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. Survey evidence suggests that vaccines could have increased preferences for challengers by lowering decision-related anxiety.

Keywords: vaccines, politics, election, challengers.

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1 Introduction

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

We provide novel evidence for the impact of vaccines on elections. Vaccines can increase political participation by decreasing the cost of voting and potentially affect relative preferences for incumbents and challengers. 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 clear eligibility rules which we show were exogenous to the pre-pandemic political equilibrium, prevailing economic conditions, and pandemic severity. Importantly, vaccines were not politicized and vaccine hesitancy was low. In addition, following an intense wave of protests before the outbreak of the pandemic, the country embarked on a path to replace the Constitution, which led to multiple high-stakes elections 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 political preferences.

Our empirical analysis is divided in two parts. The foundation of the first part is based on two pillars. First, we overcome challenges related to cherry picking and *p*-hacking in statistical analysis of observational data (Christensen and Miguel, 2018) by writing a comprehensive pre-analysis plan posted online before the elections took place. The plan offered a detailed description of the empirical analysis, which we implemented when the electoral results were made public. Second, we econometrically exploit the eligibility rules using administrative data for all 346 municipalities

in the country. The rules consisted primarily of rolling age cutoffs, chronic health prevalence, and belonging to critical economic sectors.[1] Crucially, we are able to rule out mechanical effects related to age structure as the pre-analysis plan shows that local exposure to the vaccination campaign is empirically unrelated to turnout and political preferences in the period 2012-2020. Moreover, eligibility rules were also unrelated to local economic conditions and to local variables related to pandemic severity. We use these plausibly exogenous differences in an instrumental variables framework.

We find that exogenously higher vaccination rates boosted local political participation. An increase of one standard deviation in local vaccination rates (14 percentage points, pp) is causally associated with and increase in political participation of 2.4 pp over a sample average of 48%, similar in magnitude 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. We do find that higher vaccination rates are causally associated with more votes for challengers. Our estimates imply that an additional 5,000 fully vaccinated individuals locally (10 pp of adult population) leads to 700 additional voters (1.7 pp) and 2,000 more votes for challengers (11 pp). These magnitudes imply that the higher support for challengers cannot be solely explained by higher turnout. Following the pre-analysis plan, we show the robustness of results to alternative inference methods, we characterize compliers (Abadie et al., 2002), and trace out variation in Local Average Treatment Effects using all possible subsets of variables behind the eligibility rules. The estimates appear to be fairly generalizable and causal effects are similar across different complier populations. Non-pre-specified results using the same empirical framework suggest that spatial spillovers are minimal.

Why could higher vaccination rates increase preferences for challengers? The second part of our empirical analysis was not included in the pre-analysis plan and attempts to investigate potential answers to this question.[2] We begin by showing that the higher participation and preferences for challengers cannot be attributed to increased political competition: there is little correlation between vaccination rates and the number of candidates running in elections. Additionally, we

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

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

explored variations in LATE by employing different combinations of instruments linked to eligibility rules. Our analysis indicates that individuals who responded to vaccinations by voting less for incumbents tended to be older, employed in the healthcare sector, and affected by chronic conditions. However, these characteristics are also associated with a relatively lower impact on voter turnout, suggesting that plausible explanations are more likely to be linked to shifts in the political preferences of established voters rather than skewed preferences among new voters.

In more exploratory analysis, we investigate differences in voting by interpreting incumbents as the safe (known) alternative and challengers as the uncertain (risky) option. We hypothesize that the pandemic affected people's anxiety and ability to focus, difficulted information acquisition, and therefore tilted citizens towards incumbents, the safe option in this decision under uncertainty (Tversky and Kahneman, 1992). Motivated by previous research documenting a "flight to safety" in times of uncertainty (Cohn et al., 2015; Bisbee and Honig, 2021), we present four results. First, we exploit weekly variation in vaccine-eligibility across 22,000 individuals over 32 weekly surveys around the election to show that vaccines reduced self-reported anxiety. Second, we use data from individuals surveyed multiple times to show that vaccines led to more concentration and less depression.[3] Third, we exploit eligibility rules for vaccine boosters before the Presidential and Congress Election of November 2021 and find attenuated political impacts. Given the pandemic was less severe, and the booster similarly effective (UK Health Security Agency, 2022), these results hint to the importance of pandemic-related anxiety. As we cannot directly link anxiety to voting decisions, these results only represent suggestive evidence of a potential mechanism. Fourth, we find little evidence of vaccines affecting information acquisition as measured by the proportion of invalid (blank or null) votes in local elections. Overall, we find suggestive evidence that vaccines could have affected voting through changes in risk preferences but not through information.

