DISCUSSION PAPER SERIES

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ABSTRACT

COVID-19 Vaccination Mandates and Vaccine Uptake

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 (more than 60% increase in weekly first doses) using the variation in the timing of these measures across Canadian provinces in a differencein- differences approach. Time-series analysis for each province and for France, Italy and Germany corroborates this finding, and we estimate cumulative gains of up to 5 percentage points in provincial vaccination rates and 790,000 or more first doses for Canada as a whole as of October 31, 2021 (5 to 13 weeks after the provincial mandate announcements). We also find large vaccination gains in France (3 to 5 mln first doses), Italy (around 6 mln) and Germany (around 3.5 mln) 11 to 16 weeks after the proof of vaccination mandate announcements.

JEL Classification:	I18, I12, C23
Keywords:	COVID-19, vaccine mandates, proof of vaccination, vaccine uptake, vaccine hesitancy, difference-in-differences, time-series analysis, counterfactuals

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1 Introduction

Immunization with mRNA or adenoviral vaccines has proven very effective for reducing the spread and severity of COVID-19, with fully vaccinated people benefiting from a large reduction in the risk of severe outcomes.[1] Yet, following a quick-paced uptake in early 2021, COVID-19 immunization rates in many countries slowed down significantly at about 60% of the population during the summer despite the vaccines' proven benefits (see Fig. B1). In addition, most countries or regions, even those with high vaccination rates, experienced increased viral transmission or had to maintain or re-introduce non-pharmaceutical interventions such as mask-wearing mandates or indoor capacity limits in fall 2021 because of the elevated reproduction rate of the Delta variant.[2]

A further increase in vaccination coverage therefore remains essential for reducing the health and economic impacts of COVID-19 and allowing the eventual lifting of restrictions. Moreover, the administration of vaccines with updated formulations may be necessary in the face of new variants. Public health authorities around the world have actively sought the most effective strategy to increase vaccine uptake and provide incentives for *vaccine hesitant* or procrastinating people that have had access to immunization but have not taken it up.

In response to this challenge, several local or national governments have recently introduced *proof of vaccination* mandates, which allow only vaccinated persons to attend non-essential sports or social activities, events and settings such as concerts, stadiums, museums, restaurants, bars, etc.[3] The goal of these policies is two-fold: to provide incentives for immunization and to reduce viral transmission in risky indoor or crowded settings.[4]

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) that announced and implemented such mandates during July–September, 2021. This is a period without binding vaccine supply constraints and with 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). Hence, we evaluate the mandates' impact on

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[1]See, e.g., Scobie et al. (2021), Grannis et al. (2021), Andrews et al. (2021) and Gupta et al. (2021).

[2]For example, by early August, all four of Canada's Western provinces had removed their mask-wearing mandates for public indoor spaces. By mid-September, all re-imposed a mask mandate, either preemptively or after a COVID surge. The largest provinces Ontario and Quebec did not remove their mask mandates.

[3]While such mandates are sometimes colloquially called "vaccine passports", we deliberately avoid this politicized term. Some mandates accept a recent negative test or past positive test as a substitute of vaccination or allow businesses to opt out if they abide by additional restrictions (e.g., in Alberta).

[4]Several countries have also announced or implemented vaccination mandates *for employees* (USA for all firms with more than 100 employees, Italy for all employees, Canada for all federal employees), and a broader mandate is under consideration in Austria. We do not analyze these alternative mandates.

people, such as the vaccine hesitant, that chose to remain unvaccinated for weeks or months after immunization became available to them.

While requiring proof of vaccination is expected to raise vaccine uptake, the magnitude and time profile of the increase are hard to predict: they depend on the relative importance of the factors leading to hesitancy or procrastination, such as a lack of social or economic incentives 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.[5]

The Canadian province-level data is key for our identification strategy as it allows us to use the time variation in mandate announcement dates (from Aug. 5, 2021 in Quebec to Sep. 21, 2021 in Prince Edward Island) across different geographic units in the same country via a difference-in-differences (DID) approach.[6] In contrast, the French, Italian or German mandates, or the announced U.S. vaccine mandate for employees, apply at the national level, which makes it more challenging to separate the effect of the mandate from that of time trends or other concurrent events or policies.

Fig. 1 plots the weekly vaccine first doses administered in the four most populous Canadian provinces and four European countries. All locations except Spain have a province-wide or country-wide proof of vaccination mandate. We observe a sizable boost in vaccine uptake within two weeks after the mandate *announcement* date (the dashed vertical line) in all four provinces and in France, Italy and Germany, often following a sharp decline in the pre-announcement weeks. In France, daily first-dose appointments data also shows a striking surge on the day after the mandate announcement (see Fig. B2). In contrast, Spain exhibits a steady decrease in weekly first doses over the displayed period.

Motivated by this evidence, we aim to answer two important policy questions. First, what is the magnitude of the increase in the pace of first dose vaccinations after a proof of vaccination mandate announcement? Second, how long do these gains persist and what is the cumulative effect of the mandates on vaccine uptake?

Using Canadian province-level data in a difference-in-differences (DID) statistical analysis, we find a quick and significant increase in vaccine uptake, measured by new weekly first doses, following the announcement of a proof of vaccination mandate. In our baseline

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[5]To avoid potential bias from constrained vaccine supply affecting the pre- or post-mandate pace of vaccination, we use only data after June 15, 2021, when it is safe to assume that any age-eligible person (12 or older in Canada) was able to receive a first dose of COVID-19 vaccine with minimal delay; see Table C2.

[6]In Canada, the provinces are separate public health jurisdictions with extensive powers over health policy while the vaccines are procured by the federal government and allocated to the provinces in proportion to their population. All COVID-19 vaccines used in Canada during the study period, namely BNT162b2 (Pfizer /BioNTech Comirnaty), mRNA-1273 (Moderna SpikeVax) and AZD1222 (Oxford–AstraZeneca Vaxzevria), were initially considered to be two-dose vaccines.

Figure 1: Vaccination mandates and first dose uptake
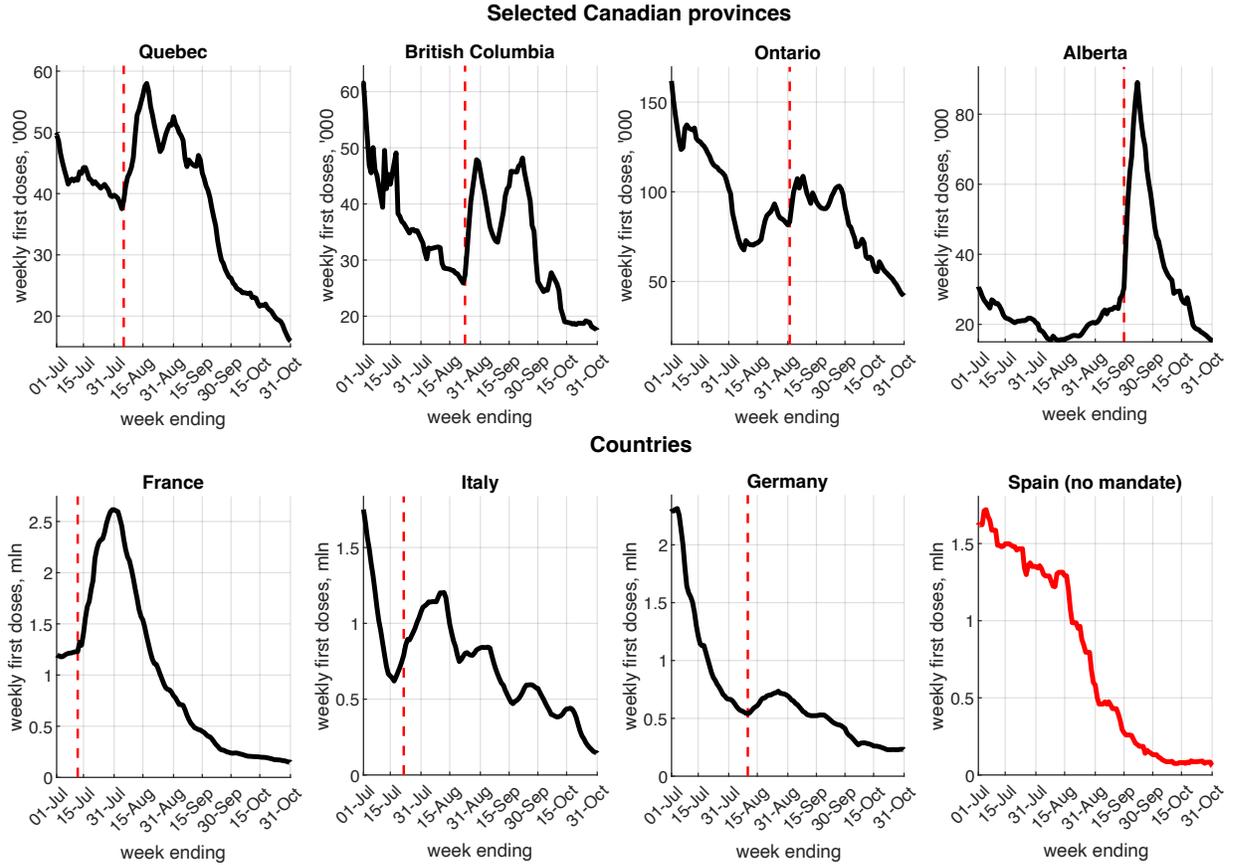
Notes: The figure plots the weekly first doses of COVID-19 vaccines administered for dates $t-6$ to t, where t is the date on the horizontal axis. The dashed red lines denote the proof of vaccination mandate announcement dates (for countries, this is the date of a *national* mandate). Spain had not announced a national proof of vaccination mandate as of October 31, 2021. We show the four most populated Canadian provinces totalling about 87% of Canada's population (see Fig. B4 for all ten provinces). Alberta also had a $100 debit card incentive for doses received between Sep. 3 and Oct. 14, 2021.

DID specification, using the period June 15 to Sep. 14, 2021 and splitting the data into five 'treated' provinces (those with the earliest announcements) and five 'control' provinces (not yet announced), we estimate a 66% (50.6 log points) average increase in weekly first doses following the announcement of a proof of vaccination mandate, relative to in absence of mandate. The increased uptake is sustained over the post-announcement period with available data. We obtain similar results, a more than 60% increase in weekly first doses on average over the post-announcement weeks, when using alternative dates and treatment groups varying from the first three to the first nine provinces to announce a mandate.

We complement the DID panel-data results with a structural break and time-series analysis, which allows us to study the mandates' longer-term impact on vaccine uptake (using

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our full data set up to October 31, 2021) and to obtain individual policy effect estimates for each province and country. We 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 all locations. The time-series analysis confirms the rapid and large increase in first-dose uptake after a mandate announcement relative to the no-mandate trend found in the DID analysis. The estimated effect varies across the provinces, from a 34% (29 log points) estimated increase in Ontario to over 300% in Alberta. We note that several factors may contribute 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 (both are negatively correlated with the size of the estimated initial effect). We find similar results for the countries – an average estimated initial increase in weekly first doses of 17% (15.4 log points) for France, 179% for Italy and 55% for Germany, relative to the respective pre-mandate trends.[7]

Simulation results using our DID estimates suggest that, as of September 14, 2021, by which date five provinces had announced proof of vaccination mandates, Canada saw about 287,000 additional first dose vaccinations, i.e., around 0.9 percentage points (p.p.) of the eligible age 12+ population, compared to the counterfactual scenario without mandates. In addition, using our time series estimates for each province, we calculate that, as of October 31, 2021, by which date all ten provinces had announced mandates, Canada's net vaccination gains grew to between 790,000 and 989,000 (2.4 to 3 p.p. of the eligible population), depending on the empirical specification. This is a significant increase in vaccine uptake considering the relatively short time in which it was achieved and the high pre-mandate first dose vaccination rate in Canada. Our simulations also imply large boosts in first-dose vaccinations relative to the no-mandate counterfactual in the three countries: 12.1 p.p. (6.5 mln additional doses) in Italy, 8 p.p. (4.6 mln doses) in France and 4.7 p.p. (3.5 mln doses) in Germany as of October 31, 2021, in our baseline specification. These estimated gains are larger than that for Canada, possibly because of the earlier mandate announcements and implementation in these countries or the lower starting vaccination rates, or they may partly reflect expanding the scope of the initial mandates, e.g., in Italy.

Vaccination mandates have been controversial, as some people perceive them as restrictions on personal freedom. This can affect compliance and increase both the direct implementation and enforcement costs, as well as the political costs of introducing a mandate. In this paper, we do not address ethical considerations; our goal is to assess the mandates'

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[7]A comparison of Fig. 1 and Fig. B2 suggests that the relatively low French estimate for the initial policy effect may reflect a lag between appointment booking and vaccine administration.

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effectiveness purely in terms of raising vaccine uptake, which can then be weighed against various costs or compared to other policies such as financial incentives (cash, gift cards, lotteries) or behavioral nudges (e.g., messages from experts, appointment reminders).

Unfortunately, there is little evidence that financial or behavioural nudges increase the vaccination rate among vaccine hesitant people. In particular, Chang et al. (2021) report results from a mid-2021 randomized controlled trial (RCT) with unvaccinated members of a large Medicare health plan in a California county where the vaccination rate at the time of the study was 77%. They examine the effect of $10 and $50 financial incentives, different public health messages, and an appointment scheduler on vaccination intentions and uptake within 30 days of the intervention. The authors find that none of the financial or behavioural treatments increased the vaccination rate among the vaccine hesitant. The proof-of-vaccination mandates we analyze target a similar group of unvaccinated people that have had the opportunity to be vaccinated for a long time. The population-weighted average first-dose vaccination rate on the dates of the provincial mandate announcements is 83.3%, which is even higher than that in Chang et al. (2021), suggesting that financial or behavioural incentives are even less likely to be effective in our setting.[8] On behavioral nudges, see also Dai et al. (2021), Alsan and Eichmeyer (2021) and Kluver et al. (2021). On financial incentives, see Barber and West (2021), Brehm et al. (2021), Sehgal (2021), and Walkey et al. (2021) on Ohio's vaccine lottery, with mixed results.

2 Data

2.1 Data sources and definitions

We use data on COVID-19 vaccination numbers, cases and deaths for all ten Canadian provinces, as well as for France, Italy, Germany and Spain.[9] Announcement and implementation dates of the proof of vaccination mandates were collected from the respective government websites and major newspapers (see Table C1).

