



COVID-19 vaccination mandates and vaccine uptake

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We evaluate the impact of government-mandated proof of vaccination requirements for access to public venues and non-essential businesses on COVID-19 vaccine uptake. We find that the announcement of a mandate is associated with a rapid and significant surge in new vaccinations (a more than 60% increase in weekly first doses), using the variation in the timing of these measures across Canadian provinces in a difference-in-differences approach. Time-series analysis for each province and for France, Italy and Germany corroborates this finding. Counterfactual simulations using our estimates suggest the following cumulative gains in the vaccination rate among the eligible population (age 12 and over) as of 31 October 2021: up to 5 percentage points (p.p.) (90% confidence interval, 3.9–5.8) for Canadian provinces, adding up to 979,000 (425,000–1,266,000) first doses in total for Canada (5 to 13 weeks after the provincial mandate announcements); 8 p.p. (4.3–11) for France (16 weeks post-announcement); 12 p.p. (5–15) for Italy (14 weeks post-announcement) and 4.7 p.p. (4.1–5.1) for Germany (11 weeks post-announcement).

Immunization has proved very effective for reducing the spread and severity of COVID-19, with large reductions in the risk of severe outcomes for vaccinated people^{1–4}. Yet, following rapid uptake in early 2021, vaccinations in many countries (Extended Data Fig. 1) slowed down notably in the summer months. In addition, even locations with high vaccination rates experienced increased viral transmission or had to maintain or reintroduce non-pharmaceutical interventions such as mask wearing or indoor capacity limits in fall 2021 because of the elevated reproduction rate of the Delta variant.

Achieving high COVID-19 vaccination coverage is therefore essential for reducing the health and economic impacts of the epidemic. Moreover, booster doses or vaccines with updated formulations may be necessary in the face of new variants. Public health authorities throughout the world have sought effective strategies to increase vaccine uptake, especially among the hesitant or procrastinating.

In response to this challenge, various local or national governments have introduced proof-of-vaccination mandates or certificates^{5–7}, which allow vaccinated persons to attend non-essential sports or social settings and events such as concerts, stadiums, museums, restaurants and bars. The goal of these policies is twofold: to provide incentives for immunization and to reduce viral transmission in risky indoor or crowded settings.

We evaluate and quantify the effect of proof-of-vaccination mandates on first-dose vaccine uptake in the ten Canadian provinces and three European countries (France, Italy and Germany). Some mandates accept a recent negative or past positive COVID test result as a substitute for vaccination or allow businesses to opt out if they abide by additional restrictions. Among the jurisdictions we consider, the mandates in France, Italy, Germany, Alberta and Saskatchewan allowed such options during the studied period. We chose these locations because they have similar economies, demographics and vaccine access, and all announced and implemented mandates in July–October 2021 (Supplementary Table 1), a period with minimal binding vaccine supply or access constraints and a high base first-dose vaccination rate (above 60% of those eligible

in the three countries and above 80% in Canada at the time of the mandate announcements; Supplementary Table 2). Hence, we evaluate the mandates' impact on people that have remained unvaccinated for weeks or months after immunization was available to them, such as the vaccine hesitant (Supplementary Table 3).

Given past evidence from HPV, Tdap and hepatitis A immunizations^{8–10}, requiring proof of vaccination is expected to raise vaccine uptake, but the magnitude and speed of the increase are hard to predict: they depend on the relative importance of the factors leading to delay or hesitancy, such as lack of social or economic incentives, misinformation, or entrenched political or religious beliefs. We use first doses as the main outcome in our statistical analysis because they most directly reflect the decision to be immunized.

Figure 1 plots the weekly vaccine first doses administered in the four most populous Canadian provinces and four European countries. All except Spain introduced a province- or country-wide mandate in the studied period. We observe a sizable boost in vaccine uptake after the mandate announcement (the dashed vertical line) in all four provinces and in France, Italy and Germany, often in contrast to a sharp decline in the pre-announcement weeks. In France, daily first-dose appointments also show a striking surge on the day after the mandate announcement (Extended Data Fig. 2). In contrast, Spain exhibits a steady decrease in weekly first doses over the displayed period.

Motivated by this evidence, we address two important policy-relevant questions. We first estimate the magnitude of the increase in first-dose vaccinations after a mandate announcement, controlling for other possible factors. Second, we evaluate how long these vaccination gains persist and the cumulative effect on vaccine uptake. We do not address ethical considerations in this paper¹¹. Our goal is to assess the mandates' effectiveness purely in terms of raising vaccine uptake, which can then be weighed against various political or enforcement costs and compared to the effectiveness of other policies, such as financial incentives (cash, gift cards, lotteries and so on)^{12–17} or behavioural nudges (for example, messages from experts or appointment reminders)^{12,18–20}, for which mixed or negative results have been reported.

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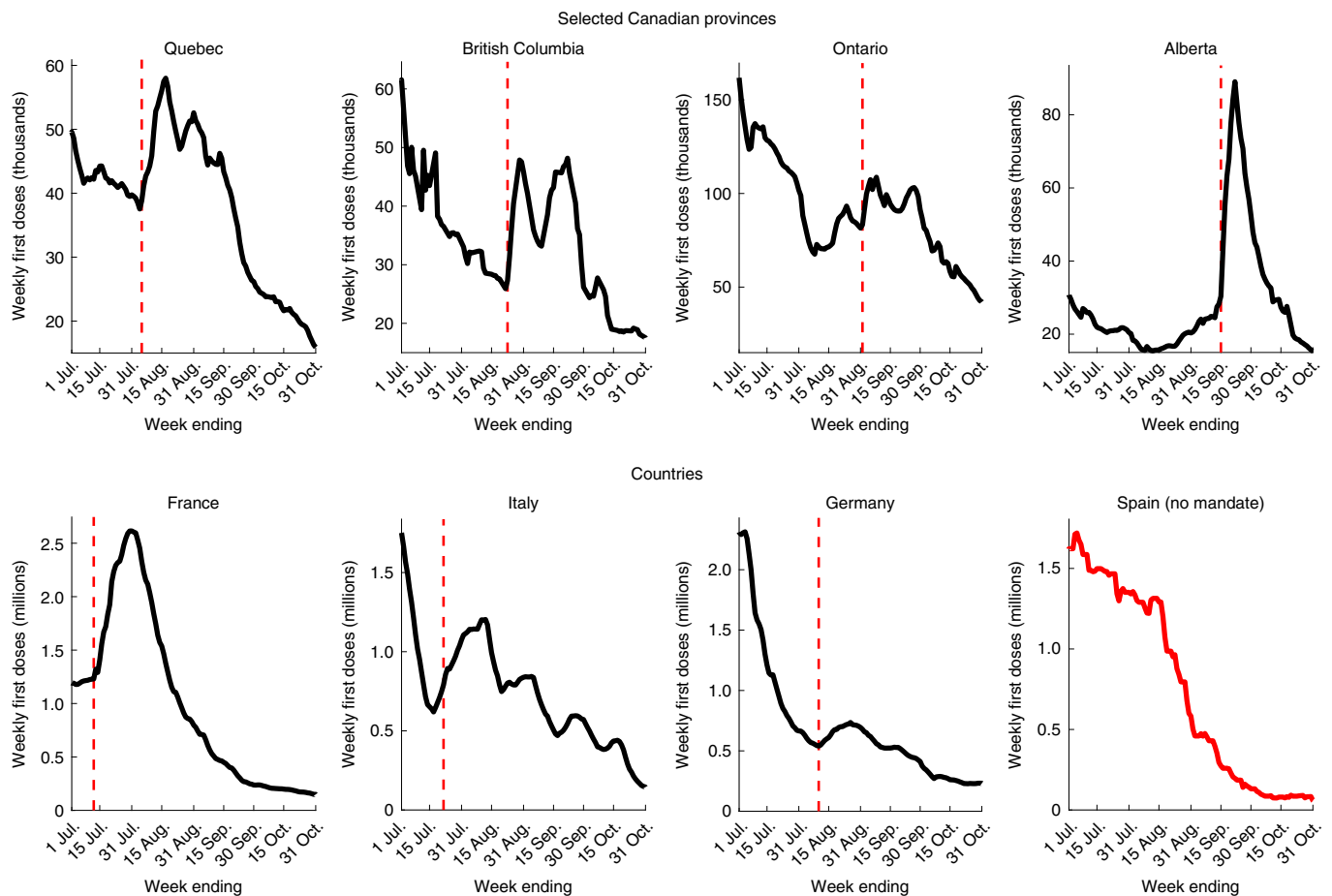


Fig. 1 | Vaccination mandates and first-dose uptake. Weekly first doses of COVID-19 vaccines administered for dates $t - 6$ to t , where t is the date on the horizontal axis. All displayed dates refer to 2021. The vertical dashed lines denote the announcement dates of the proof-of-vaccination mandates (for countries, this is the date of a national mandate). Spain had not announced a national proof-of-vaccination mandate as of 31 October 2021. We show the four most populated Canadian provinces, totalling about 87% of Canada's population (see Extended Data Figs. 3 and 4 for all ten provinces). Alberta also had a Canadian \$100 debit card incentive for doses received between 3 September and 14 October 2021.

To quantify the mandates' impact on vaccine uptake, we use Canadian provincial data in a difference-in-differences (DID) identification strategy based on the time variation in mandate announcement dates across different geographic units in the same country, Canada (from 5 August 2021 in Quebec to 21 September 2021 in Prince Edward Island; Supplementary Table 1 and Extended Data Fig. 3). In Canada, the provinces are separate jurisdictions with extensive powers over health policy, while the vaccines are procured by the federal government and allocated in proportion to provincial population. In contrast, the French, Italian and German mandates apply at the national level, which makes it more challenging to separate their effects from those of time trends or other concurrent events or policies.

We estimate a behavioural model in which the decision to receive a COVID-19 vaccine, measured by new weekly first doses, is affected by the policy setting (whether a proof-of-vaccination mandate has been announced) and the current COVID-19 epidemiological and public health conditions ('information'), proxied by weekly cases and deaths^{21,22}. We control for other potential confounding variables and unobserved heterogeneity with time and location (that is, province) fixed effects (see Methods for the details).

We complement and extend the DID results for Canada with a structural-break and time-series analysis, which allows us to study the mandates' impact on vaccine uptake over a longer period.

We obtain individual policy impact estimates for each Canadian province and for France, Italy and Germany, and identify potential factors contributing to the heterogeneity of the estimates, including the time between the mandate announcement and implementation and the fraction of the population that is already vaccinated at the time of announcement. We then use the time-series estimates in counterfactual simulations and compute the cumulative gains in vaccine uptake following the mandates over our study period ending 31 October 2021, relative to a hypothetical scenario in the absence of mandates.

Results

Figure 2 plots the raw-data time profile of weekly first doses after a proof-of-vaccination mandate announcement, with doses in the week ending on the announcement date normalized to 100. The figure shows that weekly first doses in the Canadian provinces and in France, Italy and Germany grow quickly, reach a peak one to five weeks after the announcement date and then decrease, as in the pre-announcement trend in most locations (Extended Data Fig. 4). New Brunswick, Newfoundland, Alberta, Nova Scotia, Saskatchewan and France registered increases in vaccine uptake of over 100% relative to the pre-announcement week. In other locations, such as Quebec, Ontario, Manitoba and Germany, the observed increase is more moderate, less than 50%.

