The Political Consequences of Vaccines: Quasi-Experimental Evidence from Eligibility Rules

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Abstract

Vaccines are responsible for large increases in human welfare and yet we know little about the political impacts of publicly-managed vaccination campaigns. We fill this gap by studying the case of Chile, which offers a rare combination of a high-stakes election, voluntary voting, and a vaccination process halfway implemented by election day. Crucially, the roll-out of vaccines relied on exogenous eligibility rules which we combine with a pre-analysis plan for causal identification. We find that higher vaccination rates boost political participation and, on average, decrease support for incumbents seeking reelection. An analysis of mechanisms reveals that vaccines affected incumbents differently depending on their performance in office.

Keywords: vaccines, politics, election, incumbents.

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1 Introduction

Vaccines control diseases, increase life expectancy, and are responsible for large increases in human welfare, particularly in the last century. Yet we know surprisingly little about the electoral impacts of publicly-managed vaccination campaigns. Given that electoral incentives can distort the implementation of welfare-improving policies (Besley and Case, 1995; Lizzeri and Persico, 2001; Finan and Mazzocco, 2021), this type of evidence is crucial. Vaccines also have the potential to improve the legitimacy of institutions by fostering political participation and state trust in times of health-induced crisis (Flückiger et al., 2019). We study the deployment of vaccines during one of the worst health crisis in modern history—the coronavirus pandemic—which caused millions of deaths, depressed the economy (Chetty et al., 2024), and activated ambitious economic policies (Hsiang et al., 2020). This crisis triggered an unprecedented competition for the development of vaccines and a race across nations to secure stocks for their populations.

We provide novel evidence for the impact of vaccines on elections. A simple framework highlights that vaccination can raise turnout by reducing health risks and shape support for incumbents by triggering retrospective evaluations of performance. We test these hypotheses in Chile, which offers an ideal testing ground for several reasons that we econometrically exploit for causal identification. Primarily, the central government secured a stock of vaccines and deployed the immunization using clear eligibility rules which we show were exogenous to the pre-pandemic political equilibrium, prevailing economic conditions, and pandemic severity. Importantly, vaccines were not politicized and vaccine hesitancy was low. In addition, following an intense wave of protests before the outbreak of the pandemic, the country embarked on a path to replace the Constitution, which led to multiple high-stakes elections when vaccines had only been partially delivered. These contextual features, combined with voluntary voting rules, make the setting econometrically ideal to test for the relation between vaccines, political participation, and political preferences.

Our analysis is divided in two parts. The foundation of the first part is based on two pillars. First, we overcome challenges related to cherry picking and p-hacking in analysis of observational data (Christensen and Miguel, 2018) by writing a comprehensive pre-analysis plan posted online before the elections took place. The plan offered a detailed description of the empirical analysis, which we implemented when the electoral results were made public. Second, we econometrically exploit the eligibility rules using administrative data for all 346 municipalities in the country. The rules consisted primarily of rolling age cutoffs, chronic health prevalence, and belonging to critical economic sectors.[1] Crucially, we are able to rule out mechanical effects related to age structure as the pre-analysis plan shows that local exposure to the vaccination campaign is empirically unrelated to turnout and political preferences in the period 2012-2020. Moreover, eligibility rules were also unrelated to local economic conditions and to local variables related to pandemic severity. We use these plausibly exogenous differences in an instrumental variables framework.

We find that exogenously higher vaccination rates boosted local political participation: a one standard deviation increase in vaccination (14 percentage points) raises turnout by 2.4 pp over a sample average of 48%. Although the vaccination campaign was designed and implemented by the national government, we find little evidence of partisan accountability, as vaccination rates are unrelated to vote shares for left-wing, right-wing, or independent coalitions in both local and Constitutional Assembly elections. Instead, vaccination reshaped local political competition: higher vaccination rates are causally associated with fewer votes for incumbents and more votes for challengers. Our estimates imply that an additional 5,000 fully vaccinated individuals locally (about 10 pp of the adult population) lead to roughly 700 additional voters (1.7 pp) and about 2,000 more votes for challengers (11 pp). Following the pre-analysis plan, we show that these results are robust across alternative inference methods and complier populations, and we find little evidence of spatial spillovers, indicating that the effects of vaccination are highly local.

Why could higher vaccination rates increase preferences for challengers? The second part of our analysis was not included in the pre-analysis plan and investigates potential answers.[2] We begin by showing that the higher participation and preferences for challengers cannot be attributed to increased political competition: there is little correlation between vaccination rates and the number of candidates running in elections. Most importantly, we show that the effect of vaccination on incumbent vote shares is mediated by performance. Using municipal finance data to proxy for mayoral performance, we find that vaccination rates increase support for high-performing incumbents but reduce support for low-performing ones. Moreover, when incumbents did not seek reelection, higher vaccination rates shifted votes toward challengers. These results underscore that vaccines not only mobilized voters but also appear to have sharpened retrospective evaluations, amplifying rewards for strong incumbents while penalizing weak or absent ones.

Our main contribution is to provide novel causal estimates of the impact of a large vaccination

[1] Examples of these occupations are those in the health sector, energy, gas, and water supply, public transportation, education, and public service, among others.

[2] Testing for mechanisms in the absence of knowledge about the primary set of results highlights the difficulties of writing a comprehensive pre-analysis plan. One solution could be to lay out the full set of potential mechanisms for all possible findings, but this could imply an implausible long pre-analysis plan.

process on political participation and vote shares in high-stakes elections. Existing research has mostly focused on the reverse relationship, i.e. how the political context (Desierto and Koyama, 2020; Maffioli, 2021; Pulejo and Querubín, 2021) and the characteristics of incumbent leaders (Chen et al., 2023; Cam Kavakli, 2020) shape the deployment of public health measures. To the best of our knowledge, there is no research on the political effects of large vaccination campaigns. An exception is Gutiérrez et al. (2024) which documents the relationship between vaccine eligibility and turnout in Mexico, but they lack data on vaccination and eligibility rules were less strict. The reason for the lack of evidence is presumably related to the endogeneity of public health measures, which researchers have shown to respond to political incentives. We overcome this challenge by econometrically exploiting a centralized vaccination campaign with predetermined implementation rules using a pre-analysis plan. In particular, we leverage large differences in exposure to the eligibility rules across municipalities on the eve of the election. By doing so, we are able to credibly isolate the pervasive political factors which usually affect vaccination campaigns.

Finally, we contribute to a growing literature in political economy and public health showing that disease prevalence reduces political participation and affects electoral outcomes (Urbatsch, 2017; Scheller, 2021; Morris and Miller, 2021; Gutiérrez et al., 2022; Campante et al., 2023; Mansour and Reeves, 2024), as well as to work documenting the political effects of public health policies (Haselswerdt, 2017; Clinton and Sances, 2018; Baicker and Finkelstein, 2019; Bol et al., 2021; Atal et al., 2024). In contrast to previous studies, we show that a large-scale immunization campaign can reverse the negative effects of a health crisis on political participation. We also show that the electoral consequences of vaccination depend critically on incumbent performance and reelection status. In our setting, where vaccine deployment was centrally designed and local politicians had limited control over implementation, higher vaccination rates did not translate into systematic rewards for incumbents on average, and in fact were associated with lower support for poorly performing incumbents and for successor candidates from the incumbent party. This highlights that public health policies can reshape local political competition not because incumbents control them or can easily claim credit, but because they change who participates and how voters evaluate local performance.

2 Vaccination Eligibility Rules and High-Stakes Election

We study the impact of a large vaccination campaign against the coronavirus on electoral outcomes in Chile. This country offers an ideal testing ground for three reasons. First, the country was quick in securing a diversified stock of vaccines and deployed the immunization with clear eligibility rules since December 2020. The plan to roll-out the vaccines was designed and implemented by the central government, leaving little room for local governments to affect this process.[3] Eligibility rules were based on verifiable measures such as age and occupation. Elder people and workers in certain occupations got the vaccine first on a week-to-week rolling program that started with people older than 90 years old and health personnel. Information about these rules was extensively disseminated nationally through online and traditional media (i.e. television, radio, and newspapers) and the vaccination data was made public in real time. Unlike in other countries, vaccines were *not* politicized and political parties from left to right supported the vaccination campaign, putting Chile among the countries with the highest vaccination rates (Mathieu et al., 2021).

The second crucial characteristic is that Chile faced one of the most important elections in its modern history. Five months before the pandemic outbreak, a wave of protests triggered a referendum asking citizens if they would like to replace the existing Constitution, originally drafted by the Pinochet dictatorship in 1980 (González and Prem, 2023). The referendum was held in October 2020 and 80% voted for a new Constitution. As a consequence of the vote, a new text was drafted by a Constitutional Convention composed by 155 members elected by a D'Hondt method.[4] The members of the Convention were elected the same day that mayors, councilors, and regional governors in an election with four ballots. The final important characteristic of the Chilean context is that automatic registration and voluntary voting was in place in all elections between 2012 and 2021. The combination of a high-stakes election, voluntary voting, and a massive vaccination process halfway implemented by election day constitute an ideal empirical setting.[5]

The elections we study took place in May 15-16 of 2021. Before these days, the number of infections, deaths, and the prevalence of localized lockdowns had been decreasing for several weeks but they were still high (panels A, B, and C in Figure 1). Lockdowns were dropped for the two election days to maximize electoral participation. Crucially, the vaccination process was halfway implemented as little more than 40% of the population had received the corresponding two doses for immunization (panel D in Figure 1). At this election, voters were given four different ballots. The most important election was the Constitutional Convention Election in which voters elected individuals with the goal of writing a new Constitution. Local Elections were arguably the second

[3]The stock of vaccines available locally was determined by the eligibility rules. Local governments could have affected the quality of the service with the use of waiting seats, parasols, and the use of more or less personnel on site.

[4]The proposed text was democratically rejected in a referendum held in September of 2022. A new convention, composed by elected members and experts suggested by congresspeople, wrote another text in 2023. This second text was democratically rejected in late 2023, making Chile the only country in history to reject consecutive proposals.

[5]By the time of the election there were 7.5 million people (50%) immunized with the Sinovac (84%) and Pfizer (16%) vaccines. Full immunization with the Cansino and AstraZeneca vaccines only occurred after the election.

most important and particularly relevant given that people associate local governments with most local policies affecting their daily lives (e.g. public schools). Two ballots were tied to the Local Election, one to choose the mayor and another one to choose councilors. All 345 municipalities in the country simultaneously elected one mayor and 6, 8, or 10 councilors depending on the municipality population. The fourth ballot corresponds to the Regional Governors Election, in which voters elected one governor for each of the 16 regions of the country. This was the first time in the country's history in which people democratically elected regional governors. There was scarce information about their political role, so we interpret that election as relatively low stakes.

3 Conceptual Framework

Our conceptual framework builds on standard models of turnout and retrospective voting. The central idea is that vaccination affects electoral outcomes primarily by lowering barriers to political participation and by shaping how voters act on information about incumbent performance.

Vaccination can increase turnout because it reduces health-related and practical costs of participation. When vaccination rates are higher, voters face a lower risk of infection when going to the polls, fewer individuals are incapacitated by illness, and precautionary behavior becomes less binding. As a result, the individual cost of voting declines and a larger share of the electorate is able to participate. In this sense, vaccination operates mainly as a mobilization device: it does not need to change voters' political beliefs or preferences to raise turnout, but rather enables more citizens to act on preferences they already hold. We assume that vaccination outcomes are broadly observable at the local level—through personal experience and social networks—so that local vaccination rates summarize how voters perceive the campaign in their municipality.

We summarize this idea with a simple reduced-form representation. Let the utility that individual i in municipality m derives from voting be

$$U_{im} = \beta V_m + \gamma P_{im} - c_i(V_m), \tag{1}$$

where V_m denotes the local vaccination rate, P_{im} captures the ideological or expressive component of utility from voting, and $c_i(V_m)$ is the individual cost of voting, with $c_i'(V_m) < 0$. The key assumption is that higher vaccination rates reduce participation costs by lowering perceived health risks and other barriers to voting. The term βV_m captures any remaining mobilization effect of vaccination that is not fully explained by observed health shocks, such as reductions in precautionary behavior or other participation constraints. Equation (1) therefore implies that turnout increases with vaccination rates because vaccination lowers the net cost of participation and expands the set of individuals for whom voting yields positive utility.

Importantly, higher turnout alone does not mechanically imply lower support for incumbents. Whether mobilization benefits incumbents or challengers depends on how voters evaluate those in power once they participate. We emphasize a performance-based mechanism. The pandemic generated salient information about local government performance, including how municipalities managed public services, communicated with citizens, and responded to crisis-related challenges. Vaccination does not itself generate this information. Instead, it lowers participation costs and allows voters to act on performance evaluations that were already formed during the pandemic. In this sense, vaccination facilitates performance-based voting by enabling participation, rather than by changing beliefs.

A key implication of this mechanism is that the composition of the electorate changes with vaccination. Newly mobilized voters need not be individuals who never participated in previous elections. Rather, they are voters whose participation in the 2021 election was constrained by pandemic-related health risks, illness, or precautionary behavior, and whose decision to vote is therefore more sensitive to changes in participation costs. These voters tend to be more marginal in their participation decisions and less embedded in stable incumbency-based or partisan loyalties. As a result, their inclusion in the electorate mechanically increases the weight of retrospective performance evaluations in aggregate electoral outcomes.

This logic implies heterogeneity in electoral effects. When incumbents have performed well, higher turnout should strengthen their electoral position, as more voters are able to reward effective governance. When incumbents have performed poorly, higher turnout should weaken their position, as more voters are able to punish underperformance. Moreover, when the incumbent mayor does not personally seek reelection, voters cannot reward or punish the individual responsible for past performance, and increased participation is more likely to reduce support for the incumbent party and benefit challengers.

We summarize these ideas with the following reduced-form expression for incumbent support:

$$\Pr(I_{im} = 1 \mid V_m) \quad = \quad \alpha + \delta\, V_m \times F_m - \theta\, V_m(1 - R_m), \tag{2}$$

where I_{im} is an indicator equal to one if individual i votes for the incumbent mayor (or for the incumbent party when the mayor does not run), F_m is a measure of incumbent performance, and R_m is an indicator equal to one if the incumbent mayor is running for reelection.

The interaction term $V_m \times F_m$ captures performance-based voting: when performance is strong (F_m high), higher vaccination rates increase incumbent support by allowing more voters to reward effective incumbents ($\delta > 0$); when performance is weak, higher vaccination rates reduce incumbent support by enabling more voters to punish underperformance. The term $V_m(1 - R_m)$ captures the role of reelection status: when the incumbent does not personally run, increased participation is more likely to reduce support for the incumbent party, since voters cannot directly attribute responsibility to the individual in office ($\theta > 0$).

Taken together, this framework delivers two main predictions. First, higher vaccination rates increase turnout by lowering the cost of participation and mobilizing voters who were previously constrained by pandemic-related risks. Second, the effect of vaccination on incumbent vote shares depends on incumbent performance and reelection status: well-performing incumbents benefit from higher participation, while poorly performing incumbents and successor candidates from incumbent parties are penalized.

4 Research Design

Our research design is based on a pre-analysis plan (Depetris-Chauvin and González, 2021). This methodology is relatively underused when performing observational analysis, particularly when compared to randomized controlled trials.[6] As emphasized by Christensen and Miguel (2018), the pioneering study and one of the few to this date is Neumark (2001). We follow the recommendations of Christensen and Miguel (2018) and Burlig (2018) to construct our pre-analysis plan. The study of electoral outcomes is particularly suited for this type of analysis as elections take place in a specific and verifiable date. We pre-specified all of the following econometric models, the main specification we use, and also empirically validated the research design. We then uploaded the document in the website of the Open Science Framework before the election under study.

4.1 Local exposure based on eligibility rules

Our econometric design combines four different data sources to track eligibility rules, vaccine deployment, and electoral outcomes across municipalities. First, we use individual-level data from the 2017 Census, which provides information on municipality of residence, age, gender, occupation, labor force participation, and unemployment status. Second, we use administrative electoral

[6] The use of pre-analysis plans in experimental studies has increased rapidly in the past years. The number of registered studies in the AEA registry provides evidence of this trend (Miguel, 2021).

data from the Electoral Service, including municipality-level participation and vote shares from past elections. Third, we use administrative data from the Ministry of Health with municipality-level information on the number of people vaccinated by week, COVID-related deaths and infections, and the full list of vaccination centers with their geographic location. Fourth, we use data from the nationally representative CASEN survey, which covers approximately 270,000 individuals in 324 municipalities and provides additional socioeconomic characteristics.