The main variables in our statistical analysis are defined below. Everywhere, i denotes province or country and t denotes time measured in days (date). We aggregate the data on

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[8]In an RCT study in Sweden, Campos-Mercade et al. (2021) find that a modest payment of SEK 200 (USD 24) was associated with a 4.2 p.p. increase in vaccinations, relative to a baseline rate of 71.6% in the control group, while none of three behavioral nudges had impact. However, the RCT subjects were *yet to become eligible* to be vaccinated, and hence their setting is not comparable to ours, where weeks have elapsed after widespread vaccine availability.

[9]See Table C4 for the data sources and 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.

vaccinations, cases and deaths on a weekly basis (totals for the week ending on date t, i.e., dates $t-6$ to t) to reduce the influence of day-of-the-week effects and reporting artifacts.[10]

Outcome, V_{it}. Our main outcome variable is the logarithm of administered vaccine *first doses* per 100,000 people, V_{it}, for the week ending at t (dates $t-6$ to t). We use first doses as they most directly reflect the impact of proof of vaccination mandates on the intent to be immunized.[11] Using the logarithm of weekly first doses allows us to interpret the regression coefficients as percentage changes in the outcome; moreover, the estimates are invariant to normalization, e.g., by population, as its effect is subsumed in the regression constant or fixed effects. We thus use "log weekly first doses" for simplicity throughout the paper, except where the actual scale is important.

Policy, P_{it}. Let $\hat{t}_i$ be the announcement date of the proof of vaccination mandate in jurisdiction i. We construct a binary policy variable P_{it} equal to 1 for all post-announcement dates $t \geq \hat{t}_i$ and equal to 0 for all $t < \hat{t}_i$. Proof of vaccination mandates were announced in all ten Canadian provinces over the period August 5, 2021 to September 21, 2021 (see Table C1 and Fig. B3).[12] Several European countries, including France, Italy and Germany, also introduced proof of vaccination mandates in summer 2021 (see Table C1).

Information, I_{it}. We construct variables related to the underlying 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).[13] We refer to these variables jointly as *information*, I_{it}, (see Chernozhukov et al., 2021 or Karaivanov et al., 2021) since they can inform a person's COVID-19 exposure risk assessment and/or decision to be vaccinated.[14]

Controls, W_{it}. We include province fixed effects and date fixed effects in our panel data analysis. The province fixed effects account for any time-invariant province characteristics such as sentiment towards vaccination, age distribution, etc. The date fixed effects control

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[10]Some locations, e.g., British Columbia and Nova Scotia, do not report vaccination data on weekends and then report the total for several days at once (e.g., Monday's number contains 3 days of data). In these cases, we spread the reported total equally over the affected days.

[11]In Table A3, we also report results using second doses as the outcome. We find no statistically significant effect on second-dose uptake following the announcement of mandate.

[12]We use Sep. 17, 2021 as the mandate announcement date for Newfoundland and Labrador, the date on which the premier (provincial head of government) announced the upcoming mandate. The mandate details were officially unveiled on Oct. 7 after consultations with the business and other sectors. However, using the latter date would be problematic because of a potentially strong anticipation effect following the first announcement. Our main results in Table 1 and Fig. 3 are not affected by this choice. Throughout this paper, "Newfoundland" refers to the province of Newfoundland and Labrador.

[13]Another possible information variable is hospitalizations. However, it is very strongly correlated with the COVID-19 cases and deaths. To avoid multicollinearity, we do not include it in our baseline specification. See Table A1 for results using deaths and hospitalizations as information instead of deaths and cases.

[14]To tackle 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 Chernozhukov et al. (2021).

for nation-wide trends or events, e.g., public messaging, vaccine-related international travel regulations, or political campaigning for the September 2021 federal election. In the time-series analysis, we control for time trends.

Time period. 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.[15] We explore different sample start dates in robustness checks. 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 this period.

Figure 2: First doses after mandate announcement
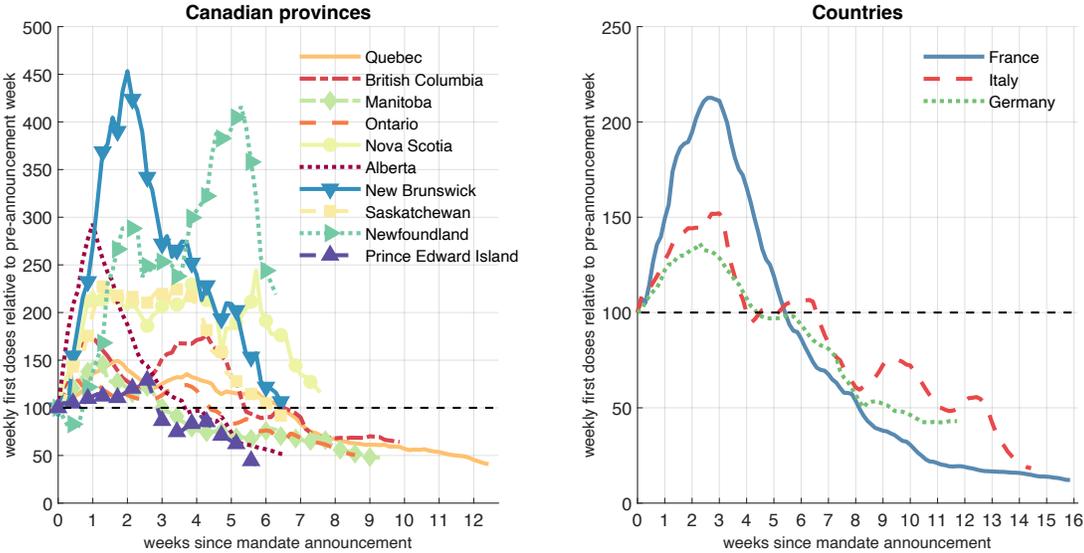
Notes: The figure plots the weekly administered first doses of COVID-19 vaccine for all dates after the mandate announcement against the number of weeks since the respective announcement date (denoted by 0 on the horizontal axis), as of Oct. 31, 2021. The weekly first doses for the week just prior to the mandate announcement are normalized to 100 for each respective province (on the left) or country (on the right).

2.2 Descriptive analysis

Fig. 2 plots the time profile of weekly first doses after a mandate announcement, with first doses in the week ending on the mandate announcement date normalized to 100. The figure only provides a raw-data illustration of the magnitude of the increase in weekly first doses following a mandate announcement. We follow up with formal analysis, controlling for information, fixed effects, pre-trends and other factors, in Sections 3 and 4.

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[15]In Canada, the provinces opened registration for first-dose vaccination for any person of age 12+ between May 10, 2021 in Alberta and May 27, 2021 in Nova Scotia, see Table C2. First-dose availability in France, Italy and Germany was similar by mid-June, at least for the 18+ age group.

Fig. 2 shows that weekly first doses in the Canadian provinces and in France, Italy and Germany grow quickly after the proof of vaccination mandate announcement, reach a peak at about 1 to 3 weeks after the announcement date and then decrease, as in the pre-announcement trend in most locations (see Fig. B4).[16] Several provinces, notably New Brunswick, Newfoundland, Alberta, Nova Scotia and Saskatchewan, as well as France, registered large increases in first-dose vaccine uptake of over 100% relative to the pre-announcement week. In other locations, e.g., Quebec, Ontario, Manitoba and Germany the observed increase is more moderate (under 50%).

3 Panel data results – Canadian provinces

3.1 Estimation

We estimate a behavioral 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 proof of vaccination mandate has been announced) and the current COVID-19 epidemiological and public health conditions, I_{it}, proxied by weekly cases and deaths. Based on the raw data patterns in Fig. 1 and Fig. B4 and the absence of vaccine supply constraints in the studied period, we assume no lag between the mandate announcement and a person's ability to receive a first vaccine dose.[17] Finally, we assume that individuals take into account the weekly public health indicators, I_{it}, for the week ending at date t, with no lag.

We use a difference-in-differences (DID) method to study the average effect of proof of vaccination mandates in Canada. In addition to information I_{it}, we control for other potential confounding variables or unobserved heterogeneity by including time and location fixed effects. We estimate the following equation:

$$V_{it} = \alpha P_{it} + \beta I_{it} + \gamma_t + \delta_i + \varepsilon_{it} \tag{1}$$

where V_{it} is log weekly first doses for location i and date t, I_{it} is information, γ_t are date fixed effects, δ_i are province fixed effects, and ε_{it} is the error term. The coefficient α on the policy variable 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} broken into separate indicator variables, one for each week after the mandate announcement date. To correct for the small number

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[16]British Columbia, Ontario, Nova Scotia, Saskatchewan and Newfoundland exhibit a second peak later.

[17]For our main model (difference-in-differences), we perform sensitivity analysis using alternative lags of up to 7 days (see Section 3.2 and Fig. B5); the results affirm our baseline choice of zero lag.

of clusters in the estimation due to there being only ten provinces, we compute and report "wild bootstrap" p-values (Cameron et al., 2008).[18]

The recent methodological literature has argued that the standard OLS two-way fixed effects (TWFE) estimator can be invalid in panel-data settings with staggered adoption like ours if there is heterogeneity in the treatment effect across groups/cohorts (provinces in this paper) and/or over time. The reason is that the TWFE estimate is a weighted average of many 2-by-2 DID treatment effects, where some of the weights can be negative or incorrect because of contamination from other periods (Goodman-Bacon, 2021).[19] In particular, Sun and Abraham (2021) develop an 'interaction-weighted' difference-in-differences estimator that is valid under these conditions and which we use to estimate (1). The Sun and Abraham estimator uses never-treated or last-treated units as the control group and is constructed as an appropriately weighted average of treatment effects for each cohort (by date of mandate announcement) and each relative time after or before the announcement.[20]

The Sun-Abraham estimator requires excluding from the estimation all time periods in which units in the control group are treated. Since the last province to announce a mandate is Prince Edward Island, on September 21, 2021, this means that we can use only data until September 20, at the latest (see Table C1). We present results for different control groups and corresponding sample periods in Table 2. We also compare our baseline results with the OLS TWFE estimates in Table A1. In addition, in Section 4 we use time-series analysis to supplement our DID results, which also allows us to use all available data until October 31.

3.2   Main results

Fig. 3 displays 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 proof of vaccination mandate. $T = 0$ denotes the week starting at the announcement date. The reference point is one week before the mandate announcement ($T = -1$). We use the Sun and Abraham (2021) treatment effect heterogeneity robust estimator and use as the 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. The chosen control group implies September 14, 2021 (the day

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[18]We use the Stata function *boottest* (see Roodman et al., 2019) clustered by province with 4,999 repetitions. Alternative ways of computing the standard errors are displayed in Table A2.

[19]See also Athey and Imbens (2018), de Chaisemartin and D'Haultfoeuille (2018), Callaway and Sant'Anna (2021) and Sun and Abraham (2021).

[20]We use the Stata function *eventstudyinteract* provided by the authors.

before mandate announcement in Alberta and New Brunswick, see Table C1) as the (latest possible) sample end date in our baseline specification. We present results with different control groups, corresponding to both earlier and later end dates, in Table 2.

Figure 3: Canadian provinces – event study
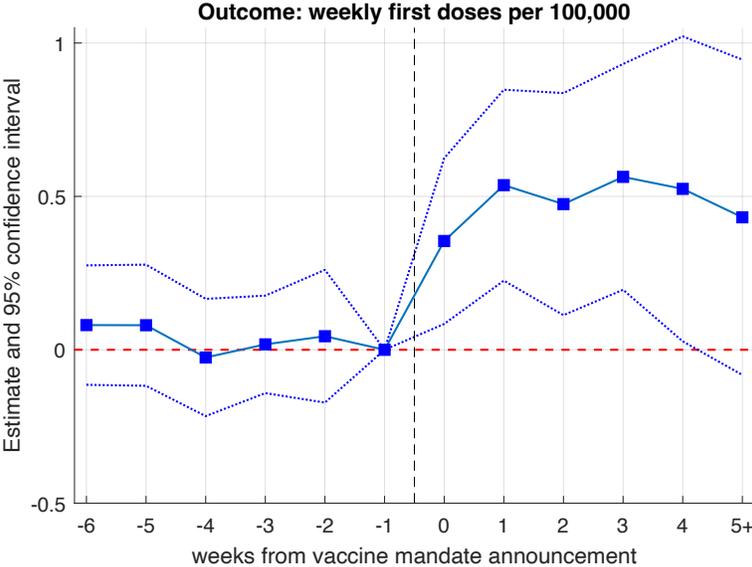
Outcome: weekly first doses per 100,000

Notes: Sun and Abraham (2021) treatment effect heterogeneity robust estimates. Sample period: June 15 to September 14, 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 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 6 weeks before ($T = -6$) to 5 or more weeks after the announcement ($T = 5$), where $T = 0$ is the week of the announcement. The reference point is one week before the announcement ($T = -1$). The dotted lines correspond to 95% confidence intervals.

We make several observations on Fig. 3. First, there is no mandate-associated pre-trend – the Sun-Abraham DID estimates before the mandate announcement are statistically indistinguishable from zero. This addresses the potential endogeneity concern 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. Third, the mandate effect on the pace of first-dose vaccinations is large in magnitude — an increase of 35 log points (42 percent) in weekly first doses in the first post-announcement week and 71% in the second week, each relative to one week before the announcement.[21] The observed quick increase

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[21] A coefficient estimate x is equivalent to $100x$ log points or a $100(e^x - 1)$ percent increase in weekly doses. The smaller $T = 0$ estimate reflects the fact that the outcome variable is a backward-looking weekly sum.

in uptake mitigates possible concerns that vaccine supply or scheduling constraints may be affecting our results (see also Fig. 2 for a raw data plot of the post-announcement increase in first-dose vaccinations by province). Fourth, the average policy effect persists over time over the six-week post-announcement period we analyze ($T = 0$ to $T = 5$). Unfortunately, the limitations of the data (the timing of the announcements) and the need for a valid not-yet-treated control group do not allow us to investigate longer horizons. We return to this question in Section 4 using time-series analysis and all available data until October 31.

