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 empower challengers irrespective of their party affiliation. We provide suggestive evidence of mental health being a mechanism behind the lower preferences for the status quo.

Keywords: vaccines, politics, election, mental health.

*This version: May 2022. The date of the election under study took place during May 15-16 of 2021. We wrote a comprehensive pre-analysis plan for all of the empirical analysis in the paper and we posted it online in May 14 of 2021 in the Open Science Framework website: https://osf.io/ynxbc/.

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1 Introduction

Vaccines allow the control of diseases, increase life expectancy, and are thus 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 disturb the implementation of welfare-improving policies such as vaccines (Besley and Case, 1995; Lizzeri and Persico, 2001), 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 has caused millions of deaths, depressed the economy (Chetty et al., 2020), and activated ambitious economic policies across de world (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.

This paper provides the first causal evidence of the impact of a large vaccination campaign on electoral outcomes. Vaccines can increase turnout by decreasing the cost of voting, but they can also increase the vote share of incumbents if their deployment is perceived as signal of competence. Alternatively, a large vaccination campaign can lead to a backlash effect for incumbents through changes in risk assessment which affect relative perceptions of the political status quo (Bisbee and Honig, 2021). We test these hypotheses in Chile, a country 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 eligibility rules which we show are exogenous to the pre-pandemic political equilibrium, prevailing economic conditions, and pandemic severity. 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 taking place 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 electoral outcomes.

The foundation of our research design is based on two features. First, we overcome challenges related to cherry picking and p-hacking in statistical analysis of observational data (Christensen and Miguel, 2018) by writing a comprehensive pre-analysis plan which we posted online before the elections took place. The plan offered a detailed description of the empirical analysis, which we then 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 and also priority occupations. The pre-analysis plan provided evidence for the differential local exposure to the vaccination campaign being plausible exogenous by estimating the correlation between local eligibility, electoral outcomes in the period 2012-2020, local economic conditions, and variables related to pandemic severity. We now use these plausibly exogenous differences in an instrumental variables framework.

We find that exogenously higher vaccination rates boosted local political participation in all of the elections we study. In particular, we estimate that an increase of one standard deviation in local vaccination rates (14 percentage points) is causally associated with and increase in political participation of 2.4 percentage points over a sample average of 48%. To put the magnitude in perspective, we show it to be similar to the impact of infections but with the reversed sign. In contrast, we find little evidence of partisan effects as exogenous differences in vaccination rates are unrelated to vote shares for left-wing, right-wing, or independent candidates. Importantly, we do find that higher vaccination rates are causally associated with fewer votes for incumbents. The change in preferences for the status quo are large as the the vote share of incumbents decreased by more than 10 pp with a 10 pp higher vaccination rate. Overall, the political consequences of vaccines are in line with an existing literature which documents how voters resort to the status quo (i.e. incumbents) during times of crisis and higher anxiety (Bisbee and Honig, 2021). Vaccines have the potential to reduce anxiety and thus lower the vote share of incumbents.

Following the pre-analysis plan, we implemented additional econometric exercises to support and interpret our findings. We begin by showing that the statistical inference is robust to the use of alternative spatially correlated errors and randomization procedures. We also use the method proposed by Abadie et al. (2002) to characterize compliers and exploit the context to trace out variation in Local Average Treatment Effects using all possible subsets of variables behind the eligibility rules. The latter exercises reveal that estimates appear to be fairly generalizable and causal effects are similar across different complier populations with perhaps larger impacts on people who presumably benefitted the most from vaccines (e.g. health personnel). Finally, non-prespecified results using the same empirical framework suggest that spatial spillovers are minimal.

To make progress on explanations for our findings, we rely on existing research documenting the negative impacts of the pandemic on mental health and psychological well-being (Holman et al., 2020; Brülhart et al., 2021). We hypothesize that deteriorated mental health problems related to anxiety pushed citizens to the status quo because of the certainties surrounding what is already known. In that sense, vaccines have the potential to *decrease* anxiety and other mental health issues and thus push citizens back to their usual relative evaluation of incumbents and challengers. We provide three pieces of evidence consistent with this explanation.[1] First, weekly representative surveys conducted by a private company reveal that vaccines are causally associated with lower levels of self-reported anxiety. Second, using administrative data we document a negative empirical relation between local vaccination rates and sick leaves from work due to mental health and behavioral disorders. And third, we exploit eligibility rules for the vaccine boosters before the Presidential and Congress Election of November 2021 and report similar but attenuated political impacts. Given that the pandemic was less severe, and the booster had similar effectiveness (UK Health Security Agency, 2022), these results highlight the importance of the scale of the crisis.

Our main contribution is to provide novel causal estimates of the impact of a large vaccination 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 (Frey et al., 2020; Cam Kavakli, 2020) can 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. The reason for the lack of evidence is presumably related to the endogeneity of public health measures which researchers have been 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 isolate the pervasive political factors which usually affect vaccination campaigns.

Related empirical studies have documented how the prevalence of a disease decreases political participation and changes vote shares (Urbatsch, 2017; Mansour et al., 2020; Campante et al., 2020; Scheller, 2021; Morris and Miller, 2021; Gutiérrez et al., 2022) and the political impact of other large public health policies (Haselswerdt, 2017; Clinton and Sances, 2018; Baicker and Finkelstein, 2019; Bol et al., 2021). Understanding the political effects of vaccines is important

[1] Importantly, none of these results were part of the pre-analysis plan. We highlight the difficulties in testing for mechanisms in the absence of knowledge about the primary set of results. 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.

because it reveals additional information about the electoral motivation of incumbent politicians to efficiently deploy immunization campaigns. In this regard, our findings are consistent with Bisbee and Honig (2021) who show that the prevalence of a disease increases preferences for the status quo. We find that higher vaccination rates increases the preference for challengers which, based on empirical evidence, we interpret as a consequence of decreased anxiety in times of crisis.

Finally, our paper also relates to research studying the factors affecting the compliance of the population to public health measures in general and vaccination campaigns in particular. Previous research has studied the role of information and historical factors in driving contemporary vaccination rates (e.g. Martínez-Bravo and Stegmann 2021; Lowes and Montero 2021). Researchers have also shown that individuals with a higher sense of civic duty are more likely to comply with public health measures (Barrios et al., 2021; Durante et al., 2021; Chen et al., 2021; Brodeur et al., 2021). Similarly, there is evidence of income, risk perceptions, and partisanship acting as mediating factors to explain differences in compliance rates (Allcott et al., 2020; Wright et al., 2020; Barrios and Hochberg, 2021). We contribute to this literature by documenting high compliance rates to a vaccination campaign with clear eligibility rules during a pandemic.