We are interested in estimating the causal impact of vaccination on electoral outcomes, including turnout and political preferences for candidates and coalitions. We observe vaccination rates and electoral outcomes by municipality and estimate the following cross-sectional regression:

$$Y_{cp} = \beta V_c + \gamma X_c + \phi_p + \epsilon_{cp}, \tag{3}$$

where Y_{cp} is an electoral outcome in municipality c, located in province p. Chile is divided into 346 municipalities, each belonging to one of 56 provinces. We use 343 municipalities in 54 provinces because one municipality lacks political data (Antarctica) and province fixed effects absorb all variation in two others (Cape Horn and Easter Island). The variable of interest is V_c, defined as the share of adults (18 years or older) who had completed a two-dose vaccination scheme by the time of the election, using adult population projections from the 2020 National Statistics Institute.[7] We include a vector of predetermined and pre-specified covariates X_c to improve precision and to control for municipality characteristics that correlate with the instrument. The error term ϵ_{cp} is allowed to be heteroskedastic and spatially autocorrelated. Since electoral outcomes arise from individual decisions, we estimate equation (3) using weighted least squares with the adult population of each municipality as weights.[8]

A leading concern with a naïve estimation of equation (3) is that vaccination rates may be correlated with omitted municipality characteristics that also affect electoral outcomes.[9] To address this concern, we employ a two-stage least squares strategy using as instruments the eligibility rules that governed access to vaccines prior to the election.

[7]By the time of the election, all eligible individuals were offered vaccines requiring two doses (Sinovac or Pfizer), and immunity was reached two weeks after receiving the second dose.

[8]The pre-analysis plan also proposed an alternative design based on distance to vaccination centers at the polling-booth level. We chose not to rely on this approach because vaccination rates are not observed at the booth level and because distance to vaccination venues is strongly correlated with predetermined political outcomes. As shown in Online Appendix Table A.1, distance predicts past turnout and vote shares even after conditioning on municipality fixed effects and controls, suggesting that it is unlikely to be exogenous.

[9]For example, education is plausibly correlated with both vaccination uptake and political participation, but many other omitted variables may also matter.

The national vaccination plan was centrally designed and implemented and followed two main pillars. First, eligibility was determined by age. Older individuals were prioritized, and by the time of the May 2021 election all residents aged 48 and above had been eligible to complete a two-dose vaccination scheme. Second, eligibility was extended to specific high-priority groups regardless of age, including individuals with chronic health conditions and workers in selected "critical" occupations. We operationalize these rules using multiple data sources. Individuals with chronic conditions are identified using administrative records from the annual influenza vaccination campaign, which registers people eligible for early vaccination due to underlying health risks. Workers in priority occupations are identified using occupational categories available in the 2017 Census; we classify as eligible those employed in health services, public transportation, education, and public administration. Combining these sources with municipality-level demographic information from the Census, we construct for each municipality the share of the adult population that was eligible to receive two vaccine doses before the election. This share constitutes our instrument Z_c.

Importantly, eligibility rules were announced and fixed before vaccination outcomes were realized and before the election took place. As a result, variation in Z_c reflects predetermined demographic and occupational composition rather than local political or economic conditions. Since vaccination progressed gradually and eligibility expanded mechanically according to the national schedule, municipalities with a larger fraction of residents in prioritized age groups, occupations, or health categories experienced faster increases in vaccination rates, providing quasi-experimental variation in exposure to vaccines. Panel (a) in Figure 2 illustrates the identifying variation. The vaccination plan mandated that, in the week preceding the election, all individuals aged 48 were eligible to be fully vaccinated. As a consequence, we observe an 18 percentage point increase in vaccination rates between 47-year-olds (40%) and 48-year-olds (58%). The 40% vaccination rate among individuals aged 47 or younger reflects those who were eligible because they worked in priority occupations or had chronic conditions. Similar discontinuities appear in other weeks as age thresholds expanded.

The pre-analysis plan proposed five specifications of equation (3). Specification (i) excludes province fixed effects ϕ_p and excludes controls X_c. Specification (ii) includes province fixed effects but no additional controls. Specification (iii) includes ϕ_p and a basic set of geographic and size controls $x_{1,c} \in X_c$: the logarithm of the distance to the national capital, the logarithm of the distance to the regional capital, an indicator for municipalities with fewer than 50,000 inhabitants, and an indicator for municipalities with populations between 50,000 and 100,000. These controls capture predetermined differences in geographic location and municipality size. Specification (iv) includes ϕ_p, $x_{1,c}$, and an extended set of controls $x_{2,c} \in X_c$ that are correlated with the instrument: turnout in the 2017 presidential election, labor force participation, the share of women in the population, female labor force participation and unemployment rates, the prevalence of permanent health conditions, average household subsidies (in logs), total COVID deaths per 10,000 inhabitants (in logs), and the number of vaccination centers per 100,000 inhabitants. Specification (v) includes ϕ_p, $x_{1,c}$, $x_{2,c}$, and controls from the 2020 plebiscite $x_{3,c} \in X_c$: turnout and the vote share in favor of the "Approve" option for a new constitution. These variables capture recent political behavior in an election also held during the pandemic. Specifications (i)–(iii) use 343 municipalities. Specifications (iv)–(v) use 324 municipalities because two of the covariates come from the CASEN survey, which is not available for all locations.

4.2 Validity of the design in the pre-analysis plan

The validity of the instrument rests on the condition that it has sufficiently strong predictive power of the endogenous variable and on the assumption that it affects the outcomes of interest only through the endogenous variable (i.e. exclusion restriction) after we condition on a small set of predetermined covariates (i.e. conditional exogeneity). Reassuringly, the instrument has a strong predictive power of the percentage of people vaccinated before the Election. Regarding the exclusion restriction, we provide suggestive evidence supporting this identification assumption using the correlation between the instrument a wide range of variables covering the political and economic dimensions of municipalities before the arrival of the pandemic and the Constitutional Convention, as well as a range of variables related to the severity of the pandemic (e.g. infections).[10]

Table 1 presents summary statistics for 19 variables describing local political participation and preferences, and the predicted power of the instrument on these variables. We have organized this table to study political participation (panel A), and preferences (panel B) in the 2020 plebiscite, and for incumbents, left-wing, right-wing, and independent candidates in all elections since 2012 when automatic registration and voluntary voting was introduced. To classify candidates as left-wing and right-wing, we follow previous work using data from these elections (Bautista et al., 2023). In the appendix we also examine 14 additional variables from the 2017 Census, 10 variables from the 2017 National Survey, three protest-related variables from the Armed Conflict Location and Event Data (ACLED) project, and four variables related to the COVID pandemic (Tables A.2-A.4). In

[10]It is important to note that the three indicators of pandemic severity the week before the election in Table A.4 were not originally specified in our pre-analysis plan. One of these three variables (i.e., Covid Deaths per 10,000 inhabitants) displays a positive and statistically significant correlation with our instrument. While we did not include these covariates as controls in our main specifications, it is reassuring that, if anything, their inclusion makes our main results somewhat stronger and more precisely estimated.

sum, we estimated the correlation between the instrument and 54 variables covering elections, the labor market, health conditions, state subsidies, and the pandemic, and we observe 7 statistically significant differences at the 10% level. The number of differences is slightly above the 5 derived from a 10% statistical test ($0.10 \times 54 = 5.4$), which in this case was reasonable to expect as we explain below. Importantly, only one of the 19 political variables is correlated with the instrument at the 5% level, which is what we expected of a 5% statistical test ($0.05 \times 19 = 0.95$).

Overall, we interpret Table 1 as supporting the validity of the research design in the sense that the instrument has little predictive power of political participation or political preferences at the local level as measured by the five elections held between 2012 and 2020. Moreover, the signs of coefficients do *not* suggest systematic political differences across municipalities with varying exposure to the vaccination process. For example, the standardized correlation between the instrument and the vote share of left-wing candidates in local elections changes from 0.29 in 2012 to -0.07 in 2016, and a similar picture emerges in the case of right-wing or independent candidates.[11]

In the Online Appendix, we confirm that the vaccination process prioritized the elder population (Tables A.2-A.4). As women tend to live longer, it was expected to observe a higher population of women in municipalities with more priority groups. Similarly, as older people are less likely to work, we also expected lower participation rates in the labor force in places more exposed to the vaccines, and more people with permanent health conditions and who receive more state subsidies. In other words, the instrument is expected to correlate with variables that characterize the elder population, including COVID deaths and the number of vaccination centers.

More critical for our research design is the lack of a correlation between the local eligibility of the population, predetermined political preferences, and economic conditions and educational levels, all which have been shown to affect political outcomes in a variety of contexts. In that sense, it is reassuring that the instrument is uncorrelated with household per capita income, poverty rates, rural population, different education measures, malnutrition, lack of health insurance, and lack of basic services. It is also reassuring that the instrument is *not* associated with the number of COVID infections and the prevalence of lockdowns, which proxy for the negative economic impacts of the pandemic and are relatively more independent of people's age at the local level.[12]

[11]The pre-analysis plan also proposed to study electoral results in the more than 40,000 booths (groups of 300 voters) using the distance from people's homes to the closest vaccination venues as shifter of take-up of the vaccination campaign. There are two problems with that design. First, we do not observe vaccination rates per booth. Second, we observe significantly more predetermined differences across treatment status. Thus the treatment is unlikely to be exogenous and contaminates the interpretation of these results. The pre-analysis plan presents details and results.

[12]Municipality-level lockdowns were decided by the central government based on real-time local data related to the pandemic. Lockdowns were associated to a decrease of 10-15% in local economic activity (Asahi et al., 2021).

5 Vaccine Deployment and Electoral Results

We organize results in two parts. First, we show that eligibility rules had a large positive impact on vaccination rates and we emphasize how these results presented in the pre-analysis plan shaped our specification decisions. Second, we present causal estimates of vaccine deployment on political participation, vote shares of incumbents, and vote shares of political coalitions.

5.1 Eligibility and compliance

Panel (b) in Figure 2 presents the relationship between local exposure originated in eligibility rules (instrument) and the share of the adult population who was fully vaccinated by election day.[13] Table 2 presents the analogue regression estimates from five specifications with different sets of controls. These results were reported in the pre-analysis plan using the same five specifications.

Four patterns emerge from panel (b) in Figure 2 and Table 2. First, the share of people eligible for the vaccine is a strong predictor of the share of adults who are fully vaccinated. Moreover, F-statistics are always larger than 49 regardless of the specification, alleviating concerns about a potential weak instrument (Stock and Yogo, 2005). Second, the first-stage coefficient is remarkably stable across different specifications and hovers between 0.66 and 0.76. The small differences in point estimates across econometric models suggest that the correlations between the instrument and predetermined (unbalanced) covariates are unlikely to be an empirical concern. If anything, the correlation becomes stronger when including these covariates as controls. Third, the first-stage coefficient is lower than one, which reveals the existence of imperfect compliance with the vaccination process, i.e. approximately 70% of the people who were eligible to get vaccinated decided to take the vaccine. And fourth, the covariates related to the only election held during the pandemic at the time (i.e. 2020 plebiscite) have significant predictive power of vaccination rates.

As mentioned in the pre-analysis plan, these results pushed us to make some empirical decisions. The most important one is that we decided to use the specification in column 5 of Table 2 to estimate the impact of the vaccination process on electoral outcomes. The reason behind this decision is the explanatory power of the covariates related to the 2020 plebiscite, which will increase the precision of our estimates, and the small set of statistically significant correlations between the instrument and predetermined covariates. We found similar results when measuring vaccination

[13]Figure A.1 in the Online Appendix depicts the geographical distribution of both the share of individuals eligible for the vaccines and the share of the adult population who was fully vaccinated by election day. Moran's I statistic is 0.17 for the latter, and 0.20 for the former; suggesting a weak positive spatial autocorrelation in both variables.

rates with one or two doses. However, we focus on specifications in which the endogenous variable is the share of adults with two doses to emphasize the importance of immunity, which takes place two weeks after the second doses. Lastly, in the pre-analysis plan we decided to add as control one lag of the corresponding dependent variable to improve the precision of our estimates.

5.2 Political participation

Table 3 presents the impact of the vaccination process on local political participation. We define the latter as the ratio between total votes and the number of people who were legally able to vote (column 1) when studying overall participation in the election. When looking at each one of the four elections, we use *valid* votes as the numerator (columns 2–5) and the same denominator, which makes turnout vary by election as invalid votes (null or blank) change across ballots. We present instrumental variables estimates in Panel A, reduced-form results in Panel B, and OLS results for comparison in Panel C. Following the pre-analysis plan, we report robust standard errors in parentheses and also adjust them for spatial autocorrelation within 50 kilometers following Conley (1999) (reported in square brackets). The latter method delivers smaller confidence intervals, and we therefore focus on the more conservative inference based on robust standard errors.

Instrumental variables estimates of equation (3) in Panel A show that an increase of 10 percentage points (pp) in vaccination rates increased local political participation by 1.7 pp (p-value$<$ 0.05). In standardized terms, an exogenous increase of one standard deviation in vaccination rates (13.8 pp) caused political participation to increase by 2.4 pp. This corresponds to approximately 5,000 additional fully vaccinated individuals being causally associated with about 700 additional votes in the election. The magnitude is sizable relative to the cross-municipality dispersion in turnout (0.27σ), but modest relative to average participation (48%). Columns 2–5 show that the effects are similar when examining each of the four elections separately (all p-values< 0.05). The statistical significance of these results is robust to randomization inference (p-values< 0.01, Figure A.2). Results in Table 3 are virtually unchanged when controlling flexibly for the set of unbalanced covariates (Table A.5).

To better understand who is mobilized by vaccination, we also estimate the effect of vaccination on turnout by age group. Using administrative data from the Electoral Service and our baseline IV specification, we find that vaccination significantly increased turnout among younger voters—particularly those below age 34—had smaller and marginally insignificant effects for individuals aged 35–44, and no detectable effect among voters older than 45. These results, reported in Figure A.3, suggest that vaccination primarily mobilized younger and more marginal voters rather than older voters with higher baseline participation. This pattern helps characterize the newly mobilized electorate and supports a participation-based interpretation of the turnout effects.

Our analysis uses relatively small administrative units and therefore it is important to check for the relevance of spillovers. We test for the most common source of contagion over space, spillovers on neighboring (contiguous) municipalities. Table A.6 presents first-stage results but replacing the share of eligible people locally by the share of eligible people in neighboring municipalities using the same five specifications than before (Table 2). Reassuringly, all within-province point estimates are smaller and indistinguishable from zero. These results constitute evidence of limited spillovers in eligibility rules and support the local nature of the exogenous variation we exploit. Table A.7 presents instrumental variables estimates of equation (3) but now replacing the dependent variable by electoral outcomes in neighboring municipalities, i.e. the first-stage is the same as in our main estimates. We again find estimates which are indistinguishable from zero.

What are the characteristics of municipalities which responded to the eligibility rules, i.e. the compliers? Our estimate represents the causal impact of vaccines as measured by the set of municipalities which empirically responded to the eligibility rules, i.e. the Local Average Treatment Effect (LATE). Therefore, it is important to characterize these municipalities to analyze the extent to which our causal estimates could be generalizable. Operationally, we follow the methodology proposed by Abadie et al. (2002). To facilitate the interpretation of the method, we convert the percentage of the population fully vaccinated to an indicator which takes the value one if the share of adults with two doses is above the median of the empirical distribution. We do the same for the instrument, i.e. the local eligibility of the population. Tables A.8 and A.9 present this analysis. Overall, treated and untreated compliers appear to be fairly similar to other locations in terms of political characteristics but experienced less lockdowns and infections. We conclude that our LATE is unlikely to be specific to a peculiar set of municipalities and is thus likely to be generalizable. We complement this claim by empirically tracing out variation in LATE below.

To further understand the magnitude of the impact of vaccines on electoral participation, we compare our estimates to the impact of infections.[14] Incapacitation effects mechanically decrease turnout by preventing infected individuals to go out and vote. However, the impact could be larger because infections also affect others for a variety of reasons such as perceptions of state ineffectiveness or fear of contagion. We leverage variation in infections within municipality over three

[14]This empirical analysis was *not* part of the pre-analysis plan but we consider it to be helpful to compare the magnitude of estimates to relatively more widespread estimates in the literature.

elections using the following two-way fixed effects econometric model:

$$Y_{ct} = \beta I_{ct} + \phi_c + \phi_t + \epsilon_{ct} \tag{4}$$

where Y_{ct} is electoral participation in municipality c in election t, I_{ct} is the average number of infected individuals per 100,000 inhabitants in the two weeks before election t, (ϕ_c, ϕ_t) represent municipality and election fixed effects respectively, and ϵ_{ct} is an error term which we allow to be correlated within municipality over time. We again use population-weighted least squares.

