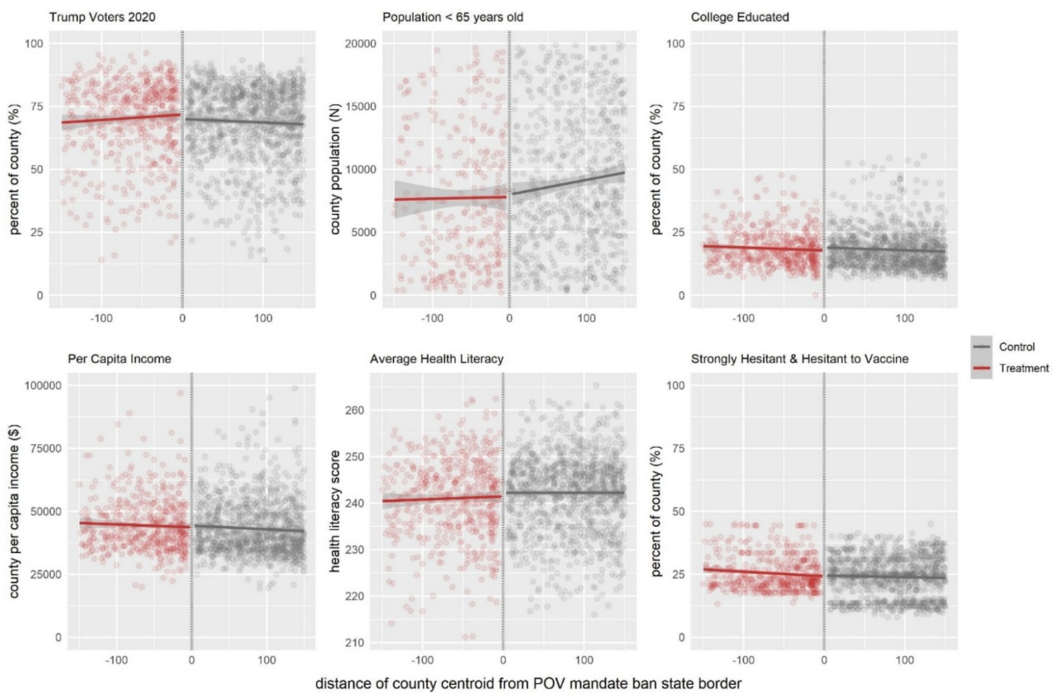

Figure S1. GRD design graphs showing continuity across the threshold of the running variable, county centroid distance, in miles, from the proof-of-vaccine (POV) mandate ban state border, among confounding factors for the executive order sample.

Further, we also analyzed the effect of POV mandate bans using a "full county" sample (not to be confused with the full sample in the main analysis that combined executive order and legislative samples). The full county sample included all counties within treatment and control states that had complete 1st-dose COVID-19 vaccination data. We still selected only neighboring states because of the staggered policy adoption with POV mandate bans and removed neighboring states that had already enacted a POV mandate ban to control for early vs. late adopter comparisons.[27] In the full county sample, only the first bimonthly time period post-POV mandate ban enactment is significant for executive order treatment and control states (δ_1 = 28.6%; p-value = 0.020). Table S1 shows the TWFEE results for executive order and legislative POV mandate ban samples.

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Table S1. Two-way fixed effects estimation (TWFEE) coefficients and standard errors (se) for percent (%) change in bimonthly, 1st-dose COVID-19 vaccinations across executive order and legislative proof-of-vaccine (POV) mandate bans for full-county samples.

	Executive Order Sample		Legislative Sample	
	Coefficient (se)	p-value	Coefficient (se)	p-value
Variables				
Treatment Counties * Post POV Mandate Ban (δ_p)				
Pre-period (ref)	--	--	--	--
Weeks 1–2	28.6% (11.5%)	0.020*	1.45% (18.3%)	0.94
Weeks 3–4	21.1% (12.2%)	0.10	10.9% (18.5%)	0.56
Weeks 5–6	20.6% (17.3%)	0.24	20.5% (24.4%)	0.41
Weeks 7–8	25.5% (17.9%)	0.17	13.2% (24.8%)	0.60
Weeks 9–10	4.52% (17.9%)	0.80	20.4% (27.8%)	0.47
Weeks 11–12	17.3% (15.3%)	0.27	20.6% (30.0%)	0.47
*P-value significant at the 0.05 level, ** 0.01 level, and *** <0.001 level.				