Table 1 shows the Sun-Abraham DID estimates of equation (1) along with p-values from wild bootstrap standard errors clustered by province in the square brackets.[22] Columns (1) and (2) (without and with controlling for weekly cases and deaths) show that the mandate announcement is associated with an average increase of about 50.5 log points or 66 percent (p-value< 0.01) in weekly first doses.

In column (3) of Table 1, we report estimates from a variant of equation (1) where the policy variable P_{it} is split into six binary variables, one for each week after the mandate announcement date, to account for dynamic effects. The results indicate a fast increase in weekly first doses of 36 log points (43 percent, $p < 0.01$) in the week beginning at the announcement date (week 0). The increase is sustained until the sample end date, as all coefficients are positive ($p < 0.02$ each week, except for "week 5+", $p < 0.1$) and indeed larger than in week 0. (We note that the later estimates are based primarily (week 3) or only (week 4) on post-mandate vaccinations in Quebec, the province with the earliest announcement.) Thus, the DID analysis provides no evidence that short-term net intertemporal substitution, which would be manifested as a negative policy estimate for the later weeks, is the mechanism behind the observed boost in vaccinations in the studied period.

In Table 2 (for estimates by week after announcement) and the top left panel of Fig. B5 (for the average estimate), we use different sets of provinces as control group, from Prince Edward Island (PEI) only (using data up to Sept. 20), to all provinces except Quebec, British Columbia and Manitoba (using data up to Aug. 31). Column (4) of Table 2 reproduces our baseline estimates from specification (3) in Table 1. We find that our main results from Table 1 remain robust to using alternative control groups.

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[22]Table A2 also reports alternative standard error specifications: regular 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 (Stata command "boottest").

Table 1: Canadian provinces – difference-in-differences results

Outcome: log weekly vaccine first doses, V_{it}			
p-values in [ ]	(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-squared	0.817	0.820	0.821
sample size, N	920	920	920
province fixed effects	X	X	X
date fixed effects	X	X	X

Notes: Sun and Abraham (2021) treatment effect heterogeneity robust estimates. Sample period: June 15 to September 14, 2021 using Alberta, New Brunswick, Saskatchewan, Prince Edward Island and Newfoundland as control group (latest treated). "week n", where $n = 0, 1, 2, ...$, 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 the square brackets. *, ** and *** denote 10%, 5% and 1% significance respectively. The results in column (3) are not directly comparable to those in Fig. 3 because, in Table 1, the reference point is the entire pre-announcement period.

3.3 Robustness analysis

Alternative initial dates. In Fig. B5, top right panel, we show that our estimates of the effect of a mandate announcement on first dose vaccine uptake are not very sensitive to the choice of initial sample date between May 1 and Jul. 15, 2021. This provides further

Table 2: Canadian provinces - different control groups

	Outcome: log weekly vaccine first doses, V_{it}					
end date	Sep. 20	Sep. 16	Sep. 15	Sep. 14	Sep. 7	Aug. 31
control group	PE	(1)+NL	(2)+SK	(3)+AB,NB	(4)+NS	(5)+ON
p-values in [ ]	(1)	(2)	(3)	(4)	(5)	(6)
week 0	0.372 ***	0.364 **	0.351 **	0.359 ***	0.384 ***	0.404 ***
	[0.001]	[0.019]	[0.017]	[0.005]	[0.009]	[0.000]
week 1	0.561 ***	0.552 ***	0.545 ***	0.543 ***	0.620 ***	0.837 ***
	[0.001]	[0.001]	[0.001]	[0.001]	[0.002]	[0.000]
week 2	0.442 **	0.469 **	0.477 **	0.498 ***	0.687 ***	0.767 ***
	[0.029]	[0.025]	[0.019]	[0.010]	[0.000]	[0.000]
week 3	0.540 ***	0.705 ***	0.695 ***	0.705 ***	0.739 ***	0.712 ***
	[0.006]	[0.002]	[0.001]	[0.001]	[0.002]	[0.002]
week 4	0.690 **	0.712 **	0.712 **	0.713 ***	0.710 **	
	[0.025]	[0.018]	[0.018]	[0.018]	[0.026]	
week 5	0.650 *	0.651 *	0.642 *	0.651 *		
	[0.059]	[0.059]	[0.060]	[0.056]		
week 6+	0.492 *	0.651 *				
	[0.089]	[0.060]				
R-squared	0.811	0.818	0.820	0.821	0.829	0.839
sample size, N	980	940	930	920	850	780
province fixed effects	X	X	X	X	X	X
date fixed effects	X	X	X	X	X	X
cases and deaths	X	X	X	X	X	X

Notes: Sun and Abraham (2021) treatment effect heterogeneity robust estimates. The table reports estimates for different treatment and control (latest treated) groups of provinces which determine the sample end date. "week n", where $n = 0, 1, 2, ...$, 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 cases and deaths 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 the square brackets. *, ** and *** denote 10%, 5% and 1% significance respectively.

reassurance that vaccine supply constraints are not a major concern for our results.

Lags. In equation (1), we assume no lag between the mandate announcement P_{it} and a person's ability to receive the first dose (the outcome V_{it}). In practice, a few days of delay may occur (e.g., from making an appointment to receiving the vaccine) even in the absence of supply constraints. The bottom left panel of Fig. B5 displays the coefficient estimates $\hat{\alpha}$ of the policy effect when assuming a lag of zero (our baseline) to seven days. The mandate effect estimates remain large and statistically significant when varying the lag length, with a slight

decrease for the longer lags. The bottom right panel of Fig. B5 shows that the R-squared value is the highest when the lag is zero, supporting our choice of baseline specification.[23]

Other robustness checks. We perform several additional robustness checks in Table A1. For easier comparison, column (1) replicates the baseline estimates from columns (2) and (3) of Table 1. Column (2) of Table A1 does not include cases and deaths, whose coefficients are not significant in the baseline specification. In column (3), we control for log weekly deaths and log average weekly hospitalizations as information. Column (4) reports the estimates of a weighted specification using the provincial populations as weights and the baseline set of information variables (cases and deaths). The estimates are smaller than in the unweighted baseline (column 1), suggesting that smaller provinces contribute more to the the estimated effect of mandate announcements on vaccine uptake. In column (5), we re-estimate equation (1) using the standard OLS two-way fixed effects (TWFE) estimator which, as discussed in Section 2.3, can be biased in the presence of heterogeneous treatment effects over cohorts and/or time. In our setting, we find that these estimates differ relatively little from our baseline Sun-Abraham treatment effect heterogeneity robust estimates.

In Table A1 column (6), we use log of daily (instead of weekly) first doses as the outcome and log of daily cases and deaths as information and find similar estimates to those in our baseline specification, although larger in week 0 since the outcome is not a moving sum. In column (7), we use the *level* of weekly first doses per 100,000 people as the outcome. The estimates are noisier than in the baseline and indicate that a mandate announcement increases weekly first dose uptake by roughly 482 per 100,000 people on average ($p < 0.06$), and up to 754 per 100,000 ($p < 0.07$) in week 4 after the announcement. Overall, all these alternative specifications confirm the robustness of our main finding of statistically significant and large impact of mandate announcements on vaccine uptake.

Randomization inference. Difference-in-differences inference from the model in equation (1) relies on asymptotic approximations requiring a sufficiently large number of provinces. While we do account for the fact that there are only 10 provinces in our sample by using wild bootstrap to compute the estimates' p-values, as an additional robustness check, we implement a variant of Fisher's randomization test (Fisher, 1935). Specifically, we estimate equation (1) 5,000 times by randomly assigning the mandate announcement date for each of the 'treated' provinces in our baseline specification. Fig. B6 plots the histogram of the randomized inference (placebo) mandate effect estimates, along with the 95% confidence band.

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[23]See Hsiang et al. (2020) for a similar lag determination approach. We also checked a specification in which we lag the information variables I_{it} (log weekly cases and deaths) with the same lag as the policy P_{it}, and the results are nearly identical.

The figure shows that only 2.88% of the 5,000 placebo estimates are larger than our baseline estimate (0.506) from column (2) of Table 1 (the solid vertical line on Fig. B6), offering assurance that our main result does not hinge on asymptotic inference approximations.

3.4 Second doses

In Table A3, we estimate the specifications in Table 1 using log of weekly *second doses* as the outcome. There are two potential mechanisms through which proof of vaccination mandates may have an impact on second dose uptake: (i) people that already had a first dose decide to receive their second dose soon after the announcement or implementation (e.g., because most mandates require full vaccination) and (ii) people that took a first dose because of the mandate take their second dose roughly 4 weeks after the first. We do not find a statistically significant effect of mandate announcements on second dose uptake in our sample period. This is consistent with second doses being more spread out after the announcement. We also plot weekly second doses over time on Fig. B7 and, unlike the sharp increases for first doses, we only see relatively small and gradual second-dose increases after the mandate announcements in some of the (smaller) provinces.

3.5 Mandate implementation

Our main results use the regional variation in the timing of announcement of proof of vaccination mandates to examine their effect on vaccine uptake. In addition, there is variation in the interval between the mandates' announcement and *implementation* (coming into force), as well as in the implementation dates themselves – see Table C1 and Fig. B4.[24] We checked whether we can detect additional effects on first-dose uptake, associated with the mandates' implementation, by adding to equation (1) an indicator variable that equals to 1 post-implementation and 0 otherwise. The estimate on this indicator variable is positive but not statistically significantly different from zero (not displayed). However, we cannot conclude that there do not exist implementation effects, since even the latest feasible sample end date (Sep. 20, 2021) necessitated by the requirement to have a not yet treated control group is only soon after the mandate implementation date in 3 of the 10 provinces (see Table C1).

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[24]The mandates were implemented in all provinces between Sep. 3 and Oct. 22, 2021.

4 Time series analysis

4.1 Estimation

We complement our panel data analysis for Canada by time-series analysis for each of the ten Canadian provinces as well as for France, Italy and Germany. While the Sun and Abraham (2021) difference-in-differences approach in Section 3 allows us to reliably estimate the impact of mandate announcements on new first doses in Canada by exploiting the variation in the timing of the mandates across provinces, it can only be used with data from before the last treatment date and hence it does not allow us to evaluate the longer-term effect of the mandates. In contrast, the time-series approach allows us to use all available data until October 31, 2021 to estimate the mandate effect on vaccine uptake and the additional vaccinations compared to the counterfactual of no mandate separately for each province and country.

We use the *interrupted time-series analysis* (ITSA) framework (e.g., Linden, 2015) and estimate the following equation for each country or province[25]

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

where V_t are log weekly first doses for the week ending at date t, c is a constant, I_t is proxy for information analogous to its counterpart in equation (1), and η_t is the error term. In our baseline time-series specification, we construct the policy variable P_t as the weekly average from date $t-6$ to t of the 'mandate announced' indicator P_{it} defined in Section 2.1.[26] We include 7-day and 14-day lagged values of V_t (instead of $t-1$ or $t-2$ autoregressive terms) 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 which takes 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 and the post-announcement time trend (with slope $\tau_1 + \tau_2$) in weekly first doses.

The time-series estimation approach requires stronger identifying assumptions than the DID approach since there is no control group. Specifically, we need to assume that the time-

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[25]We omit the subscripts i for simplicity since all variables refer to the same location.

[26]Using a weekly average of the policy indicator 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 Tables A5 and A6.

series process of weekly first doses changed after the mandate announcement only because of the policy, i.e., that it would have followed the same pre-trend if no mandate had been announced. Another needed assumption is exogeneity of the announcement date. We also cannot control for fixed effects in a flexible way beyond including a constant and time trends. Under these assumptions, the coefficient on the policy variable P_t captures the average level 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 time trend of first doses after the mandate announcement, relative to the pre-announcement trend.

4.2 Structural break at the announcement date

We first show the impact of the proof of vaccination mandate announcements on first dose uptake in Table A4, where we perform a structural break test (Chow (1960) test for a known break point) for the log weekly first doses, V_t. The presence of a break point in V_t at the mandate announcement date indicates an abrupt change (shift) in the first-dose time series process. We use a bandwidth of 50 days before and 35 days after the mandate announcement date.[27] The before-after bandwidth was chosen to reduce the size distortion of the test since the outcome variable is a weekly sum and the error terms are serially correlated.[28] Columns (1) and (2) in Table A4 use the log of weekly first doses, V_t. For all ten Canadian provinces and all three countries, we strongly reject the null hypothesis that the mandate announcement date is not a structural break ($p < 0.01$).

We also perform the structural break test using first-differenced weekly first doses in Table A4 column (3). The differenced series is stationary, and the error terms are not serially correlated, which alleviates concerns about size distortion in the Chow test. The null hypothesis of no structural break at the announcement date is rejected with $p < 0.05$ in most locations except Saskatchewan and three Atlantic provinces.[29] Overall, the structural break test results suggest that a mandate announcement is strongly associated with a trend break in first-dose vaccine uptake in all or most locations depending on the specification.

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[27]The results are also robust to picking June 15, 2021 as the initial date for the test.

[28]As shown in Giles and Scott (1992), the size of the Chow test is distorted upward (over-rejection of the null) in the presence of positive serial correlation in the error terms. The size distortion is mitigated if the sample split is unequal before vs. after the break point.

[29]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.

4.3 Results

In Table 3, we report the results from estimating the time-series model (2) for each country and province in our data.[30] We include log weekly cases and deaths as information, I_t. In columns (1)-(3), we report the estimated coefficients on the mandate announcement variable, P_t, the time trend, T_t, and the interaction time trend, T_t^a, in equation (2). Since the outcome and information variables are level variables that can introduce non-stationarity, we use the augmented Dickey-Fuller (ADF) unit root test to test whether the residuals η_t are stationary, to avoid a spurious regression.[31] 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 coefficient estimates $\hat{\pi}$ on the mandate announcement variable P_t in Table 3 are statistically significantly positive ($p < 0.01$) and large for all three countries. Italy has the largest estimated boost to weekly first doses after the mandate announcement (102.5 log points or 179% increase relative to the pre-announcement trend), followed by Germany (55% increase) and France (17% increase).[32] The baseline time trend is downward sloping ($\hat{\tau}_1 < 0$) in all countries and indicates a steep decrease in vaccinations of 1.7% to 2.9% per day. The post-announcement trend in first doses turns less steep in all three countries (the post-announcement slope change estimate $\hat{\tau}_2$ is positive, although only statistically significant for Germany), which suggests a lack of net intertemporal substitution as of October 31.