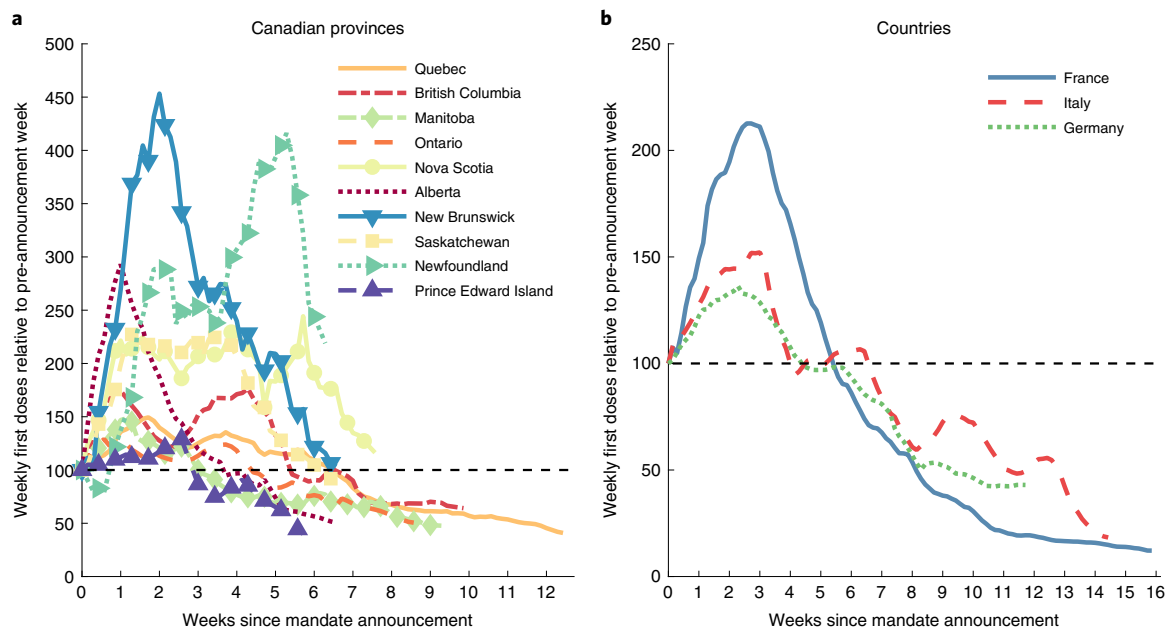


Fig. 2 | First doses after mandate announcement. **a, b**, Weekly administered first doses of COVID-19 vaccine for all dates after the mandate announcement against the number of weeks since the respective announcement date (denoted by 0 on the horizontal axis), as of 31 October 2021. The weekly first doses for the week just prior to the mandate announcement are normalized to 100 for each respective province (**a**) or country (**b**) (dashed lines).

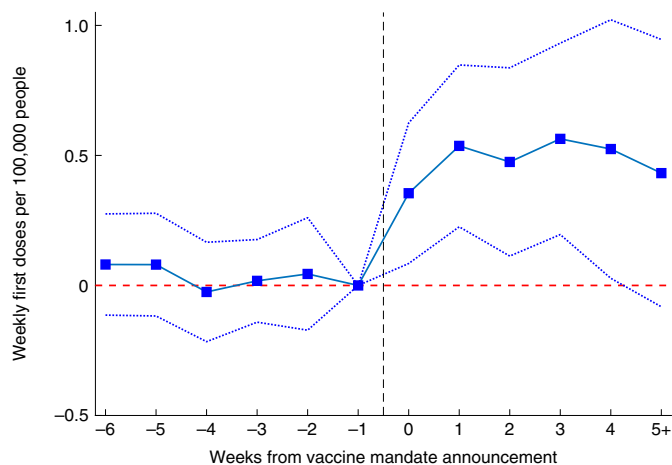


Fig. 3 | Canadian provinces—event study. Sun and Abraham²³ treatment effect heterogeneity robust estimates (Methods). The sample period is 15 June to 14 September 2021, using Alberta, New Brunswick, Saskatchewan, Prince Edward Island and Newfoundland as the control group (latest treated). The outcome variable, V_{it} , is log weekly first doses per 100,000 people administered for dates $t - 6$ to t inclusive. The figure plots the estimates (denoted by squares) from a variant of equation (1) where the mandate announcement variable P_{it} is replaced by the interaction of being in the ‘treatment’ group (announced mandate) with a series of dummies for each week ranging from six weeks before ($T = -6$) to five or more weeks after the announcement ($T = 5$), where $T = 0$ denotes the week starting at the announcement date. The reference point is one week before the announcement ($T = -1$). The dotted lines correspond to 95% CIs.

Canadian provinces—DID analysis. We use a DID statistical method, the Sun and Abraham²³ treatment effect heterogeneity robust estimator (Methods), to study the average effect of proof of vaccination mandates in Canada. In Fig. 3, we plot the results from

an event study analysis of weekly first doses administered in the ten Canadian provinces, from six weeks before to five or more weeks after the announcement of a mandate. We use as a control group (latest treated) the last five provinces to announce a mandate (Alberta, New Brunswick, Saskatchewan, Newfoundland and Prince Edward Island). We chose this control group as the latest-in-time representative set of provinces containing a mix of smaller and larger provinces by population. This choice implies 15 June 2021 to 14 September 2021 (the day before mandate announcement in Alberta and New Brunswick; Supplementary Table 1) as our baseline DID time period. We present results with different control groups corresponding to earlier or later sample end dates in Supplementary Table 4. First, Fig. 3 shows a lack of mandate-associated pre-trend in the data—the DID estimates before the mandate announcement are statistically indistinguishable from zero. This addresses the potential endogeneity concern (parallel trends assumption) that provinces that announced a mandate may have had a different trend in first-dose vaccinations than provinces that did not announce a mandate. Second, the impact of the mandate announcement on first-dose vaccine uptake is realized relatively quickly and is large in magnitude—an increase of 42% (35 log points; $P = 0.015$; 95% confidence interval (CI), 9–62) in new weekly doses in the first post-announcement week and 71% (54 log points; $P = 0.001$; 95% CI, 21–86) in the second week, each relative to one week before the announcement. The observed quick increase in uptake mitigates possible concerns that vaccine supply or scheduling constraints may be affecting our results. Third, the policy effect persists over the six-week post-announcement period that we analyse ($T = 0$ to $T = 5$). Unfortunately, data limitations (the timing of the announcements) and the need for a not-yet-treated control group in the DID method do not allow us to investigate longer horizons.

Table 1 displays DID estimates of the mandate effect on first-dose uptake, relative to the pre-mandate period, controlling for information (cases and deaths) and time and location fixed effects. Columns 1 and 2 show that a mandate announcement is associated with an average increase of about 66% (50.6 log points; $P = 0.001$; 95% CI, 25–77; in column 2) in weekly first doses. In column 3, we report

Table 1 | Canadian provinces—DID estimates

Outcome: log weekly vaccine first doses, V_{it}			
	(1)	(2)	(3)
Mandate announced, P_{it}	0.504*** (0.002)	0.506*** (0.001)	
Week 0			0.359*** (0.005)
Week 1			0.543*** (0.001)
Week 2			0.498** (0.010)
Week 3			0.705*** (0.001)
Week 4			0.713** (0.018)
Week 5+			0.651* (0.056)
log weekly cases, C_{it}		0.002 (0.967)	0.002 (0.958)
log weekly deaths, D_{it}		0.048 (0.365)	0.046 (0.415)
R^2	0.817	0.820	0.821
Sample size, N	920	920	920
Province fixed effects	X	X	X
Date fixed effects	X	X	X

Sun and Abraham²³ treatment effect heterogeneity robust estimates. Columns 1 and 2 report the average mandate effect estimate over the post-announcement period, without and with controlling for weekly cases and deaths. Column 3 decomposes the effect by post-announcement week. The sample period is 15 June to 14 September 2021, using Alberta, New Brunswick, Saskatchewan, Prince Edward Island and Newfoundland as the control group (latest treated). Week n , where $n=0,1,2,\dots$, is a binary variable that takes value 1 for the days in the n th week immediately after the announcement date (week 0 is the week starting at the announcement date) and value 0 otherwise. The C_{it} and D_{it} variables are log weekly totals for dates $t-6$ to t . P values from wild bootstrap (boottest) standard errors clustered by province with 4,999 repetitions are reported in parentheses; *, ** and *** denote 10%, 5% and 1% significance (two-sided), respectively. A coefficient estimate x is equivalent to 100 x log points or a 100($e^x - 1$) per cent increase in weekly doses. The results in column 3 are not directly comparable to Fig. 3 because in Table 1 the reference point is the entire pre-announcement period. The column 3 estimates for week 4 and onwards are based on post-mandate vaccinations in Quebec, the province with the earliest announcement. The smaller $T=0$ estimate for week 0 reflects the fact that the outcome variable is a backward-looking weekly sum.

estimates where the mandate policy variable is decomposed into six binary variables (one for each week after the announcement date) to account for dynamic effects. The results indicate a sharp increase in weekly first doses of 43% (36 log points; $P=0.005$; 95% CI, 14–58) in the week beginning at the announcement date (week 0). The increase is sustained throughout the post-announcement sample period, as all estimates in column 3 are positive ($P<0.06$) and larger than the week 0 estimate. The DID analysis thus yields no evidence that short-term intertemporal substitution (which would be manifested as negative estimates in the later weeks) is the mechanism behind the observed boost in first-dose vaccinations in the studied period.

We perform a range of robustness checks and sensitivity analysis on our main DID results. Our result of a more than 60% increase in weekly first doses on average over the post-announcement weeks remains robust when using alternative dates and treatment groups varying from the first three to the first nine provinces to announce a mandate (Supplementary Table 4 and Extended Data Fig. 5). We also allow for alternative initial sample dates or lags in the policy or

information variables (Extended Data Fig. 5). Additional robustness checks include using different control variables (hospitalizations), population weights, ordinary least squares (OLS) two-way fixed effects (TWFE), daily data, levels data, alternative ways to compute the standard errors and using randomized (placebo) announcement dates (see Extended Data Fig. 6, Supplementary Figs. 1 and 2, and Supplementary Tables 5 and 6 for the details). A possible concern is that case or death counts may be correlated with the mandate announcements (while also affecting vaccination rates as information, for which we control). We ran event-study analyses analogous to that in Fig. 3 but using weekly cases or deaths as the outcome and find that the estimates before the announcements are statistically indistinguishable from zero (Supplementary Fig. 3). This suggests the absence of significant differential pre-trends in cases or deaths across the treated and control provinces.

Most provincial proof-of-vaccination mandates required two doses to be considered adequately vaccinated with a vaccine offered in Canada during the study period: Pfizer (Comirnaty), Moderna (SpikeVax) or AstraZeneca (Vaxzevria); there were limited exceptions in Quebec (a prior infection could count as a first dose) and British Columbia (only one dose was required between 13 September and 23 October 2021). In Supplementary Table 7, we use the log of weekly second doses as the outcome. We do not find a statistically significant effect of mandate announcements on second-dose uptake in our sample period, consistent with second doses being more evenly spread out over time. Extended Data Fig. 7 also shows that, unlike the sharp boost in first doses, there are only small or gradual post-mandate increases in second doses in some of the provinces. One possible explanation for the lack of large increases in second doses a few weeks after the spikes in first doses is that mandates may have encouraged some people that already had their first doses to obtain their second dose sooner, thus shifting some second doses forward and dampening the lagged effect.