However, we believe the estimates for later bimonthly time periods are no longer significant among executive order counties because the full county sample included counties that were not comparable to those in POV mandate ban states. This was shown when looking at characteristic differences between treatment and control counties for the main model GRD sample and the full county sample (Table S2). Further, our GRD design selection mechanism relies on evidence showing spatial proximity as predictors of vaccine uptake and removes investigator bias and potential unknown confounding that may bias other methods for selecting comparable treatment and control groups (i.e. propensity score matching).[22]

Table S2. Averages and t-test p-values for time-constant factors related to COVID-19 vaccinations for geographic regression discontinuity (GRD) and full-county samples.

	GRD Sample		p-value	Full-County Sample		p-value
	Treatment	Control		Treatment	Control	
Trump votes 2020 (% of county)	69.6%	64.5%	<0.001***	69.4%	60.9%	<0.001***
Population Size (<65-year-olds; N)	67,078	73,091	0.59	68,670	97,750	0.012*
Health Literacy Scores	243.6	244.0	0.22	242.5	244.2	<0.001***
Proportion ≥65 years old (%)	18.6%	18.7%	0.67	18.8%	18.8%	0.94
College Degree (%)	18.3%	19.2%	0.006**	18.1%	20.2%	<0.001***
Per Capita Income 2019 ($)	$44,692	$44,818	0.79	$44,367	$46,250	<0.001***
*P-value significant at the 0.05 level, ** 0.01 level, and *** <0.001 level.						

Appendix A.2. Study Period

In addition to testing various study samples, we also tested various study periods with a specific emphasis on various pre-period inclusions. We limited our main analysis study period to eight weeks pre-POV mandate ban enactment. This was to accommodate early POV mandate ban adopters with pre-periods extending into January 2021, which had limited data availability and access for persons under 65 years old and would present a downward bias for these POV mandate ban comparisons. Further, for those counties with data, the population under 65 years old that had access to COVID-19 vaccines in January 2021 were essential workers, immunocompromised, and not representative of the general population.[34] However, when testing this in the executive order sample we still found that the significant positive effects remains for the first two time periods post POV mandate ban when including up to 12 weeks pre POV mandate ban enactment (Weeks 1–2; δ_1 = 26.0%; standard errors

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(se) = 9.69%; p-value = 0.012 and Weeks 3–4; δ_2 = 28.5%; se = 11.5% ; p-value = 0.019). This was also true when restricting the study pre-period up to four weeks pre POV mandate ban enactment.

Appendix A.2. Study Period

We performed many sensitivity analyses on our main model specification, including the tests shown above to determine our sample and study period. First, we looked at differing post-period time points, including weekly and monthly values. The main model specifications looked at bimonthly time periods post POV mandate ban to smooth out delays in reporting and inaccuracies that were present even when aggregated to weekly values. Table S3 shows the interaction term δ_p with weekly time periods (n = 14) for treatment counties. The weekly time periods for the executive order sample showed alternating significance for the percent change in weekly, 1st-dose COVID-19 vaccinations and no change in significance for the legislative sample. Part of our rationale to aggregate weekly values into bimonthly estimates was due to the inconsistent nature of COVID-19 vaccination reporting in the first half of 2021.[17] Further, when aggregated into monthly time periods, we saw significant estimates similar to those when using bimonthly estimates (Table S4).

Table S3. Two-way fixed effects estimation (TWFEE) coefficients and standard errors (se) for percent (%) change in weekly, 1st-dose COVID-19 vaccinations across executive order and legislative proof-of-vaccine (POV) mandate bans samples using weekly time periods.