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[30]The estimates' p-values are calculated using the Newey-West heteroskedasticity and autocorrelation (HAC) robust estimator (Newey and West, 1987) with 3 lags. The lag length was chosen as the closest integer to $T^{1/4}$, where T is the sample size.

[31]The ADF test results in column (4) use 13 lags chosen as the integer part of $12(T/100)^{1/4}$, as suggested in Schwert (1989). For fewer lags the test tends to yield smaller p-values.

[32]France's estimate is sensitive to including log weekly cases in I_t. We obtain a larger estimate ($\hat{\pi} = 0.468$, or 60% increase) when using deaths and hospitalizations as information variables, see Table A5.

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

Notes: Time period – June 15 to October 31, 2021. Each row is a separate time-series regression as specified in (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_{it} 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 (HAC) standard errors with 3 lags are in the square brackets. *, ** and *** denote 10%, 5% and 1% significance respectively. Column (4) reports augmented Dickey-Fuller (ADF) test statistics and p-values using 13 lags (Schwert, 1989).

Turning to the Canadian provinces, in our baseline specification in Table 3, we obtain statistically significantly positive and large estimates $\hat{\pi}$ of the increase in weekly first doses after mandate announcement for all provinces except for Quebec.[33] The statistically significant policy estimates range from 0.293 in Ontario (equivalent to a 34% increase in first doses relative to the pre-announcement trend) to 1.455 in Alberta (a 328% increase). This range of estimates is consistent with our DID estimate in Table 1 for the average policy effect for Canada, although the two estimation methods are not directly comparable. The estimated effect of the mandate announcement tends to be larger for provinces experiencing a surge in cases at the same time (for which we control in I_t), namely Alberta, Saskatchewan and New Brunswick. These provinces also announced their mandates relatively late (mid-September) and set a relatively short time interval between mandate announcement and implementation (see Table C1). 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 Fig. B8.

As in the results for the countries, the estimates of the baseline time trend in first doses, τ_1, are negative for all ten provinces (although not statistically significant in the two smallest provinces, Newfoundland and Prince Edward Island). The estimates $\hat{\tau}_2$ of the post-announcement change in slope of the time trend are large and statistically significantly negative for Alberta, New Brunswick, Saskatchewan, and PEI (British Columbia's estimate is also negative but not statistically significant). This result is consistent with the mandate effect on new first doses diminishing over time in these provinces.[34]

In Table A5, we also present results from two alternative specifications of equation (2): using log weekly deaths and log average weekly hospitalizations as information, in columns (1) and (2), and using a binary (instead of weekly-averaged) policy variable P_t. We obtain results that are very similar to our baseline findings in Table 3 and confirm the positive and large estimated effect of mandate announcements on first dose vaccine uptake.[35]

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[33]The estimate for Quebec is 0.389 and statistically significantly different from zero when we use deaths and hospitalizations as information I_t; see Table A5.

[34]We note that with hospitalizations replacing cases as control variable (see column (2) of Table A5), $\hat{\tau}_2$ is negative, albeit sometimes without statistical significance, for all provinces but Manitoba and Nova Scotia. Estimates for the latter two provinces show very negative $\hat{\tau}_1$, possibly due to the abrupt decrease in vaccinations about four weeks before their mandate announcements (see Fig. B4). Thus, we cannot rule out that a negative change in trend may be quite general for Canadian provinces.

[35]The policy effect estimates are positive but not statistically significantly different from zero in one specification in column (1) and 3 specifications in column (3) of Table A5, out of 26 total specifications.

5 Counterfactual analysis

We use the difference-in-differences estimate $\hat{\alpha}$ of the average effect of mandate announcements on weekly first doses from Table 1 to evaluate the counterfactual of no proof of vaccination mandates in any Canadian province. Define the counterfactual log weekly first doses $\tilde{V}_{it}^{did}$ per 100,000 people in province i for the week ending at date t as

$$\tilde{V}_{it}^{did} = \hat{V}_{it}^{did} - \hat{\alpha}P_{it}, \tag{3}$$

where $\hat{V}_{it}^{did}$ is the fitted value of log weekly first doses for the week ending at t in equation (1), and $\hat{\alpha}$ is the DID estimate on the policy variable P_{it} (i.e., 0.506 in Table 1, column (2)). Averaging across the provinces i, we obtain $\tilde{V}_t^{did}$ as the estimated counterfactual log weekly first doses per 100,000 people in Canada in the absence of mandates.

We also use the time-series location-specific estimates of the mandate effect from Section 4 and Table 3 to construct no-mandate counterfactual weekly vaccine doses separately for each province and for France, Italy and Germany. Accounting for the lagged terms in (2), define the counterfactual log weekly doses per 100,000 people, $\tilde{V}_t^{ts}$ as

$$\tilde{V}_t^{ts} = \hat{V}_t^{ts}, \text{ for all } t < \hat{t} \text{ and}$$
$$\tilde{V}_t^{ts} = \hat{V}_t^{ts} - \hat{\pi}P_t - \hat{\tau}_2 T_t^a + \hat{\lambda}_1 \left(\tilde{V}_{t-7}^{ts} - \hat{V}_{t-7}^{ts} \right) + \hat{\lambda}_2 \left(\tilde{V}_{t-14}^{ts} - \hat{V}_{t-14}^{ts} \right), \text{ for all } t \geq \hat{t}, \tag{4}$$

where $\hat{t}$ is the mandate announcement date, $\hat{V}_t^{ts}$ is the fitted value of log weekly first doses for the week ending at t in regression equation (2), $\hat{\pi}$ is the coefficient estimate of the mandate announcement variable P_t from column (1) of Table 3, $\hat{\tau}_2$ is the estimate of the interaction time trend T_t^a from column (2) of Table 3, and $\hat{\lambda}_1$ and $\hat{\lambda}_2$ are the coefficient estimates on the lagged terms V_{t-7} and V_{t-14} in equation (2) (not displayed). The policy effect estimate $\hat{\pi}$, as well as $\hat{\tau}_2$, $\hat{\lambda}_1$ and $\hat{\lambda}_2$ are specific for each respective country or province.

5.1 DID counterfactual – Canada

In Fig. 4, we start from the actual total doses on the day before the mandate announcement and add up forward in time using the weekly counterfactual doses $\tilde{V}_t^{did}$ defined in equation (3).[36] We then compare the counterfactual total doses (the dotted red line on Fig. 4) with the actually observed total doses (the black diamonds).

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[36]Since the Sun-Abraham Stata code *eventstudyinteract* does not support computing the regression fitted value, we use the observed values V_{it} in place of $\hat{V}_{it}^{did}$ in (3), i.e., assume the same residuals as in the data.

Figure 4: Canada – observed vs. no-mandate counterfactual first doses as of Sep. 14, 2021
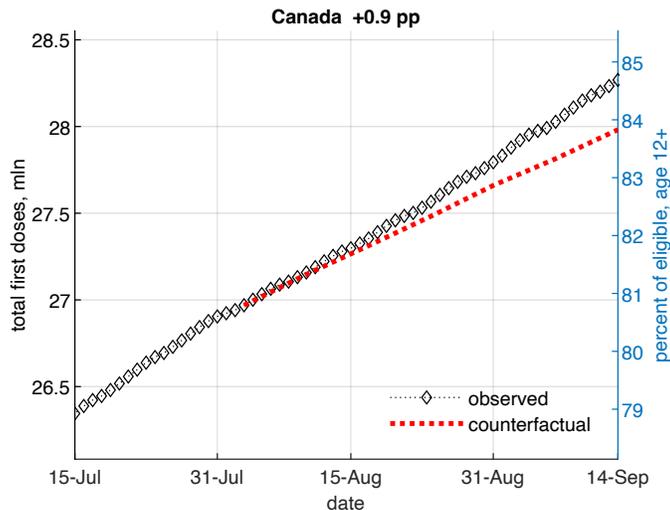
Notes: The figure plots the observed (black diamonds) and the expected no-mandate counterfactual (dotted red line) cumulative first doses (in millions) by date. The counterfactual uses the P_{it} coefficient estimate from column (2) of Table 1. The number in the caption indicates the percentage point increase in first doses relative to the no-mandate counterfactual as of September 14, 2021.

We estimate that, as of September 14, 2021, the sample end date in Table 1, the proof-of-vaccination mandates in the five Canadian provinces that announced mandates by that date have led to 287,000 additional first doses, or an increase in new first-dose vaccinations of 0.9 p.p. of the eligible population relative to the no-mandate counterfactual. This number equals approximately 4.4 percent of all unvaccinated eligible people of age 12 or older at the time of the first proof-of-vaccination mandate announcement in Canada (August 5, 2021 in Quebec). This is a sizable increase in vaccine uptake from a very high base rate (above 80% with first dose) achieved in a short time (less than 6 weeks) among vaccine hesitant people.

5.2 Time series counterfactuals

5.2.1 Canadian provinces

We next use the time-series estimates in Table 3 to compute counterfactual weekly doses, $\tilde{V}_t^{ts}$, as defined in (4), and counterfactual total first doses for each province. The results are displayed in Fig. 5.[37] The left vertical axis displays total first doses in millions. The right axis displays the total doses in terms of the percentage of eligible people (age 12 and above; see Table C3). The number next to the province name is the estimated percentage-point

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[37]In Figure B9, we also display these results in terms of actual vs. counterfactual log weekly doses.

(p.p.) increase in first doses relative to the no-mandate counterfactual, as of October 31, 2021. In Table A6, we display the gains in thousands of doses, along with estimated gains from alternative specifications.

Figure 5: Canadian provinces – observed vs. no-mandate counterfactual first doses as of October 31, 2021 (time-series estimates)
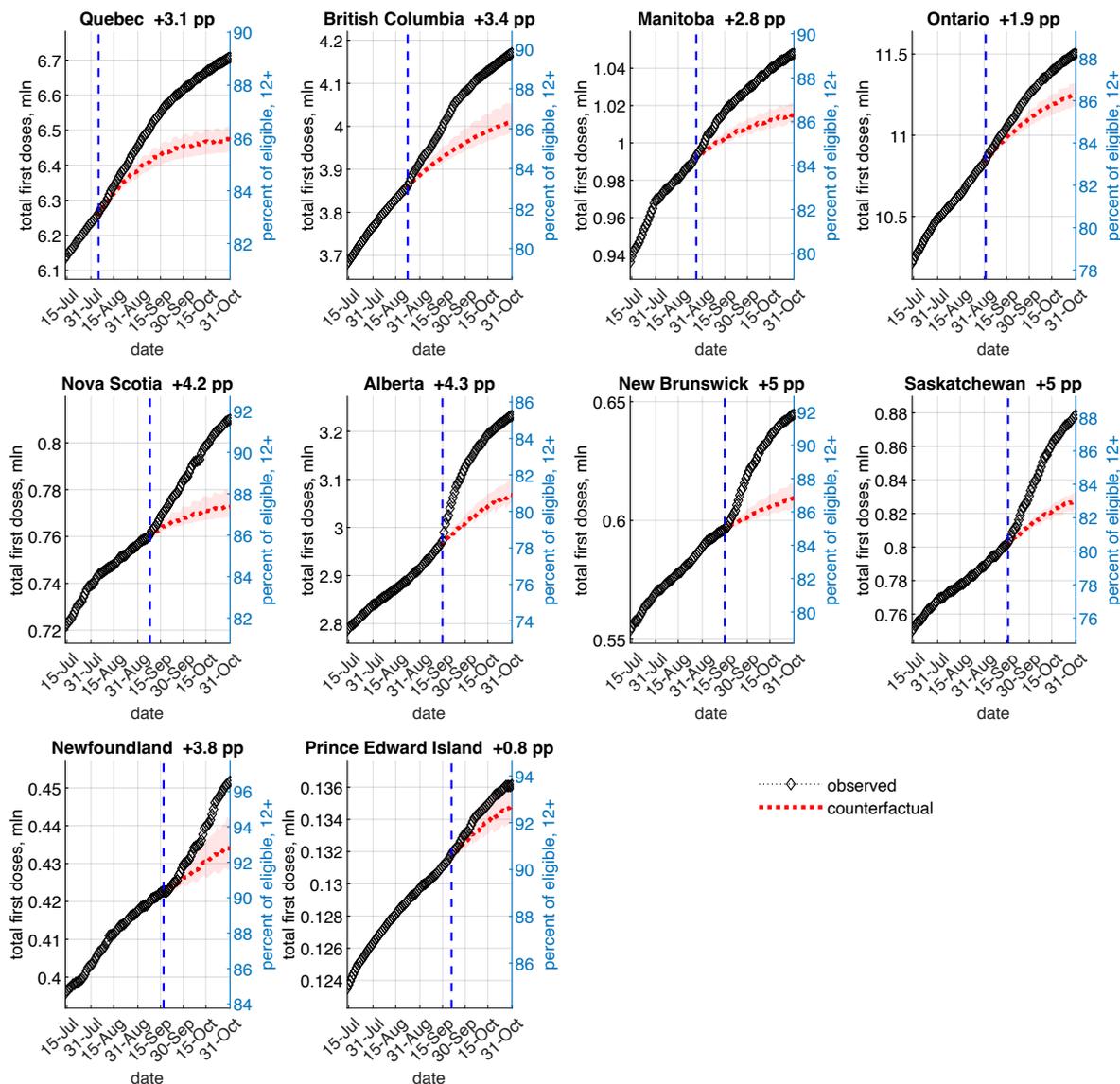
Notes: The figure plots the observed (black diamonds) and the expected no-mandate counterfactual (dotted red lines) cumulative first doses by date and province. The shaded areas denote 5-95 percent confidence bands. The vertical dashed line denotes the mandate announcement date. The counterfactuals are computed using the estimates in Table 3. The number next to each province name indicates the percentage point increase in first doses relative to the no-mandate counterfactual, as of Oct. 31, 2021.