We also do not find statistically significant effects on first-dose uptake associated with the mandates' implementation dates additional to the announcement date effect; see the Supplementary Information for the details.

Time-series analysis. We complement and extend the DID panel-data results with a structural break and time-series analysis using the “interrupted time-series analysis” method²⁴, which models the relationship between the outcome (weekly first doses) and the policy variable (mandate announcement) and controls for time trends, lagged outcomes and information (weekly cases and deaths); see Methods for the details. This allows us to estimate the mandates' longer-term impact on vaccine uptake separately for each of the ten Canadian provinces, as well as for France, Italy and Germany, using all the data up to 31 October 2021.

We first test for the presence of a structural break at the announcement date and find that we can reject the null hypothesis of no break for each of the provinces and countries. See Supplementary Table 8 and Methods for the details.

In Table 2, we report the time-series estimates for each country and province in our data. In columns 1–3, we report the estimated coefficients on the mandate announcement variable, $\hat{\pi}$, and on two time trends: a linear daily time trend (estimate $\hat{\tau}_1$) and an ‘interaction’ time trend reflecting the post-mandate trend slope change (estimate $\hat{\tau}_2$). Since the outcome and information variables are level variables, in column 4, we use the augmented Dickey–Fuller (ADF) unit root test to test whether the residuals are stationary, to avoid a spurious regression. We reject the null hypothesis that the residual contains a unit root at the 95% confidence level for all locations except Newfoundland, where we can reject the null at the 90% level.

The estimates $\hat{\pi}$ for the initial rise in weekly first doses after mandate announcement in Table 2 are large and statistically

Table 2 | Time series estimates

Outcome: log weekly vaccine first doses

	Policy, $\hat{\pi}$ (1)	Time trend, $\hat{\tau}_1$ (2)	Trend change, $\hat{\tau}_2$ (3)	ADF test (4)
Countries				
France	0.154*** (0.004)	-0.018*** (0.000)	0.003 (0.229)	-3.970 (0.002)
Italy	1.025*** (0.000)	-0.029*** (0.001)	0.005 (0.556)	-3.365 (0.012)
Germany	0.438*** (0.000)	-0.017*** (0.000)	0.006** (0.012)	-3.235 (0.018)
Canadian provinces				
Quebec	0.064 (0.647)	-0.029** (0.010)	0.017* (0.079)	-5.192 (0.000)
British Columbia	0.734*** (0.000)	-0.013*** (0.008)	-0.004 (0.400)	-4.587 (0.000)
Manitoba	0.757*** (0.000)	-0.034*** (0.000)	0.012** (0.014)	-4.735 (0.000)
Ontario	0.293*** (0.007)	-0.011** (0.033)	0.002 (0.739)	-4.166 (0.001)
Nova Scotia	0.595*** (0.002)	-0.025*** (0.000)	0.021** (0.021)	-4.698 (0.000)
Alberta	1.455*** (0.000)	-0.015*** (0.000)	-0.024*** (0.000)	-3.372 (0.012)
New Brunswick	1.313*** (0.000)	-0.011** (0.019)	-0.017*** (0.004)	-4.416 (0.000)
Saskatchewan	0.898*** (0.000)	-0.008*** (0.001)	-0.013** (0.013)	-3.800 (0.003)
Newfoundland	0.410* (0.075)	-0.007 (0.241)	0.005 (0.742)	-2.689 (0.076)
Prince Edward Island	0.378** (0.010)	-0.002 (0.565)	-0.014** (0.019)	-2.904 (0.045)
Sample size for each row	139	139	139	139

The time period is 15 June to 31 October 2021. Each row is a separate time-series regression as specified in equation (2). All rows include 7-day and 14-day lags of the outcome variable and log weekly deaths D_t and log weekly cases C_t as information, I_t . Column 1 reports the estimate $\hat{\pi}$ on the mandate announcement policy variable P_t in equation (2). Column 2 reports the estimate $\hat{\tau}_1$ on the linear time trend T_t . Column 3 reports the estimate $\hat{\tau}_2$ of the post-announcement trend slope change. P values computed using Newey–West heteroskedasticity and autocorrelation robust standard errors with three lags are shown in parentheses; *, ** and *** denote 10%, 5% and 1% significance (two-sided), respectively. Column 4 reports ADF one-sided test statistics and P values using 13 lags chosen as the integer part of $12(T/100)^{0.4}$; see ref. 43.

significantly positive for all three countries: 17% for France (15.4 log points; $P=0.004$; 95% CI, 5.0–25.8), 179% for Italy (103 log points; $P<0.001$; 95% CI, 53–152) and 55% for Germany (43.8 log points; $P<0.001$; 95% CI, 31.4–56.1), relative to the respective pre-mandate trends. A comparison of Fig. 1 and Extended Data Fig. 2 suggests that the relatively low French estimate may reflect a lag between appointment booking and vaccine administration. We also obtain statistically significantly positive and large estimates of the increase in weekly first doses after mandate announcement, $\hat{\pi}$, for all provinces except Quebec. The estimate for Quebec is 0.389 ($P<0.001$; 95% CI, 0.265–0.514) and statistically significantly different from zero when we use deaths and hospitalizations as information;

see Supplementary Table 9. The estimated mandate effect varies across the provinces—for example, a 34% (29 log points; $P=0.007$; 95% CI, 8.2–50.5) initial increase in Ontario versus 326% (145 log points; $P<0.001$; 95% CI, 110–181) in Alberta. The range of the provincial estimates is consistent with our DID estimate in Table 1 for the average mandate effect for Canada; however, the two estimation methods are not directly comparable.

The estimated mandate effect on vaccine uptake tends to be larger for provinces experiencing a surge in cases at the same time (for which we control)—namely, Alberta, Saskatchewan and New Brunswick (Extended Data Fig. 4). These provinces also announced their mandates relatively late (mid-September) and set a relatively short time interval between mandate announcement and implementation (Supplementary Table 1). A larger fraction of eligible unvaccinated people at the time of mandate announcement is also positively associated with a larger policy effect estimate. We illustrate these associations in Extended Data Fig. 8.

The baseline time trend in first doses is downward sloping ($\hat{\tau}_1 < 0$) in all countries and provinces (although not statistically significant in the two smallest provinces, Newfoundland and Prince Edward Island) and indicates a steady decrease in new vaccinations of 0.8% to 3.4% per day in the studied period in the absence of a mandate. The post-mandate announcement trend in first doses turns less steep in the three countries (the estimate $\hat{\tau}_2$ is positive, although statistically significant only for Germany), which suggests a lack of net intertemporal substitution as of 31 October. However, the estimates $\hat{\tau}_2$ are statistically significantly negative for Alberta, New Brunswick, Saskatchewan and Prince Edward Island, which is consistent with the mandate effect on new first doses diminishing over time in these provinces.

In Supplementary Table 9, we report additional results from two alternative time-series specifications: using weekly deaths and average hospitalizations as information control variables or using a binary, instead of weekly-averaged, policy variable. The results are very similar to those in Table 2 and confirm the large and positive estimated effect of mandate announcements on first-dose vaccine uptake. We provide further details on the time-series model fit and out-of-sample projections in the Supplementary Information.

Counterfactuals. We use our estimates of the mandate effect on vaccine uptake from Table 2 to compute the cumulative increase in new vaccinations for each province and for France, Italy and Germany, relative to the counterfactual scenario of the absence of a mandate. Counterfactual weekly and cumulative doses are computed as explained in the Methods. We report the vaccinations gains both in levels (million doses) and in percentage points (p.p.) relative to the vaccination rate at the mandate announcement date.

We find large cumulative increases in the first-dose vaccination rate for all provinces relative to in the absence of a mandate: from 1.9 p.p. (90% CI, -0.3–3.0) in Ontario to 5 p.p. (90% CI, 4.0–5.6) in Saskatchewan and 5 p.p. (90% CI, 3.9–5.8) in New Brunswick, with all other provinces in between, as of 31 October 2021 (Figs. 4 and 5). These estimated total gains in uptake add up to 2.9 p.p. (90% CI, 1.3–3.8) of the eligible population or 979,000 new first doses (90% CI, 425,000–1,266,000) for Canada as a whole (Supplementary Table 10). This is a sizeable increase in vaccine uptake considering the relatively short period in which it was achieved (within 6 to 10 weeks of the mandate announcement for most provinces; 13 weeks for Quebec) and the very high pre-mandate first-dose vaccination rate in Canada (over 80% on average at the time of the mandate announcements; Supplementary Table 2). For example, Saskatchewan’s 5 p.p. estimated gain amounts to about one quarter of all remaining unvaccinated eligible people in the province as of the mandate announcement date (16 September 2021).

We do not find evidence of net intertemporal substitution (a decrease in the cumulative first-dose gains from pulling