Table 4 presents results. Column 1 shows that an increase of 1 percentage point (pp) in active infection rates locally (500 people) decreases political participation by 5.8 pp. Columns 2-5 confirm the finding using different measures of pandemic intensity. At first sight, column 1 suggests that the impact of infections is larger than the one of vaccines because an increase of 1 pp in vaccination rates increases political participation by only 0.2 percentage points. However, vaccination rates are on average 48% with a standard deviation (σ) of 9 pp while infection rates are on average 0.15% with a standard deviation of 0.19 pp. A more similar magnitude is revealed when comparing standardized effects: an increase of 1σ in vaccination rates increases turnout by 1.5 pp (0.171×9), while the same increase in infections decreases turnout by 1.1 pp (5.8×0.19).

5.3 Partisanship and incumbency

Since the vaccination campaign was designed and implemented by the national government, a natural accountability hypothesis is that voters may reward or punish the national ruling coalition in areas where vaccination was more successful. We therefore begin by examining whether higher vaccination rates affected vote shares for the incumbent national coalition in both local elections and the Constitutional Assembly election.

Table 5 studies vote shares in the Constitutional Convention election, where candidates were grouped into left-wing, right-wing, and independent lists. This election provides a particularly clean test of national-level accountability, since it did not feature incumbents in the usual sense and was widely perceived as a defining moment in the country's political trajectory. Instrumental variables estimates in columns 1–3 show small and statistically insignificant effects of vaccination on coalition vote shares, suggesting little systematic reward or punishment of national political coalitions. Columns 4–6 turn to coalition vote shares in the local mayoral election. Again, we find imprecise and unstable estimates for left-wing, right-wing, and independent candidates.[15] Taken

[15]Point estimates suggest that some votes may have flowed away from independent candidates in more vaccinated

together, the results indicate that vaccination rates did not translate into clear shifts in support across political coalitions, either in national or local contests.

We find limited and unstable effects of vaccination on coalition vote shares. This suggests that voters did not systematically attribute the success of the vaccination campaign to national parties or coalitions. Instead, the political effects of vaccination appear to operate primarily through changes in political participation and local electoral competition rather than through national partisan accountability. This finding motivates our subsequent focus on turnout and incumbent versus challenger support at the municipal level. If vaccination mainly affects voters' willingness to participate and their evaluation of local incumbents, rather than their support for national coalitions, then turnout and local incumbent vote shares are the most informative outcomes to study.

Columns 7–8 therefore examine the impact of vaccination rates on the vote share of incumbent mayors. Instrumental variables estimates reveal a negative and statistically significant relationship between vaccination and incumbent support. An exogenous increase of 10 percentage points in vaccination rates is associated with a decline of roughly 20 percentage points in the vote share of the incumbent party or about 11 percentage points in the vote share of the incumbent mayor.[16] Given an average electorate of about 18,500 voters per municipality, these magnitudes imply that 5,000 additional vaccinated individuals translate into roughly 2,000 fewer votes for the incumbent.

Overall, Tables 3 and 5 show that exogenous increases in vaccination rates boosted political participation without benefitting specific political parties. The effects appear to be highly local, as we find no evidence of spatial spillovers (Table A.7). Moreover, once attention is shifted from coalitions to individual incumbents, vaccination is associated with a systematic decline in support for those in power. These results are robust to controlling for a wide range of potential confounders, including past turnout and incumbent vote shares, geographic and population characteristics, outcomes from the 2020 plebiscite, and the subset of variables that are statistically unbalanced in Table 1 (see Figure A.4).

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municipalities, although standard errors are large. One potential explanation is a candidate-supply margin: we find a small increase in the number of left- and right-wing candidates and no robust change in independent entry in more vaccinated municipalities (Table A.12). In the Appendix, we also repeat these coalition analyses for the Councilors and Regional Governors elections and likewise find little evidence of systematic partisan effects.

[16] A new law enacted shortly before the pandemic prevented many incumbents from seeking reelection by imposing term limits. All coefficients display similar statistical significance under alternative inference procedures (Figure A.2).

5.4 Discussion

Vaccines unexpectedly reduced the vote shares of incumbent mayors, despite a large literature showing that incumbents obtain political returns from policies that benefit the population. This finding cannot be explained by changes in the average number of candidates competing in local elections (0.3 per 1,000 inhabitants, see Table A.11).[17] In addition, the new voters brought by the higher political participation are unlikely to be the sole explanation behind this finding because the increase in turnout is smaller than the decrease in votes for incumbents. Moreover, we observe lower support for incumbents of *all* parties. Support for right-wing parties (incumbents in the central government) was unchanged by vaccines in local and constitutional elections, except in the case of regional governors. The latter election was held for the first time in history, voters had little information to resort to, and right-wing parties benefitted from vaccines (Table A.13).

We offer two exercises to illuminate potential explanations for the lower support for incumbent mayors. In the first (pre-registered) exercise, we trace out variation in the LATE by using all combinations of instruments behind the eligibility rules $Z_c = \{z_{1c}, \dots, z_{Jc}\}$. We have $J = 6$ with $j = A, B, C, D, E, F$, thus $2^6 = 64$ different subsets of instruments.[18] Panels (a) and (b) in Figure 3 present results. Visual inspection of small and large point estimates with subsets of instruments reveals that people who responded to the vaccines by voting less for incumbents are older (E), work in health (A), and suffer from chronic conditions (F). These characteristics are also related to relatively *lower* effect on turnout, suggesting that plausible explanations are more likely to be related to changes in political preferences of *established* voters than to skewed preferences among *new* voters. For example, health workers and the elderly could have been disappointed by how incumbent mayors assisted the vaccination campaign promoted by the central government, and consequently decided to support challengers. Unfortunately, without voting data disaggregated by municipality and demographic groups we cannot fully test this hypothesis. The second exercise, and the more exploratory analysis of mechanisms, is presented in the next section.

[17]We estimate equation (3) with the number of competitors per 1,000 voters in each election as dependent variable. Vaccination rates are unrelated to the number of candidates in local elections. Thus the weaker performance of incumbent mayors cannot be attributed to political competition. This result was not part of the pre-analysis plan.

[18]This analysis requires a more stringent exclusion restriction, namely that each j is affecting the outcome of interest (turnout, incumbent vote share) only through changes in local vaccination rates.

6 Why Vaccines Weakened Some Incumbents

6.1 Incumbent performance

The framework in Section 3 suggests that vaccines increase political participation, but that the electoral consequences for incumbents depend on how voters evaluate local officials once they decide to participate. In particular, mobilization is expected to hurt incumbents when perceived performance is weak, as newly participating voters are more likely to punish underperformers, while high-performing incumbents should be better positioned to capture their support. When incumbents do not seek reelection, mobilization is instead likely to shift preferences toward challengers. These predictions motivate our analysis of heterogeneity by incumbent performance.

To proxy for mayoral performance during the 2017–2020 term, we draw on administrative data on municipal finances. Specifically, we compute the change in the municipal deficit per capita between the 2017–2020 period (D_t) and the preceding term (D_{t-1}) using data from the *Sistema Nacional de Información Municipal* (SINIM).[19] We classify municipalities as high- or low-performing based on a median split in order to maximize statistical power and maintain balanced sample sizes across groups. By construction, half of municipalities are categorized as low-performing and half as high-performing.

Consistent with the framework, Table 6 shows that higher vaccination rates are strongly and significantly associated with lower incumbent vote shares in municipalities led by low-performing mayors. By contrast, when focusing on municipalities where incumbents were able to seek reelection, vaccination rates are positively associated with the vote share of high-performing incumbents, while the relationship remains weak or negative among low performers. These patterns indicate that performance is central to explaining why vaccination weakened some incumbents but bolstered others.

To assess whether these findings depend on the specific measure of performance, we conduct two additional robustness exercises. First, we reclassify municipalities using a measure of own-source municipal income that excludes transfers from the central government, and therefore depends more directly on mayoral effort and local revenue management. Second, we use changes in standardized test scores (SIMCE) in public schools as an alternative and highly salient indicator

[19] Measuring mayoral performance is inherently challenging, as many dimensions of local governance are difficult to observe systematically. Municipal finances provide a concrete and comparable metric that is regularly reported, audited, and discussed in the media. In the Chilean context, persistent increases in municipal deficits are commonly interpreted as signals of weak management or governance capacity rather than as deliberate redistribution.

of municipal performance. This measure is motivated by experimental evidence showing that voters in Chile respond electorally to information about SIMCE outcomes when evaluating incumbent mayors (Cox et al., 2024). Across both measures, the qualitative pattern is unchanged: vaccination rates are consistently associated with larger electoral losses for incumbents in low-performing municipalities than in high-performing ones (Table A.14).[20]

Taken together, these results reinforce the interpretation that vaccination reshaped local electoral outcomes by strengthening performance-based voting rather than by uniformly penalizing incumbents.

6.2 Alternative explanations

In addition to the mechanisms highlighted in the conceptual framework, we explored other channels through which vaccination could shape electoral behavior. These analyses are more exploratory and are reported in detail in Online Appendix B.

Decision-making. Vaccination may have altered how citizens processed political choices by reducing anxiety, improving focus, and lowering depression. Survey evidence shows that vaccinated individuals were less worried about infection, more optimistic about the country's future, and better able to concentrate. Together, these effects suggest that vaccines improved individuals' capacity to evaluate alternatives beyond the incumbent, weakening the typical advantage of the "safe" status quo during crises, as Bisbee and Honig (2021) document.[21] Importantly, we find no evidence that vaccination distorted information acquisition, as rates of invalid voting remained unchanged.

Booster analysis. We also study the Presidential and Congressional elections of November 2021, held six months after our main case. By then, most of the population had already completed the primary vaccination schedule, uncertainty about the disease had declined, and vaccine campaigns were largely focused on booster doses. This setting provides a natural test of whether the political effects of vaccination attenuate as the pandemic becomes less salient and vaccination becomes widespread. Consistent with this interpretation, we find that the positive effect of vaccination on turnout persists but is substantially smaller than in May 2021, while the effects on incumbent or challenger support are weak or statistically indistinguishable from zero. These re-

[20]We are not aware of comprehensive municipality-level indicators of pandemic management quality. While such measures would be desirable, the consistency of results across multiple performance proxies suggests that our findings do not hinge on a narrow interpretation of fiscal outcomes.

[21]Bisbee and Honig (2021) use both experimental and observational data to show that higher COVID-19 anxiety increases support for establishment candidates (i.e., incumbents), consistent with a "flight to safety" mechanism.

sults suggest that the May 2021 elections likely capture an upper bound of the political effects of vaccination, when uncertainty and the perceived importance of immunization were highest. The full analysis is presented in Online Appendix B.3.

These additional analyses should be viewed as exploratory and point to alternative mechanisms that could also contribute to the observed electoral impacts of vaccination. Although we cannot fully ruled these out, they help situate our findings within a broader set of plausible behavioral responses to the pandemic.

7 Conclusion

The causal impact of vaccination campaigns on elections has been elusive to estimate given the political factors driving the implementation of these policies. We exploited eligibility rules and other appealing characteristics of the Chilean context to show that increases in vaccination rates are causally associated with more political participation and, on average, empower outsiders by decreasing the votes of incumbents, irrespective of their party affiliation. We combined several sources of administrative data to show that vaccines benefit incumbents only when overall performance had been relatively good and the same incumbent mayor seeks reelection.

The higher political participation associated with vaccination highlights the potential for effective public health policies to strengthen democratic legitimacy in times of crisis. At the same time, our findings show that vaccination can reshape electoral competition in ways that are not uniformly favorable to those in power. In our context, where vaccine deployment was centrally designed and governed by clear eligibility rules, higher vaccination rates did not systematically benefit incumbents and were associated with lower support for poorly performing mayors and for incumbent parties when the mayor did not seek reelection. This suggests that even policies with broad public support may have complex and heterogeneous electoral consequences.

Importantly, our results should not be interpreted as evidence about incumbents' incentives to deploy vaccines in settings where local politicians directly control vaccine distribution. In Chile, local mayors had limited influence over eligibility, procurement, and timing. In institutional environments where vaccine deployment is decentralized and politically controlled, poorly performing incumbents might instead have strong incentives to expand access aggressively in order to compensate for weaknesses in other domains. Our findings therefore speak to the electoral consequences of vaccination, not to the strategic incentives governing its implementation.

Finally, several features of our setting are important for interpreting external validity. First, Chile is a relatively well-functioning democracy, and the vaccination campaign was centrally coordinated with clear eligibility rules. This institutional structure limits the scope for local political manipulation and credit claiming. Second, although we exploit exogenous variation in vaccination rates, the campaign affected the entire country, complicating assessment of its aggregate national political impact. Third, the May 2021 elections took place in an unusual political context marked by social unrest and a constitutional process, which may limit extrapolation to routine elections. Finally, while we show that the electoral effects of vaccination depend critically on incumbent performance and reelection status, more work is needed to understand how these mechanisms operate in settings where politicians have greater control over public health policy implementation.

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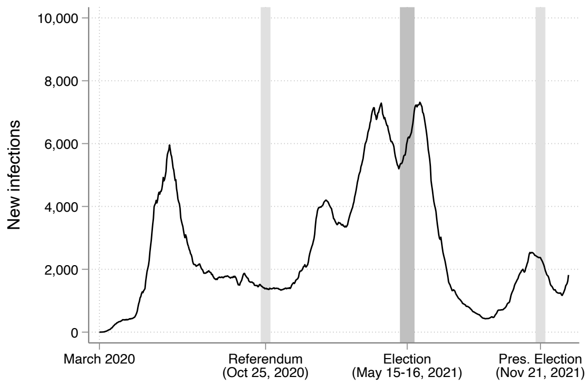
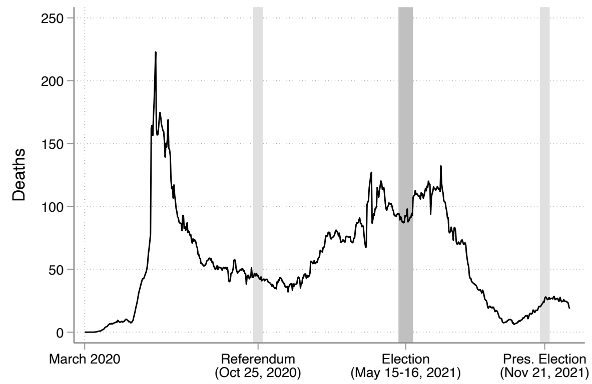
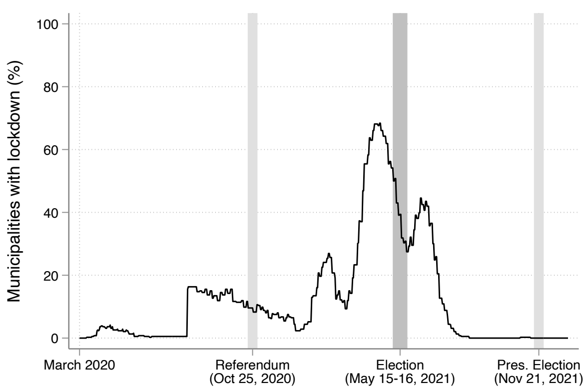
Figure 1: Pandemic and vaccination during study period