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 has deployed the immunization with clear eligibility rules since December 2020. The plan to roll-out the vaccines was crafted and implemented by the central government, leaving little room for local governments to affect this process.[2] Eligibility rules were based on verifiable measures such as age and occupation. Elder people and workers in certain occupations have gotten the vaccine first on a week-to-week rolling program that started with people older than 90 years old and health personnel. Moreover, information about these rules was extensively disseminated through online and traditional media (i.e. television, radio, and newspapers). Importantly, all the vaccination data has been made public in real time.

The second crucial characteristic is that Chile faces one of the most important elections in its

[2]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.

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, 2022). The referendum was held in October 2020 and 80% voted for a new Constitution. As a consequence of the vote, a new text is currently being drafted by a Constitutional Convention composed by 155 members elected by a D'Hondt method. The members of the Convention were selected the same day than 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 have been in place in all elections since 2012. The combination of a high-stakes election, voluntary voting, and a massive vaccination process halfway implemented by election day constitute an ideal empirical setting.[3]

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 all still high (panels A, B, and C in Figure 1). 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 most important and particularly relevant given that people associate local governments with most local policies affecting their daily lives. Two ballots were tied to the Local Election, one to choose the mayor and another one to choose the members of the local council. 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.[4]

3 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

[3]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.

[4]This was the first time in the country's history in which people democratically elected regional governors. There was scarce information about their practical role, so we think of this election as relatively low stakes.

compared to randomized controlled trials.[5] 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.

3.1 Local exposure based on eligibility rules

Our econometric design combines four different data sources to track eligibility rules, vaccine deployment, and electoral results across municipalities. First, individual-level data from the 2017 Census with the municipality of residence and age, gender, occupation, labor force participation, and unemployment status. Second, administrative electoral data from the Electoral Service including municipality-level participation and vote shares from 2012. Third, administrative data from the Ministry of Health with municipality-level information on the number of people vaccinated by week. the number of deaths and infections related to the pandemic, and the full list of vaccination centers with their geographic location. And fourth, data from a nationally representative survey of approximately 270,000 individuals in 324 municipalities in 2017 known as CASEN survey.

We are interested in estimating the causal impact of vaccination on electoral outcomes, i.e. participation in the election and the corresponding political preferences for candidates, parties, and coalitions. We observe vaccination rates and electoral outcomes at the municipality level. Then we can write the relationship of interest as the following cross-sectional regression equation:

$$Y_{cp} \quad = \quad \beta V_c + \gamma X_c + \phi_p + \epsilon_{cp} \tag{1}$$

where Y_{cp} is an electoral outcome in municipality c, located in province p. Chile is divided in 346 municipalities, each located in one of 56 provinces. We use 343 municipalities in 54 provinces because one municipality lacks political data (Antarctica) and province fixed effects ϕ_p absorb all variation in two others (Cape Horn and Easter Island).[6] The right-hand side variable of interest

[5]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).

[6]When analyzing vote shares we aggregated political parties into political coalitions. We created the mapping from

is V_c which we 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 of the National Statistics Institute.[7] We also include a set of predetermined (and pre-specified) covariates X_c to improve precision and control for municipality characteristics that correlate with the instrument. We use a mean zero error term ϵ_{cp} that we allow to be robust to heteroskedasticity or spatially autocorrelated. Finally, given that electoral outcomes arise from individual-level decisions, we estimate equation (1) using weighted least squares with the local adult population as weight.

A leading concern with a naïve estimation of equation (1) is omitted variables which can explain both the vaccination rates and electoral outcomes.[8] In order to estimate the causal effect of vaccines, we employ a two-stage least squares strategy using as instruments $Z_c = \{z_{1c}, \ldots, z_{Jc}\}$ the eligibility rules. Given that country-wide vaccination process take months (or even years) to reach a large fraction of the population, the central government released a roll out plan shortly before the first vaccines arrived. The first pillar of the plan stated that older people and those with a chronic condition get a vaccine first. By the time of the election, all Chileans and foreign residents of 48 years old or older had been eligible for two doses. The second pillar states that workers in certain "critical" occupations could also get the vaccine.[9] The existence of these eligibility rules allows us to construct the share of the local population that was offered a vaccine before the election (Z_c).[10] Note that we constructed the instrument Z_c *before* the election took place.

Panel (a) in Figure 2 presents some of the identifying variation visually. The vaccination plan mandated that the week before the election all 48 year old individuals were eligible to be fully vaccinated. As a consequence, we observe an 18 percentage point increase in vaccination rates from 47 (40%) to 48 yr old people (58%). Note that people younger than 48 yr old could still have been vaccinated if they worked in priority occupations or suffered from a chronic disease.

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political parties to left- and right-wing coalitions in the pre-analysis plan. Appendix A presents this classification.

[7]By te time of the election all eligible people in the country were offered a vaccine with a two doses scheme (Sinovac or Pfizer). Immunity is reached two weeks after receiving the second dose.

[8]One example is education, presumably associated with vaccination and electoral participation. However, there are potentially many omitted variables and even the bias in β is difficult to bound or to put a sign on.

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

[10]We identified people with a chronic condition using administrative data from the annual vaccination campaign related to the influenza disease. In terms of occupations, we are restricted by the categories in the 2017 Census and we use the following: health personnel, public transportation, education, and public workers.

The pre-analysis plan proposed five specifications of equation (1): (i) without province fixed effects ϕ_p and without controls X_c; (ii) including province fixed effects ϕ_p and without controls X_c; (iii) including ϕ_p and the basic controls $x_{1,c} \in X_c$ the log of the distance (in km.) from the municipality to the capital, the log of the distance (in km.) to the regional capital, one indicator for municipality with less than 50,000 inhabitants, and one indicator for those hosting between 50,000 and 100,000 people, all of which aim to capture basic predetermined differences in geographic location and size. (iv) Including ϕ_p, $x_{1,c}$, and the following extended controls $x_{2,c} \in X_c$ which we found to be correlated with the instrument: turnout in the 2017 presidential election, labor participation rate, share of women in population, labor participation and 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 (v) including ϕ_p, $x_{1,c}$, $x_{2,c}$ and the following controls from the 2020 plebiscite $x_{3,c} \in X_c$: turnout and vote share for the option in favor of a new constitution (i.e. "Approve" option). These controls aim to capture predetermined political differences across municipalities in a recent election also held during the pandemic. For specifications 1-3 we observe 343 municipalities. However, we only observe 324 when we use specifications 4-5 because two covariates come from the 2017 National Survey which is not implemented in some locations.

3.2 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).

Table 1 presents summary statistics for 17 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 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., 2021). In the appendix we also examine 14 additional variables from the 2017 Census, 10 variables from the 2017 National Survey, and four variables related to the COVID pandemic (Tables A.1-A.3). In sum, we estimated the correlation between the instrument and 46 variables covering elections, the labor market, health conditions, state subsidies, and the pandemic, and we observe 8 statistically significant differences at the 10% level. The number of differences is slightly above the 5 derived from a 10% statistical test ($0.10 \times 46 = 4.6$), which in this case was reasonable to expect as we explain below. Importantly, only one of the 17 political variables is correlated with the instrument at the 5% level, which is what we expected of a 5% statistical test ($0.05 \times 17 = 0.85$).