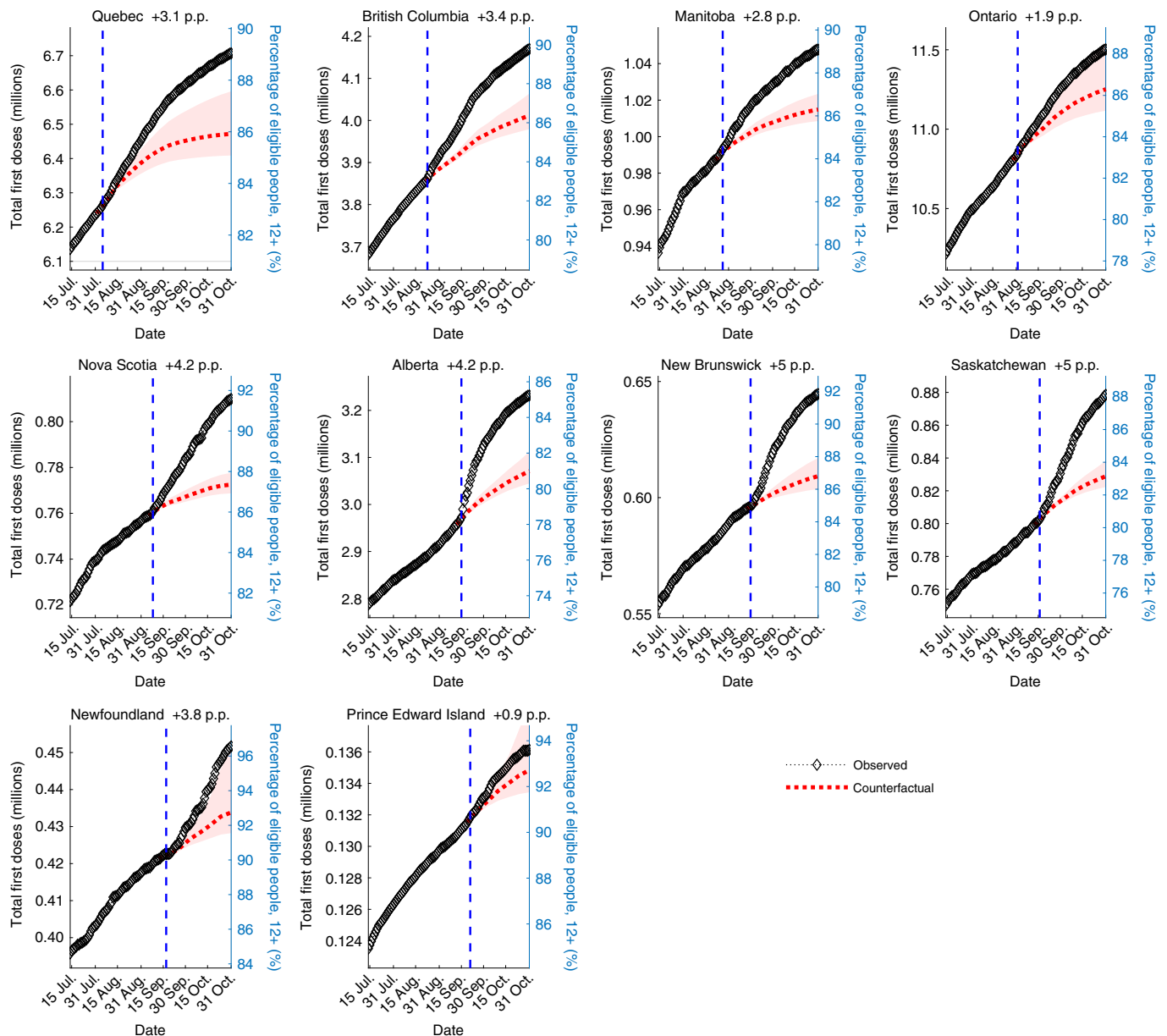


Fig. 4 | Canadian provinces—observed versus no-mandate counterfactual first doses as of 31 October 2021 (time-series estimates). Observed (diamonds) and estimated mean counterfactual (dotted lines) cumulative first doses for each province by date. All displayed dates refer to 2021. The counterfactuals are computed using the estimates in Table 2. The shaded areas denote 5th–95th percentile confidence bands computed using 1,000 draws from the estimated asymptotic joint distribution of the parameters in equation (3). The vertical dashed lines denote the mandate announcement dates. The number next to each province name indicates the mean estimated p.p. increase in first doses relative to the no-mandate counterfactual, as of 31 October 2021.

vaccinations forwards in time) as of 31 October 2021, except in Prince Edward Island. However, the strongly negative $\hat{\tau}_2$ estimates in Table 2 and the trends in Extended Data Fig. 9 indicate that Alberta, New Brunswick and Saskatchewan are projected to exhibit net intertemporal substitution after the end of October 2021. That said, given the flattening of the counterfactual curves in Fig. 4, the cumulative gains are likely to remain sizeable.

In Extended Data Fig. 10, we also plot a counterfactual computed using the DID estimate for Canada from Table 1 (that is, using the same policy estimate for all provinces). We find that, as of the DID sample end date of 14 September 2021, the five Canadian provinces that announced mandates by that date had benefited from 287,000 additional first doses (90% CI, 239,000–333,000) or a vaccination

rate increase of 0.9 p.p. (90% CI, 0.7–1.0) for the eligible population, relative to the no-mandate counterfactual.

We also estimate large increases in first-dose vaccinations relative to the no-mandate counterfactual in the three countries: 8 p.p. (90% CI, 4.3–10.8) or 4.59 million (90% CI, 2.47–6.25) additional first doses in France, 12 p.p. (90% CI, 5.0–15.1) or 6.48 million (90% CI, 2.67–8.14) doses in Italy, and 4.7 p.p. (90% CI, 4.1–5.1) or 3.47 million (90% CI, 3.06–3.81) doses in Germany as of 31 October 2021 using our main Table 2 specification (Figs. 5 and 6 and Supplementary Table 10). These estimated gains are larger than those for Canada, possibly because of the earlier mandates in these countries or the lower starting vaccination rates, and they may partly reflect expanding the scope of the initial mandates—for

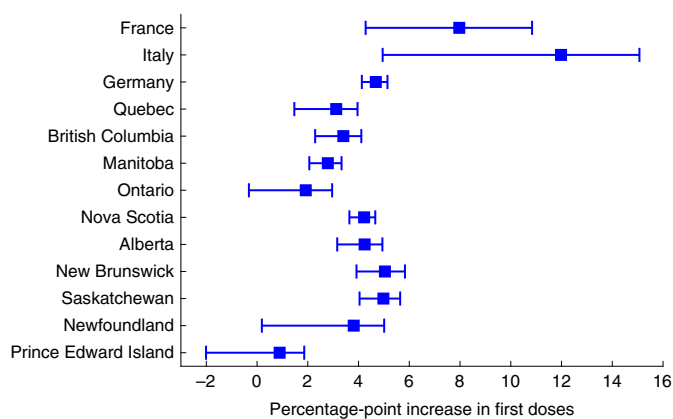


Fig. 5 | Counterfactuals—p.p. increase in vaccine uptake. Total estimated p.p. increase (mean and 90% CI) in the first-dose vaccination rate after mandate announcement, as of 31 October 2021.

example, to inter-regional travel and employment in September and October in Italy.

The counterfactuals assume that all explanatory variables except the mandate announcement (for example, cases, deaths and time trends) remain fixed at their observed values and that the model parameters remain stable. These assumptions are more plausible over relatively short periods. Hence, these simulations should be interpreted primarily as an illustration of the estimated impact of the mandates on vaccine uptake rather than policy guidance.

Discussion

We find that government-mandated proof-of-vaccination requirements or certificates have sizable and statistically significant impacts on COVID-19 vaccine uptake, with large observed increases in first-dose vaccinations in the first several weeks after mandate announcement and lasting cumulative gains relative to the pre-announcement trend. This includes robust DID evidence using the variation in the timing of mandate announcements within the same country, Canada. Our results are of similar magnitude and consistent with the findings of other authors using different data samples and methodologies: Mills and Rüttenauer²⁵ use a synthetic

control approach and estimate large increases in vaccinations in France (8.6 million), Italy (4 million) and Israel (2.1 million) from 20 days before to 40 days after COVID-19 certification mandate implementation, while Oliu-Barton et al.²⁶ compute counterfactuals on the basis of an innovation diffusion model of vaccine uptake and attribute vaccination rate increases of 13 p.p. for France, 6.2 p.p. for Germany and 9.7 p.p. for Italy to the announcement of COVID-19 certificate requirements, with associated additional gains from averted deaths and gross domestic product losses.

The estimated mandate effect on uptake varies across the Canadian provinces, with the timing of announcement and implementation and the percentage unvaccinated playing a role. The estimated impact also differs across France, Italy and Germany. Further research on understanding this heterogeneity and on other potentially important factors (for example, the role of government communication or the media, the degree of political polarization and the amount of social trust) can complement our study.

The unambiguous and large increases in vaccine uptake that we find compare favourably to the mixed evidence from using financial incentives (cash, gift cards, lotteries and so on)^{12–17} or behavioural nudges^{12,18–20}. Financial incentives for vaccination have been criticized for the optics of putting a low dollar value on being vaccinated compared with the social benefits, because of the perceived unfairness in rewarding people who delayed their vaccination or because of potential moral hazard problems (for example, expecting future payments). Financial incentives may even have the perverse effect of validating vaccine concerns among unvaccinated individuals¹². Others²⁷ have argued that, given the already high vaccination rates in developed countries, behavioural nudges may not be very effective, which is consistent with the findings of ref. ¹² and ref. ¹³. However, vaccination mandates have also been controversial, as some people perceive them as restrictions on personal freedom. This can affect compliance and increase the direct implementation and enforcement costs, as well as the political costs of introducing a mandate.

In terms of external validity, in 2020, Lazarus et al.²⁸ conducted a survey on vaccine hesitancy across 19 countries comprising around 55% of the world population. The participants were asked, “If a COVID-19 vaccine is proven safe and effective and is available, will you take it?” Canada, Italy and Germany placed around the middle in the self-reported vaccine hesitancy rate (29% to 31%), while France had a higher hesitancy rate (41%). In this regard,

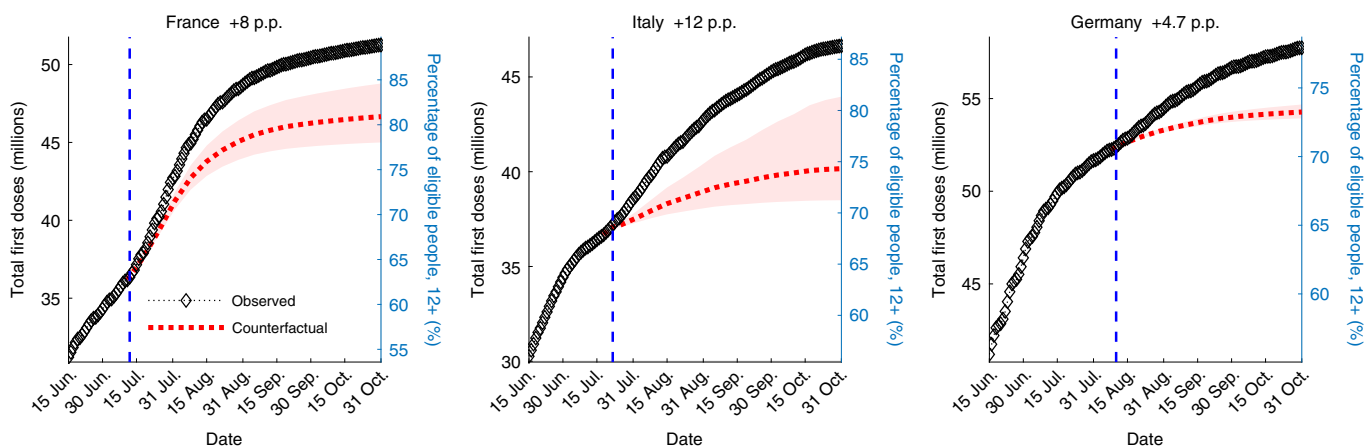


Fig. 6 | Countries—observed versus no-mandate counterfactual first doses. Observed (diamonds) and estimated mean counterfactual (dotted lines) cumulative first doses for each country by date. All displayed dates refer to 2021. The counterfactuals are computed using the estimates in Table 2. The shaded areas denote 5th–95th percentile confidence bands computed using 1,000 draws from the estimated asymptotic joint distribution of the parameters in equation (3). The vertical dashed lines denote the mandate announcement dates. The number next to each country name indicates the mean estimated p.p. increase in first doses relative to the no-mandate counterfactual, as of 31 October 2021.

our results on the large impact on new first-dose vaccinations in these countries can be useful to public health authorities in other places looking for an effective strategy to increase vaccine uptake.

We conclude by listing some limitations and areas for further research. We have abstracted from assessing the mandates' impact on health outcomes (cases, hospitalizations or deaths). However, vaccine effectiveness estimates from the medical literature can be used to study this further, ideally controlling for possible changes in behaviour. Our focus is on vaccination and certification requirements for non-essential settings, although some locations (such as Italy) expanded the mandates to inter-regional travel or employment, and other countries (such as Greece and Austria) have proposed even broader mandates, which we do not analyse. For data limitation reasons, we also could not study the effect by age group as in ref. 25 or the possible role of past personal sickness outcomes as in ref. 29. See also the Methods for limitations and required assumptions related to the time-series analysis and counterfactuals and our ways of tackling them.

Naturally, a full cost–benefit analysis of proof-of-vaccination mandates is beyond the scope of this paper. In particular, the costs of imposing and enforcing the mandates—economic, political or personal—are very hard to estimate, as is the social value of vaccinating an additional person^{30,31}. One component of the latter is avoided health-care costs; for example, Barber and West¹⁴ estimate that the Ohio vaccine lottery saved the state US\$66 million in intensive care unit costs. Our results are a step towards quantifying the benefits of proof-of-vaccination requirements.