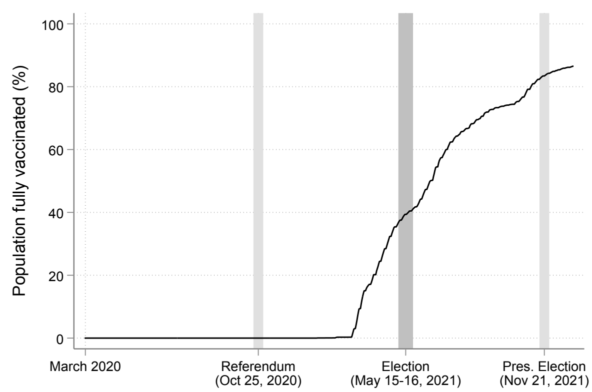
(a) Infections



(b) Deaths



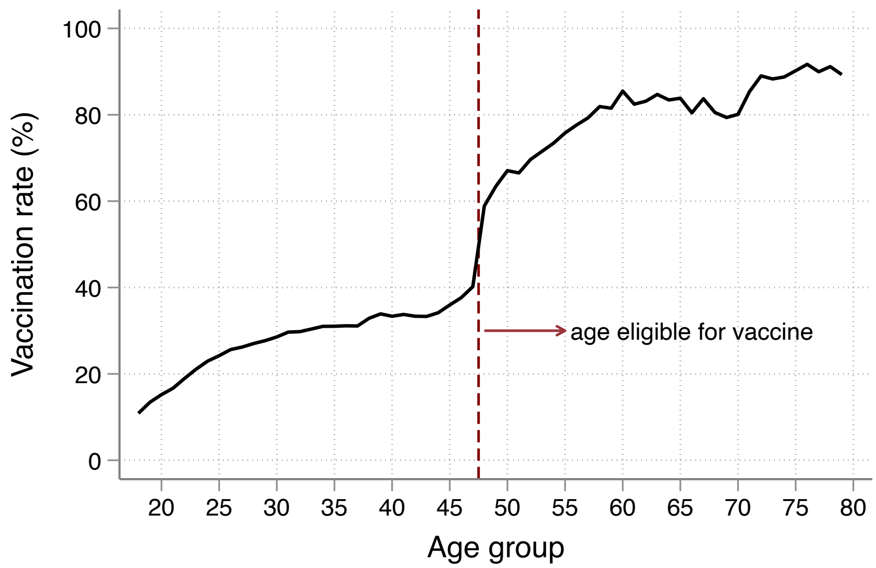
(c) Lockdowns



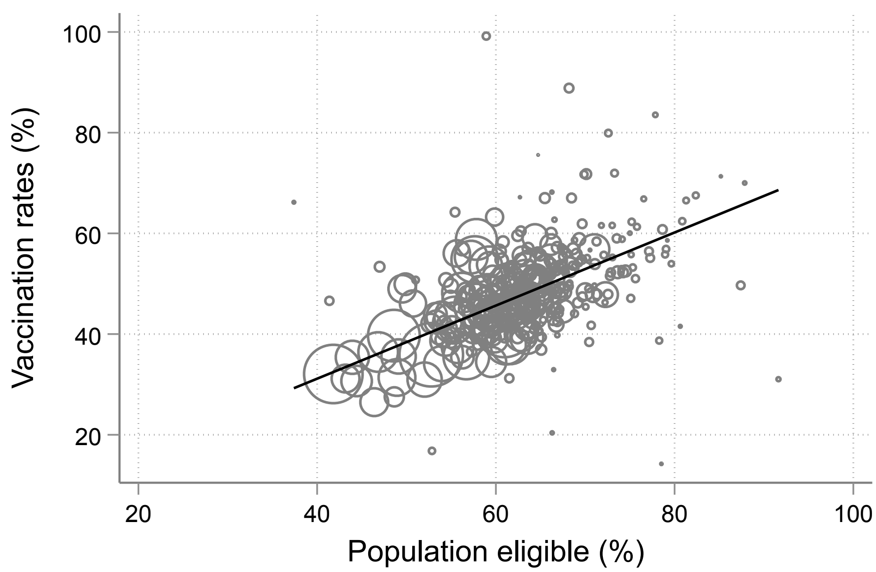
(d) Share fully vaccinated

Notes: Administrative data from the Ministry of Health (panels A and B) and Ministry of the Interior (panel C). We present seven days moving averages in panels A and B. The latter panel omits the announcement of 1,057 deaths in July 17 of 2020 which were related to the pandemic but did not have a date. Lockdowns in panel C are simply calculated the ratio of municipalities under lockdown over the total number of municipalities. Municipality-level lockdowns were decided by the central government using information about the local incidence of the pandemic. Panel D plots the cumulative percentage of the population who is fully vaccinated with two doses. The vertical dark gray line in May 15-16 marks the date of the election under study. The vertical light gray lines mark the date of the referendum to decide whether to write a new Constitution (October 25, 2020) and the Presidential and Congress Election (November 21, 2021).

Figure 2: Eligibility rules and vaccination rates



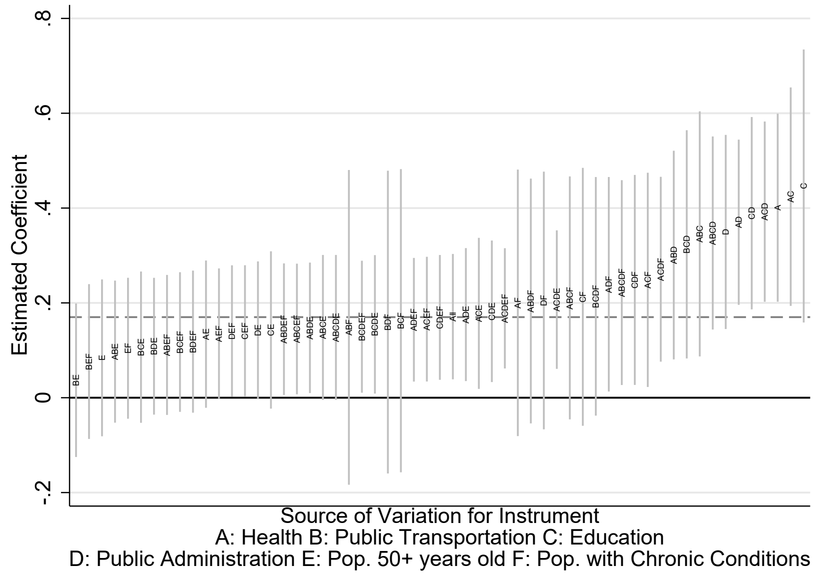
(a) Vaccination rate by election day



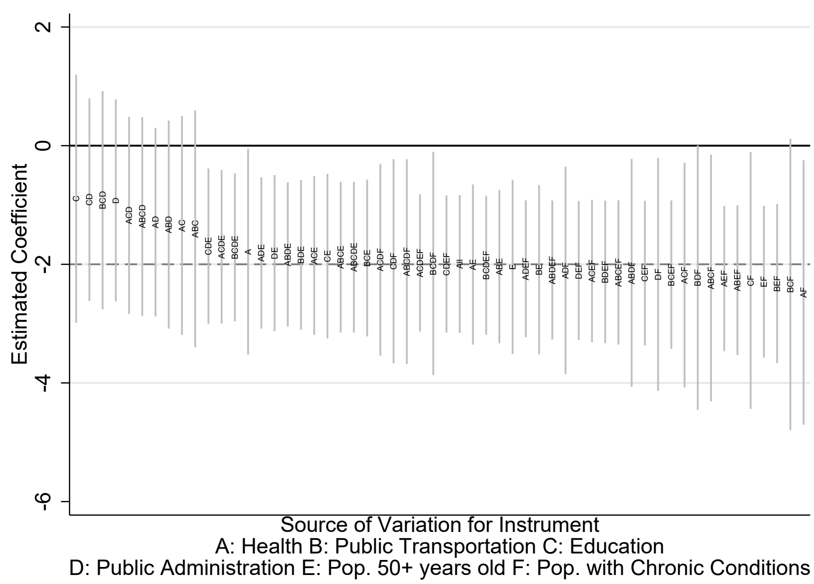
(b) First-stage

Notes: Panel (a) presents the vaccination rate by age group the day before the election we study. The last age group which was eligible for the vaccine were 48 yr old people. We observe an 18 percentage point increase in vaccination rates from 47 (40%) to 48 yr old people (58%). People younger than 48 yr old could have been vaccinated if they worked in priority occupations or had a chronic disease. Panel (b) shows the first-stage, i.e. the Municipality-level empirical relationships between vaccination rates (*y*-axis) and population eligible to get the vaccine (*x*-axis).

Figure 3: Variation in Local Average Treatment Effects



(a) Effect of vaccines on political participation



(b) Effect of vaccines on vote share for incumbents

Notes: These figures depict different IV estimates using combinations of all the different sources of variation in the instrument to trace out variation in LATE. Estimations with First-stage statistics (Kleibergen-Paap rk Wald F statistic) below 10 were excluded. The dependent variable in A is turnout in the 2021 elections while in B is vote share of Incumbent in 2021 mayoral elections. The dashed horizontal lines denote point estimates from columns (1) from Panels B in Tables 3 and 5, respectively.

Table 1: Descriptive statistics and validity of the research design

	Mean st. dev.	Univariate regression of covariate on instrument (mean instrument 64.3, st. dev. 9.27) unconditional	conditional on province F.E.	conditional on province F.E. and controls	Standardized effect from (4)
Panel A: Political participation	(1)	(2)	(3)	(4)	(5)
Turnout 2020 Plebiscite	43.9	-0.267*	0.178	0.208	0.18
	10.4	(0.140)	(0.214)	(0.172)	
Turnout 2017 Presidential Election	46.1	-0.2	-0.401**	-0.410***	0.35
	10.9	(0.161)	(0.173)	(0.154)	
Turnout 2016 Local Election	47.3	0.641***	0.333***	0.034	0.03
	12.2	(0.090)	(0.100)	(0.098)	
Turnout 2013 Presidential Election	49.1	0.076	-0.202	-0.229	-0.20
	10.5	(0.172)	(0.181)	(0.145)	
Turnout 2012 Local Election	53.6	0.562***	0.298***	0.059	0.05
	10.8	(0.100)	(0.079)	(0.079)	
Panel B: Political preferences					
Supports new constitution 2020	75.7	-0.19*	0.074	0.177	0.17
	9.9	(0.101)	(0.141)	(0.170)	
Supports convention 2020	71.8	-0.199**	0.045	0.163	0.18
	8.4	(0.091)	(0.128)	(0.151)	
Vote share right-wing 2017	46.7	0.088	-0.074	-0.212	-0.23
	8.6	(0.110)	(0.134)	(0.153)	
Vote share right-wing 2016	36.7	-0.299	-0.082	-0.005	0.00
	19.7	(0.268)	(0.293)	(0.352)	
Vote share right-wing 2013	23.7	-0.124	-0.08	-0.156	-0.21
	7.0	(0.085)	(0.124)	(0.150)	
Vote share right-wing 2012	35.6	-0.122	-0.25	-0.137	-0.07
	18.1	(0.265)	(0.264)	(0.334)	
Vote share left-wing 2017	53.3	-0.088	0.074	0.212	0.23
	8.6	(0.111)	(0.134)	(0.153)	
Vote share left-wing 2016	41.8	0.183	-0.054	-0.147	-0.07
	18.5	(0.220)	(0.279)	(0.337)	
Vote share left-wing 2013	64.7	0.135*	0.122	0.15	0.20
	7.0	(0.080)	(0.110)	(0.132)	
Vote share left-wing 2012	44.7	0.22	0.535*	0.558	0.29
	17.7	(0.189)	(0.316)	(0.356)	
Vote share independent 2016	17.9	0.158	0.074	0.052	0.02
	22.8	(0.329)	(0.435)	(0.516)	
Vote share independent 2012	16.0	-0.014	-0.254	-0.411	-0.18
	20.9	(0.321)	(0.443)	(0.523)	
Vote share incumbent 2016 (Mayor)	52.5	0.023	-0.068	-0.150	-0.12
	11.4	(0.210)	(0.224)	(0.233)	
Vote share incumbent 2016 (Councilors)	44.2	0.071	0.029	-0.014	-0.01
	8.5	(0.118)	(0.134)	(0.133)	
Vote share incumbent 2012 (Mayor)	47.1	0.063	0.228	0.337	0.20
	14.9	(0.170)	(0.215)	(0.246)	
Municipalities	343				

Notes: The inclusion of vote share for incumbents (both major and councilors) was not originally specified in our pre-analysis plan. Column 1 reports the mean and standard deviation for 20 variables from previous elections (listed at the left). Columns 2 to 4 report point estimates and robust standard errors from OLS regressions of each covariate on the instrument (i.e., share of people in priority groups). Column 2 shows unconditional results, column 3 conditions on 54 province fixed effects, and column 4 conditions on province fixed effects and a restricted set of controls including distance to the national capital (in logs), distance to the regional capital (in logs) and two indicators of population size (i.e., less than 50 thousand inhabitants and between 50 thousands and 100 thousands inhabitants). All regressions are weighted by local adult population in 2020. Statistical significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 2: Eligibility rules and vaccination rates

	Dependent variable: Share of adults with two doses				
	(1)	(2)	(3)	(4)	(5)
Share of eligible people	0.729***	0.716***	0.693***	0.755***	0.662***
	(0.054)	(0.088)	(0.102)	(0.102)	(0.094)
R-squared	0.398	0.514	0.529	0.743	0.766
Avg. dependent variable	49.86	49.86	49.86	48.58	48.58
Mean of instrument	64.17	64.17	64.17	63.52	63.52
Province fixed effects		X	X	X	X
Basic controls			X	X	X
Unbalanced covariates				X	X
2020 Plebiscite controls					X
Observations	343	343	343	324	324

Notes: The share of target population is computed as the sum of population working in health services, transportation, education, and public administration, population with chronic diseases, and population older than 50 years old; all as shares of adult population. The basic set of controls includes distance to national capital (in logs), distance to regional capital (in logs) and two indicators of population size (i.e., less than 50 thousand inhabitants and between 50 thousands and 100 thousands inhabitants). The set of unbalanced covariates includes turnout in 2017 presidential election, labor participation rate, share of women in population, labor participation rate of women, unemployment rate of women, prevalence of permanent health conditions, average household subsidy (in logs), total covid deaths per 10,000 inhabitants (in logs), and number of vaccination centers per 100,000 inhabitants. All covid figures are measured until first day of the vaccination campaign (December 23, 2020). 2020 Plebiscite controls include turnout and vote share for approval. Regressions are weighted by voting age population. Robust standard errors in parenthesis. Statistical significance: *** p <0.01, ** p <0.05, * p <0.1.

Table 3: Vaccination and political participation

	General turnout	Share of valid votes: Mayor	Share of valid votes: Constitutional convention	Share of valid votes: Councilors	Share of valid votes: Governors
Panel A: Instrumental variables	(1)	(2)	(3)	(4)	(5)
Share of adults with two doses	0.171**	0.177**	0.157***	0.203***	0.155***
	(0.067)	(0.069)	(0.056)	(0.064)	(0.057)
	[0.006]	[0.005]	[0.001]	[0.002]	[0.003]
Panel B: Reduced form					
Share of eligible people	0.109**	0.113***	0.100***	0.129***	0.099***
	(0.043)	(0.043)	(0.034)	(0.038)	(0.036)
	[0.002]	[0.001]	[0.000]	[0.000]	[0.000]
Panel B: OLS					
Share of adults with two doses	0.052	0.049	0.063*	0.060	0.069**
	(0.041)	(0.041)	(0.034)	(0.039)	(0.034)
	[0.101]	[0.132]	[0.006]	[0.038]	[0.006]
Observations	324	324	324	324	324
Province fixed effects	X	X	X	X	X
Full set of controls	X	X	X	X	X
First-stage F-statistic	49.97	49.97	49.97	49.97	49.97
Avg. dependent variable	47.86	46.86	40.89	45.39	42.15
St. dev. dependent variable	8.7	8.6	6.8	8.6	7.2
Standardized effect (Panel A)	0.27	0.28	0.32	0.33	0.30

Notes: All regressions are weighted by the local adult population. Robust standard errors in parenthesis. P-values from standard errors adjusted for spatial autocorrelation in brackets (Conley, 1999). Statistical significance: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 4: Infections and political participation (n.p.s)

	Dep variable: General turnout (mean 45.4, st. dev. 8.9)				
	(1)	(2)	(3)	(4)	(5)
Share of active cases in adult pop.	-5.809**				
	(2.555)				
Share of active cases in adult pop. (logs)		-10.201***			
		(3.232)			
Active cases (logs)			-2.219***		
			(0.380)		
Intensity of lockdown before election				-0.277***	
				(0.070)	
Lockdown on election day					-3.092***
					(0.812)
Observations	1,029	1,029	1,029	686	686
Municipality fixed effects	X	X	X	X	X
Election fixed effects	X	X	X	X	X
R-squared	0.782	0.784	0.792	0.801	0.798

Notes: Additional empirical analysis which was not pre-specified (n.p.s). This table considers the 3 elections taking place during the pandemic. These elections are the 2020 Plebiscite (October 25th, 2020), May 2021 Election (May 15th and 16th, 2021), and 2021 Presidential Election (November 21st, 2021). Regressions are weighted by population in municipality. Active cases refer to the the average daily active cases considering up to two weeks before each election. Intensity of lockdown refers to the total number of days with active lockdowns considering the previous two weeks of the election. Lockdowns were no longer operative before the last election (i.e., presidential), therefore specifications in columns 4 and 6 only includes observations for the first two elections. Share of active cases in adult population have a mean value of 0.15 and standard deviation of 0.19. Robust standard errors clustered at the municipality level in parenthesis. Statistical significance: *** p <0.01, ** p <0.05, * p <0.1.