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.1-A.3). 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

[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 exposure to the vaccination campaign (see Appendix B). Unfortunately, we observe significantly more predetermined differences across treatment status (Table A.4). These differences imply that the treatment is unlikely to be exogenous and thus contaminates the interpretation of these results. Nevertheless, we present this "booth-level analysis" anyways for transparency.

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]

4 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.

4.1 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. 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

[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).

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 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.

4.2 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 to 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 use robust standard errors (in parentheses) but also follow Conley (1999) to adjust them for spatial autocorrelation within 50 kilometer (in square brackets). The latter method to calculate standard errors always delivers smaller confidence intervals and thus we only discuss results using the former to be conservative.

Instrumental variables estimates of equation (1) 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 terms of standardized effects, an exogenous increase of one standard deviation (σ) in vaccination rates (13.8 pp) caused political participation to increase by 2.4 pp. This is, approximately 5000 additional fully vaccinated individuals are causally associated with 500 additional votes at this election. The economic magnitude is relatively large when compared to the standard deviation (σ) in turnout across municipalities (0.27σ) but modest when compared to the average participation (48%). Columns 2-5 reveal that this number is similar when looking at each one of the four elections separately (all p-values<0.05). The statistical significance of these results is robust to the use of randomization inference (p-values<0.01, Figure A.1).[13]

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 (1) 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

[13]The booth-level analysis in Table A.5 shows little empirical relationship between distance to the closest vaccination venue and political participation, suggesting factors such as information, travel costs, or state presence were unlikely to be relevant for turnout decisions, or simply their impacts within municipalities offset each other.

[14]This empirical analysis was *not* part of the pre-analysis plan but we consider it to be helpful to compare the

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

$$Y_{ct} \quad = \quad \beta I_{ct} + \phi_c + \phi_t + \epsilon_{ct} \tag{2}$$

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).

4.3 Partisanship and incumbency

Did specific candidates (e.g. incumbents) or political parties (e.g. left-wing) benefit from the higher political participation derived from vaccines? Did vaccines have an effect on political preferences for different candidates? To answer these questions, we begin by studying vote shares at the local election. Columns 1-5 in Table 5 present the impact of vaccination rates on vote shares for incumbents and candidates from different coalitions. Instrumental variables estimates in column 1 reveal a negative relationship between vaccination rates and votes for incumbent mayors/parties. An exogenous increase of 10 pp in vaccination rates is associated with 20 pp fewer votes for the incumbent. This coefficient is also statistically significant when using spatial errors or randomiza-

magnitude of estimates to relatively more widespread estimates in the literature.

tion inference (see panel B in Figure A.1). This estimate becomes smaller, but it is still statistically significant, when looking at the subsample of mayors who decided to run for reelection (column 2), suggesting that voters punished incumbent political parties more than incumbent mayors.[15]

The fact that voters preferred challengers when vaccination was higher is related to a literature documenting an increase in preferences for the status quo during times of anxiety (Morgenstern and Zechmeister, 2001; Bisbee and Honig, 2021). Similarly, we should expect citizens to vote against the status quo (i.e. incumbent parties) once the anxiety is removed. As recent global evidence suggests, the deployment of vaccines can be interpreted as a public policy that successfully removes anxiety during times of crisis management. In particular, Fetzer et al. (2020) show that across the globe weak government response is associated with more worries and depression.

In contrast to the impact of vaccines on incumbents and challengers, columns 3-5 in Table 5 shows imprecisely estimated effects for political coalitions (left, right, independents). The point estimates suggest perhaps that some votes flowed from independent candidates to left-wing candidates but standard errors are large. The booth-level analysis again reveals similar patterns but, in terms of magnitude, with significantly smaller effects (Table A.10). In the Appendix we repeat these analyses but studying vote shares in the Councilors and Regional Governors election (Tables A.11, A.12). We find little evidence of partisan effects in these lower-stakes elections.

Columns 6-8 in Table 5 study partisan votes to elect the Constitutional Convention as dependent variable. Note that given this was an extraordinary election, there were no incumbent candidates.[16] This is also one of the most important elections in the country's history in which for the first time a group of individuals was democratically elected to write a new Constitution. Once again, instrumental variables estimates reveal little evidence of partisan effects derived from the vaccination process, with small and statistically insignificant coefficients. The booth-level analysis again shows impacts which are similar but small in magnitude. The point estimates show that people who lived closer from vaccination venues were more likely to vote for independent candidates, suggesting a positive impact of vaccines on outsider candidates (Table A.10).

Overall, Tables 3 and 5 show that exogenous increases in vaccination rates boosted local po-

[15] A new law enacted shortly before the pandemic outbreak prevented many incumbents to run for reelection as it established that incumbent mayors could only remain in power for a maximum of three periods (12 years).

[16] Those who had been elected by popular vote before the 2021 election could be considered incumbents. We looked for the 1,100 candidates in the group of all elected politicians since 1990 and found that more than 95% were never elected. Besides being a loose definition of incumbency, the context lacks enough variation to study this matter.

litical participation without benefitting specific political parties. Importantly, the impacts appear to be highly local as we fail to find evidence of spatial spillovers. Additionally, our analysis of incumbents show that higher vaccination rates favored the challengers and harmed those in power.

4.4 Local average treatment effects

The context offers a somewhat rare opportunity to trace out variation in the LATE by using all possible combinations of pre-determined variables behind the eligibility rules $Z_c = \{z_{1c}, \ldots, z_{Jc}\}$. In this case we have that $J = 6$ which we label as $j = A, B, C, D, E, F$. Therefore, we can construct $2^6 = 64$ different subsets of instruments. As mentioned in the pre-analysis plan, we focus on subsets with sufficiently strong first stage which we decided to be F-test>10 but the conclusion is similar with other cutoffs. Of course, this analysis requires a more stringent exclusion restriction, namely that each j is affecting the outcome only through changes in local vaccination rates.

Figure 3 presents results from this exercise. Panel (a) presents the LATEs of vaccines on political participation and panel (b) on the vote share of incumbents. For comparison, we highlight the benchmark estimates with a horizontal dashed line. Point estimates are labelled with the instruments employed for estimation and vertical gray lines represent 95 percent confidence intervals. These figures reveal economically meaningful variation in the impact of vaccines on political participation, with LATEs ranging from 0.02 to 0.40 (benchmark of 0.17). This is, a 10 pp increase in local vaccination rates is causally associated with a 0.2-4 pp increase in political participation. In the case of incumbent vote shares, the same LATEs vary from -1.0 to -2.5 (benchmark of -2.0). In addition, an examination of strong first-stages together with high and low LATEs shows that the people who increased their political participation and voted for the incumbent the most after getting the vaccine are health/education personnel and those with chronic conditions.