Methods

Data and definitions. We use data on COVID-19 vaccination numbers, cases and deaths for all ten Canadian provinces and for France, Italy, Germany and Spain; see Supplementary Table 11 for the details. We collected the Canadian data from the official provincial dashboards or equivalent sources. We use the Our World in Data dataset for the country data. Announcement and implementation dates of the proof-of-vaccination mandates or certificates were collected from government websites and major newspapers (Supplementary Table 1).

The main variables used in our statistical analysis are defined next. Everywhere, i denotes province or country, and t denotes time measured in days (date). We aggregate the data on vaccinations, cases and deaths on a weekly basis (totals for the week ending on date t —that is, dates $t-6$ to t) to reduce the influence of day-of-the-week effects or reporting artefacts (for example, lumping weekend data in Monday's report; in the latter case, we distribute the reported total equally over the affected dates).

- Outcome, V_{it} . The main outcome variable is the logarithm of administered vaccine first doses per 100,000 people, for the week ending at t (dates $t-6$ to t). We use first doses, as they most directly reflect the impact of the mandates on the intent to be immunized and avoid potential issues related to second-dose scheduling or availability. In Supplementary Table 7, we also report results using second doses as the outcome. All COVID-19 vaccines used in Canada during the study period—namely, Pfizer (Comirnaty), Moderna (SpikeVax) and AstraZeneca (Vaxzevria)—were originally considered two-dose. Using the logarithm of weekly first doses allows us to interpret the regression coefficients as percentage changes; moreover, the estimates are invariant to normalization—for example, by population (subsumed in the regression constant or fixed effects). We thus use 'log weekly first doses' for simplicity throughout the text, except where the actual scale is important.
- Policy, P_{it} . Let t_i be the mandate announcement date in jurisdiction i . We construct a binary policy variable P_{it} equal to 1 for all post-announcement dates $t \geq t_i$ and equal to 0 for all $t < t_i$. Proof-of-vaccination mandates were announced in all ten Canadian provinces over the period 5 August 2021 to 21 September 2021 (Extended Data Fig. 3 and Supplementary Tables 1 and 12). Last to announce were the four Atlantic provinces, which had the lowest per capita case rates in August 2021, and Alberta and Saskatchewan, which had the highest per capita case rates in August 2021 (Extended Data Fig. 4). France, Italy and Germany introduced proof-of-vaccination certificates in July–August 2021 (Supplementary Table 1).
- Information, I_{it} . We include control variables related to the concurrent COVID-19 epidemiological situation, specifically log of weekly cases, C_{it} , and log of weekly deaths, D_{it} , for the week ending at date t (dates $t-6$ to t). We refer to these variables jointly as 'information', I_{it} (refs. 32,33), since they can inform a person's COVID-19 exposure risk assessment and/or decision to be vaccinated—for example, as shown in refs. 21,22. Another possible information variable is hospitalizations. However, it is strongly correlated with COVID-19

cases and deaths, so all three cannot be included at the same time (see Supplementary Tables 5 and 9 for results using deaths and hospitalizations as information). To address zero weekly values, which sometimes occur in the smaller provinces for deaths or cases (4.4% of all observations for cases and 10.7% for deaths), we replace $\log(0)$ with -1 , as in ref. 32.

- Other controls, W_{it} . We include province fixed effects and date fixed effects in our DID analysis. The province fixed effects account for any time-invariant province characteristics such as sentiment towards vaccination, age structure, education and political alignment. The date fixed effects control for national trends or events—for example, public messaging, vaccine-related international travel regulations or campaigning for the 2021 federal election. In the time-series analysis, we control for time trends.
- Time period. We use the period 15 June 2021 to 31 October 2021. The start date was chosen to ensure that possible constraints on obtaining a first dose related to eligibility or supply are minimal or non-existent. This helps avoid potential bias from constrained vaccine supply affecting the pre- or post-mandate pace of vaccination. In Canada, the provinces opened registration for first-dose vaccination for any person of age 12 or older between 10 May 2021 in Alberta and 27 May 2021 in Nova Scotia (Supplementary Table 3). First-dose availability in France, Italy and Germany was similar by mid-June, at least for the 18-plus age group. We explore different sample start dates in the robustness checks (Extended Data Fig. 5). The sample end date is based on data availability at the time of statistical analysis and writing. First doses for the 5–11 age group were not approved in the studied period.

DID estimation. We estimate a behavioural model in which the decision to receive a COVID-19 vaccine, measured by new weekly first doses, is affected by the policy setting, P_{it} (whether a mandate has been announced), and current COVID-19 epidemiological information about public health conditions, I_{it} , proxied by weekly cases and deaths. On the basis of the raw data patterns in Fig. 1 and Extended Data Fig. 4 and the absence of vaccine supply constraints in the studied period, we assume no lag between a mandate announcement and a person's ability to receive a first vaccine dose and no information lag. We perform sensitivity analysis using alternative lags (Extended Data Fig. 5); the results affirm our baseline choice of no lag.

In the DID analysis, we estimate:

$$V_{it} = \alpha P_{it} + \beta I_{it} + \gamma_t + \delta_i + \varepsilon_{it}, \quad (1)$$

where γ_t are date fixed effects, δ_i are province fixed effects and ε_{it} is an error term. The coefficient α on P_{it} captures the average effect of the mandate announcement on weekly first doses over all post-announcement dates. To capture dynamic effects of the mandates, we also estimate a version of equation (1) with P_{it} split into separate indicator variables, one for each week after the mandate announcement date (Table 1).

To correct for the small number of clusters in the estimation since there are only ten provinces, we report 'wild bootstrap' P values (we use the Stata package `boottest` clustered by province with 4,999 repetitions^{34,35}). The use of clustered standard errors allows the error terms to be serially correlated within each province. Alternative methods for computing the standard errors are explored in Supplementary Table 6, including clustering at the province level (Stata command `cluster`), wild bootstrap standard errors clustered at the province level and wild bootstrap standard errors two-way clustered by province and date allowing for spatio-temporal correlation (Stata command `boottest`).

The recent methodological literature^{23,36–39} has argued that the standard OLS TWFE estimator can be invalid in panel-data settings with staggered adoption like ours if there is heterogeneity in the treatment effect across cohorts (provinces in our data) and/or over time. The reason is that the TWFE estimate is a weighted average of many two-by-two DID treatment effects, where some of the weights can be negative or incorrect because of contamination from other periods.

In particular, Sun and Abraham²³ develop a DID estimator that is valid under these conditions, which we use to estimate equation (1). The Sun and Abraham estimator uses never-treated or last-treated units as the control group and is constructed as the weighted average of treatment effects for each cohort (by date of mandate announcement) and each relative time after or before the announcement (we use the Stata function `eventstudyinteract` provided by the authors). Specifically, to calculate the average treatment effect α , we replace αP_{it} in equation (1) with

$$\sum_{g \in C} \sum_{l \neq -1} \alpha_{i,l} (\mathbb{1}[i = g] D_{it}^l),$$

where C is the set of never-treated or last-treated provinces (control group), $D_{it}^l = \mathbb{1}[t - t_i = l]$ is a 'relative time' indicator and t_i is the date of treatment for province i . Under parallel trends and no anticipation, Sun and Abraham²³ show that $\alpha_{i,l}$ is consistent for the province–time-specific treatment effect. Then, the average treatment effect for each relative-time period, α_l , is the appropriately weighted (by the sample share of each treated province in relative time l) average of $\alpha_{i,l}$ across the units i , and α in equation (1) is computed as the simple average of α_l across l .

The estimator requires excluding all time periods in which units in the control group are treated. Since the last province to announce a mandate was Prince Edward Island, on 21 September 2021, we can use only data until 20 September at the latest (Supplementary Table 1). We present results for different control groups and corresponding sample periods in Supplementary Table 4. We also compare our main results with the OLS TWFE estimates in Supplementary Table 5.

Time-series estimation. We estimate the following “interrupted time-series analysis”²⁴ specification for each country or province (we omit the subscripts i for simplicity since all variables refer to the same location):

$$V_t = c + \lambda_1 V_{t-7} + \lambda_2 V_{t-14} + \pi P_t + \tau_1 T_t + \tau_2 T_t^a + \mu I_t + \eta_t, \quad (2)$$

where V_t are log weekly first doses for the week ending at date t , c is a constant, I_t is a proxy for information (analogous to its counterpart in equation (1)) and η_t is the error term. In our baseline specification in Table 2, we construct the policy variable P_t as the weekly average from date $t-6$ to t of the ‘mandate announced’ indicator P_t , defined above. This is consistent with the weekly vaccination and information variables V_t and I_t and improves the fit in the time-series regressions. We also present results without weekly averaging in Supplementary Tables 9 and 10. We include 7-day and 14-day lagged values of V_t (instead of first and second lags) since the outcome variable V_t is a weekly total.

We include two time trends in equation (2): T_t is a linear daily time trend initialized at the sample start date $t=0$, and T_t^a is an ‘interaction’ time trend that takes the value 0 at all dates until the announcement date (inclusive) and increases by 1 for each day afterwards. The coefficients τ_1 (slope) and τ_2 (change in slope at the announcement date) characterize, respectively, the baseline (pre-announcement) time trend (with slope τ_1) and the post-announcement time trend (with slope $\tau_1 + \tau_2$) in weekly first doses.

Standard errors and P values are calculated using the Newey–West⁴⁰ heteroskedasticity and autocorrelation robust estimator, with three lags. The lag was chosen as the closest integer to $T^{1/4}$, where T is the sample size.

The time-series approach requires stronger identification assumptions than the DID approach since there is no control group. Specifically, we need to assume exogeneity of the announcement date and that the time-series process for weekly first doses changes after the announcement only because of the policy—that is, it would have followed the same pre-trend if no mandate had been announced. We also cannot control for fixed effects in a flexible way beyond including a constant and time trends. Under these assumptions, the coefficient on P_t captures the average shift in first-dose uptake attributed to the mandate announcement. Similarly, the coefficient on the interaction time trend T_t^a measures the slope change in the trend of first doses after the mandate announcement relative to the pre-announcement trend.

Structural break at the announcement date. We perform a structural break Chow test⁴¹ for a known break point using the log weekly first doses, V_t , in Supplementary Table 8. The presence of a break point at the mandate announcement date indicates an abrupt change or shift in the first-dose time-series process. We use a bandwidth of 50 days before and 35 days after the announcement date. The unequal before–after bandwidth is chosen to reduce the size distortion of the test, since the outcome variable is a weekly sum, and the error terms are serially correlated⁴². Columns 1 and 2 in Supplementary Table 8 use the log of weekly first doses, V_t , while column 3 uses first-differenced weekly first doses. The differenced series is stationary, and the error terms are not serially correlated, which alleviates concerns about size distortion in the test. The power of the Chow test is weaker in this specification since the first-differenced V_t series is a growth rate being used to test for a level shift. The differenced series is also noisier, as it captures daily fluctuations. Overall, the structural break test results show that a mandate announcement is strongly associated with a trend break in first-dose vaccine uptake in all or most locations.