Fig. 5 shows a relatively quick divergence and sizable gap between the observed and counterfactual cumulative first-dose numbers. The largest absolute gains in new vaccinations (left axis of Fig. 5 and column (1) in Table A6) are naturally in the four largest provinces (Ontario, Quebec, Alberta and British Columbia). As a proportion of the eligible population (right axis), however, we estimate the largest mandate impacts on vaccine uptake to be in Saskatchewan and New Brunswick (5 p.p.), followed by Alberta (4.3 p.p.) and Nova Scotia (4.2 p.p.). These gains occurred within 6 to 10 weeks of the mandate announcement for most provinces (13 for Quebec) and are on top of a high base vaccination rate (see Table C3). For example, Saskatchewan's 5 p.p. estimated gain amounts to about one quarter of all unvaccinated eligible people on the mandate announcement date (Sep. 16, 2021).

We do not find evidence of significant net intertemporal substitution (decrease in the cumulative first-dose gains due to pulling vaccinations forward in time) as of October 31, 2021, except in PEI. However, the strongly negative τ_2 estimates in Table 3 column (3) and the trends in Fig. B9 indicate that Alberta, New Brunswick and Saskatchewan are projected to exhibit net intertemporal substitution soon after the end of October 2021. That said, given the flattening of the counterfactual curves, cumulative gains are likely to remain significant.

5.2.2 Countries

We also use our time-series estimates in Table 3 to estimate the gains in first dose uptake for France, Italy and Germany, relative to the counterfactual assuming the absence of a proof of vaccination mandate. The results are displayed in Fig. 6 and Table A6. The estimated increases in first-dose uptake after the mandate announcements are large in all three countries: 12.1 p.p. (6.54 mln additional first doses) in Italy, 8 p.p. (4.60 mln doses) in France, and 4.7 p.p. (3.46 mln doses) in Germany. The larger estimated effect in Italy could be partly attributed to the complementary vaccination requirements for inter-regional travel and for employment introduced in September and October.

All counterfactual simulations assume that all explanatory variables except the mandate announcement (e.g., deaths or time fixed effects) would remain fixed at their observed values, and that the estimated model coefficients remain stable. This is a strong assumption that is more plausible over relatively short periods. These results should therefore be interpreted with caution and as an illustration of the estimated impact of the proof of vaccination mandates on vaccine uptake.

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Figure 6: Countries – observed vs. no-mandate counterfactual first doses
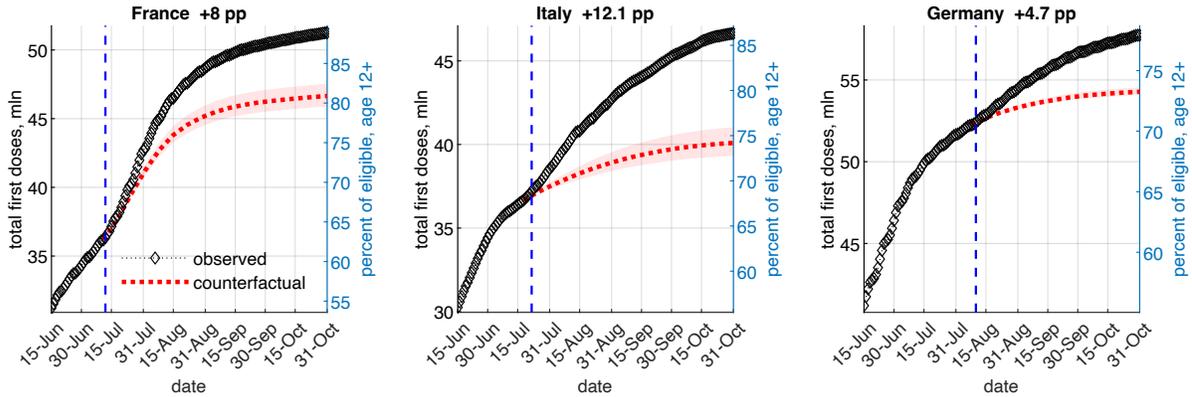
Notes: The figure plots the observed (black diamonds) and the expected counterfactual (dotted red line) cumulative first doses for each country by date. The shaded areas denote 5–95 percent confidence bands. The vertical dashed line denotes the mandate announcement date. The counterfactuals are computed using the estimates from Table 3. The number next to each country name indicates the percentage point increase in first doses relative to the no-mandate counterfactual, as of October 31, 2021.

6   Discussion and conclusions

We evaluate the impact of government-mandated proof of vaccination requirements on COVID-19 vaccine uptake. Using difference-in-differences analysis and the variation of timing of mandate announcements across the Canadian provinces, we estimate a statistically significant and large (about 66% on average) increase in weekly first-dose vaccine uptake in Canada over the first weeks after the announcement of a proof of vaccination mandate, relative to in absence of mandate. We obtain similar results from time-series analysis for each Canadian province and for France, Italy and Germany — a sizable rise in new first-dose vaccinations after a mandate announcement relative to the pre-announcement trend.

The estimated mandate announcement effect varies across the Canadian provinces, with the provinces exhibiting a larger initial gain tending to drop back toward the baseline pace of vaccination more quickly. Using simulations based on our time-series results, we estimate that, as of October 31, 2021, all the provinces and all three countries achieved large net increases in the first-dose vaccination rate relative to the hypothetical scenario of absence of proof of vaccination mandate (2.4 to 3 percentage points (p.p.) of the vaccine-eligible population for Canada as a whole, 8 p.p. for France, 12.1 p.p. for Italy and 4.7 p.p. for Germany). These time-series estimates may include effects from complementary measures (e.g., vaccination requirements for health workers or for inter-regional travel, etc.) adopted after the original proof of vaccination mandates, particularly in the European countries.

In terms of external validity, in June 2020, Lazarus et al. (2021) conducted a survey on vaccine hesitancy across 19 countries comprising around 55% of the world population.[38] Canada, Italy and Germany placed around the middle in self-reported vaccine hesitancy rate (29% to 31%), while France had a higher hesitancy rate (41%). In this regard, our results on the large impact of proof of vaccination mandates on first-dose uptake in these four countries can be useful to public health authorities in other countries looking for an effective strategy to increase vaccine uptake.

Our objective is not to engage in normative or ethical debates related to vaccination mandates (e.g., Gostin, 2021), but to provide a benchmark range of estimates for their effectiveness in terms of new first dose uptake, which can be compared to other approaches such as monetary incentives or behavioural nudges.

Monetary incentives for vaccination have been criticized by many for the 'optics' of putting a (low) dollar value on being vaccinated,[39] because of the perceived unfairness in "rewarding" people who delayed their vaccination, or, if payments are unavailable to those that were vaccinated earlier, because of potential moral hazard problems (e.g., expecting future payments). Related to this, Chang et al. (2021) find that financial incentives may even have the perverse effect of validating vaccine concerns among unvaccinated individuals aged 40 or above and those that supported Donald Trump in the 2020 U.S. presidential election (see also Hsieh, 2021). Others have argued that, given the already high vaccination rates in developed countries, behavioural nudges may not be very effective (Thaler, 2021), which is consistent with the findings of Chang et al. (2021) and Campos-Mercade et al. (2021).

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.[40] Our results are a first step toward quantifying the benefits of proof of vaccination requirements.

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[38]The participants were asked: "If a COVID-19 vaccine is proven safe and effective and is available, will you take it?"

[39]For example, U.S. President Joseph Biden advocated in July 2021 that state and local governments pay $100 to anyone willing to receive a first dose. However, the public health value of vaccination is likely much higher (see Castillo et al., 2021).

[40]For example, one component of the social value of vaccination consists of avoided healthcare costs. Barber and West (2021) estimate that the Ohio vaccine lottery program saved the state USD 66 million in intensive care unit costs.

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Author contributions

Competing interests

Data availability

All data sources are described in Appendix C. The data used in the study are available at: https://github.com/C19-SFU-Econ/dataV.

Appendix A. Additional Tables

Table A1: Robustness

p-values in [ ]	Outcome: log weekly first vaccine doses, V_{it}					daily outcome (6)	level outcome (7)
	baseline (1)	no cases & deaths (2)	deaths & hospitaliz. (3)	population weighted (4)	OLS TWFE (5)		
A. All post-announcement dates							
mandate announced P_{it}	0.506 ***	0.504 ***	0.507 ***	0.363 ***	0.494 **	0.513 ***	481.6 *
	[0.001]	[0.002]	[0.000]	[0.003]	[0.020]	[0.001]	[0.052]
R-squared	0.820	0.817	0.821	0.888	0.812	0.577	0.662
B. By week after the mandate announcement							
week 0	0.359 ***	0.344 **	0.362 ***	0.229 **	0.378 ***	0.517 ***	309.9 *
	[0.005]	[0.020]	[0.002]	[0.027]	[0.007]	[0.001]	[0.082]
week 1	0.543 ***	0.546 ***	0.545 ***	0.328 *	0.570 **	0.394 ***	505.3 *
	[0.001]	[0.003]	[0.000]	[0.088]	[0.018]	[0.002]	[0.056]
week 2	0.498 **	0.509 **	0.503 **	0.335 *	0.520 *	0.431 ***	510.0 *
	[0.010]	[0.016]	[0.010]	[0.056]	[0.054]	[0.000]	[0.088]
week 3	0.705 ***	0.693 ***	0.693 ***	0.451 ***	0.677 ***	0.672 ***	676.3 *
	[0.001]	[0.001]	[0.000]	[0.000]	[0.004]	[0.001]	[0.027]
week 4	0.713 **	0.723 **	0.705 ***	0.370	0.672 **	0.758 ***	754.1 *
	[0.018]	[0.020]	[0.008]	[0.109]	[0.022]	[0.003]	[0.068]
weeks 5+	0.651 *	0.678 **	0.658 **	0.286	0.650 *	0.510 *	732.3
	[0.056]	[0.041]	[0.046]	[0.423]	[0.052]	[0.060]	[0.119]
R-squared	0.821	0.819	0.823	0.896	0.816	0.580	0.663
sample size, N	920	920	920	920	920	920	920
province fixed effects	X	X	X	X	X	X	X
date fixed effects	X	X	X	X	X	X	X

Notes: Sun and Abraham (2021) treatment effect heterogeneity robust estimates. Sample period: June 15 to September 14, 2021 using Alberta, New Brunswick, Saskatchewan, Prince Edward Island and Newfoundland as control group (latest treated); "week n", where $n = 0, 1, 2, ...$, 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. All specifications include log weekly cases C_{it} and log weekly deaths D_{it} as information controls unless otherwise specified. P-values from wild bootstrap (boottest) standard errors clustered by province with 4,999 repetitions are reported in the square brackets. Column (1) reproduces the baseline estimates from columns (2)–(3) of Table 1. Column (2) does not control for cases and deaths. Column (3) uses log average weekly hospitalizations and log weekly deaths as information. Column (4) reports the estimates from a weighted version of equation (1) with the provinces' populations used as weights. Column (5) displays results from estimating (1) via the standard OLS two-way fixed effects (TWFE) method. Column (6) uses log *daily* first doses as the outcome. Column (7) uses the *level* of weekly first doses per 100,000 people as the outcome. *, ** and *** denote 10%, 5% and 1% significance respectively.

Table A2: Robustness – standard errors

	Outcome: log weekly first doses, V_{it}		
	(1)		(2)
mandate announced	0.506		
	(0.001)	***	
	[0.001]	***	
	{0.000}	***	
week 0			0.359
			(0.004) ***
			[0.005] ***
			{0.005} ***
week 1			0.543
			(0.002) ***
			[0.001] ***
			{0.000} ***
week 2			0.498
			(0.006) ***
			[0.010] **
			{0.009} ***
week 3			0.705
			(0.000) ***
			[0.001] ***
			{0.001} ***
week 4			0.713
			(0.003) ***
			[0.018] **
			{0.016} **
week 5+			0.651
			(0.011) **
			[0.056] *
			{0.055} *
R-squared	0.820		0.821
sample size, N	920		920
province fixed effects	X		X
date fixed effects	X		X
cases and deaths	X		X

Notes: P-values from standard clustering (Stata command "cluster") by province in the ( ) parentheses; wild bootstrap (Stata command "boottest") with one-way clustering by province and 4,999 repetitions in the [ ] square brackets, and wild bootstrap with two-way clustering by province and date with 4,999 repetitions in the { } curly braces. "week n", where $n = 0, 1, 2, ...,$ 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 cases and deaths variables are log weekly totals for dates $t - 6$ to t. *, ** and *** denote 10%, 5% and 1% significance respectively.

Table A3: Second doses

Outcome: log weekly vaccine second doses, S_{it}			
p-values in [ ]	(1)	(2)	(3)
mandate announced, P_{it}	0.099	0.116	
	[0.405]	[0.298]	
week 0			0.041
			[0.630]
week 1			0.117
			[0.252]
week 2			0.145
			[0.195]
week 3			0.201
			[0.176]
week 4			0.177
			[0.243]
week 5+			0.185
			[0.120]
R-squared	0.848	0.853	0.854
sample size, N	920	920	920
province fixed effects	X	X	X
date fixed effects	X	X	X
log weekly cases and deaths		X	X

Notes: Sun and Abraham (2021) treatment effect heterogeneity robust estimates. Time period: June 15 to September 14, 2021 using Alberta, New Brunswick, Saskatchewan, Prince Edward Island and Newfoundland as control group (latest treated); "week n", where $n = 0, 1, 2, ...$, 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 cases and deaths variables are log weekly totals for dates $t - 6$ to t. P-values from wild bootstrap (boottest) standard errors clustered by province with 5,000 repetitions are reported in the square brackets. *, ** and *** denote 10%, 5% and 1% significance respectively.

Table A4: Test for structural break at the mandate announcement date

	announcement date	Outcome: log weekly vaccine first doses		
		H_0 rejection p-value		
		(1)	(2)	(3)
Quebec	Aug. 5, 2021	0.0000	0.0000	0.0000
British Columbia	Aug. 23, 2021	0.0000	0.0000	0.0000
Manitoba	Aug. 27, 2021	0.0000	0.0000	0.0021
Ontario	Sep. 1, 2021	0.0000	0.0000	0.0002
Nova Scotia	Sep. 8, 2021	0.0000	0.0000	0.2067[†]
Alberta	Sep. 15, 2021	0.0000	0.0000	0.0001
New Brunswick	Sep. 15, 2021	0.0000	0.0000	0.0423
Saskatchewan	Sep. 16, 2021	0.0000	0.0000	0.2299[†]
Newfoundland	Sep. 17, 2021	0.0000	0.0000	0.2392[†]
Prince Edward Island	Sep. 21, 2021	0.0000	0.0000	0.9409[†]
France	Jul. 12, 2021	0.0000	0.0000	0.0238
Italy	Jul. 22, 2021	0.0000	0.0000	0.0412
Germany	Aug. 10, 2021	0.0000	0.0000	0.0005
first-differenced outcome				X
controls: cases, deaths and time trend			X	X

Notes: The table reports the test statistics for the Chow structural break test with known break point. The null hypothesis H_0 is that there is no structural break at the mandate announcement date. Column (1) uses equation (2) but only including V_{t-7} and V_{t-14}. Column (2) in addition controls for log weekly deaths, log weekly cases and the linear time trend T_t. Column (3) uses the first difference of log weekly first doses and includes deaths, cases and a time trend. We use a bandwidth of 50 days before and 35 days after the announcement date (T = 86). [†]denotes that H_0 cannot be rejected at the 90% confidence level.