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) shape the deployment of public health measures. To the best of our knowledge, there is no research on the political effects of large vaccination campaigns. An

[3]Recent research in the U.S. and the U.K. also shows the psychological benefits of vaccines (Bagues and Dimitrova, 2021; Agrawal et al., 2021; Chaudhuri and Howley, 2022). Related research documents how the pandemic deteriorated psychological well-being (Holman et al., 2020; Brülhart et al., 2021; Altindag et al., 2022).

exception is Gutiérrez et al. (2023) which documents the relationship between vaccine eligibility and turnout in Mexico, but they lack data on vaccination and eligibility rules were less strict. The reason for the lack of evidence is presumably related to the endogeneity of public health measures, which researchers have shown to respond to political incentives. We overcome this challenge by econometrically exploiting a centralized vaccination campaign with predetermined implementation rules using a pre-analysis plan. In particular, we leverage large differences in exposure to the eligibility rules across municipalities on the eve of the election. By doing so, we are able to credibly isolate the pervasive political factors which usually affect vaccination campaigns.

Related studies show that the prevalence of a disease decreases political participation and changes vote shares (Urbatsch, 2017; Mansour et al., 2020; Scheller, 2021; Morris and Miller, 2021; Gutiérrez et al., 2022; Campante et al., 2023) and the political impact of other large public health policies (Haselswerdt, 2017; Clinton and Sances, 2018; Baicker and Finkelstein, 2019; Bol et al., 2021; Atal et al., 2024). Understanding the political effects of vaccines is important because it reveals information about the electoral motivation of incumbents 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, evidence which we tentatively interpret as a consequence of decreased anxiety in times of crisis. In that sense, we speak to a broader literature documenting the psychological determinants of political participation and candidate choice. For example, previous research shows that emotions play a key role in political behavior (Brader and Marcus, 2013; Passarelli and Tabellini, 2017), that social concerns drive political participation (e.g. Gerber et al. 2008; Dellavigna et al. 2017), and that seemingly superfluous factors affect candidate choice (e.g. Shue and Luttmer 2009; Dellavigna 2009; Ajzenman and Durante 2023). In contrast, we emphasize the potential of anxiety-changing policies in shaping political preferences.

2 Vaccination Eligibility Rules and High-Stakes Election

We study the impact of a large vaccination campaign against the coronavirus on electoral outcomes in Chile. This country offers an ideal testing ground for three reasons. First, the country was quick in securing a diversified stock of vaccines and deployed the immunization with clear eligibility rules since December 2020. The plan to roll-out the vaccines was designed and implemented by

the central government, leaving little room for local governments to affect this process.[4] Eligibility rules were based on verifiable measures such as age and occupation. Elder people and workers in certain occupations got the vaccine first on a week-to-week rolling program that started with people older than 90 years old and health personnel. Information about these rules was extensively disseminated nationally through online and traditional media (i.e. television, radio, and newspapers) and the vaccination data was made public in real time. Unlike in other countries, vaccines were *not* politicized and political parties from left to right supported the vaccination campaign, putting Chile among the countries with the highest vaccination rates (Mathieu et al., 2021).

The second crucial characteristic is that Chile faced one of the most important elections in its modern history. Five months before the pandemic outbreak, a wave of protests triggered a referendum asking citizens if they would like to replace the existing Constitution, originally drafted by the Pinochet dictatorship in 1980 (González and Prem, 2023). The referendum was held in October 2020 and 80% voted for a new Constitution. As a consequence of the vote, a new text was drafted by a Constitutional Convention composed by 155 members elected by a D'Hondt method.[5] The members of the Convention were elected the same day that mayors, councilors, and regional governors in an election with four ballots. The final important characteristic of the Chilean context is that automatic registration and voluntary voting was in place in all elections between 2012 and 2021. The combination of a high-stakes election, voluntary voting, and a massive vaccination process halfway implemented by election day constitute an ideal empirical setting.[6]

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

[4]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.

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

[6]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.

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

3 Research Design

Our research design is based on a pre-analysis plan (Depetris-Chauvin and González, 2021). This methodology is relatively underused when performing observational analysis, particularly when compared to randomized controlled trials.[7] 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

[7]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).

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:

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

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).[8] The right-hand side variable of interest 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.[9] 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.[10] 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 takes 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

[8]When analyzing vote shares we aggregated political parties into political coalitions. We created the mapping from political parties to left- and right-wing coalitions in the pre-analysis plan. Appendix A presents this classification.

[9]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.

[10]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.