Overall, we derive three conclusions from the empirical variation in the LATEs. First, the positive impact of vaccines on political participation and incumbent vote shares are robust findings which do *not* depend on a single instrument. Second, individuals who presumably benefit the most from vaccines – e.g. health personnel working at public hospitals – are those who responded the most in terms of the political outcomes we examine. Finally, the use of a single variable combining six possible instruments does *not* deliver an unusually large or small LATE.

5 Vaccines and Mental Health

Can the lower preferences for the status quo derived from vaccines be explained by changes in the mental health of vaccinated individuals? An existing literature proposes that during times of crisis individuals resort to the certainty provided by the status quo (Bisbee and Honig, 2021). The status quo becomes attractive because anxiety can increase the level of risk aversion by increased difficulties in assessing probabilities of uncertain outcomes or simple due to a political endowment effect. This section presents three pieces of evidence consistent with anxiety playing an important role in explaining the increased participation and preference for incumbents arising from vaccines. For transparency, we highlight that none of the following results were part of the pre-analysis plan.

5.1 Anxiety in high-frequency surveys

To test for increased anxiety, 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 more than 150 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 allows means the eligibility rules were fixed within a survey. We use the surveys conducted in May (5-7), June (2-3), July (30-1), August (4-6), and September (1-3). Each survey was responded by more than 700 adults, for a total of almost 3,500 survey respondents.

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

$$y_{ij(i)} \quad = \quad \beta \text{ Eligible}_i + f(x_i) + \phi_{j(i)} + \eta_{ij(i)} \tag{3}$$

where y_{ij} is the response of person i who's age by the time of the survey is j. 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 "Eligible$_i$" takes the value of one if i was eligible for the second doses of the vaccine during the days in which the survey was conducted. Our preferred specification also includes non-parametric controls for gender and education $f(x_i)$. Crucially for the identification strategy, regression (3) includes a complete set of age fixed effects ϕ_j

which allows us to econometrically compare individuals of the exact same age but who answered the survey when they were and were not eligible for the vaccine. Finally, η_{ij} is an error term clustered by age to allow for arbitrary correlation within age cohorts.

Table 6 presents estimates of equation (3). Column 1 and 2 use as dependent variable an indicator for people who reported being *worried* about getting infected by the virus, and present results from specifications with and without controls $f(x_i)$. Both columns reveal than 57% of respondents were worried, a number which decreases to 50-51% among those who were eligible for the vaccine. Using the 70% take-up rate from Table 2, we can infer that being fully vaccinated decreases concerns about being infected by 9 percentage points (pp, i.e. 0.06/0.70) or 16% of the sample mean (0.09/0.57). Column 3 repeats the estimation but using an indicator for people who reported being *very worried* about getting infected, a little more than one-third of the sample. Being age-eligible decreased worriness by 5 pp, which using the first-stage leads to conclude that vaccines decreased anxiety by 7 pp or 19% of the sample mean (0.07/0.36). The last column uses the entire 1-5 response as dependent variable and an ordered probit to account for the ordinal nature of this measure. We again find evidence of decreased worriness derived from the vaccines.[17]

5.2 Mental health in administrative data

Are there economic consequences of vaccines which could be mediated by anxiety levels? To measure the link between mental health and labor outcomes, we use administrative data measuring sick leaves. When a worker in the formal Chilean labor market has a health problem which impedes their normal work activities, a physician can give them a sick leave to recover at home while still receiving a wage (with a cap). More than 550,000 sick leaves are given every month, with approximately one-third given because of mental and behavioral disorders (ICD-10 codes, F00-F99). Considering that 5.5 of the 8 million workers in the country belong to the formal labor market, sick leaves are sizable as they represent approximately 10% of the formal labor force. Moreover, sick leaves because of mental and behavioral disorders represent more than 3% of the labor force each month, approximately half of the non-pandemic unemployment rate. We hypothesize that vaccines improve mental health and thus decrease sick leaves due to mental disorders.

To estimate the impact of vaccines on sick leaves, we construct a panel dataset of municipalities

[17] These results are in line with contemporary research in the U.K. showing that vaccines boost psychological well-being by improving people's social life and enjoyment of daily activities (Bagues and Dimitrova, 2021).

observed each month during 2021, the year in which fully vaccinated individuals went from 0% to more than 93%. Econometrically, we exploit month-to-month variation using the following model:

$$\ell_{ct} = \beta V_{ct} + \phi_c + \phi_{c(t)} + \phi_t + \epsilon_{ct} \tag{4}$$

where ℓ_{ct} is the number of sick leaves per worker in municipality c in month t, V_{ct} is the cumulative share of the adult population who is fully vaccinated with two doses in month t, ϕ_c is a full set of municipality fixed effects, ϕ_t represent month fixed effects, and $\phi_{c(t)}$ are municipality-specific linear trends to absorb variation in the cumulative nature of V_{ct}. Note that we use sick leaves per worker instead of per people as these are only available for individuals in the labor force. We measure the local labor force using the 2017 census. The error term ϵ_{ct} is clustered by municipality, and we estimate the regression using weighted least squares with local population as weights.

Table A.13 reports estimates of equation (4). We find that a 10 percentage point (pp) increase in local vaccination rates from one month to the other – i.e. 5000 more people get the second doses of the vaccine – is associated with 0.0044 pp fewer sick leaves, a decrease of 0.6% from the monthly average ($0.0044 \times 10/7.06$), although this estimate is not statistically different from zero. In contrast, the association between vaccines and mental health is statistically different from zero (p-value<0.05). In particular, sick leaves due to mental health decrease by 0.0010 pp or 0.5% from the monthly average ($0.0010 \times 10/2.14$). Put differently, when 5000 more people get fully vaccinated locally in a month, we observe three fewer sick leaves due to mental disorders from a baseline of 470. However, recall that sick leaves are only available for workers in the *formal* labor market who on average correspond to 70% of the labor force. Extrapolating this relationship to the informal labor market, the reduction in sick leaves due to mental health increases to 4.3 (3/0.70).

5.3 Booster analysis

The May election we study took place when the pandemic was still causing many infections, lockdowns were common, the medium- and long-run impact of the disease were uncertain, and the second doses of the vaccine was seen as key for immunity and the return to normal. Anxiety is likely to play a large role under those conditions and, in that sense, our estimates could be interpreted as an upper bound. As such, we hypothesize that under less severe pandemic times, less uncertainty about the disease, and a more limited role for the vaccines, our main findings should be attenuated. To test for these ideas, we repeat the analysis but now in the Presidential and Congress Election of November 2021, six months after the May election we studied previously.