Table A5: Time series estimates – alternative specifications

| p-values in [ ] | Outcome: log weekly vaccine first doses | | | |
| | deaths and hospitalizations | | binary P_t | |
	policy $\hat{\pi}$ (1)	trend change $\hat{\tau_2}$ (2)	policy $\hat{\pi}$ (3)	trend change $\hat{\tau_2}$ (4)
Countries				
France	0.468 ***	-0.009 **	0.040	0.004
	[0.000]	[0.026]	[0.321]	[0.205]
Italy	0.516 **	0.014	0.799 ***	-0.000
	[0.035]	[0.132]	[0.003]	[0.967]
Germany	no hospitalizations		0.274 ***	0.007 ***
	data available		[0.001]	[0.002]
Canadian provinces				
Quebec	0.389 ***	-0.003	0.016	0.018 **
	[0.000]	[0.387]	[0.841]	[0.027]
British Columbia	0.626 ***	-0.007 ***	0.566 ***	-0.003
	[0.000]	[0.000]	[0.000]	[0.507]
Manitoba	0.735 ***	0.019 **	0.586 ***	0.012 *
	[0.000]	[0.018]	[0.002]	[0.066]
Ontario	0.322 ***	-0.002	0.209 **	0.002
	[0.003]	[0.459]	[0.022]	[0.763]
Nova Scotia	0.726 ***	0.002	0.470 ***	0.026 ***
	[0.007]	[0.782]	[0.003]	[0.003]
Alberta	1.401 ***	-0.028 ***	0.983 ***	-0.015 ***
	[0.000]	[0.000]	[0.000]	[0.001]
New Brunswick	1.220 ***	-0.014 **	0.723 ***	-0.007
	[0.000]	[0.041]	[0.006]	[0.431]
Saskatchewan	0.809 ***	-0.009 *	0.644 ***	-0.000
	[0.000]	[0.087]	[0.000]	[0.945]
Newfoundland	0.327	-0.001	0.126	0.011
	[0.146]	[0.944]	[0.534]	[0.431]
Prince Edward Island	0.323 *	-0.019 ***	0.362 ***	-0.012 **
	[0.053]	[0.002]	[0.005]	[0.047]
sample size for each row, T = 139				

Notes: Time period – June 15 to October 31, 2021. All rows include 7-day and 14-day lags of the outcome variable. Columns (1) and (3) report the coefficient estimate $\hat{\pi}$ on the "mandate announced" policy variable P_t in equation (2). Columns (2) and (4) report the estimate $\hat{\tau_2}$ on the post-announcement (interaction) time trend. Columns (1) and (2) use log weekly deaths and log average weekly hospitalizations as information; columns (3) and (4) use a binary (not weekly averaged) policy variable P_t. P-values computed using Newey-West heteroskedasticity and autocorrelation robust (HAC) standard errors with 3 lags are in the square brackets. *, ** and *** denote 10%, 5% and 1% significance respectively.

Table A6: Counterfactuals – estimated additional first doses relative to no mandate

specification:	baseline cases and deaths		deaths and hospitalizations		binary P_t	
	max (1)	Oct. 31 (2)	max (3)	Oct. 31 (4)	max (5)	Oct. 31 (6)
Countries (million doses)						
France	4.60	4.60	6.42	4.79	2.94	2.94
Italy	6.54	6.54	6.26	6.26	5.83	5.83
Germany	3.46	3.46	no hosp. data		3.40	3.40
Canadian provinces (thousands doses)						
Quebec	236	236	177	177	230	230
British Columbia	160	160	116	116	153	153
Manitoba	33.1	33.1	36.1	36.1	32.7	32.7
Ontario	253	253	195	195	213	213
Nova Scotia	37.3	37.3	34.9	34.9	37.3	37.3
Alberta	165	165	142	142	162	162
New Brunswick	35.2	35.2	33.5	33.5	27.6	27.6
Saskatchewan	50.2	50.2	42.2	42.2	55.0	55.0
Newfoundland	17.8	17.8	13.4	13.4	14.6	14.6
Prince Edward Island	1.3	1.2	0.6	-0.2	1.5	1.5
Canada (total)		989		790		927

Notes: The table displays the difference between total observed first-dose vaccinations and the estimated counterfactual first doses in the absence of vaccination mandate. Columns (1) and (2) use the baseline specification from Table 3 with log weekly cases and deaths as information to compute the counterfactuals. Columns (3)–(4) and (5)–(6) use the alternative specifications from Table A5, with log weekly deaths and log average weekly hospitalizations as information or using a binary policy variable instead of weekly average. The odd-numbered columns ('max') display the maximum value of the difference (the maximum gains) over the whole post-announcement period; the even-numbered columns ('Oct. 31') display the estimated difference as of the sample end date, October 31, 2021.

Appendix B. Additional Figures

Figure B1: Share of people with at least one dose – example countries
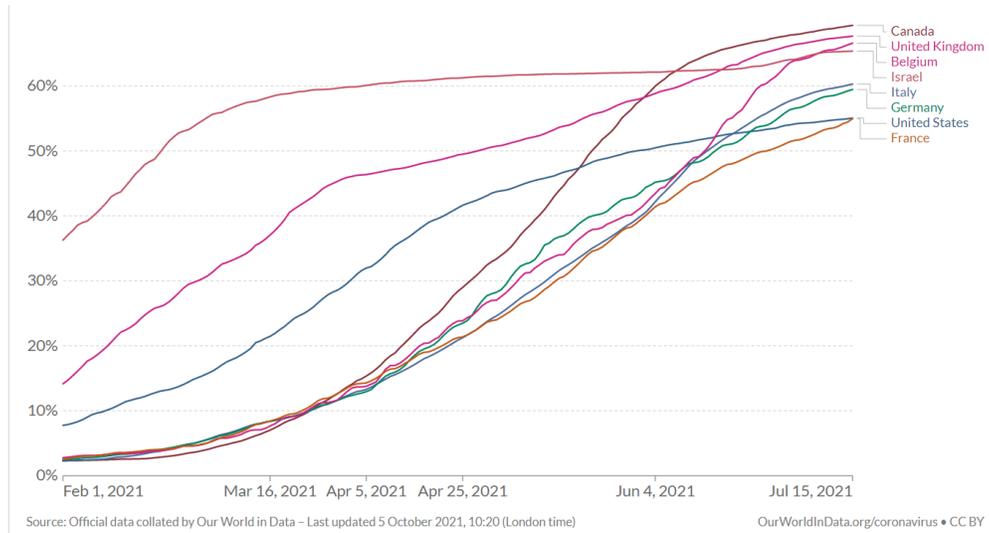
Notes: The Figure plots the share of the population who have received at least one dose of a COVID-19 vaccine in selected countries. Source: Our World in Data.

Figure B2: France – first-dose vaccination appointments
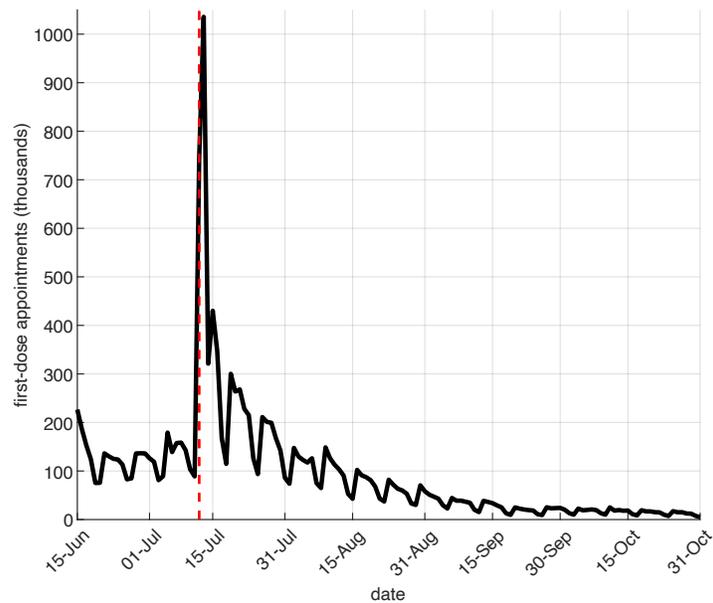
Notes: The figure plots the daily first-dose vaccination appointments by date made on Doctolib, a booking website accounting for about 2/3 of cumulative COVID-19 vaccinations in France as of Q4 2021. Source: doctolib.fr. The dashed red line denotes the mandate announcement date, July 12, 2021.

Figure B3: Canada – proof of vaccination mandates over time
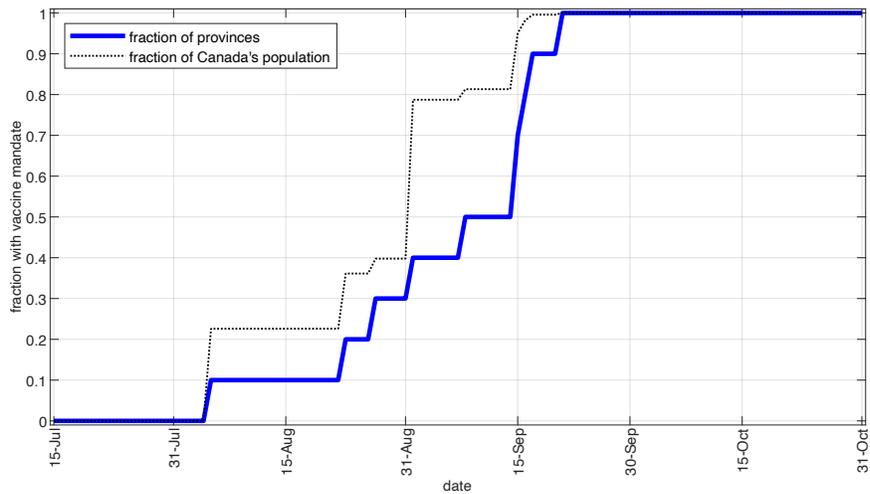
Notes: The figure plots the cumulative fraction of provinces and the cumulative fraction of Canada's population for which a proof of vaccination mandate has been announced. See Table C1 for the exact dates of mandate announcement in each province.

Figure B4: Canadian provinces – first doses per 100,000 people
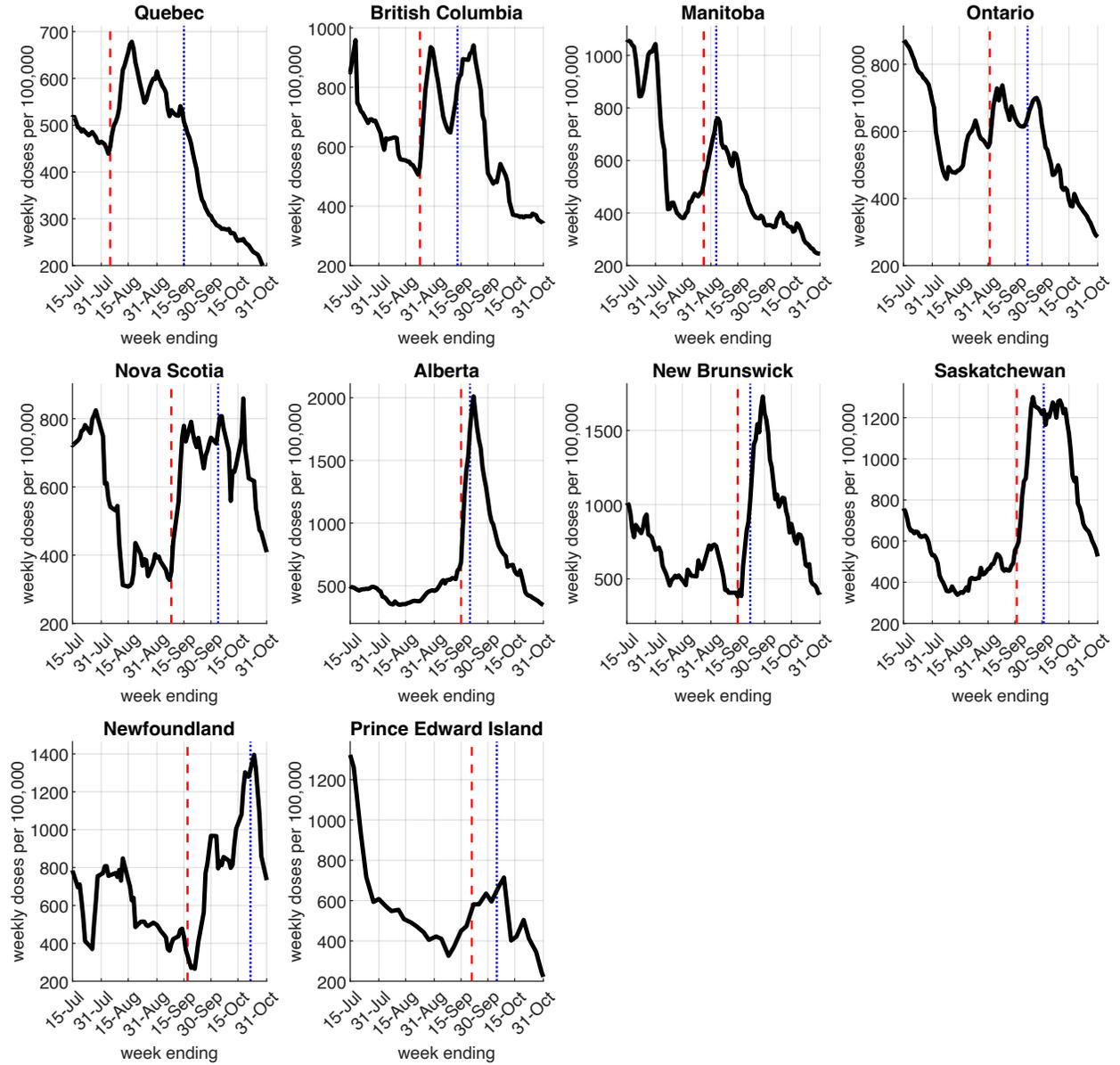
Notes: The figure plots the weekly administered COVID-19 vaccine first doses per 100,000 people for dates $t-6$ to t, where t is the date on the horizontal axis. The dashed red lines denote the proof of vaccination mandate announcement date for each province. The dotted blue lines denote the mandate implementation (enforcement) date for each province (see Table C1).