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. 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).[11] 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%). The 40% of people who were vaccinated and were 47 years old or younger either worked in priority occupations or suffered from a chronic disease. The striking 50% increase in vaccination rates due to age-eligibility is similar in other weeks.

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.

[11] 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.

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).[12]

Table 1 presents summary statistics for 19 variables describing local political participation and preferences, and the predicted power of the instrument on these variables. We have organized this table to study political participation (panel A), and preferences (panel B) in the 2020 plebiscite, and for incumbents, left-wing, right-wing, and independent candidates in all elections since 2012 when automatic registration and voluntary voting was introduced. To classify candidates as left-wing and right-wing, we follow previous work using data from these elections (Bautista et al., 2023). In the appendix we also examine 14 additional variables from the 2017 Census, 10 variables from the 2017 National Survey, and four variables related to the COVID pandemic (Tables A.1-A.3). In sum, we estimated the correlation between the instrument and 51 variables covering elections, the labor market, health conditions, state subsidies, and the pandemic, and we observe 7 statistically significant differences at the 10% level. The number of differences is slightly above the 5 derived from a 10% statistical test ($0.10 \times 51 = 5.1$), which in this case was reasonable to expect as we explain below. Importantly, only one of the 19 political variables is correlated with the instrument at the 5% level, which is what we expected of a 5% statistical test ($0.05 \times 19 = 0.95$).

Overall, we interpret Table 1 as supporting the validity of the research design in the sense that the instrument has little predictive power of political participation or political preferences at the local level as measured by the five elections held between 2012 and 2020. Moreover, the signs

[12]It is important to note that the three indicators of pandemic severity the week before the election in Table A.3 were not originally specified in our pre-analysis plan. One of these three variables (i.e., Covid Deaths per 10,000 inhabitants) displays a positive and statistically significant correlation with our instrument. While we did not include these covariates as controls in our main specifications, it is reassuring that, if anything, their inclusion makes our main results somewhat stronger and more precisely estimated.

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.[13]

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 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.[14]

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.

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

[14]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).

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.[15] Table 2 presents the analogue regression estimates from five specifications with different sets of controls. These results were reported in the pre-analysis plan using the same five specifications.

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

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

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

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 5,000 additional fully vaccinated individuals are causally associated with 700 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.2). Results in Table 3 are affected virtually little when controlling non-linearly for the set of unbalances variables (Table A.4).

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.5 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.6 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.7 and A.8 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.[16] Incapacitation effects mechanically decrease turnout by preventing infected individuals to go out and vote. However, the impact could be larger because infections also affect others for a variety of reasons such as perceptions of state ineffectiveness or fear of contagion. We leverage variation in infections within municipality over three elections using the following two-way fixed effects econometric model:

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

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 con-

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

firm 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 coalition or 11 pp fewer votes for the incumbent *mayor*.[17] Given that 18,500 individuals voted in the average municipality, these numbers imply that an additional 5,000 fully vaccinated individuals lead to 2,000 fewer votes for the incumbent mayor (11%×18,500).

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).[18] Columns 6-8 reveal similar findings. In the latter columns, we study partisan votes to elect the Constitutional Convention as dependent variable. Note that given this was an extraordinary election, there were no incumbent candidates.[19] This is also an important election in the country's history as for the

[17] A new law enacted shortly before the pandemic outbreak prevented many incumbents to run for reelection by establishing a maximum of three periods in office (12 years). All these coefficients display similar statistical significance when using spatial errors or randomization inference (see panel B in Figure A.2).

[18] Point estimates suggest that some votes flowed from independent to left-wing candidates but standard errors are large. In the Appendix, we repeat these analyses but studying vote shares in the Councilors and Regional Governors election (Tables A.9, A.11) and find little evidence of partisan effects in these lower-stakes elections.

[19] 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.

first time a group of individuals was democratically elected to write a new Constitution and thus we consider it high stakes. Instrumental variables estimates again reveal little evidence of partisan effects derived from vaccines, with small and statistically insignificant coefficients.

Overall, Tables 3 and 5 show that exogenous increases in vaccination rates boosted local political 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 Why did vaccines weaken incumbent mayors?

Vaccines unexpectedly reduced the vote shares of incumbent mayors, despite a large literature showing that incumbents obtain political returns from policies that benefit the population. This finding cannot be explained by changes in the average number of candidates competing in local elections (0.3 per 1,000 inhabitants, see Table A.10).[20] In addition, the new voters brought by the higher political participation are unlikely to be the explanation behind this finding because the increase in turnout is smaller than the decrease in votes for incumbents. Moreover, we observe lower support for incumbents of *all* parties. Support for right-wing parties (incumbents in the central government) was unchanged by vaccines in local and constitutional elections, except in the case of regional governors. The latter election was held for the first time in history, voters had little information to resort to, and right-wing parties benefitted from vaccines (Table A.11).