Table 7 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 7 show that vaccination rates are again causally associated with more political participation. However, the point estimate is 30% smaller than in May (0.12 versus 0.17). In terms of preference for the status quo, it is difficult to define who is the incumbent in the Presidential Election because the incumbent was not among the seven candidates. Therefore, we use two measures of incumbency. First, a measure of deviations from the political center derived from vote shares and an order of the seven candidates in a unidimensional left-right spectrum. Second, we use 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 left-right spectrum but some attenuated evidence for challengers when vaccination rates were higher (-0.23 versus 1.99). In all, we confirm the existence of attenuated impacts in the November election.

6 Conclusion

The causal impact of vaccination campaigns on the political sphere 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 empower outsiders by decreasing the votes of incumbents, irrespective of their party affiliation. We combined surveys, administrative data, and a replication analysis in another election to argue that psychological factors related to anxiety appear to be key to explain the increased preferences for the status quo.

The higher political participation derived from compliance with the vaccination campaign shows that it is possible to increase the legitimacy of political institutions in times of crisis using effective public policies. However, the magnitude of our estimates also constitute a cautionary tale as the public policy we study was one of the most effective in the world (Ritchie et al., 2022). As such, it is hard to imagine that political participation could increase more than a couple of percentage points with less effective vaccination processes or different policies during other crises given the large scale of disruption during the pandemic. Relatedly, the increased preferences for incumbents derived from higher vaccination rates suggests that electoral incentives could distort the implementation of this type of public policies. In the hands of incumbent politicians and upcoming elections, the deployment of vaccines could be slowed down to increase the likelihood of reelection. In that sense, our results point towards the appealing of centralized policy implementation with clear rules during times of crisis to cope with potentially distortionary policies.

Finally, three characteristics of this study are important to interpret its external validity. First, Chile is a relatively well-functioning democracy and the incumbent government invested a significant amount of public resources to organize the elections we study. Therefore, the impact of vaccines materialized under contextual factors which likely make our estimates an upper bound. Second, even though we exploited exogenous changes in vaccination rates to estimate the causal impact of vaccines, the entire country was affected by the vaccination campaign. This fact imposes a challenge to gauge the national contribution of vaccines to the electoral outcomes in the high-stakes election we study. Third, our preferred interpretation for the results is that vaccines successfully decreased the health cost associated to vote and decreased the power of incumbents through lower anxiety related to the disease. However, more work needs to be done in order to pin down the range of mechanisms through which vaccines can affect the political equilibrium.

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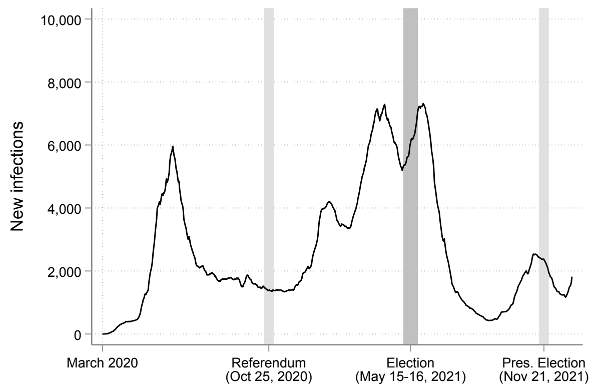
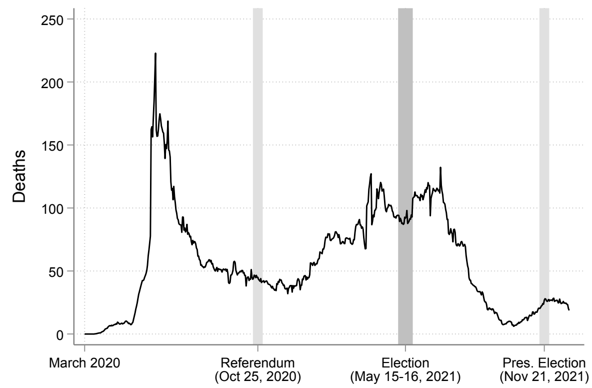
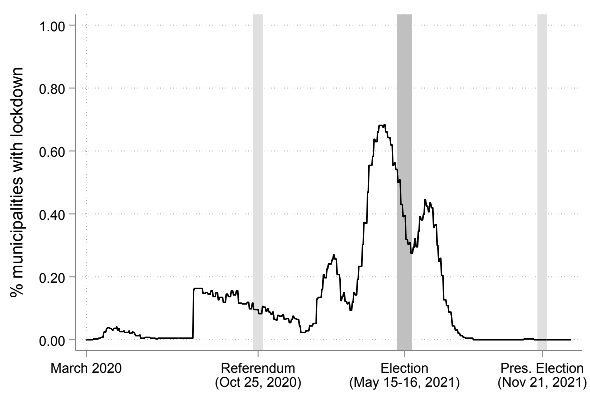
Figure 1: Pandemic and vaccination during study period