Counterfactuals. Calling \hat{t} the mandate announcement date, we compute the counterfactual log weekly doses per 100,000 people, \hat{V}_t^{cs} , iteratively using

$$\hat{V}_t^{cs} = V_t, \quad \forall t < \hat{t} \text{ and}$$

$$\hat{V}_t^{cs} = V_t - \hat{\pi} P_t - \hat{\tau}_2 T_t^a + \hat{\lambda}_1 (\hat{V}_{t-7}^{cs} - V_{t-7}) + \hat{\lambda}_2 (\hat{V}_{t-14}^{cs} - V_{t-14}), \quad \forall t \geq \hat{t}, \quad (3)$$

where $\hat{\pi}$ is the coefficient estimate of the mandate announcement variable P_t , from column 1 of Table 2, $\hat{\tau}_2$ is the estimate of the interaction time trend T_t^a from column 2 of Table 2, and the terms multiplied by the estimates $\hat{\lambda}_1$ and $\hat{\lambda}_2$ account for the lagged values V_{t-7} and V_{t-14} in equation (2). The policy effect estimate $\hat{\pi}$, $\hat{\tau}_2$, $\hat{\lambda}_1$ and $\hat{\lambda}_2$ are specific for each respective country or province. To plot Extended Data Fig. 10, we compute the counterfactual mean as $\hat{V}_t^{DID} = V_t - \hat{\alpha} P_t$, where $\hat{\alpha}$ is the DID estimate on the policy variable P_t from Table 1, column 2.

Reporting summary. Further information on research design is available in the Nature Research Reporting Summary linked to this article.

Data availability

All data used in this paper are publicly available at <https://github.com/C19-SFU-Econ/dataV>. Source data are provided with this paper.

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Author contributions

All authors contributed equally to this research.

Competing interests

The authors declare no competing interests.

Additional information

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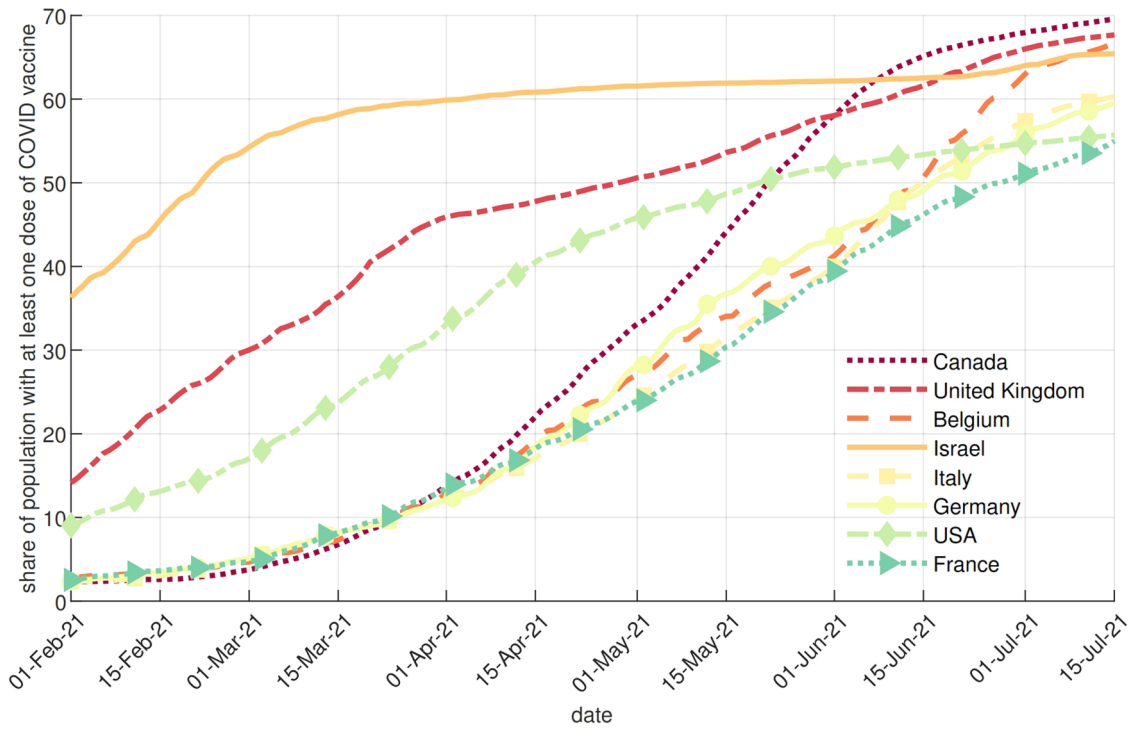
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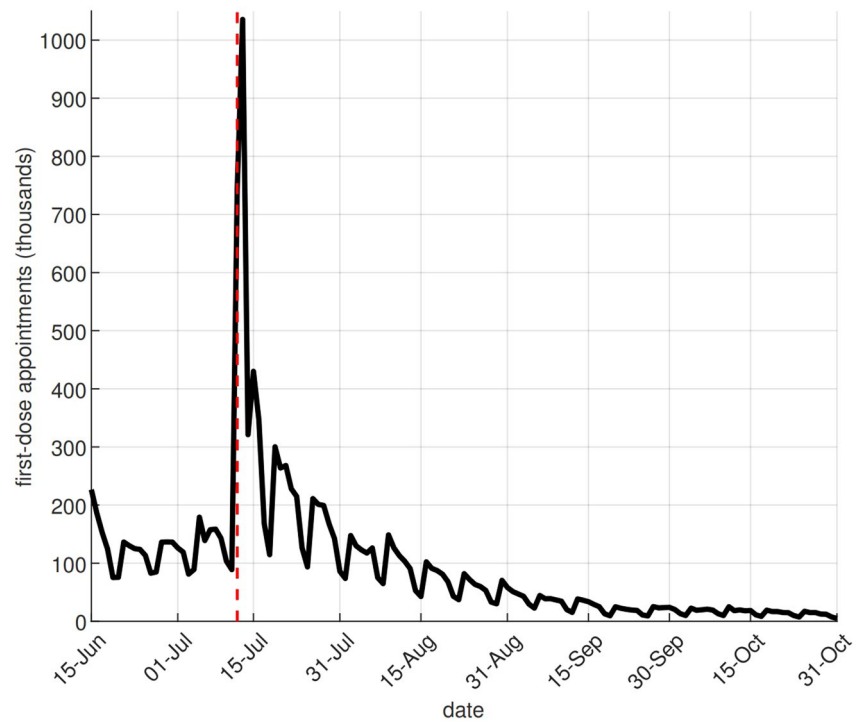
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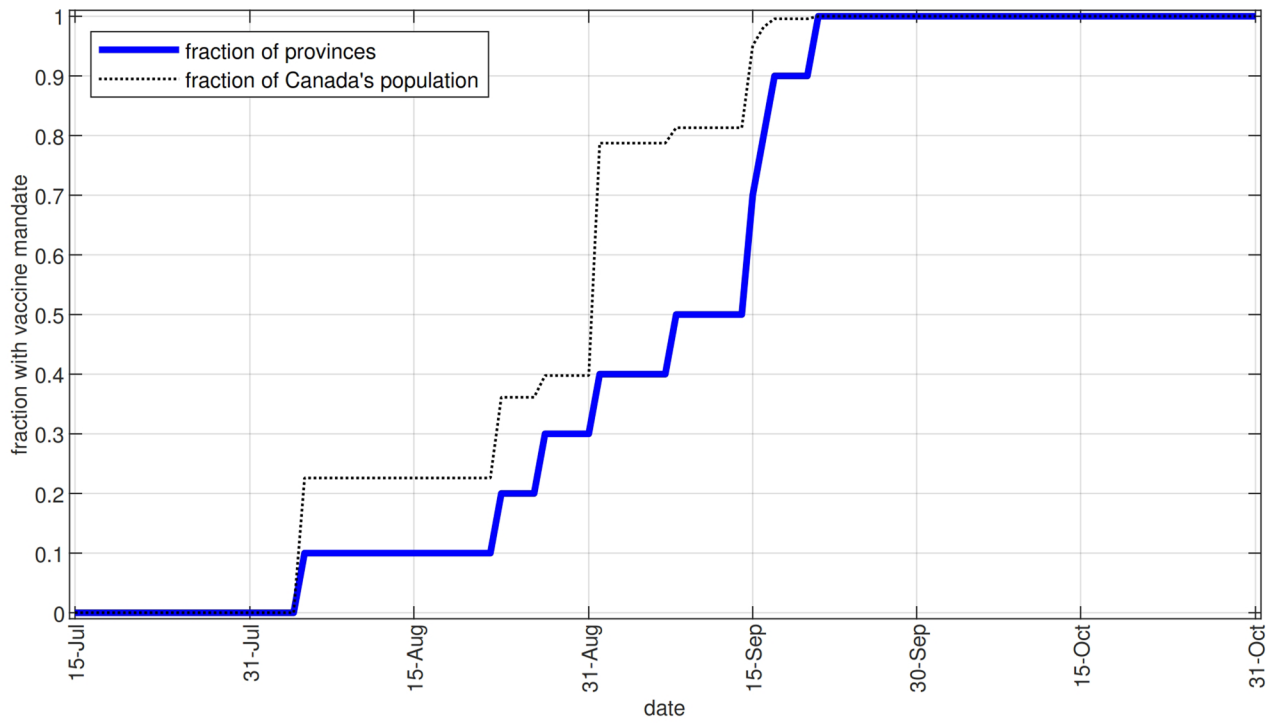
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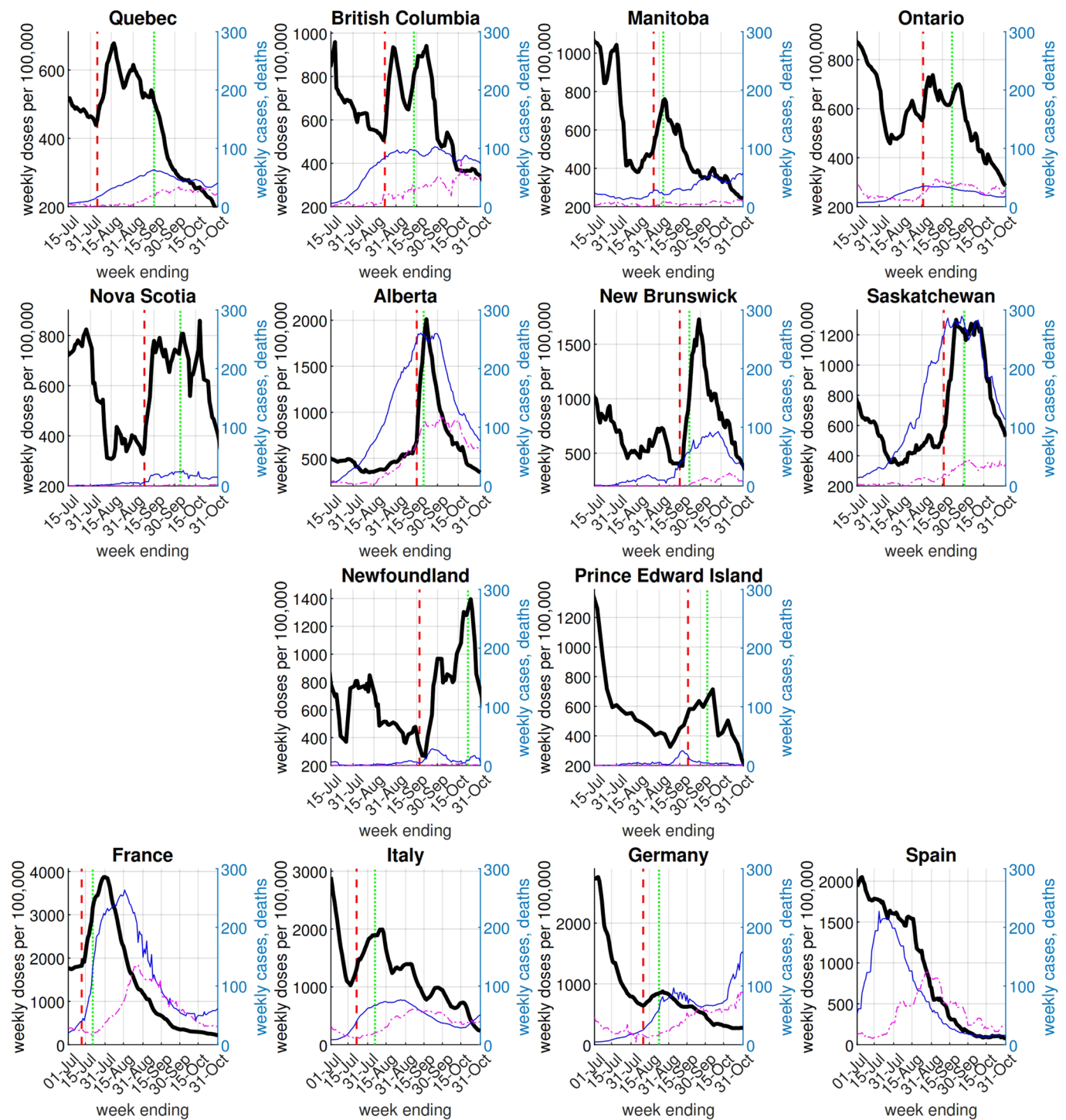
Extended Data Fig. 1 | Share of people with at least one dose - example countries. Notes: The figure plots the share of the population that has received at least one dose of a COVID-19 vaccine in selected countries. Source: Our World in Data, <https://ourworldindata.org>.