Figure B5: Robustness
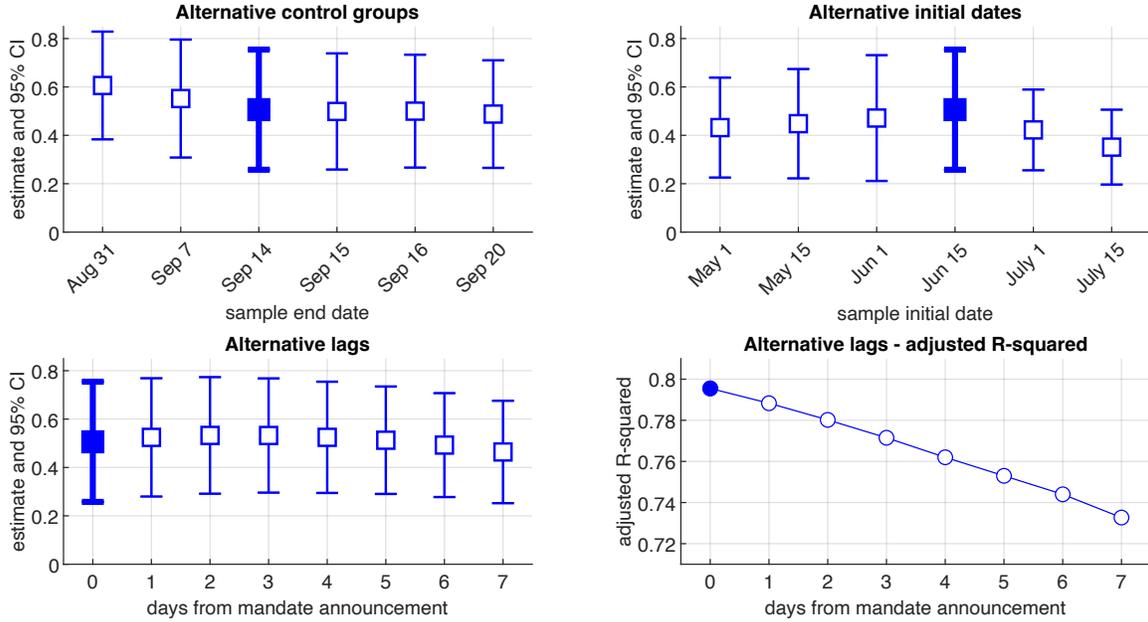
Notes: The top left panel shows the estimates $\hat{\alpha}$ for the mandate announcement policy variable P_{it} in equation (1) with different sample end dates and corresponding control groups, relative to the baseline (Sep. 14, in bold). The top right panel shows the coefficient estimate for P_{it} with different initial sample dates (May 1 to July 15, 2021) relative to the baseline (June 15, 2021, in bold). The bottom left panel shows the estimates from a variant of equation (1) when using no lag in the announcement P_{it} (in bold, our baseline) and when using lags of up to 7 days, i.e., using P_{it-k} for $k = 1, 2, ..., 7$. The bottom right panel displays the adjusted R-squared values corresponding to the different lags.

Figure B6: Random assignment of announcement dates
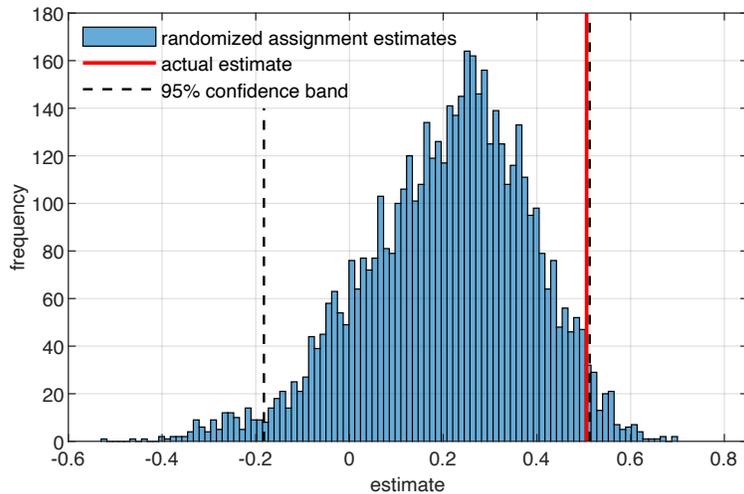
Notes: We estimate equation (1) 5,000 times using the Sun and Abraham (2021) treatment effect heterogeneity robust estimator after randomly assigning the date of mandate announcement for each province which has announced a mandate by Sep. 14. The figure plots the histogram of these placebo inference estimates, along with the 2.5-th and 97.5-th percentiles (dashed lines). The solid vertical red line corresponds to the baseline estimate from column (2) in Table 1.

Figure B7: Canadian provinces - second doses per 100,000 people
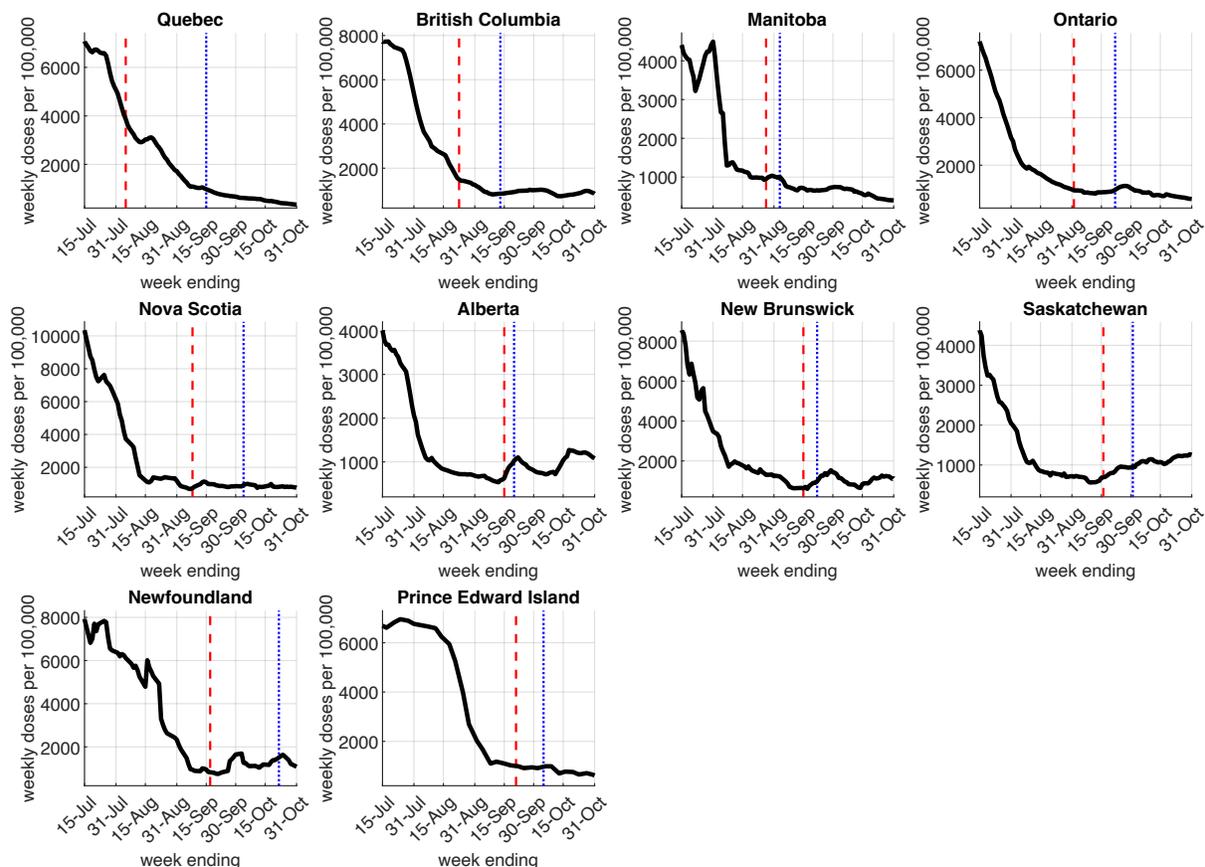
Notes: The figure plots the weekly administered COVID-19 vaccine *second* doses per 100,000 people for dates $t-6$ to t, where t is the date on the horizontal axis. The dashed red lines denote the vaccination proof mandate announcement date for each province. The dotted blue lines denote the mandate implementation (enforcement) date for each province (see Table C1).

Figure B8: Time-series policy estimates – correlations
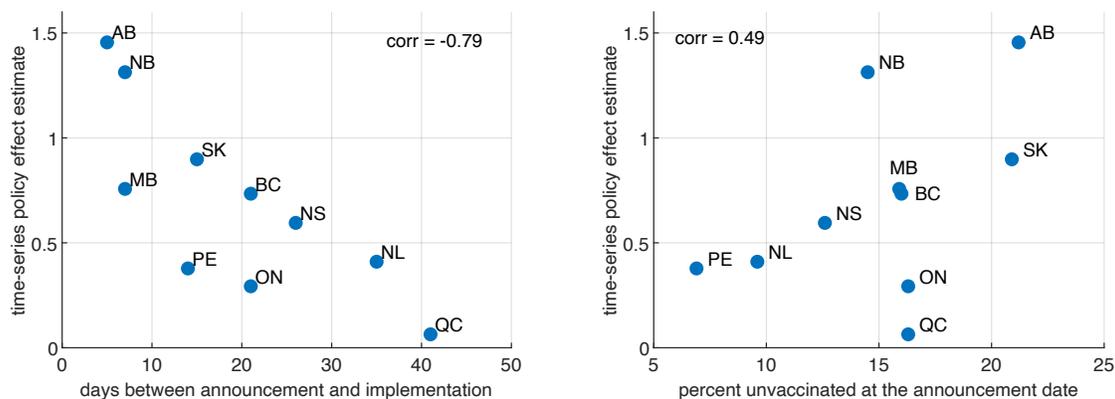
Notes: The figure plots the time-series policy estimates, $\hat{\pi}$ from Table 3 column (1) against the number of days between mandate announcement and implementation (left) and the percent remaining unvaccinated eligible people at the announcement date (right). The figure is for illustration; no causal claims are made.

Figure B9: Canadian provinces - observed vs. no-mandate counterfactual weekly first doses as of October 31, 2021 (time-series estimates)
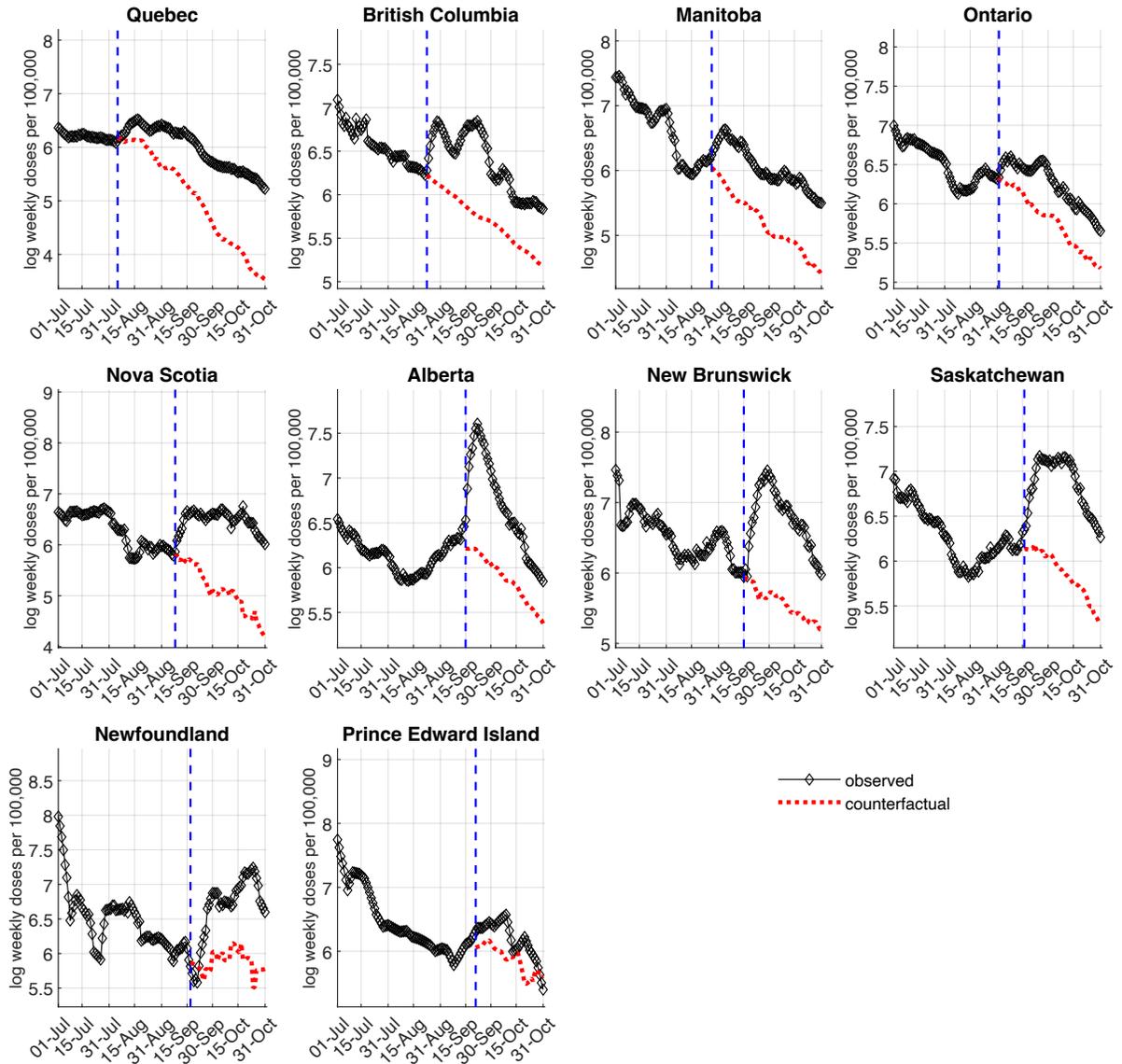
Notes: The figure plots the observed (black diamonds) and the expected no-mandate counterfactual (dashed red lines) log weekly first doses per 100,000 people. We use the estimates from Table 3 to compute the counterfactuals, as specified in (4). The dashed blue vertical lines denote the mandate announcement date for each province.