Table 5: Vaccination, partisanship, and incumbents

	Dependent variable: Vote share							
	Constitutional convention			Local election (mayor)			Local election (mayor)	
	Left wing	Right wing	Independent	Left wing	Right wing	Independent	Incumbent	Incumbent (reelection law not binding)
Panel A: Instrumental variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Share of adults with two doses	0.020	0.053	0.057	0.929	0.319	-1.015	-1.995***	-1.141**
	(0.165)	(0.060)	(0.164)	(0.640)	(0.327)	(0.672)	(0.593)	(0.451)
	[0.921]	[0.457]	[0.529]	[0.056]	[0.296]	[0.010]	[0.004]	[0.001]
Panel B: Reduced form								
Share of eligible people	0.013	0.035	0.038	0.618	0.204	-0.660	-1.367***	-0.918**
	(0.123)	(0.044)	(0.124)	(0.478)	(0.236)	(0.484)	(0.398)	(0.397)
	[0.921]	[0.472]	[0.555]	[0.045]	[0.335]	[0.024]	[0.001]	[0.000]
Panel C: OLS								
Share of adults with two doses	-0.117	0.062*	0.131	0.901***	0.125	-0.910**	-0.440	-0.183
	(0.082)	(0.035)	(0.083)	(0.343)	(0.150)	(0.354)	(0.271)	(0.300)
	[0.003]	[0.024]	[0.001]	[0.006]	[0.442]	[0.000]	[0.010]	[0.208]
Municipalities	324	324	324	324	324	324	324	233
Province fixed effects	X	X	X	X	X	X	X	X
Full set of controls	X	X	X	X	X	X	X	X
Avg. dependent variable	17.83	21.07	18.19	37.12	28.02	32.74	39.44	42.94
Standardized effect (Panel A)	0.02	0.06	0.05	0.40	0.16	-0.35	-1.54	-0.60

Notes: All regressions are weighted by adult population. Robust standard errors in parentheses. P-values from standard errors adjusted for spatial autocorrelation in brackets (Conley, 1999). Statistical significance: *** p <0.01, ** p <0.05, * p <0.1.

Table 6: The role of incumbent performance (n.p.s)

	Dependent variable: Vote share for the incumbent				
Sample:	All municipalities			Municipalities with incumbents who could run for reelection	
	All	Low-performing public finance	High-performing public finance	High-performing public finance	Low-performing public finance
Panel A: Instrumental Variables	(1)	(2)	(3)	(4)	(5)
Share of adults with two doses	-1.995***	-1.311**	-0.622	1.321*	-0.218
	(0.593)	(0.588)	(0.655)	(0.773)	(0.489)
Panel B: Reduced form					
Share of eligible people	-1.367***	-1.035*	-0.498	1.081	-0.227
	(0.398)	(0.557)	(0.682)	(0.951)	(0.466)
Panel C: OLS					
Share of adults with two doses	-0.440	-0.681	0.028	0.717**	-0.144
	(0.271)	(0.415)	(0.391)	(0.319)	(0.589)
Municipalities	324	165	159	115	118
Province fixed effects	X	X	X	X	X
Full set of controls	X	X	X	X	X
First-stage F-statistic	53.57	29.82	35.27	13.91	34.16
Avg. dependent variable	39.64	38.48	40.84	43.97	41.95

Notes: Additional empirical analysis which was not pre-specified (n.p.s). We split municipalities into "high-performing" and "low-performing" by the median of the distribution in a measure of performance in municipal public finances. The measure of performance is the change in municipal deficit per capita between the mayoral terms of 2017-2020 (term of incumbent) and 2013-2016 (previous term). Deficit is defined as spending minus revenues (per capita). All regressions are weighted by adult population. Robust standard errors in parenthesis. Statistical significance: *** p <0.01, ** p <0.05, * p <0.1.

ONLINE APPENDIX

The Political Consequences of Vaccines:
Quasi-Experimental Evidence from Eligibility Rules

Emilio Depetris-Chauvin and Felipe González

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A Definition of Political Coalitions

We examine the impact of the vaccination process on two sets of outcomes Y_c. The first is *Turnout*, defined as total votes in election ℓ (including null and blank votes) over total number of people who are eligible to vote (i.e. *electores*), with ℓ being Local Elections (mayor), Local Elections (councilors), Constitutional Convention, and Governors. The second set of outcomes are *Vote Shares*, defined as votes for option j in the election over total number of votes, with j being defined as explained bellow.

A.1 Local Election

1.1 *Incumbent*, defined as the incumbent mayor running for reelection or the candidate from his/her coalition when the mayor is not running.

1.2 *Left-wing*, defined as those running in the following coalitions: Unidad por el Apruebo, Chile Digno Verde y Soberano, Unidos por la Dignidad, Dignidad Ahora,

1.3 *Right-wing*, defined as those running in the following coalitions: Chile Vamos, Republicanos, Independientes Cristianos, Ciudadanos Independientes, Nuevo Tiempo.

1.4 *Independent*, defined as those running in the following coalitions: Ecologistas e Independientes, Independientes fuera de pacto.

1.5 *Councilors*, same outcomes as the previous four but defined in the separate local election for councilors.

A.2 Constitutional Convention Election

2.1 *Left-wing*, defined as candidates running in the following lists: Lista del Apruebo (YB), Apruebo Dignidad (YQ), Partido Humanista (XG), Partido Ecologista (XA).

2.2 *Right-wing*, defined as candidates running in the list Vamos por Chile (XP).

2.3 *Independent*, defined as candidates in any of the 74 lists (A-ZZ) that are different from the five lists composed by candidates from left- or right-wing political parties.

2.4 *Invalid*, defined as null or blank votes over the total number of casted votes. This measure attempts to capture the level of confusion or disinformation in the population. Recent media articles suggest that some people appear to believe that they have to vote for multiple candidates. The confusion is understandable given that this is the first time a Constitutional Convention will be elected and there are reserved seats for women and indigenous people.

A.3 Regional Governors Election

3.1 *Left-wing*, defined as candidates in the following coalitions: Unidad Constituyente, Frente Amplio, Igualdad para Chile, Humanicemos Chile, Partido de Trabajadores Revolucionarios, Por Dignidad Regional,

3.2 *Right-wing*, defined as candidates in the following coalitions: Chile Vamos, Partido Republicano, Unión Patriótica, Partido Nacional Ciudadano, Independientes Cristianos,

3.3 *Independents*, defined as candidates in the following coalitions: Ecologistas e Independientes, Regionalistas Verdes, Independientes fuera de pacto.

B Alternative Explanations

B.1 Vaccines and anxiety in high-frequency surveys

This section explores why vaccines might increase preferences for challengers. This analysis was *not* part of the pre-analysis plan and we view it as more exploratory. We interpret the decision between the incumbent and a challenger through the lens of decision-making under uncertainty (Tversky and Kahneman, 1992). The incumbent represents the safe (known) alternative, while the challenger represents the uncertain (risky) option. Voters need to acquire information about challengers. We interpret the pandemic as affecting people's anxiety and ability to focus (Fetzer et al., 2022), difficulting information acquisition, and therefore tilting citizens towards incumbents, the safe and known alternative. Vaccines can reverse this process and thus potentially empower challengers. This hypothesis is consistent with previous research in political science and financial economics showing that during times of crisis or uncertainty, agents resort to the certainty provided by the status quo and safer assets (Cohn et al., 2015; Bisbee and Honig, 2021).

We use high-frequency surveys conducted in 2021 by an independent private firm. The surveys were implemented on a weekly basis and aim to be representative of the entire country. As such, the probabilistic sampling was geographically stratified, which led to respondents living in hundreds of municipalities located in all of the 16 regions in the country, with 90% living in urban and 10% in rural areas. Crucially, each weekly survey was conducted in less than three days, which means that the eligibility rules were fixed within a given survey. We use all surveys conducted from the first week of February 2021 until the first week of September 2021. Each survey was responded by more than 700 adults and thus we observe more than 22,000 survey respondents.

In order to exploit the roll-out of the vaccines following the weekly eligibility rules, we estimate the following regression equation using data from the surveys around the election:

$$y_{ij(i)} = \beta V_i + f(x_i) + \phi_{j(i)} + \eta_{ij(i)} \tag{5}$$

where $y_{ij(i)}$ is the response of person i who's age by the time of the survey is $j(i)$. As dependent variables, we use two indicators, one for individuals who reported being *worried* and another one for those *very worried* about getting infected. The indicator V_i takes the value of one if i was fully vaccinated by the time of the survey. Similarly as before, we provide instrumental variables estimates using as instrument an indicator which takes the value of one if i was eligible to be fully vaccinated when surveyed. Our preferred specification also includes non-parametric controls for gender and education $f(x_i)$. Crucially for the identification strategy, equation (5) includes a complete set of age fixed effects $\phi_{j(i)}$ which allows us to econometrically compare individuals of the same age but who answered the survey when they were and were not eligible for the vaccine. Finally, $\eta_{ij(i)}$ is an error term clustered by age to allow for arbitrary correlation within age cohorts.

Table A.16 presents estimates of equation (5).[22] Column 1 and 2 use as dependent variable an indicator for people who reported being *worried* or *very worried* about getting infected by the virus. We observe 57% of respondents to be worried and 36% to be very worried. We find that vaccines decrease the probability of being worried or very worried by 5-6 percentage points, a decrease of 10-13% over the respective sample means. Column 3 confirms this result using the ordinal 1-5 response as dependent variable and an ordered probit for estimation. The lower concerns about the pandemic are mirrored in the optimism reported by survey respondents (columns 4-5). In particular, vaccines increase the probability of being optimistic about the future of the country by 7 percentage points, an increase of 18% over the sample mean.

B.2 Vaccines and the ability to focus in repeated surveys

We use waves of a nationally representative survey conducted by an independent research team in charge of studying the evolution of mental health during the pandemic. They implemented four waves of the same survey in July 2020, November 2020, April 2021, and August 2021. Approximately 1,500 individuals were surveyed in each wave, allowing us to examine how mental health evolved within individuals over time. We use these surveys to test for the empirical relationship between vaccines, concentration, and depression. Table A.15 provides descriptive statistics for the six measures related to mental health that can be tracked in all four waves of the survey.

Given that the same individuals were surveyed four times within a two-year period, the data allows us to control for unobserved heterogeneity across individuals. Econometrically, we exploit within-individual variation over time using the following econometric model:

$$y_{it} = \beta E_{it} + \gamma f(x_i) + \phi_i + \phi_t + \epsilon_{it} \tag{6}$$

where y_{it} is an indicator for the response of individual i in month t, E_{it} takes the value of one if individual i was eligible for the vaccine in month t, ϕ_i is a full set of individual fixed effects, and ϕ_t represent wave fixed effects. Unfortunately, the survey did not ask for the vaccination status and thus we rely on the 70% take-up rate from sections 5.1 and B.1 to discuss instrumental variables estimates. We also include a flexible vector of controls $f(x_i)$ which include age fixed effects, individual-level controls (gender, education), and the number of covid cases in the region. The error term ϵ_{it} is clustered by municipality, and we employ weighted least squares with survey weights.

[22]To save space, we do not report the first-stage estimates in Table A.16. However, these estimates can be easily derived from the results presented in the reduced form (Panel B) and IV (Panel C). It is reassuring to note that the implied first-stage coefficient (i.e., 0.67) is remarkably close to the 70% take-up rate obtained in section 5.1 using variations in vaccination rates and the share of eligible individuals at the municipality level.

Panel A in Table A.17 presents estimates of equation (6) without individual fixed effects for comparison. Panel B in the same table shows estimates with individual fixed effects. Columns 1 and 2 show that when individuals were eligible for the vaccine, their concentration improved. If we considered a take-up rate of 70% for the vaccine, then individuals who were fully vaccinated reported feeling overwhelmed 12 percentage points less than those not vaccinated. Moreover, vaccination leads to a 15 percentage points decrease in the probability of feeling not able to focus, a large decrease from a sample average of 40%. We do not find evidence of sleeping problems varying systematically with vaccines (column 3) and, consistent with the evidence from section B.1, individuals fully vaccinated report significantly lower feelings of depression (columns 4-6). The results are similar if we replace the dependent variable by the 1-4 ordered response (Table A.18)

B.3 Booster analysis

The May 2021 election took place when the pandemic was still generating high infection rates, lockdowns were common, uncertainty about the medium- and long-run consequences of the disease was substantial, and completing the vaccination schedule was perceived as essential for returning to normality. Under these conditions, vaccination plausibly played a central role in shaping political behavior, so our baseline estimates can be interpreted as capturing an upper bound of vaccine effects. By contrast, the November 2021 Presidential and Congressional elections occurred in a markedly different environment. More than 90% of the population had completed the primary vaccination schedule, uncertainty about the disease had declined, and vaccination campaigns focused mainly on booster doses. If vaccination primarily operates by reducing uncertainty and lowering participation costs, its political effects should be weaker in this later context. We use the November elections to test this hypothesis.

Table A.19 presents first-stage and instrumental variables results. Column 1 begins by estimating the impact of eligibility rules on vaccination rates. Eligibility rules changed weekly and thus the share of eligible people is different the week before the November election than the week of the May election. In addition, more than 90% of the population had the two doses and rules were in place mostly for booster vaccines (third doses). The estimate shows that compliance with the vaccine was lower, with less than a quarter of people who were offered the vaccine actually taking it, but still highly significant and different from zero with a F-statistic of 10.2.

Columns 2 and 5 in Table A.19 show that more vaccination leads to more political participation in the presidential and congress elections, but the point estimate is 30% smaller than in May. As the incumbent president was not a candidate in the presidential election, we propose two measures: (1) deviations from the political center derived from vote shares and an order of the seven candidates in a unidimensional left-right spectrum, and (2) vote shares of right-wing candidates as proxy for candidates from parties which were politically closer to the incumbent President. We find little evidence of impacts on the former but some evidence of higher preferences for challengers (left-wing) when vaccination rates were higher. Yet the magnitude of the coefficient is significantly smaller than in the May election. Studying preferences for incumbents in the Congress Election, we again fail to find evidence for vaccines tilting voters towards challengers (column 6). In all, we confirm the existence of attenuated impacts in the November election in terms of turnout and null or attenuated effects in terms of vaccines increasing preferences for challengers.

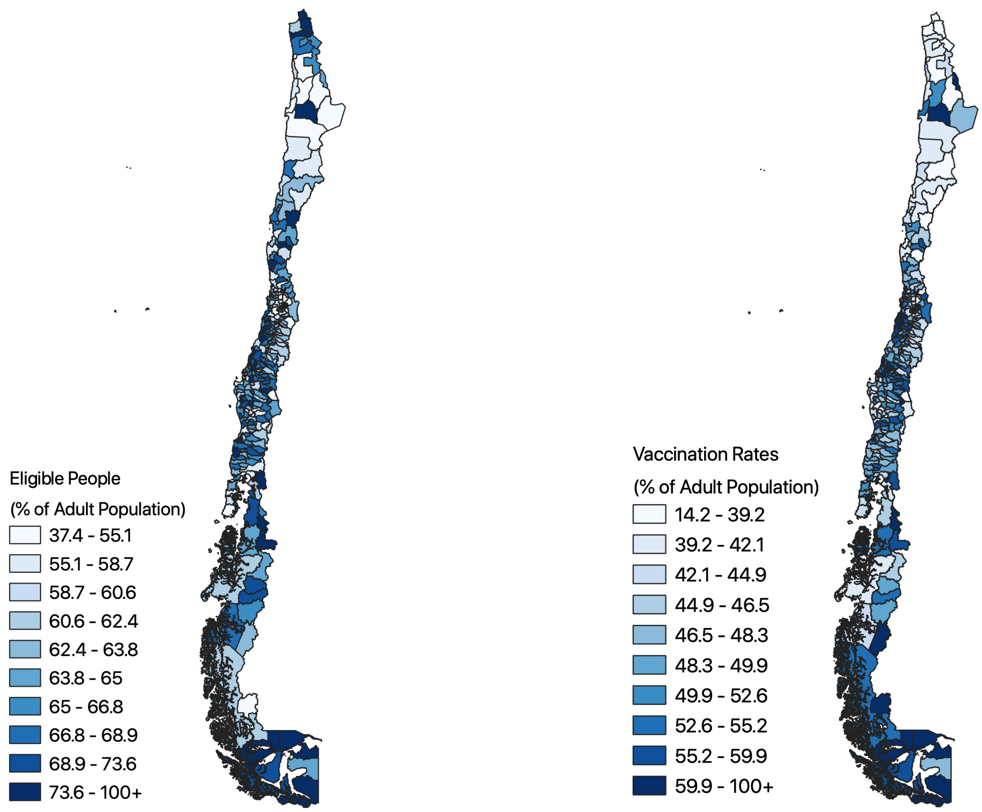
B.4 Information acquisition as measured by invalid votes

The vaccines could have distorted the acquisition of information about candidates. There are at least two ways in which information acquisition could have been affected. First, vaccines could have changed the exposure to political campaigns, leading to fewer interactions between candidates and voters and to low information levels. Second, the overload of information about the disease and other events (e.g. vaccination) could have displaced information about candidates in the election. Limited attention and cognitive restrictions implies that voters might select and store a finite amount of information. If any of these or related informational mechanisms is at play, we should observe that voters had fewer information about candidates. Note that uninterested or apathetic voters can fall in any of these two categories, as collecting information is a decision.