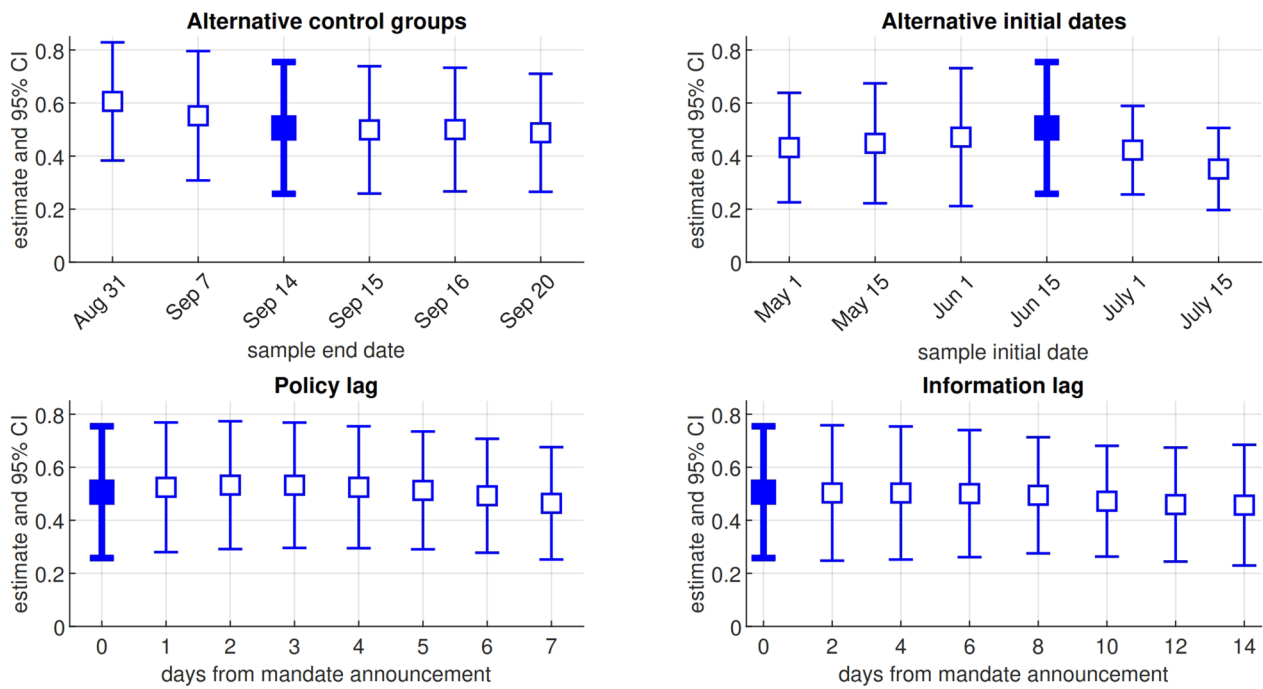
Extended Data Fig. 2 | France - first-dose vaccination appointments. Notes: The figure plots the daily first-dose vaccination appointments by date made on Doctolib, a booking website accounting for about 2/3 of cumulative COVID-19 vaccinations in France as of Q4 2021. Source: <https://about.doctolib.fr/vaccination/statistiques.html>. The vertical dashed line denotes the mandate announcement date, July 12, 2021.



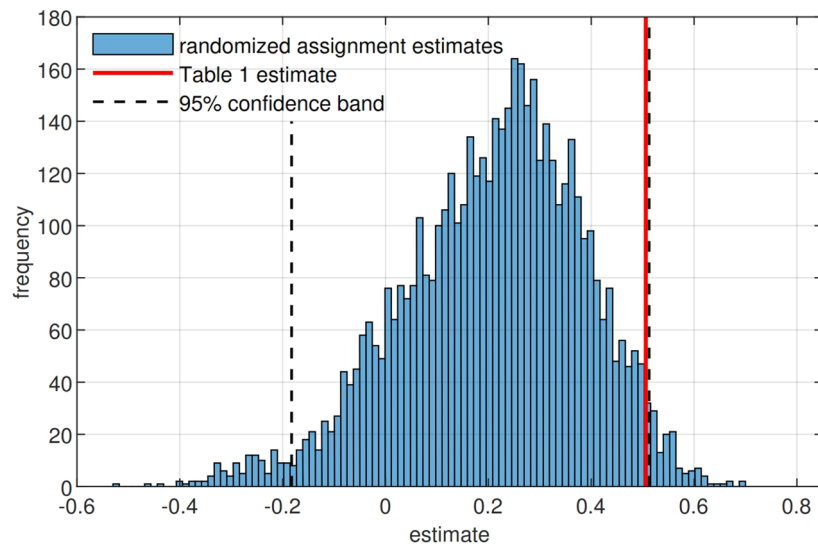
Extended Data Fig. 3 | Canada - proof of vaccination mandates over time. Notes: The figure plots the cumulative fraction of provinces and the cumulative fraction of Canada's population for which a proof of vaccination mandate has been announced. See Supplementary Table 1 for the exact dates of mandate announcement in each province.



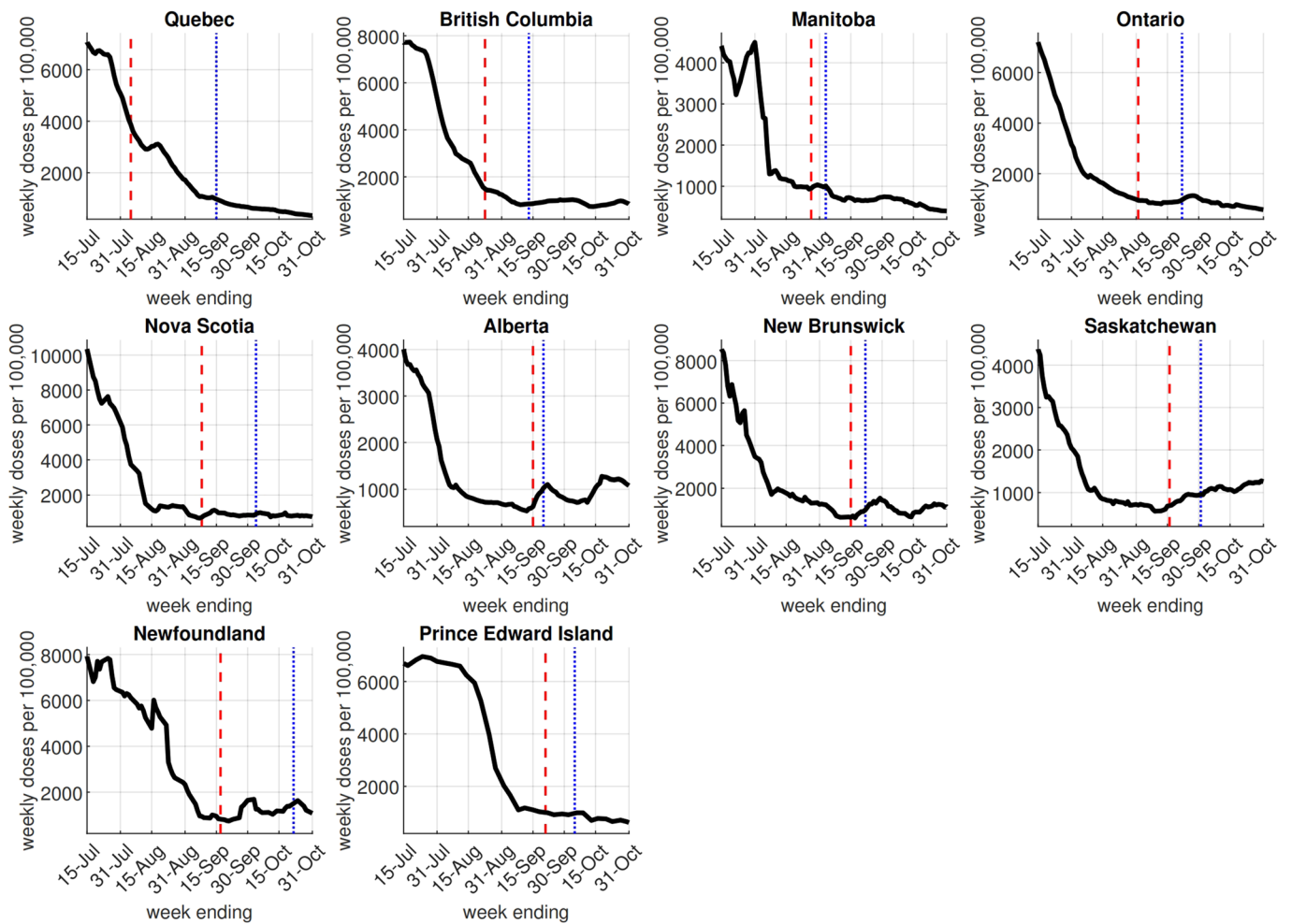
Extended Data Fig. 4 | First doses per 100,000 people. Notes: The figure plots the weekly administered COVID-19 vaccine first doses per 100,000 people for dates $t-6$ to t (thick solid line), where t is the date on the horizontal axis. The vertical dashed lines denote the mandate announcement date for each province. The vertical dotted lines denote the mandate implementation (enforcement) date for each province (see Supplementary Table 1). The thin solid lines plot weekly cases per 100,000 (right axis) in each location; the thin dash-dotted lines plot weekly deaths (for the provinces) or weekly-averaged daily deaths (for the countries) (right axis).