Appendix C. Supplementary Information

Table C1: Proof of vaccination mandate announcement and implementation dates

	Announcement date	Implementation date
Canadian provinces		
Quebec	August 5, 2021	September 15, 2021
British Columbia	August 23, 2021	September 13, 2021
Manitoba	August 27, 2021	September 3, 2021
Ontario	September 1, 2021	September 22, 2021
Nova Scotia	September 8, 2021	October 4, 2021
Alberta	September 15, 2021	September 20, 2021
New Brunswick	September 15, 2021	September 22, 2021
Saskatchewan	September 16, 2021	October 1, 2021
Newfoundland and Labrador[1]	September 17, 2021	October 22, 2021
Prince Edward Island	September 21, 2021	October 5, 2021
Countries		
France	July 12, 2021	July 21, 2021
Italy	July 22, 2021	August 6, 2021
Germany	August 10, 2021	August 23, 2021

Notes: Some jurisdictions implemented the proof of vaccination mandates in stages, e.g., in terms of the extent of vaccination required (at least one dose or full vaccination) or in terms of the scope of the mandate (e.g., only for very large events or across a wide array of settings); in these cases, we report the date on which the first stage with wide application started to be enforced. See Table C5 for more details on the mandates' scope. [1]Newfoundland's mandate was announced by the provincial premier on Sep. 17, 2021, but the full details were officially unveiled on Oct. 7, 2021.

Table C2: Canada – age 12+ eligibility dates for first vaccine dose

Quebec	May 25, 2021
British Columbia	May 19, 2021
Manitoba	May 14, 2021
Ontario	May 23, 2021
Nova Scotia	May 27, 2021
Alberta	May 10, 2021
New Brunswick	May 26, 2021
Saskatchewan	May 20, 2021
Newfoundland and Labrador	May 17, 2021
Prince Edward Island	May 18, 2021

Notes: The table reports the first date at which any person of age 12 or higher was eligible to register and receive first dose of a COVID-19 vaccine in the respective province.

Table C3: Vaccine eligible population (age 12 or older)

	Total eligible	Percent unvaccinated on mandate announcement date
Quebec	7,532,499	16.3%
British Columbia	4,644,795	16.0%
Manitoba	1,176,020	15.9%
Ontario	13,038,060	16.3%
Nova Scotia	884,351	12.6%
Alberta	3,791,765	21.2%
New Brunswick	701,871	14.5%
Saskatchewan	996,908	20.9%
Newfoundland and Labrador	467,761	9.6%
Prince Edward Island	145,432	6.9%
France	57,668,019	37.0%
Italy	54,036,939	31.2%
Germany	74,100,000	29.3%

Notes: The table reports the number of vaccine eligible people (age 12 or older) by province or country. The numbers are approximate (best estimates). Sources: Canada (link), France (link), Italy (link), Germany (link).

Table C4: Data sources

Location	Vaccinations	Cases	Deaths
Canadian provinces			
Alberta (AB)	link	link	link
British Columbia (BC)	link[1]	link	link
Ontario (ON)	link	link	link
Quebec (QC)	link	link	link
Saskatchewan (SK)	link	link	link
Nova Scotia (NS)	link[1]	link	link
Manitoba (MB)	link	link	link
New Brunswick (NB)	link	link	link
Newfoundland and Labrador (NL)	link[2]	link	link
Prince Edward Island (PE)	link	link	link
Countries			
France, Italy, Germany	Our World In Data (OWID)		

Notes: 1. The BC and NS vaccinations data was manually collected from the official government bulletins. 2. The source for NL vaccination data is the COVID-19 Canada Open Data Working Group, which archives official information from the provincial dashboards. Hospitalizations data (used in robustness checks) is from the COVID-19 Tracker aggregator database.

Table C5: Proof of vaccination mandates scope

Canadian Provinces

Quebec	Health and social facilities (hospitals, rehabilitation, etc.); Private seniors' residences; indoor sports and physical activities; outdoor organized sports and physical activities involving frequent or prolonged contact; Outdoor events and festivals with more than 50 spectators or participants; Sporting events or shows (play, concert, musical or comedy shows etc.) in outdoor stadiums or outdoor stage; Agricultural fairs; Festivals or celebrations; Walks, marathons, cycling circuits; Golf tournaments; Immersive or walking tours; Performance venues; Stadiums and arenas; Auditoriums; Movie theatres; Indoor sport events at which the number of spectators exceeds 250 people seated in assigned seats; Bars and restaurants, including patios; Fast food restaurant dining rooms, including patios; Nightclubs; Microbreweries; Distilleries; Shopping mall food courts and food stores; Arcades, theme parks, amusement parks and centres, recreation centres, and water parks; Casinos and gambling halls, including bingo; Tourist and recreational cruises; Conventions and conferences.
British Columbia	Indoor ticketed sporting events with more than 50 people; Indoor concerts, theatre, dance and symphony events with more than 50 people; Licensed restaurants and cafes and restaurants and cafes that offer table service (indoor and patio dining); Liquor tasting rooms in wineries, breweries or distilleries; Pubs, bars and lounges (indoor and patio); Nightclubs, casinos and movie theatres; Gyms, exercise and dance facilities or studios; Businesses offering indoor exercise/fitness; Indoor adult group and team sports for people 22 or older; Indoor organized events with more than 50 people (e.g., weddings, funeral receptions, organized parties, conferences, trade fairs and workshops); Indoor organized group recreational classes and activities with more than 50 people; Post-secondary student housing; Spectators at indoor youth sporting events with more than 50 people.
Manitoba	Indoor and outdoor ticketed sporting events and concerts; Indoor theatre/dance/symphony events; Restaurants (indoor and patio dining); Nightclubs and all other licensed premises; Casinos, bingo halls and VLT lounges; Movie theatres; Fitness centres, gyms and indoor sporting and recreational facilities (excluding youth recreational sport); Organized indoor group recreational classes and activities and indoor recreational business.
Ontario	Restaurants, bars and other food and drink establishments; food or drink establishments with dance facilities, including nightclubs, restoclubs, and outdoor areas of these establishments; meeting and event spaces; facilities used for sports and recreational fitness activities and personal physical fitness training, including: gyms, fitness, sporting and recreational facilities; pools; leagues, sporting events; waterparks; indoor areas of facilities where spectators watch events; casinos, bingo halls and other gaming establishments; concert venues, theatres and cinemas; bathhouses, sex clubs and strip clubs; horse racing tracks, car racing tracks and other similar venues; commercial film and TV productions where there is a studio audiences; any of the following outdoor areas that have a normal capacity of 20,000 or more people: outdoor meeting and event spaces; outdoor facilities used for sports and recreational fitness activities; outdoor concert venues, theatres and cinemas.
Nova Scotia	Full-service restaurants where patrons sit at tables to be served, both indoors and on patios; food establishments (like fast food and coffee shops) where people sit to eat and drink, both indoors and on patios (not including food courts, takeout, drive-thru or delivery); liquor licensed (drinking) establishments (like bars, wineries, distillery tasting rooms, craft taprooms and liquor manufacturers), both indoors and on patios; casinos and gaming establishments, both indoors and on patios; fitness establishments (like gyms and yoga studios) and sport and recreation facilities (like arenas, pools and large multipurpose recreation facilities); businesses and organizations offering indoor and outdoor recreation and leisure activities (like climbing facilities, dance classes, escape rooms, go-carts, indoor arcades, indoor play spaces, music lessons, pottery painting, shooting ranges and outdoor adventure); indoor and outdoor festivals, special events and arts and culture events (like theatre performances, concerts and movie theatres); indoor and outdoor sports practices, games, competitions and tournaments (participants and spectators); indoor and outdoor extracurricular school-based activities, including sportsbus, boat and walking tours; museums, Art Gallery of Nova Scotia and public library programs; indoor and outdoor events and activities like receptions, social events, conferences and training that are hosted by a business or organization; indoor and outdoor wedding ceremonies and funerals (including receptions and visitation) that are hosted by a business or organization; community meetings in rental spaces or where the public may be present; training hosted by a business or organization.
Alberta[1]	Restaurants and food courts; Nightclubs, casinos, bingo halls, VLT lounges; Entertainment and recreation centres (e.g., bowling, racing, arcades, billiards halls, etc.); Museums, art galleries; Movie theatres; Recreation facilities for physical, performance or recreational activity; Conferences, meeting spaces, halls, and rented space; Weddings and funerals held in public facilities; Professional sporting or performance events (spectator); Indoor adult sport and performance activities (participants); Private social events held in public facilities; Adult recreational activities (e.g., classes, groups); Amenities in hotels and condos, including fitness rooms, pools and game rooms.

New Brunswick	Indoor festivals, performing arts and sporting events; Indoor and outdoor dining and drinking at restaurants, pubs and bars; Nightclubs, amusement centres, pool halls, bowling alleys and casinos; Movie theatres; Gyms, indoor group exercise, indoor pools and recreation facilities; Indoor organized gatherings like weddings, funerals, parties, conferences and workshops; Indoor organized group recreational classes and activities; Visiting a long-term care facility.
Saskatchewan[2]	Restaurants, including in hotels or other lodgings (incl. outdoor patios); Nightclubs, bars, taverns buses and other establishments that serve alcohol; theatres; cinemas; bingo halls, casinos and other gaming establishments; concerts; live-music venues; fitness centres and gyms; stand-alone liquor and cannabis retail sales locations; and facilities hosting sporting events where tickets are required that have GST charged.
Newfoundland and Labrador	Gatherings of any size held for socializing, celebration, ceremony or entertainment hosted at a recognized business or organization, a rental room, community centre, or other venues used to host gatherings (e.g. weddings, funerals, birthday parties, baby showers, faith-based gatherings); Conferences, conventions and trade shows; Arenas; Indoor gyms and fitness facilities, yoga studios, and dance studios; Places where sports or recreational activities are practiced indoors; Places where group music, art, dance, and drama activities are practiced indoors, including bands, choirs, dance and music classes; Indoor entertainment facilities (arcades, trampoline parks, bowling alleys, billiard halls, golf, laser tag, indoor playgrounds, and paintball); Bars and lounges; Restaurants (indoor seated dining only, including food courts; does not apply to outdoor patios, take-out, delivery, or drive-thru services); Cinemas and performance spaces; Bingo halls; Personal service establishments including spas, esthetic services, hair salons, barber shops, body piercing, tattooing and tanning salons; Long-term care homes, personal care homes, assisted living facilities, community care homes (visitors only).
Prince Edward Island	Indoor and outdoor organized gatherings/events, including sporting events and recreational activities, and spectators at youth sporting events or recreational activities; Concerts, and arts, theatre, and music events; Wedding and funeral receptions and wakes; Conferences, trade fairs and workshops; Group activities and classes like pottery, art and choir; Adult group and team sports for people 19 years of age and older; Food premises and licensed premises (indoor and patio dining), including: Restaurants, Cafes, Bars, Liquor tasting rooms in wineries, breweries or distilleries; Casinos and movie theatres; Indoor gyms, exercise/dance facilities/studios, swimming pools and skating rinks; Outdoor facilities for organized gatherings/events; Arcades and bowling alleys; Meetings where members of the general public may be present.

Countries

France[3]	Marquees, theaters, sporting or cultural performance venues, conference rooms; trade shows and exhibition fairs; outdoor establishments including zoological, amusement and theme parks; stadiums, sports establishments, swimming pools, sports halls; casinos, gaming halls and "bowling alleys"; outdoor sit/stand festivals; cinemas and theaters; monuments, museums and exhibition halls; libraries (excluding university and specialized libraries); sports competition; other events, cultural, sporting, fun or festive, organized in public space or in a place open to the public; worship establishments for events not of a religious nature; ships and boats, cruise ship type; discotheques, dance clubs and bars; fairgrounds, from a threshold of 30 stands or attractions; commercial catering activities (bars and restaurants, including on terraces); trade fairs and exhibitions, and professional seminars; health, social and medico-social services and establishments; long-distance travel by inter-regional public transport (domestic flights, TGV journeys, Intercity and night trains, inter-regional coaches); department stores and shopping centers of more than 20,000 m².
Italy[3]	Travel between different regions by air, train, ship, ferry or coach; Restaurants, bars, ice cream parlours and pastry shops for consumption at table indoors; Performances open to the public, sporting events, both outdoors and indoors; Museums and places of culture, shows; Swimming pools and gyms; Private parties, such as wedding receptions; Festivals and trade fairs; Conventions and congresses; Spas and fitness centres; Gaming halls and betting shops, bingo halls and casinos.
Germany[3]	Visitor access to hospitals, nursing and care homes and assisted living facilities for the disabled; Access to indoor cafés and restaurants; Participation in indoor events and celebrations (e.g. information events, cultural events or sporting events); Using personal care services (e.g. hairdressers, beauticians, cosmetic services); Indoor sports (e.g. at gyms, swimming pools or sports centres); Accommodation (hotels, guesthouses): a test is to be taken for arrival and twice a week for the duration of the stay.

Notes: 1. Alberta's mandate allows presenting a recent negative COVID-19 test in lieu of vaccination and allows businesses to opt out of the proof of vaccination requirement and instead be subject to stricter COVID-19 prevention protocols, e.g., capacity limits, distancing, etc. 2. Saskatchewan's mandate allows presenting a recent negative COVID-19 test in lieu of vaccination. 3. France's, Italy's and Germany's mandates allow presenting a recent negative COVID-19 test or a past positive test in lieu of vaccination.