Given the lack of data measuring how informed voters were, we can only test this hypothesis using proxies of information. We argue that less information about candidates could have been associated with more invalid or null votes.[23] Therefore, we repeat our main estimation strategy but replace the dependent variable by the percentage of invalid (blank or null) votes in the corresponding election. The results in Table A.20 suggest that vaccines did not affect the amount of information about candidates as vaccination rates are unrelated to the percentage of invalid votes in local elections. Overall, the sign of coefficients is unstable across elections and the point estimates are relatively small when compared to the impact of vaccines on incumbents. We conclude that information acquisition is unlikely to be the main explanation behind our findings.

[23]This interpretation is far from being the only possible one and there is an active research agenda studying how to interpret and what drives invalid voting. See Kouba and Lysek (2018) for a relatively recent meta-analysis.

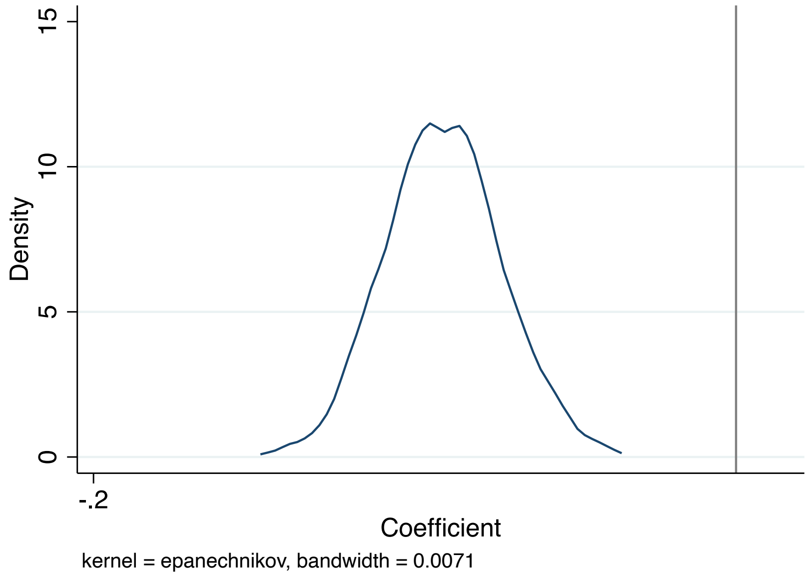
Figure A.1: Geographic Distribution of Eligible and Vaccinated People



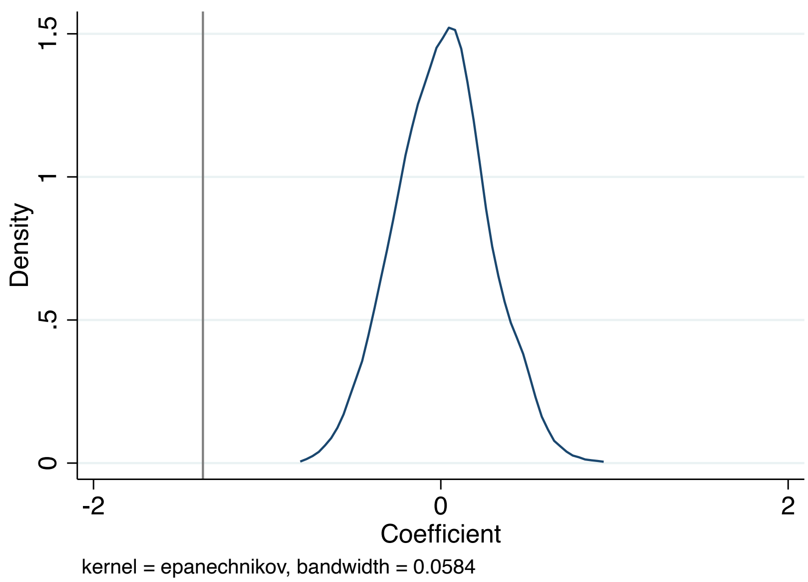
(a) Share of Eligible People (deciles) (b) Share of Vaccinated People (deciles)

Note: These figures present the geographic distribution across Chilean municipalities of the proportion of eligible (left figure) and vaccinated people (right figure) by the date of the election. Share of vaccinated people is defined as the number of people with two doses over the total number of people older than 18 years old (i.e. adult population) as measured by the 2020 projections.

Figure A.2: Randomization inference



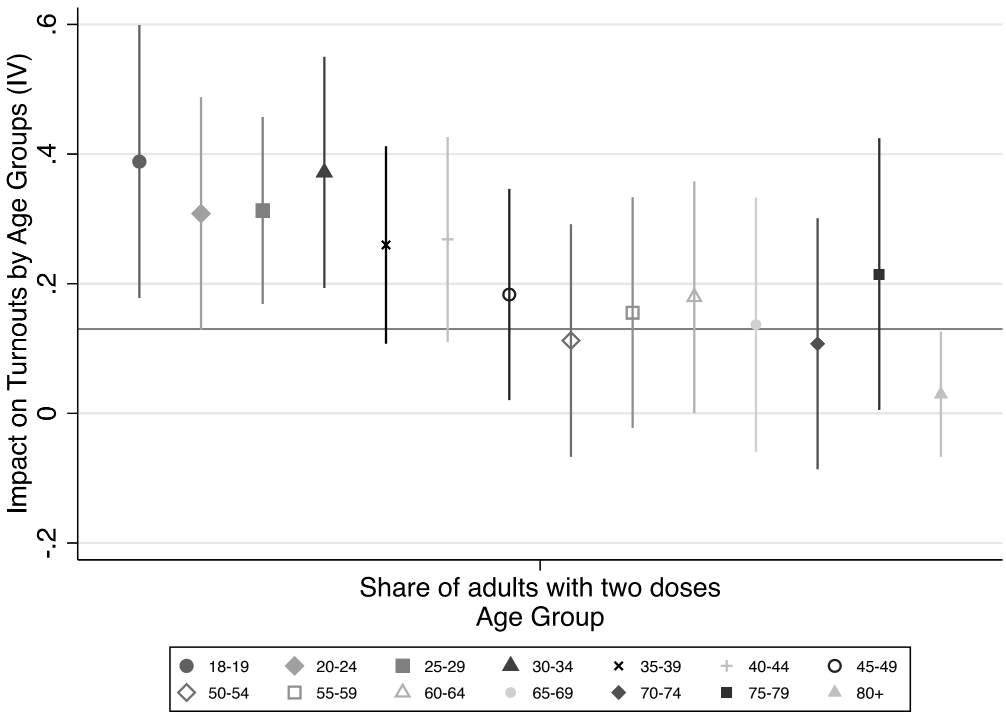
(a) Effect of vaccines on political participation



(b) Effect of vaccines on vote share for incumbents

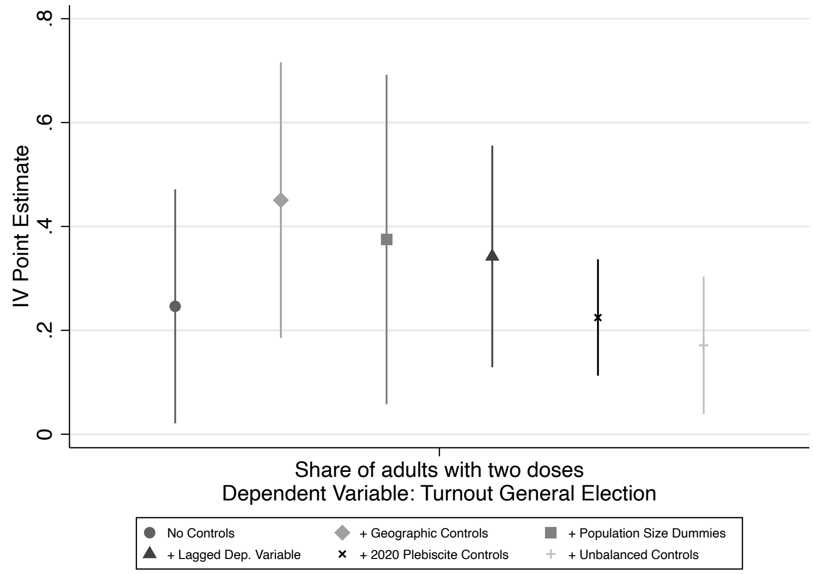
Note: These figures presents the distribution of point estimates from a series of regressions in which predicted share of adults with two doses are randomly assigned across municipalities 1,000 times. The dependent variable in A is turnout in the 2021 elections while in B is vote share of Incumbent in 2021 mayoral elections. The vertical lines denote point estimates from columns (1) from Panels B in Tables 3 and 5, respectively.

Figure A.3: Turnout by age bracket

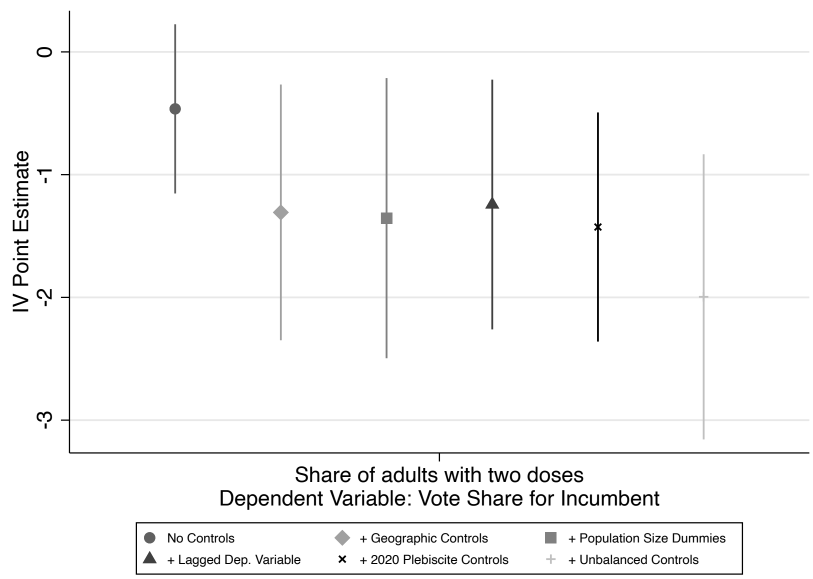


Note: This figure reports instrumental variables estimates of equation (3) using turnout by age bracket as the dependent variable. For each age group, turnout is defined as the number of valid votes cast by individuals in that age bracket divided by the total number of registered voters in the same bracket. Estimates are obtained using the baseline specification with province fixed effects and the full set of controls. Vertical lines denote 95 percent confidence intervals based on robust standard errors. The figure shows that vaccination significantly increased turnout among younger voters, with smaller and statistically insignificant effects for older age groups.

Figure A.4: Robustness of results to control variables



(a) Effect of vaccines on political participation



(b) Effect of vaccines on vote share for incumbents

Note: These figures presents IV estimates of equation (3) using different combinations of control variables. The dependent variable in panel (a) is turnout in the 2021 elections while in panel (b) is the vote share for the incumbent in 2021 mayoral elections. The vertical lines denote the 95 percent confidence interval.

Table A.1: (In)Validity of the booth-level design

	Mean st. dev.	Univariate regression of covariate on closest distance to vaccination venue: unconditional	conditional on municipality F.E.	conditional on municipality F.E. and controls	Standardized effect from (4)
Panel A: Political participation	(1)	(2)	(3)	(4)	(5)
Turnout 2020 Plebiscite	51.25	-0.081	0.681	0.545	0.04
	11.6	(0.539)	(0.558)	(0.579)	
Turnout 2017 Presidential Election	46.4	0.764	1.039	1.099	0.09
	10.6	(0.638)	(0.680)	(0.762)	
Turnout 2016 Local Election	35.0	3.705***	1.391**	0.624	0.03
	16.1	(1.382)	(0.684)	(0.511)	
Turnout 2013 Presidential Election	51.3	1.319	0.632	1.306	0.06
	14.6	(0.917)	(3.931)	(4.095)	
Turnout 2012 Local Election	46.1	3.299***	0.183	0.960	0.05
	14	(0.651)	(3.399)	(3.576)	
Panel B: Political preferences					
Supports new constitution 2020	77.5	-1.155	-1.084*	-1.318**	-0.09
	12.1	(1.013)	(0.619)	(0.634)	
Supports convention 2020	74.5	-1.264	-1.071*	-1.269**	-0.09
	12.2	(0.903)	(0.552)	(0.559)	
Vote share right-wing 2017	44.5	1.453	1.052**	1.336**	0.09
	11.6	(0.992)	(0.509)	(0.532)	
Vote share right-wing 2016	40.5	3.802*	0.413	0.694*	0.02
	21.6	(2.065)	(0.354)	(0.405)	
Vote share right-wing 2013	25.8	1.450	1.336*	1.672**	0.11
	11.3	(1.299)	(0.681)	(0.810)	
Vote share right-wing 2012	38.5	2.895	0.993**	1.294*	0.05
	19.7	(1.767)	(0.502)	(0.672)	
Vote share left-wing 2017	55.5	-1.453	-1.052**	-1.336**	-0.09
	11.6	(0.992)	(0.509)	(0.532)	
Vote share left-wing 2016	41.4	-2.835*	-0.425	-0.668*	-0.03
	20.4	(1.658)	(0.369)	(0.401)	
Vote share left-wing 2013	63.5	-0.423	-1.038*	-1.465**	-0.10
	10.7	(1.099)	(0.607)	(0.729)	
Vote share left-wing 2012	48.2	-2.698	-1.022**	-1.227*	-0.05
	18.7	(1.636)	(0.512)	(0.688)	
Vote Share Independent 2016	18.2	-0.968	0.011	-0.026	-0.00
	22.1	(1.431)	(0.236)	(0.239)	
Vote Share Independent 2012	13.2	-0.197	0.029	-0.067	-0.00
	18.7	(1.418)	(0.194)	(0.169)	

Notes: Column 1 reports the mean and standard deviation for 17 variables from previous elections (listed at the left). Columns 2 to 4 report point estimates and robust standard errors from OLS regressions of each covariate on the average distance from people's homes in a booth to the closest vaccination venue within their municipality of residence (Closest distance to vaccination venue). Column 2 shows unconditional results, column 3 conditions on municipality fixed effects, and column 4 conditions on municipality fixed effects and a restricted set of controls including percentage of women, average age, total number of people registered in the booth, and the distances from people's homes to the booth and the municipal hall. Due to missing data on the number of voters registered at the booth level for the 2012 and 2013 elections, balance tests for turnouts in those elections are performed for a restricted sample. Robust standard errors clustered at the municipality level in parenthesis. Statistical significance: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.2: Descriptive statistics from the 2017 Census

		Univariate regression of covariate on instrument (mean instrument 64.3, st. dev. 9.27)			
	Mean st. dev.	unconditional	conditional on province F.E.	conditional on province F.E. and controls	Standardized effect from (4)
	(1)	(2)	(3)	(4)	(5)
Population women	49.0	0.037*	0.041	0.060*	0.10
	5.6	(0.021)	(0.042)	(0.033)	
Population 0 to 4 yrs old	6.4	-0.037	-0.027	-0.036	-0.30
	1.1	(0.024)	(0.031)	(0.024)	
Population 5 to 12yrs old	10.8	-0.006	0.008	-0.027	-0.14
	1.7	(0.055)	(0.076)	(0.057)	
Population 12 to 18 yrs old	9.3	0.021	0.032	0.011	0.06
	1.7	(0.046)	(0.068)	(0.052)	
Labor Participation Rate	59.8	-0.582***	-0.434***	-0.400***	-0.38
	9.7	(0.056)	(0.059)	(0.058)	
Labor Participation Rate, women	47.0	-0.698***	-0.540***	-0.448***	-0.40
	10.3	(0.093)	(0.109)	(0.097)	
Unemployment Rate	7.0	0.030*	0.022	0.031	0.13
	2.3	(0.016)	(0.018)	(0.020)	
Unemployment Rate, women	11.5	0.112***	0.091**	0.070*	0.15
	4.3	(0.035)	(0.035)	(0.039)	
Poor Household Rate (extensive)	6.4	-0.067**	-0.044	-0.037	-0.12
	2.9	(0.032)	(0.050)	(0.049)	
Poor Household Rate (intensive)	1.4	-0.013	-0.009	-0.007	-0.09
	0.7	(0.008)	(0.011)	(0.010)	
Rural Population	0.4	0.009***	0.005***	0.001	0.03
	0.3	(0.001)	(0.002)	(0.001)	
Population with Primary Education	0.3	0.004***	0.003**	0.001	0.10
	0.1	(0.001)	(0.001)	(0.001)	
Population with Secondary Education	0.4	0.001	0.001	0.001	0.17
	0.1	(0.001)	(0.001)	(0.001)	
Population with Tertiary Education	0.2	-0.005***	-0.005	-0.003	-0.31
	0.1	(0.002)	(0.003)	(0.003)	
Municipalities	343				

Notes: Column 1 reports the mean value and standard deviation for 14 demographic and labor market variables from 2017 Census (listed at the left). Columns 2 to 4 report point estimates and robust standard errors from OLS regressions of each covariate on our instrument (i.e., share of people in priority groups). Column 2 shows unconditional results, column 3 conditions on 54 province fixed effects, and column 4 conditions on province fixed effects and a restricted set of controls including distance to the national capital (in logs), distance to the regional capital (in logs) and two indicators of population size (i.e., less than 50 thousand inhabitants and between 50 thousands and 100 thousands inhabitants). All regressions are weighted by local adult population in 2020. Statistical significance: *** p <0.01, ** p <0.05, * p <0.1.