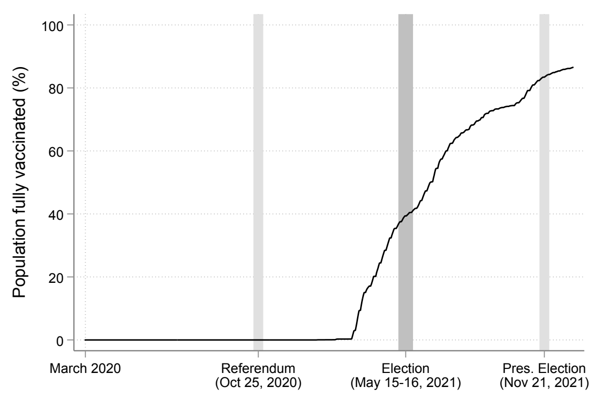
(a) Infections



(b) Deaths



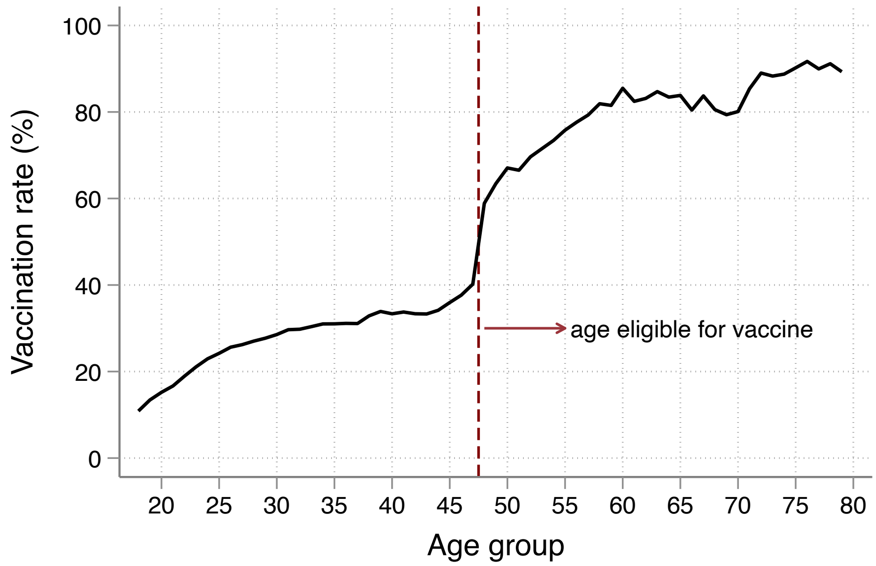
(c) Lockdowns



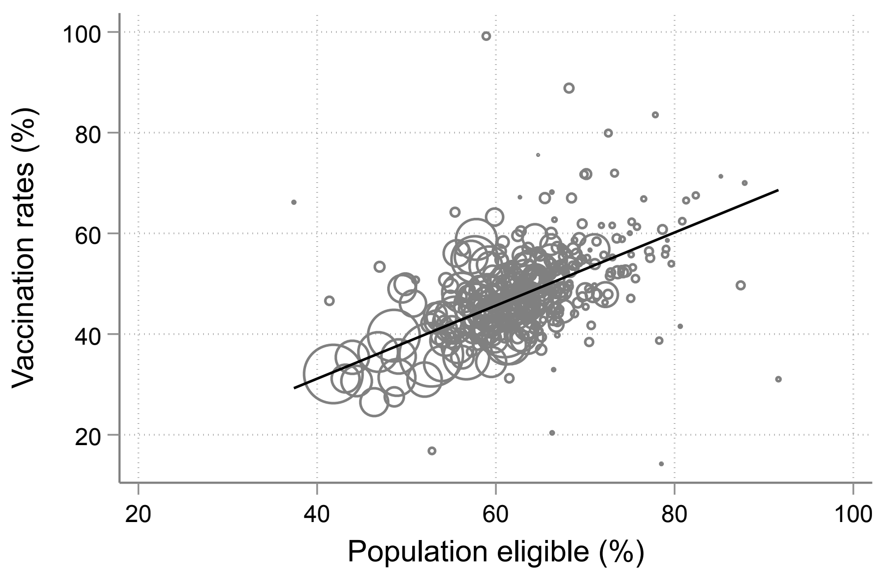
(d) Share fully vaccinated

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



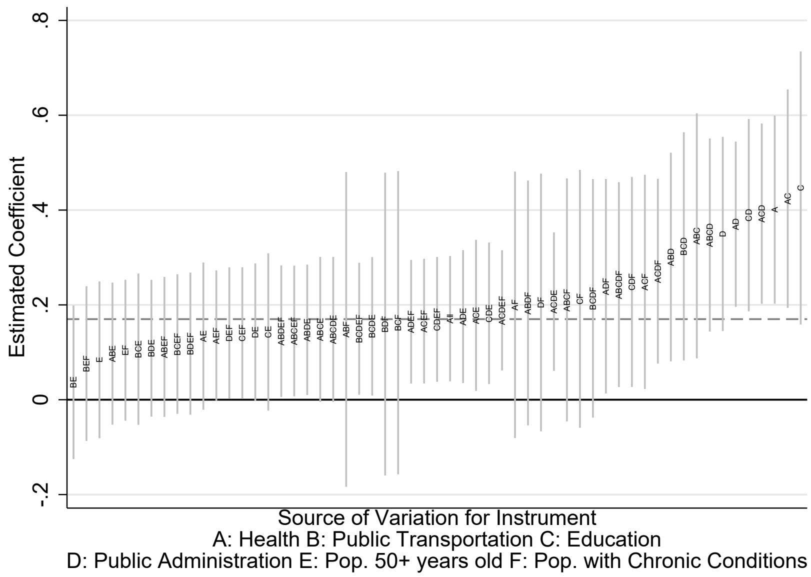
(a) Vaccination rate by election day



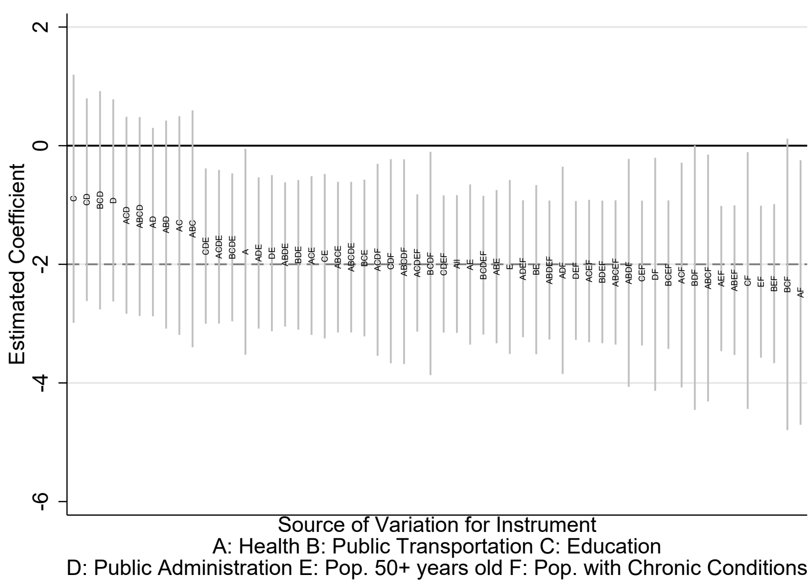
(b) First-stage

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)	
Municipalities	343				

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 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

		Share of valid votes			
	General turnout	Mayor	Constitutional convention	Councilors	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 aases 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							
	Local election (mayor)					Constitutional convention		
	Incumbent	Incumbent (reelection law not binding)	Left wing	Right wing	Independent	Left wing	Right wing	Independent
Panel A: Instrumental variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Share of adults with two doses	-1.995***	-1.141**	0.929	0.319	-1.015	0.020	0.053	0.057
	(0.593)	(0.451)	(0.640)	(0.327)	(0.672)	(0.165)	(0.060)	(0.164)
	[0.004]	[0.001]	[0.056]	[0.296]	[0.010]	[0.921]	[0.457]	[0.529]
Panel B: Reduced form								
Share of eligible people	-1.367***	-0.918**	0.618	0.204	-0.660	0.013	0.035	0.038
	(0.398)	(0.397)	(0.478)	(0.236)	(0.484)	(0.123)	(0.044)	(0.124)
	[0.001]	[0.000]	[0.045]	[0.335]	[0.024]	[0.921]	[0.472]	[0.555]
Panel C: OLS								
Share of adults with two doses	-0.440	-0.183	0.901***	0.125	-0.910**	-0.117	0.062*	0.131
	(0.271)	(0.300)	(0.343)	(0.150)	(0.354)	(0.082)	(0.035)	(0.083)
	[0.010]	[0.208]	[0.006]	[0.442]	[0.000]	[0.003]	[0.024]	[0.001]
Municipalities	324	233	324	324	324	324	324	324
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	39.44	42.94	37.12	28.02	32.74	17.83	21.07	18.19
Standardized effect (Panel A)	-1.54	-0.60	-0.24	-0.86	-0.49	0.02	0.06	0.05