	Executive Order Sample		Legislative Sample	
	Coefficient (se)	p-value	Coefficient (se)	p-value
Variables				
Treatment Counties * Post POV Mandate Ban (δ_p)				
Pre-period (ref)	--	--	--	--
Week 1	48.3% (13.0%)	0.001**	-1.41% (17.2%)	0.94
Week 2	17.4% (16.5%)	0.30	-9.08% (19.2%)	0.64
Week 3	49.5% (14.3%)	0.002**	-5.08% (16.0%)	0.75
Week 4	16.9% (15.3%)	0.28	13.0% (20.3%)	0.53
Week 5	39.5% (15.9%)	0.019*	25.3% (24.5%)	0.31
Week 6	30.2% (12.9%)	0.026*	-9.32% (24.5%)	0.71
Week 7	32.8% (14.9%)	0.036*	-8.17% (25.6%)	0.75
Week 8	42.5% (17.4%)	0.021*	16.3% (25.1%)	0.52
Week 9	13.3% (20.2%)	0.52	35.3% (26.2%)	0.19
Week 10	40.0% (17.2%)	0.027*	5.45% (31.3%)	0.86
Week 11	19.7% (13.8%)	0.16	20.0% (29.3%)	0.50
Week 12	37.0% (15.2%)	0.021*	18.6% (28.4%)	0.52
*P-value significant at the 0.05 level, ** 0.01 level, and *** <0.001 level.				

Table S4. Two-way fixed effects estimation (TWFEE) coefficients and standard errors (se) for percent (%) change in monthly, 1st-dose COVID-19 vaccinations across executive order and legislative POV mandate bans samples using monthly time periods.

	Executive Order Sample		Legislative Sample	
	Coefficient (se)	p-value	Coefficient (se)	p-value
Variables				
Treatment Counties * Post POV Mandate Ban (δ_p)				
Pre-period (ref)	--	--	--	--
Weeks 1–4	33.4% (10.1%)	0.002**	-0.738% (15.0%)	0.96
Weeks 5–8	36.6% (14.6%)	0.018*	5.95% (21.8%)	0.79
Weeks 9–12	25.4% (13.7%)	0.07	19.3% (26.8%)	0.48
*P-value significant at the 0.05 level, ** 0.01 level, and *** <0.001 level.				

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In addition, we conducted sensitivity checks to confirm that our model was robust to varying designated distances to select treatment and control counts and potential spillover effects (Table S5 & S6). We tested TWFEE for county samples within 125, 100, and 75 miles of the POV mandate ban state border for both legislative and executive order samples. We see largely the same results as the main model, with the exception that Weeks 5–6 is no longer significantly different for the 100-mile buffer in the executive order sample. We also tested the potential for spillover effects. These sensitivity tests removed counties with county centroid distances within 5, 10, and 25 miles from the POV mandate ban state border. We saw no changes in the estimate's significance for either executive order or legislative samples, suggesting that our results were robust to potential spillover bias. Estimates were also assessed when removing multiple county controls. The estimates for the executive order sample were similar in significance for all bimonthly time periods, except that Weeks 9–10 were also significantly different than the pre-POV mandate ban period compared to controls (δ_5 = 39.3%; se = 18.6%; p-value = 0.044) when we removed multiple controls.

Table S5. Two-way fixed effects estimation (TWFEE) coefficients and standard errors (se) for percent (%) change in bimonthly, 1st-dose COVID-19 vaccinations across executive order and legislative proof-of-vaccine (POV) mandate ban samples for varying geographic regression discontinuity (GRD) distance buffers.