Table A.3: Additional descriptive statistics

		Univariate regression of covariate on instrument (mean instrument 64.3, st. dev. 9.27)			
	Mean st. dev.	unconditional	conditional on province F.E.	conditional on province F.E. and controls	Standardized effect from (4)
Panel A: 2017 National Survey	(1)	(2)	(3)	(4)	(5)
Log household income	12.5	-0.016***	-0.009	-0.006	-0.18
	0.3	(0.004)	(0.006)	(0.007)	
Poverty Rate	12.4	0.228***	-0.018	-0.038	-0.05
	7.3	(0.040)	(0.050)	(0.058)	
Poverty Rate, multidimensional	26.1	0.095	0.156	0.031	0.03
	10.5	(0.095)	(0.120)	(0.124)	
Self-reported health score	18.1	0.135***	0.062**	0.053	0.15
	3.2	(0.031)	(0.031)	(0.038)	
Permanent health condition	12.7	0.189***	0.098**	0.101**	0.20
	4.6	(0.034)	(0.039)	(0.040)	
Malnutrition	7.4	0.052	0.046	0.018	0.04
	3.9	(0.042)	(0.060)	(0.057)	
Lack of health insurance	5.3	-0.166***	-0.082	-0.091	-0.20
	4.3	(0.041)	(0.067)	(0.075)	
Lack of social security	36.4	0.079	0.281**	0.204	0.17
	11.5	(0.124)	(0.137)	(0.145)	
Lack of basic services	14.3	0.313***	0.138*	0.008	0.01
	12.6	(0.062)	(0.075)	(0.053)	
Log household subsidy	9.5	0.034***	0.021***	0.017***	0.37
	0.4	(0.004)	(0.005)	(0.005)	
Panel B: 2019 protests					
Protests per 100.000 inhab.	0.11	0.002***	0.000	0.001	0.01
	0.69	(0.001)	(0.000)	(0.000)	
Indicator for protest	0.56	-0.011***	-0.006	-0.001	-0.03
	0.50	(0.003)	(0.004)	(0.006)	
Indicator for riot	0.44	-0.013***	-0.006	0.003	0.05
	0.50	(0.003)	(0.004)	(0.005)	
Municipalities (panel A)	323				
Municipalities (panel B)	343				

Notes: Column 1 reports the mean and standard deviation for 10 demographic and labor market variables from 2017 Census (panel A) and 3 variables related to the 2019 protests (panel B). Columns 2 to 4 report point estimates and robust standard errors from OLS regressions of each covariate on our instrument (i.e., share of people in priority groups). Column 2 shows unconditional results, column 3 conditions on 54 province fixed effects, and column 4 conditions on province fixed effects and a restricted set of controls including distance to the national capital (in logs), distance to the regional capital (in logs) and two indicators of population size (i.e., less than 50 thousand inhabitants and between 50 thousands and 100 thousands inhabitants). The source for the protest variables in panel B is the Armed Conflict Location and Event Data (ACLED). We count the number of protests between 6 October and 15 November (2019). Regressions are weighted by local adult population in 2020. Statistical significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.4: Descriptive statistics for the pandemic before the vaccines and before the election

		Univariate regression of covariate on instrument (mean instrument 64.3, st. dev. 9.27)			
	Mean st. dev.	unconditional	conditional on province F.E.	conditional on province F.E. and controls	Standardized effect from (4)
	(1)	(2)	(3)	(4)	(5)
Before Vaccination Campaign					
Share of lockdown days	7.0	-0.310**	-0.137	0.002	0.00
	9.7	(0.151)	(0.113)	(0.104)	
COVID infections per 10,000	277.7	-4.595**	1.042	1.701	0.10
	159.7	(1.931)	(1.788)	(1.842)	
COVID deaths per 10,000	5.8	-0.161**	0.256**	0.278**	0.50
	5.2	(0.076)	(0.112)	(0.111)	
Vaccination centers per 100,000	24.3	0.540***	0.445***	0.351***	0.07
	48.4	(0.080)	(0.139)	(0.103)	
Week Before Election					
Share of lockdown days	45.4	-0.110	0.537	0.736	0.14
	46.6	(0.595)	(0.549)	(0.648)	
COVID infections per 10,000	31.44	0.329***	0.191	0.168	0.06
	26.1	(0.111)	(0.136)	(0.160)	
COVID deaths per 10,000	0.59	0.013***	0.023***	0.023***	0.23
	0.89	(0.003)	(0.004)	(0.005)	
Municipalities	343				

Notes: The inclusion of the three indicators of pandemic severity the week before the election was not originally specified in our pre-analysis plan. Column 1 reports the mean value and standard deviation for 4 variables related to the pandemic (listed at the left). All pre-vaccination campaign covid figures are measured until first day of the vaccination campaign (December 23, 2020). Columns 2 to 4 report point estimates and robust standard errors from OLS regressions of each covariate on our instrument (i.e., share of people in priority groups). Column 2 shows unconditional results, column 3 conditions on 54 province fixed effects, and column 4 conditions on province fixed effects and a restricted set of controls including distance to the national capital (in logs), distance to the regional capital (in logs) and two indicators of population size (i.e., less than 50 thousand inhabitants and between 50 thousands and 100 thousands inhabitants). All regressions are weighted by local adult population. Statistical significance: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.5: Vaccination and political participation when controlling non-linearly for unbalances

	General turnout	Share of valid votes: Mayor	Constitutional convention	Councilors	Governors
Panel A: Instrumental variables	(1)	(2)	(3)	(4)	(5)
Share of adults with two doses	0.136***	0.130***	0.118***	0.179***	0.123***
	(0.049)	(0.049)	(0.040)	(0.046)	(0.041)
Panel B: Reduced form					
Share of eligible people	0.105**	0.100**	0.091**	0.138***	0.095**
	(0.043)	(0.044)	(0.037)	(0.039)	(0.038)
Panel B: OLS					
Share of adults with two doses	0.061*	0.054	0.078***	0.074**	0.079***
	(0.036)	(0.036)	(0.028)	(0.033)	(0.030)
Observations	324	324	324	324	324
Province fixed effects	X	X	X	X	X
Basic set of controls	X	X	X	X	X
Quintiles of unbalanced covariates	X	X	X	X	X
First-stage F-statistic	73.46	73.46	73.46	73.46	73.46
Avg. dependent variable	47.86	46.86	40.89	45.39	42.15
St. dev. dependent variable	8.7	8.6	6.8	8.6	7.2

Notes: All regressions are weighted by the local adult population. The set of unbalanced covariates (discretionalized into quintiles) are turnout in 2017 presidential election, labor participation rate, share of women in population, labor participation rate of women, unemployment rate of women, prevalence of permanent health conditions, average household subsidy (in logs), total covid deaths per 10,000 inhabitants (in logs), and number of vaccination centers per 100,000 inhabitants. Robust standard errors in parenthesis. Statistical significance: *** p <0.01, ** p <0.05, * p <0.1.

Table A.6: Spatial spillovers of eligibility rules

Dependent variable: Share of adults with two doses					
	(1)	(2)	(3)	(4)	(5)
Share of eligible people	0.299*	-0.206	-0.166	-0.077	-0.080
in neighboring municipalities	(0.153)	(0.278)	(0.275)	(0.185)	(0.165)
R-squared	0.059	0.328	0.381	0.646	0.701
Avg. dependent variable	49.86	49.86	49.86	48.58	48.58
Province fixed effects		X	X	X	X
Basic controls			X	X	X
Unbalanced covariates				X	X
2020 Plebiscite controls					X
Observations	340	340	340	323	323

Notes: The share of eligible people in neighboring municipalities is computed as the population-weighted mean of the share of target population in neighboring municipalities. The share of target population in each municipality is computed as the sum of population working in health services, transportation, education, and public administration, population with chronic diseases, and population older than 50 years old; all as shares of adult population. The basic set of controls includes distance to national capital (in logs), distance to regional capital (in logs) and two indicators of population size (i.e., less than 50 thousand inhabitants and between 50 thousands and 100 thousands inhabitants). The set of unbalanced covariates includes turnout in 2017 presidential election, labor participation rate, share of women in population, labor participation rate of women, unemployment rate of women, prevalence of permanent health conditions, average household subsidy (in logs), total covid deaths per 10,000 inhabitants (in logs), and number of vaccination centers per 100,000 inhabitants. All covid figures are measured until first day of the vaccination campaign (December 23, 2020). 2020 Plebiscite controls include turnout and vote share for approval. Regressions are weighted by voting age population. Robust standard errors in parenthesis. Statistical significance: *** p <0.01, ** p <0.05, * p <0.1.

Table A.7: Spatial spillovers of political impacts

Dependent variable measured in neighboring municipalities:				
	Turnout		Vote share incumbent mayor	
	(1)	(2)	(3)	(4)
Share of adults with two doses	-0.123	-0.112	-0.125	-0.118
	(0.103)	(0.071)	(0.256)	(0.273)
Observations	323	323	323	323
Avg. dependent variable	43.39	43.39	37.9	37.9
First-stage F-statistic	48.1	53.4	53.3	48.1
Full set of controls	X	X	X	X
Lagged dep. variable (neighbors)		X		X

Notes: The share of target population in each municipality is computed as the sum of population working in health services, transportation, education, and public administration, population with chronic diseases, and population older than 50 years old; all as shares of adult population. The full set of controls includes distance to national capital (in logs), distance to regional capital (in logs) and two indicators of population size (i.e., less than 50 thousand inhabitants and between 50 thousands and 100 thousands inhabitants), turnout in 2017 presidential election, labor participation rate, share of women in population, labor participation rate of women, unemployment rate of women, prevalence of permanent health conditions, average household subsidy (in logs), total covid deaths per 10,000 inhabitants (in logs), and number of vaccination centers per 100,000 inhabitants, and the 2020 Plebiscite controls. All covid figures are measured until first day of the vaccination campaign (December 23, 2020). Regressions are weighted by voting age population. Robust standard errors in parenthesis. Statistical significance: *** p <0.01, ** p <0.05, * p <0.1.

Table A.8: Characterization of compliers I

	Treated	Untreated	Full sample
	(1)	(2)	(3)
Turnout 2020 Plebiscite	46.57	47.36	51.37
Turnout 2017 Presidential Election	39.28	45.53	44.57
Turnout 2016 Local Election	40.14	37.49	34.27
Turnout 2013 Presidential Election	39.28	45.53	44.57
Turnout 2012 Local Election	49.98	46.66	42.65
Supports new constitution 2020	78.64	74.88	78.00
Supports convention 2020	74.63	71.58	74.77
Vote share right-wing 2017	42.64	47.80	44.01
Vote share right-wing 2016	36.35	32.78	38.20
Vote share right-wing 2013	19.49	22.42	24.27
Vote share right-wing 2012	31.63	31.41	35.89
Vote share left-wing 2017	57.36	52.20	55.99
Vote share left-wing 2016	40.99	56.94	40.74
Vote share left-wing 2013	67.92	64.70	63.09
Vote share left-wing 2012	57.19	49.66	46.67
Vote share Independent 2016	13.36	5.52	15.60
Vote share Independent 2012	7.77	14.10	12.09

Notes: This table presents an empirical characterization of the complier municipalities. See Abadie et al. (2002) for details. The treatment in this exercise is an indicator that takes the value one if the share of adults with two doses is above the median of the empirical distribution.

Table A.9: Characterization of compliers II

	Treated	Untreated	Full sample
	(1)	(2)	(3)
Census			
Population Women	51.10	51.16	51.08
Population 0 to 4 yrs old	6.08	7.31	6.63
Population 5 to 12yrs old	10.45	11.79	10.73
Population 12 to 18 yrs old	9.64	10.15	9.50
Labor Participation Rate	56.63	63.63	62.88
Labor Participation Rate, women	44.67	51.52	51.93
Unemployment Rate	8.30	7.65	7.19
Unemployment Rate, women 2017	12.64	11.37	10.13
Poor Household Rate (extensive)	7.39	6.44	6.13
Poor Household Rate (intensive)	1.62	1.42	1.35
Rural Population	0.20	0.17	0.12
Population with Primary Education	0.31	0.29	0.24
Population with Secondary Education	0.39	0.36	0.37
Population with Tertiary Education	0.14	0.19	0.23
Survey			
Log household income	12.42	12.54	12.72
Poverty Rate	11.47	12.69	8.51
Poverty Rate, multidimensional	22.90	21.41	21.12
Self-reported health score	18.91	18.67	17.28
Permanent health condition	13.43	11.74	11.38
Malnutrition	8.43	6.77	6.64
Lack of health insurance	5.52	6.55	6.28
Lack of social security	35.50	34.17	34.73
Lack of basic services	7.12	6.84	6.67
Log household subsidy	9.57	9.31	9.15
Pandemic			
Share of lockdown days	10.83	12.39	15.42
COVID infections per 10,000	323.41	301.01	341.61
COVID deaths per 10,000	8.06	4.51	8.81
Vaccination centers per 100,000	12.06	2.09	7.10
2019 protests			
Protests per 100,000	0.06	0.03	0.05
Indicator protest	0.64	0.57	0.85
Indicator riot	0.66	0.54	0.82

Notes: This table presents an empirical characterization of the complier municipalities. See Abadie et al. (2002) for details. The treatment in this exercise is an indicator that takes the value one if the share of adults with two doses is above the median of the empirical distribution.

Table A.10: Partisanship in local councilors elections

	Vote Share for			
	Incumbent	Left-Wing	Right-Wing	Independent
Panel A: Instrumental variables	(1)	(2)	(3)	(4)
Share of adults with two doses	-0.116	-0.081	0.110	0.084
	(0.150)	(0.203)	(0.142)	(0.155)
	[0.318]	[0.706]	[0.333]	[0.192]
First-Stage Statistic	47.96	51.96	51.09	52.21
Panel B: Reduced Form				
Share of people in priority groups	-0.076	-0.054	0.073	0.022
	(0.111)	(0.153)	(0.106)	(0.146)
	[0.308]	[0.704]	[0.351]	[0.794]
R-squared	0.483	0.767	0.887	0.449
Panel C: OLS				
Share of adults with two doses	-0.010	-0.026	0.092	-0.042
	(0.085)	(0.120)	(0.072)	(0.098)
	[0.848]	[0.618]	[0.020]	[0.390]
R-squared	0.482	0.767	0.888	0.449
Mean of dep variable	17.44	56.36	35.05	33.95
Std deviation of dep variable	8.38	11.99	12.54	10.96

Notes: The unit of observation in Panels A, B, and C is a municipality. The number of observations in Panels A, B, and C is 324. Regressions are weighted by voting age population. Robust standard errors in parenthesis. P-values from standard errors adjusted for spatial autocorrelation in brackets (Conley, 1999). Statistical significance: *** p <0.01, ** p <0.05, * p <0.1. First-stage statistic reports the Kleibergen-Paap rk Wald F statistic.