Notes: All regressions are weighted by 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 6: Eligibility and pandemic concerns in high-frequency surveys (n.p.s)

	Dependent variable			
	Worried		Very Worried	Concern (Ordered)
	(1)	(2)	(3)	(4)
Eligible for vaccine	-0.063***	-0.064***	-0.053**	-0.145***
	(0.021)	(0.021)	(0.021)	(0.046)
Estimation method	OLS	OLS	OLS	Ordered Probit
R-squared	0.031	0.039	0.043	–
Avg. dependent variable	0.57	0.57	0.36	3.55
Age fixed effects	X	X	X	X
Individual controls		X	X	X
Observations	3,481	3,481	3,481	3,481

Notes: Additional empirical analysis which was not pre-specified (n.p.s). 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. Sample consists in 5 waves of survey implemented the first week of each month from May to September 2021. Concern 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. Individual controls are a gender dummy and 9 education dummies. Robust standard errors in parenthesis. Statistical significance: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 7: 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: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

ONLINE APPENDIX

The Political Consequences of Vaccines:

Quasi-Experimental Evidence from Eligibility Rules

Emilio Depetris-Chauvin and Felipe González

A Definition of Political Coalitions **iii**

A.1 Local Election iii

A.2 Constitutional Convention Election iii

A.3 Regional Governors Election iv

B Booth-Level Analysis **iv**

B.1 Motivation for research design iv

B.2 Econometric strategy iv

List of Figures

A.1 Randomization inference vi

List of Tables

A.1 Descriptive statistics from the 2017 Census vii

A.2 Descriptive statistics from the 2017 National Survey viii

A.3 Descriptive statistics for the pandemic before the vaccines ix

A.4 Descriptive statistics and validity of the booth-level design x

A.5 Vaccination centers and political participation xi

A.6 Spatial spillovers of eligibility rules xii

A.7 Spatial spillovers of political impacts xiii

A.8 Characterization of compliers I xiv

A.9 Characterization of compliers II xv

A.10 Vaccination centers, partisanship, and incumbents xvi

A.11 Vaccination and partisanship in local councilors elections xvii
A.12 Vaccination and partisanship in governors election xviii
A.13 Vaccination and sick leaves xix

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 Booth-Level Analysis

B.1 Motivation for research design

The pre-analysis plan also proposed to exploit the location of vaccination centers as a source of within-municipality exposure to vaccines. People who live farther away from vaccination centers can make different decisions for a number of reasons. For example, travel costs might affect their choice, they might have differential information about the vaccination process, different perception of state presence (Flückiger et al., 2019), might have been primed to think about the pandemic, our could have experienced more feelings of anxiety.

The location of vaccination centers was unknown ex-ante. Therefore, the distance from people's homes to these places should be a valid source of variation. Moreover, people are assigned randomly to booths within their municipality of residence based on their national ID number and the explicit goal of reaching 350 voters per booth. Therefore, the average distance from people registered in a booth to the closest vaccination venue should vary quasi-randomly across booths. Unfortunately, we cannot replicate the municipality-level research design because the vaccination data is not available at the booth level.

Table A.4 presents the same analysis for the booth-level econometric design. In this case the exogenous variable is the distance to the closest vaccination venue, we replace province by municipality fixed effects, and the controls by the distance from people's homes to the booth and the municipal hall, the percentage of women, the average age of people, and the total number of people in the booth. Reassuringly, the distance to vaccination centers within municipalities is uncorrelated with political participation in all elections before the pandemic. However, people who live farther away from vaccination centers vote relative more for right-wing parties and voted more against the Constitutional Convention, although estimates are of small economic magnitude.

B.2 Econometric strategy

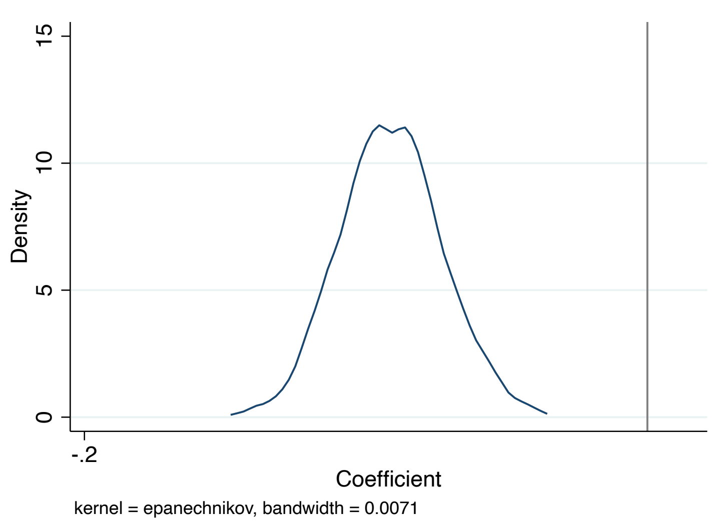
To implement this strategy, we use three data sources. First, the list of all people who have the right to vote in the 2021 Election, approximately 15 million individuals. These data is known as Electoral Registry, it is constructed by the Electoral Service and for each person we observe their age, gender, home address, and the booth in which they can vote. Second, electoral outcomes at the booth-level. Booths are groups of 350 people and there are approximately 45,000 in the country located inside 2,500 polling stations. Third, the location of approximately 1,400 vaccination centers.

We geocoded the home addresses of the 15 million people in the Electoral Registry, and the location of all booths and vaccination centers to estimate the following cross-sectional equation:

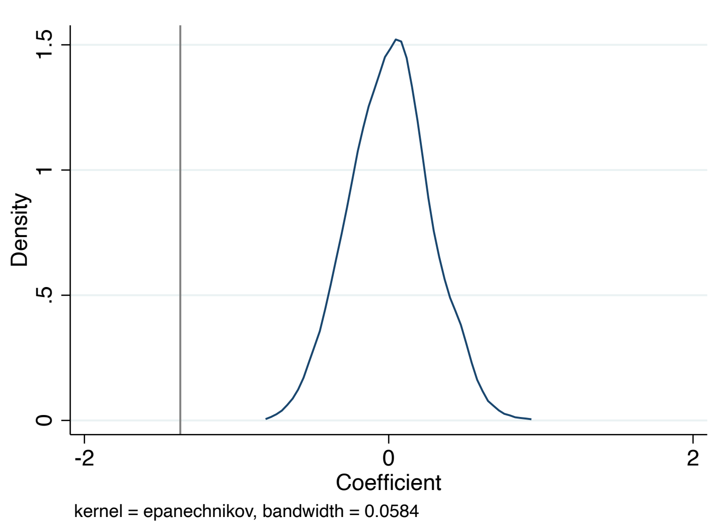
$$y_{ij} = \tau d_i + \gamma x_i + \phi_j + \varepsilon_{ij} \tag{5}$$

where y_{ij} is an electoral outcome in booth i located in municipality j and d_i is a vector of averages distances from people's homes in booth i to specific locations. We use political participation and vote shares as outcomes but now measured in 45,000 booths instead of 343 municipalities. In contrast to the previous strategy, our interest is now on the average distance from people's homes in a booth to the closest vaccination venue within their municipality of residence. As geographic controls, we include the distance from people's homes to the booth and the municipal hall for a total of three distance variables. Equation (5) also includes a vector for the characteristics of people in a booth, x_i: the percentage of women, the average age, and the total number of people registered in the booth. We are unfortunately constrained by data availability to include more characteristics of people as controls. In order to make comparisons within municipalities we include a full vector of municipality-level fixed effects ϕ_j and we allow the error term ε_{ij} to be correlated within municipalities.

Figure A.1: 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.

Table A.1: Descriptive statistics from the 2017 Census

	Mean st. dev.	Univariate regression of covariate on instrument (mean instrument 64.3, st. dev. 9.27)			Standardized effect from (4)
		unconditional	conditional on province F.E.	conditional on province F.E. and controls	
	(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.2: Descriptive statistics from the 2017 National Survey

		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)
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)	
Municipalities	323				

Notes: Column 1 reports the mean value and standard deviation for 12 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: Descriptive statistics for the pandemic before the vaccines

	Mean st. dev.	Univariate regression of covariate on instrument (mean instrument 64.3, st. dev. 9.27)			Standardized effect from (4)
		unconditional	conditional on province F.E.	conditional on province F.E. and controls	
	(1)	(2)	(3)	(4)	(5)
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)	
Municipalities	343				

Notes: Column 1 reports the mean value and standard deviation for 4 variables related to the pandemic (listed at the left). All 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.4: Descriptive statistics and 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.5: Vaccination centers and political participation

	General Turnout	Share of Valid Votes			
		Mayor	Const. Conv.	Councilors	Governors
	(1)	(2)	(3)	(4)	(5)
Distance to closest vaccination center	0.887	0.863	0.830	0.808	0.954
	(0.609)	(0.591)	(0.675)	(0.608)	(0.618)
Booths	42,163	42,163	42,163	42,163	42,163
Municipal fixed effects	X	X	X	X	X
Full set of controls	X	X	X	X	X
Standardized effect	0.07	0.07	0.07	0.07	0.08
R-squared	0.469	0.470	0.519	0.464	0.460
Mean of dep variable (Panel D)	43.5	42.64	38.73	40.92	40.97

Notes: All specifications at the booth-level includes municipality fixed effects and controls percentage of women, the average age, and the total number of people registered in the booth, and for the distances from people's homes to the booth and the municipal hall. Regressions at the municipality level are weighted by voting age population. Robust standard errors in parenthesis (clustered at the municipality level for the booth-level analysis). 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

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: Vaccination centers, partisanship, and incumbents

	Dependent variable: Vote share							
	Local election (mayor)					Constitutional convention		
	Incumbent	Incumbent (reelection law not binding)	Left wing	Right wing	Independent	Left wing	Right wing	Independent
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Closest distance to vaccination venue	0.663	0.419	-0.568	-0.658**	1.200**	-0.381	1.271*	-0.736*
	(0.571)	(0.595)	(0.504)	(0.299)	(0.577)	(0.266)	(0.662)	(0.386)
Booths	42,156	31,029	42,156	42,156	42,156	42,154	42,154	42,154
Municipality fixed effects	X	X	X	X	X	X	X	X
Full set of controls	X	X	X	X	X	X	X	X
Standardized effect	0.03	0.02	-0.02	-0.02	-0.05	-0.03	0.10	-0.05
R-squared	0.926	0.908	0.935	0.968	0.926	0.741	0.807	0.797
Avg. dependent variable	48.38	53.13	41.91	29.26	26.82	34.1	18.9	39.1

Notes: Standard errors clustered at the booth-level. Statistical significance: *** p <0.01, ** p <0.05, * p <0.1.

Table A.11: Vaccination and partisanship in local councilors elections

	Vote Share for			
	Incumbent	Left-Wing	Right-Wing	Independent
Panel A: Reduced Form	(1)	(2)	(3)	(4)
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 B: IV				
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 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
Panel D: Booth Analysis (OLS)				
Closest distance to vaccination venue	0.012	-1.062*	0.937	0.022
	(0.271)	(0.544)	(0.624)	(0.101)
R-squared	0.739	0.811	0.837	0.856
Mean of dep variable (Panels ABC)	17.44	56.36	35.05	33.95
Std deviation of dep variable (Panels ABC)	8.38	11.99	12.54	10.96

Notes: The unit of observation in Panels A, B, and C (D) is a municipality (booth). The number of observations in Panels A, B, and C is 324 and 42,154 in Panel D. Regressions at the municipality level are weighted by voting age population. Robust standard errors in parenthesis (clustered at the municipality level for the booth-level analysis). 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.12: Vaccination and partisanship in governors election

	Vote Share for		
	Left-Wing	Right-Wing	Independent
Panel A: Reduced Form	(1)	(2)	(3)
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 B: IV			
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 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
Panel D: Booth Analysis (OLS)			
Closest distance to vaccination venue	-0.665	1.041*	-0.188
	(0.477)	(0.614)	(0.189)
R-squared	0.880	0.838	0.915
Mean of dep variable (Panels ABC)	46.54	23.21	19.07
Std deviation of dep variable (Panels ABC)	15.96	9.94	14.00

Notes: The unit of observation in Panels A, B, and C (D) is a municipality (booth). The number of observations in Panels A, B, and C is 324 and 42,160 in Panel D. Robust standard errors in parenthesis (clustered at the municipality level for the booth-level analysis). 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.13: Vaccination and sick leaves

Dependent variable: Sick leaves per worker		
	All	Mental and behavioral disorders
	(1)	(2)
Share of adults with two doses	-0.0044	-0.0010**
	(0.0032)	(0.0005)
Observations	4,151	4,151
Municipalities	346	346
Municipality fixed effects	X	X
Month fixed effects	X	X
Municipality-specific linear trends	X	X
Avg. dependent variable	7.06	2.14
R-squared	0.963	0.984

Notes: All regressions are weighted by adult population. Share of adults with two doses is computed as of end of the previous month. Standard errors clustered at the municipality-level in parenthesis. Statistical significance: *** p <0.01, ** p <0.05, * p <0.1.