	125 miles		100 miles		75 miles	
	Coefficient (se)	p-value	Coefficient (se)	p-value	Coefficient (se)	p-value
Executive Order POV Mandate Ban States						
Interaction Term (δ_p)						
Weeks 1–2	33.0% (9.77%)	0.002**	31.4% (9.80%)	0.003**	32.8% (9.75%)	0.002**
Weeks 3–4	36.5% (11.3%)	0.003**	35.9% (12.3%)	0.007**	40.5% (12.3%)	0.003**
Weeks 5–6	34.9% (15.5%)	0.032*	32.5% (17.0%)	0.07	34.4% (16.5%)	0.046*
Weeks 7–8	39.5% (16.4%)	0.023*	40.8% (17.5%)	0.027*	44.8% (16.8%)	0.013*
Weeks 9–10	34.3% (16.5%)	0.047*	29.9% (17.9%)	0.11	36.6% (17.9%)	0.05
Weeks 11–12	39.1% (17.5%)	0.034*	38.3% (19.2%)	0.06	36.6% (17.8%)	0.049*
Legislative POV Mandate Ban States						
Interaction Term (δ_p)						
Weeks 1–2	-8.65% (15.5%)	0.58	-12.0% (14.8%)	0.42	-12.4% (14.2%)	0.39
Weeks 3–4	1.14% (16.9%)	0.95	-1.09% (16.5%)	0.95	-2.74% (15.7%)	0.86
Weeks 5–6	5.08% (20.8%)	0.81	3.15% (19.9%)	0.88	1.29% (19.4%)	0.95
Weeks 7–8	0.526% (23.8%)	0.98	-1.81% (23.3%)	0.94	-6.87% (23.6%)	0.77
Weeks 9–10	18.3% (25.6%)	0.48	17.9% (25.0%)	0.48	14.0% (25.2%)	0.58
Weeks 11–12	17.5% (28.1%)	0.54	15.7% (27.7%)	0.57	11.0% (27.7%)	0.70
*P-value significant at the 0.05 level, ** 0.01 level, and *** <0.001 level.						

Table S6. Two-way fixed effects estimation (TWFEE) coefficients and standard errors (se) for percent (%) change in weekly, 1st-dose COVID-19 vaccinations across executive order and legislative proof-of-vaccine (POV) mandate ban samples for varying spillover buffer.

	5-mile buffer		10-mile buffer		25-mile buffer	
	Coefficient (se)	p-value	Coefficient (se)	p-value	Coefficient (se)	p-value
Executive Order POV Mandate Ban States						
Interaction Term (δ_p)						

Weeks 1–2	32.6% (9.83%)	0.002**	32.3% (9.58%)	0.002**	35.5% (9.44%)	0.001**
Weeks 3–4	34.5% (11.3%)	0.005**	34.3% (10.9%)	0.004**	38.2% (11.6%)	0.003**
Weeks 5–6	35.0% (14.5%)	0.022*	34.3% (14.4%)	0.024*	35.6% (14.5%)	0.020*
Weeks 7–8	36.9% (15.6%)	0.025*	36.0% (15.6%)	0.028*	39.0% (15.1%)	0.015*
Weeks 9–10	26.9% (16.7%)	0.12	25.1% (15.9%)	0.126	26.9% (16.2%)	0.11
Weeks 11–12	28.2% (13.9%)	0.05	28.1% (13.9%)	0.052	29.4% (13.0%)	0.031*
Legislative POV Mandate Ban States						
Interaction Term (δ_p)						
Weeks 1–2	-5.10% (16.9%)	0.77	-3.60% (17.7%)	0.84	1.30% (19.8%)	0.95
Weeks 3–4	4.11% (17.9%)	0.82	6.91% (17.7%)	0.70	10.9% (19.9%)	0.59
Weeks 5–6	7.96% (22.3%)	0.72	10.2% (22.1%)	0.65	14.0% (25.0%)	0.58
Weeks 7–8	4.13% (24.7%)	0.87	6.87% (24.4%)	0.78	9.24% (25.5%)	0.72
Weeks 9–10	20.1% (26.7%)	0.46	21.9% (26.4%)	0.41	23.9% (28.0%)	0.40
Weeks 11–12	19.4% (28.8%)	0.51	22.6% (28.7%)	0.44	24.8% (29.6%)	0.41
*P-value significant at the 0.05 level, ** 0.01 level, and *** <0.001 level.						

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