Table A.11: Number of competitors per election

	Dep. variable: Competitors per 1,000 voters			
	Mayors	Constitution	Governors	Councilors
Panel A: Instrumental variables	(1)	(2)	(3)	(4)
Share of adults with two doses	0.002	0.065**	0.005**	0.019
	(0.002)	(0.029)	(0.002)	(0.012)
Panel B: Reduced Form				
Share of eligible people	0.002	0.044**	0.004**	0.013
	(0.001)	(0.021)	(0.002)	(0.008)
Panel C: OLS				
Share of adults with two doses	-0.001	0.003	-0.000	0.003
	(0.001)	(0.013)	(0.001)	(0.013)
Observations	324	324	324	324
Province fixed effects	X	X	X	X
Full set of controls	X	X	X	X
Avg. dependent variable	0.3	4.7	0.4	2.2

Notes: Additional empirical analysis which was not pre-specified (n.p.s). See Table 2 for the description of the full set of controls. All regressions are weighted by voting age population. Robust standard errors in parenthesis. Statistical significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.12: Number of competitors in local elections by coalition

	Dep. variable: Competitors per 1,000 voters			
	Total	Left-wing	Right-wing	Independent
Panel A: Instrumental variables	(1)	(2)	(3)	(4)
Share of adults with two doses	0.002	0.002***	0.001**	-0.001
	(0.002)	(0.001)	(0.000)	(0.001)
Panel B: Reduced Form				
Share of eligible people	0.002	0.002***	0.001*	-0.000
	(0.001)	(0.000)	(0.000)	(0.001)
Panel C: OLS				
Share of adults with two doses	-0.001	0.000	0.000	-0.001**
	(0.001)	(0.000)	(0.000)	(0.001)
Observations	324	324	324	324
Province fixed effects	X	X	X	X
Full set of controls	X	X	X	X
Avg. dependent variable	0.259	0.087	0.066	0.106

Notes: Additional empirical analysis which was not pre-specified (n.p.s). See Table 2 for the description of the full set of controls. All regressions are weighted by voting age population. Robust standard errors in parenthesis. Statistical significance: *** p <0.01, ** p <0.05, * p <0.1.

Table A.13: Partisanship in governors election

	Vote Share for		
	Left-Wing	Right-Wing	Independent
Panel A: Instrumental variables	(1)	(2)	(3)
Share of adults with two doses	-0.091	0.193***	-0.038
	(0.121)	(0.070)	(0.091)
	[0.486]	[0.067]	[0.619]
First-Stage Statistic	49.21	49.21	49.21
Panel B: Reduced Form			
Share of people in priority groups	-0.060	0.128**	-0.025
	(0.089)	(0.051)	(0.068)
	[0.497]	[0.090]	[0.623]
R-squared	0.949	0.940	0.954
Panel C: OLS			
Share of adults with two doses	-0.001	0.115***	-0.058
	(0.061)	(0.043)	(0.047)
	[0.993]	[0.059]	[0.103]
R-squared	0.949	0.941	0.954
Mean of dep variable	46.54	23.21	19.07
Std deviation of dep variable	15.96	9.94	14.00

Notes: The unit of observation in Panels A, B, and C is a municipality. The number of observations in Panels A, B, and C is 324. Robust standard errors in parenthesis. P-values from standard errors adjusted for spatial autocorrelation in brackets (Conley, 1999). Statistical significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. First-stage statistic reports the Kleibergen-Paap rk Wald F statistic.

Table A.14: Robustness to measures of incumbent performance

	Low-performing municipalities	High-performing municipalities
	(1)	(2)
Panel A: Municipal income		
Share of adults with two doses	-3.302***	-2.405**
	(1.194)	(1.042)
Observations	155	169
Province fixed effects	X	X
Full set of controls	X	X
Panel B: Standardized test scores		
Share of adults with two doses	-2.257*	-0.999*
	(1.285)	(0.537)
Observations	174	150
Province fixed effects	X	X
Full set of controls	X	X

Notes: This table reports instrumental-variables estimates of the effect of vaccination rates on incumbent vote share, allowing for heterogeneity by alternative measures of incumbent performance. In each panel, municipalities are classified as low- or high-performing based on a median split of the corresponding performance measure. Panel A uses a measure of municipal income per capita that excludes transfers from the central government and therefore reflects own-source revenues that depend more directly on mayoral effort and local revenue management. Panel B uses changes in standardized test scores (SIMCE) in public schools between mayoral terms as a salient indicator of municipal performance. All regressions are weighted by adult population and include province fixed effects and the full set of controls described in Table 2. Robust standard errors are reported in parentheses. Statistical significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.15: Summary statistics mental health measures

Variable		Ordered measure				Indicator measure				
		Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max	Observations
Feel overwhelmed	overall	3.28	0.83	1	4	0.83	0.38	0	1	N = 5783
	between		0.64	1	4		0.28	0	1	n = 2184
	within		0.59	1.03	5.28		0.29	0.08	1.58	T-bar = 2.65
Not able to focus	overall	2.38	0.71	1	4	0.40	0.49	0	1	N = 5783
	between		0.55	1	4		0.39	0	1	n = 2184
	within		0.51	0.38	4.38		0.33	-0.35	1.15	T-bar = 2.65
Sleeping problems	overall	3.26	0.83	1	4	0.84	0.37	0	1	N = 5783
	between		0.64	1	4		0.27	0	1	n = 2184
	within		0.60	1.01	5.51		0.28	0.09	1.59	T-bar = 2.65
Cannot enjoy	overall	2.45	0.80	1	4	0.45	0.50	0	1	N = 5783
	between		0.61	1	4		0.38	0	1	n = 2184
	within		0.58	0.45	4.70		0.36	-0.30	1.20	T-bar = 2.65
Feel depressed	overall	2.99	0.89	1	4	0.74	0.44	0	1	N = 5783
	between		0.67	1	4		0.32	0	1	n = 2184
	within		0.65	0.74	4.99		0.34	-0.01	1.49	T-bar = 2.65
Worse mood	overall	3.33	0.90	1	5	0.85	0.35	0	1	N = 5783
pre-pandemic	between		0.70	1	5		0.26	0	1	n = 2184
	within		0.62	0.58	6.08		0.26	0.10	1.60	T-bar = 2.65

Notes: Additional empirical analysis which was not pre-specified (n.p.s). Sample consists in 4 waves of survey implemented in July 2020, November 2021, March 2022, and August 2022. All dependent variables are ordered variables increasing in levels of mental or psychological distress. Original questions allows a 4-scale answer: much less than usual, less than usual, the same as usual, more than usual. “Feel Overwhelmed” asks “Have you been feeling constantly overwhelmed and tense?”; “Feel not Able to Focus” asks “Have you been able to focus on what you’re doing?”; “Sleeping Problems” asks “Have your worries caused you to lose a lot of sleep?”; “Cannot Enjoy” asks “Have you been able to enjoy your normal daily activities?”; “Depressed” asks “Have you been feeling unhappy and depressed?”; “Worse Mood Pre-Pandemia” asks “In comparison to your mood prior to the Corona Virus pandemic, how have you been feeling?”.

Table A.16: Vaccines and anxiety in high-frequency surveys (n.p.s)

Dependent variable:	Concern about Covid			Optimism about country	
	Indicator worried	Indicator very worried	Ordered	Indicator optimistic	Ordered
Panel A: OLS	(1)	(2)	(3)	(4)	(5)
Fully vaccinated	0.019*	0.013	0.095***	0.028***	0.082***
	(0.009)	(0.008)	(0.029)	(0.009)	(0.020)
Panel B: Reduced form					
Eligible for vaccine	-0.042***	-0.035***	-0.126***	0.049***	0.129***
	(0.008)	(0.009)	(0.023)	(0.010)	(0.022)
Panel C: IV					
Fully vaccinated	-0.063***	-0.052***	-0.188***	0.073***	0.192***
	(0.013)	(0.013)	(0.035)	(0.015)	(0.031)
First-stage statistic	1335	1335	1335	1297	1297
Avg. dependent variable	0.58	0.36	3.58	0.40	3.1
Age fixed effects	X	X	X	X	X
Individual controls	X	X	X	X	X
Covid control	X	X	X	X	X
Observations	22,269	22,269	22,269	22,116	22,116

Notes: Additional empirical analysis which was not pre-specified (n.p.s). Sample consists in 32 waves of survey implemented from February to September 2021. Fully vaccinated takes value 1 if individual declares having at least two doses of the vaccine. Eligible takes value 1 if individual's age is such that individual is eligible for the second dose of the vaccine at the time of the survey. Concern about covid is based on the question "how worried are you about contracting covid?" and follows a 5-point scale taking value of 1 (none), 2 (a little), 3 (some), 4 (quite a lot) and 5 (a lot). The variable worried takes value of 1 if concern is above 3, 0 otherwise. The variable very worried takes value of 1 if concern takes value of 5, 0 otherwise. Optimism about the country is based on the question "how do you feel about the future of your country?" and follows a 5-point scale taking value of 1 (very pessimistic), 2 (pessimistic), 3 (neither pessimistic nor optimistic), 4 (optimistic) and 5 (very optimistic). The variable optimistic takes value of 1 if optimism about the country is above 3, 0 otherwise. Individual controls are a gender dummy and 9 education dummies. Covid control represents a two-weeks average of daily COVID infections per 10,000 at the regional level. Robust standard errors clustered at the wave-region level in parenthesis. Regressions are weighted using sampling weights. Statistical significance: *** p <0.01, ** p <0.05, * p <0.1.

Table A.17: Vaccine-eligibility and mental health in surveys (n.p.s)

	Feel overwhelmed	Feel not able to focus	Sleeping problems	Cannot enjoy	Feel depressed	Worse mood pre-covid
Panel A	(1)	(2)	(3)	(4)	(5)	(6)
Eligible for vaccine	-0.088***	-0.066***	-0.029	-0.101***	-0.047**	-0.101***
	(0.021)	(0.022)	(0.018)	(0.023)	(0.022)	(0.019)
Panel B: Individual fixed effects						
Eligible for vaccine	-0.083***	-0.106***	-0.020	-0.099***	-0.026	-0.093***
	(0.021)	(0.021)	(0.020)	(0.025)	(0.024)	(0.019)
Observations (panel A)	5,770	5,770	5,770	5,770	5,764	5,770
Observations (panel B)	5,242	5,242	5,242	5,242	5,242	5,237
Avg. dependent variable	0.83	0.40	0.84	0.45	0.74	0.85
Age fixed effects	X	X	X	X	X	X
Individual controls	X	X	X	X	X	X
Covid control	X	X	X	X	X	X

Notes: Additional empirical analysis which was not pre-specified (n.p.s). Sample consists in 4 waves of survey implemented in July 2020, November 2021, March 2022, and August 2022. Eligible for Vaccine takes value 1 if individual's age is such that individual is eligible for the second dose of the vaccine at the time of the survey. All dependent variables are dummies denoting high levels of mental or psychological distress. Original questions allows a 4-scale answer: much less than usual, less than usual, the same as usual, more than usual. "Feel Overwhelmed" asks "Have you been feeling constantly overwhelmed and tense?"; "Feel not Able to Focus" asks "Have you been able to focus on what you're doing?"; "Sleeping Problems" asks "Have your worries caused you to lose a lot of sleep?"; "Cannot Enjoy" asks "Have you been able to enjoy your normal daily activities?"; "Depressed" asks "Have you been feeling unhappy and depressed?"; "Worse Mood Pre-Pandemia" asks "In comparison to your mood prior to the Corona Virus pandemic, how have you been feeling?". Individual controls are a gender dummy and 8 education dummies. Covid control represents a two-weeks average of daily COVID infections per 10,000 at the regional level. Robust standard errors clustered at the wave-region level in parenthesis. Regressions are weighted using sampling weights. Statistical significance: *** p <0.01, ** p <0.05, * p <0.1.

Table A.18: Eligibility and mental health in surveys, ordered responses (n.p.s)

	Feel overwhelmed	Feel not able to focus	Sleeping problems	Cannot enjoy	Feel depressed	Worse mood pre-covid
Panel A	(1)	(2)	(3)	(4)	(5)	(6)
Eligible for vaccine	-0.158***	-0.081***	-0.078*	-0.211***	-0.129***	-0.353***
	(0.043)	(0.031)	(0.040)	(0.034)	(0.042)	(0.042)
Panel B: Individual fixed effect						
Eligible for vaccine	-0.173***	-0.121***	-0.088**	-0.198***	-0.110***	-0.358***
	(0.042)	(0.033)	(0.041)	(0.036)	(0.043)	(0.045)
Observations (panel A)	5,770	5,770	5,770	5,770	5,764	5,770
Observations (panel B)	5,242	5,242	5,242	5,242	5,242	5,237
Avg. dependent variable	3.28	2.38	3.26	2.45	2.99	3.33
Age fixed effects	X	X	X	X	X	X
Individual controls	X	X	X	X	X	X
Covid control	X	X	X	X	X	X

Notes: Additional empirical analysis which was not pre-specified (n.p.s). Sample consists in 4 waves of survey implemented in July 2020, November 2021, March 2022, and August 2022. Eligible for Vaccine takes value 1 if individual's age is such that individual is eligible for the second dose of the vaccine at the time of the survey. All dependent variables are ordered variables increasing in levels of mental or psychological distress. Original questions allows a 4-scale answer: much less than usual, less than usual, the same as usual, more than usual. "Feel Overwhelmed" asks "Have you been feeling constantly overwhelmed and tense?"; "Feel not Able to Focus" asks "Have you been able to focus on what you're doing?"; "Sleeping Problems" asks "Have your worries caused you to lose a lot of sleep?"; "Cannot Enjoy" asks "Have you been able to enjoy your normal daily activities?"; "Depressed" asks "Have you been feeling unhappy and depressed?"; "Worse Mood Pre-Pandemia" asks "In comparison to your mood prior to the Corona Virus pandemic, how have you been feeling?". Individual controls are a gender dummy and 8 education dummies. Covid control represents a two-weeks average of daily COVID infections per 10,000 at the regional level. Robust standard errors clustered at the wave-region level in parenthesis. Regressions are weighted using sampling weights. Statistical significance: *** p <0.01, ** p <0.05, * p <0.1.

Table A.19: Booster and electoral outcomes in November 2021 (n.p.s.)

		Presidential Election			Congress Election	
	Share of adults with three doses	Turnout	Deviation from political center	Right-wing vote share	Turnout	Vote share incumbents
	(1)	(2)	(3)	(4)	(5)	(6)
Share of eligible people	0.218***					
	(0.068)					
Share of adults with *three* doses		0.117**	-0.004	-0.227*	0.120*	-0.005
		(0.059)	(0.004)	(0.117)	(0.066)	(0.004)
Observations	318	318	318	318	318	318
Province fixed effects	X	X	X	X	X	X
Full set of controls	X	X	X	X	X	X
Avg. dependent variable	47.01	46.90	0.58	53.08	42.46	27.75
St. dev. dependent variable	11.48	5.68	0.44	10.35	6.09	14.15
First-stage F-statistic	–	10.2	10.2	10.2	10.2	10.2

Notes: Additional empirical analysis which was not pre-specified (n.p.s). The share of eligible people is computed following the eligibility rules up to the week of the Presidential and Congress Election (November 15-19, 2021). See Table 2 for the description of the full set of controls. All regressions are weighted by voting age population. Robust standard errors in parenthesis. Statistical significance: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.20: Blank and null votes per election

	Dep. variable: % of invalid (blank/null) votes			
	Mayors	Constitution	Governors	Councilors
Panel A: Instrumental variables	(1)	(2)	(3)	(4)
Share of adults with two doses	-0.005	0.004	-0.013	-0.024**
	(0.007)	(0.024)	(0.024)	(0.011)
Panel B: Reduced Form				
Share of eligible people	-0.003	0.003	-0.009	-0.017**
	(0.005)	(0.019)	(0.019)	(0.008)
Panel C: OLS				
Share of adults with two doses	0.002	-0.016	-0.038***	-0.014***
	(0.004)	(0.012)	(0.014)	(0.005)
Observations	324	324	324	324
Province fixed effects	X	X	X	X
Full set of controls	X	X	X	X
Avg. dependent variable	1.0	6.3	5.5	2.3

Notes: Additional empirical analysis which was not pre-specified (n.p.s). See Table 2 for the description of the full set of controls. All regressions are weighted by voting age population. Robust standard errors in parenthesis. Statistical significance: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.