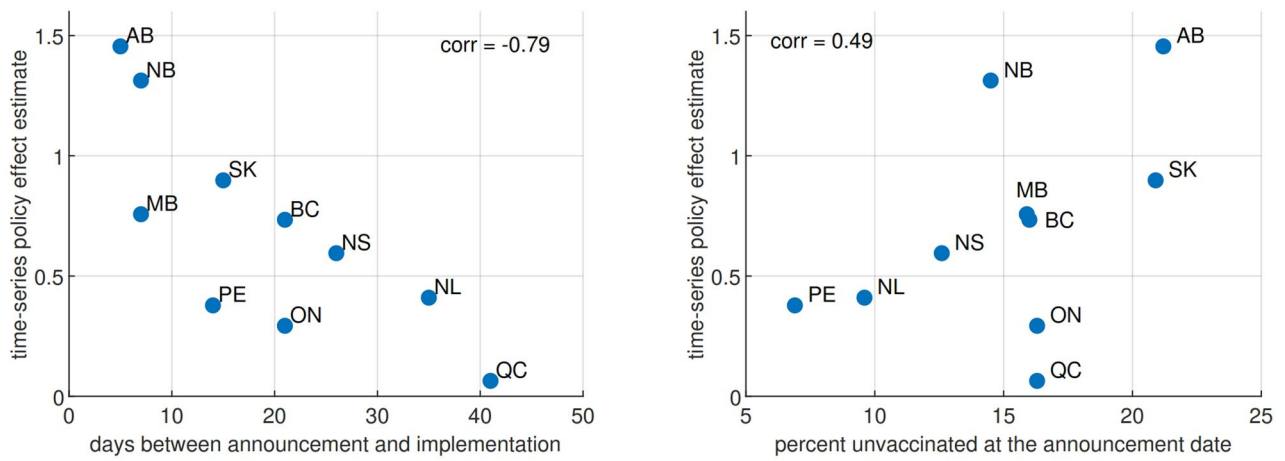
Extended Data Fig. 5 | Robustness. Notes: The figure plots the coefficient estimate, $\hat{\alpha}$ (denoted by square) and 95% confidence intervals (denoted by vertical error bars) of the mandate announcement variable P_{it} in equation (1). The upper left panel shows the estimates for different sample end dates and corresponding control groups including the baseline (Sep. 14, in bold). The upper right panel shows the estimates for different initial sample dates (May 1 to July 15, 2021), including the baseline (June 15, 2021, in bold). The lower left panel shows the estimates from a variant of equation (1) when using no lag in the policy announcement P_{it} (in bold, our baseline) and using lag of up to 7 days, that is, P_{it-k} for $k=1, 2, \dots, 7$. The lower right panel displays the estimates from a variant of equation (1) when using no lag for the information (cases and deaths) I_{it} (in bold, our baseline) or using lag of up to 14 days, that is, I_{it-k} for $k=2, 4, 14$.



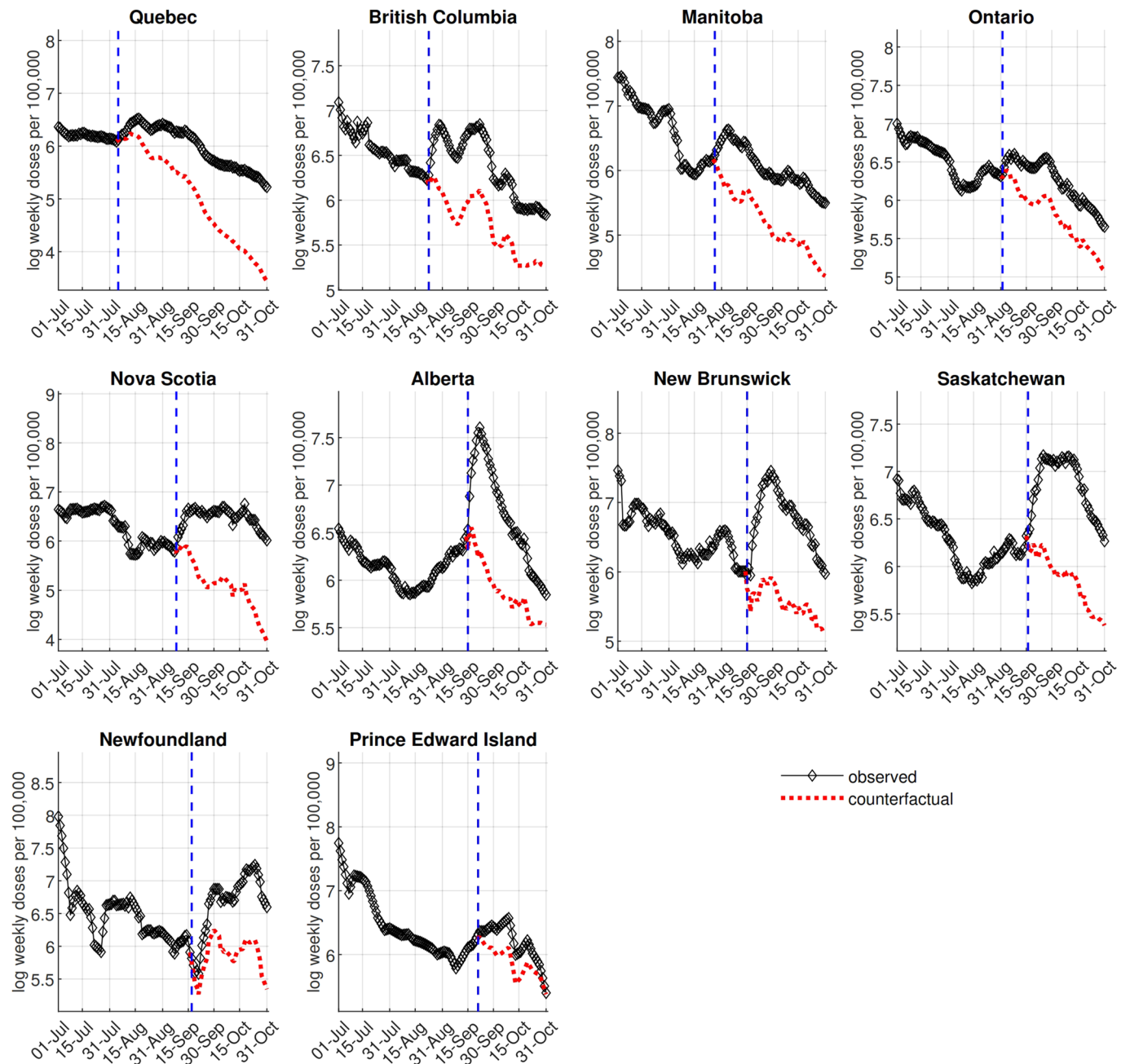
Extended Data Fig. 6 | Random assignment of announcement dates. Notes: We estimate equation (1) 5,000 times using the Sun and Abraham treatment effect heterogeneity robust estimator after randomly assigning the date of mandate announcement for each province which has announced a mandate by Sep. 14. The figure plots the histogram of these placebo inference estimates, along with the 2.5-th and 97.5-th percentiles (dashed lines). The solid vertical line corresponds to the baseline estimate from column (2) in Table 1.



Extended Data Fig. 7 | Canadian provinces - second doses per 100,000 people. Notes: The figure plots the weekly administered COVID-19 vaccine second doses per 100,000 people for dates $t-6$ to t , where t is the date on the horizontal axis. The vertical dashed lines denote the vaccination proof mandate announcement date for each province. The vertical dotted lines denote the mandate implementation (enforcement) date for each province (see Supplementary Table 1).

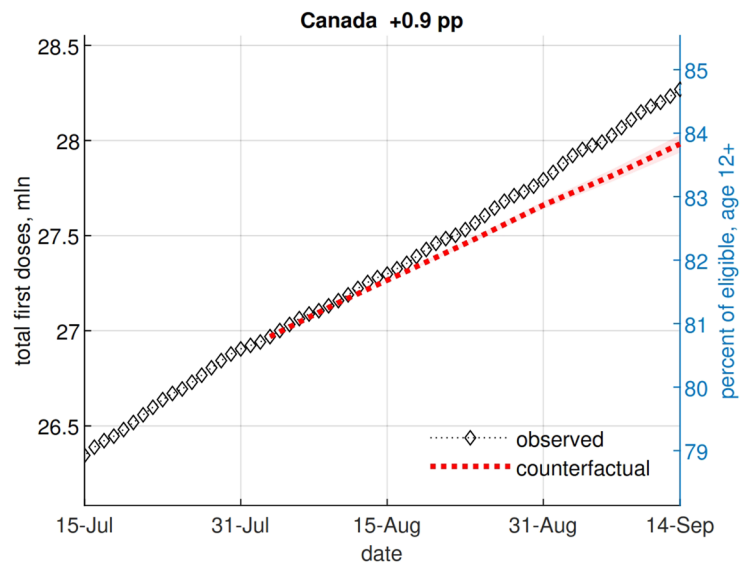


Extended Data Fig. 8 | Time-series policy estimates - correlations. Notes: The figure plots the time-series policy estimates, $\hat{\pi}$ from Table 2, column (1) against the number of days between mandate announcement and implementation (left) and the percent remaining unvaccinated eligible people at the announcement date (right). The figure is for illustration; no causal claims are made.



Extended Data Fig. 9 | Canadian provinces - observed vs. no-mandate counterfactual weekly first doses as of October 31, 2021 (time-series estimates).

Notes: The figure plots the observed (diamonds) and the estimated mean no-mandate counterfactual (dotted line) log weekly first doses per 100,000 people. We use the estimates from Table 2 to compute the counterfactuals, as specified in Eq. (3). The vertical dashed lines denote the mandate announcement date for each province.



Extended Data Fig. 10 | Canada - observed vs. no-mandate counterfactual first doses as of September 14, 2021 (DID estimate). Notes: The figure plots the observed (diamonds) and the estimated mean no-mandate counterfactual (dotted line) cumulative first doses (in millions) by date, with 5-95 percentile confidence bands (the shaded area). The counterfactual uses the policy P_{it} coefficient estimate from column (2) of Table 1. The number in the caption indicates the percentage point increase in first doses relative to the no-mandate counterfactual as of September 14, 2021.

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All data sources are publicly accessible and are described in Appendix C of the paper.
The data used in the study are publicly available at: <https://github.com/C19-SFU-Econ/dataV>

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Behavioural & social sciences study design

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Study description	quantitative
Research sample	COVID-19 vaccination, case and death numbers for each Canadian province from official sources (see Appendix C Table C4) and for three countries (France, Italy, Germany) from the Our World in Data database (ourworldindata.org).
Sampling strategy	not applicable; we use all available data for the specified geographic locations.
Data collection	We use data on COVID-19 vaccination numbers, cases and deaths for all ten Canadian provinces, as well as for France, Italy, and Germany (see Table C4 for the data sources and details). The Canadian data was collected from the official provincial dashboards or equivalent sources. We use the Our World In Data dataset for the country data. Announcement and implementation dates of the proof of vaccination mandates were collected from the respective government websites and major newspapers (see Table C1).
Timing	We use the period June 15, 2021 to October 31, 2021. The start date is chosen to ensure that possible constraints to obtain a first dose related to eligibility or vaccine supply are minimal or non-existent. The sample end date is based on data availability at the time of statistical analysis and writing.
Data exclusions	no data were excluded in the specified sample time period; the difference-in-differences statistical method used requires using untreated locations as the control group which determines the sample end date for this part of the analysis (see Section 3)
Non-participation	not applicable
Randomization	not applicable

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