We offer two exercises to illuminate potential explanations for the lower support for incumbent mayors. In the first (pre-registered) exercise, we trace out variation in the LATE by using all combinations of instruments behind the eligibility rules $Z_c = \{z_{1c}, \ldots, z_{Jc}\}$. We have $J = 6$ with $j = A, B, C, D, E, F$, thus $2^6 = 64$ different subsets of instruments.[21] Panels (a) and (b) in Figure 3 present results. Visual inspection of small and large point estimates with subsets of instruments reveals that people who responded to the vaccines by voting less for incumbents are older (E), work in health (A), and suffer from chronic conditions (F). These characteristics are also related to relatively *lower* effect on turnout, suggesting that plausible explanations are more likely to be

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

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

related to changes in political preferences of *established* voters than to skewed preferences among *new* voters. For example, health workers and the elderly could have been disappointed by how incumbent mayors assisted the vaccination campaign promoted by the central government, and consequently decided to support challengers. Unfortunately, without voting data disaggregated by municipality and demographic groups we cannot fully test this hypothesis. The second exercise, and the more exploratory analysis of mechanisms, is presented in the next section.

5 Vaccines and Decision-Making

This section explores why vaccines might increase preferences for challengers. This analysis was *not* part of the pre-analysis plan and we view it as more exploratory. We interpret the decision between the incumbent and a challenger through the lens of decision-making under uncertainty (Tversky and Kahneman, 1992). The incumbent represents the safe (known) alternative, while the challenger represents the uncertain (risky) option. Voters need to acquire information about challengers. We interpret the pandemic as affecting people's anxiety and ability to focus (Fetzer et al., 2020), difficulting information acquisition, and therefore tilting citizens towards incumbents, the safe and known alternative. Vaccines can reverse this process and thus potentially empower challengers. This hypothesis is consistent with previous research in political science and financial economics showing that during times of crisis or uncertainty, agents resort to the certainty provided by the status quo and safer assets (Cohn et al., 2015; Bisbee and Honig, 2021).

5.1 Vaccines and anxiety in high-frequency surveys

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

In order to exploit the roll-out of the vaccines following the weekly eligibility rules, we estimate

the following regression equation using data from the surveys around the election:

$$y_{ij(i)} = \beta V_i + f(x_i) + \phi_{j(i)} + \eta_{ij(i)} \tag{3}$$

where $y_{ij(i)}$ is the response of person i who's age by the time of the survey is $j(i)$. As dependent variables, we use two indicators, one for individuals who reported being *worried* and another one for those *very worried* about getting infected. The indicator V_i takes the value of one if i was fully vaccinated by the time of the survey. Similarly as before, we provide instrumental variables estimates using as instrument an indicator which takes the value of one if i was eligible to be fully vaccinated when surveyed. Our preferred specification also includes non-parametric controls for gender and education $f(x_i)$. Crucially for the identification strategy, equation (3) includes a complete set of age fixed effects $\phi_{j(i)}$ which allows us to econometrically compare individuals of the same age but who answered the survey when they were and were not eligible for the vaccine. Finally, $\eta_{ij(i)}$ is an error term clustered by age to allow for arbitrary correlation within age cohorts.

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

5.2 Vaccines and the ability to focus in repeated surveys

We use waves of a nationally representative survey conducted by an independent research team in charge of studying the evolution of mental health during the pandemic. They implemented four waves of the same survey in July 2020, November 2020, April 2021, and August 2021. Approxi-

[22] To save space, we do not report the first-stage estimates in Table 6. However, these estimates can be easily derived from the results presented in the reduced form (Panel B) and IV (Panel C). It is reassuring to note that the implied first-stage coefficient (i.e., 0.67) is remarkably close to the 70% take-up rate obtained in section 4.1 using variations in vaccination rates and the share of eligible individuals at the municipality level.

mately 1,500 individuals were surveyed in each wave, allowing us to examine how mental health evolved within individuals over time. We use these surveys to test for the empirical relationship between vaccines, concentration, and depression. Table A.12 provides descriptive statistics for the six measures related to mental health that can be tracked in all four waves of the survey.

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

$$y_{it} = \beta E_{it} + \gamma f(x_i) + \phi_i + \phi_t + \epsilon_{it} \tag{4}$$

where y_{it} is an indicator for the response of individual i in month t, E_{it} takes the value of one if individual i was eligible for the vaccine in month t, ϕ_i is a full set of individual fixed effects, and ϕ_t represent wave fixed effects. Unfortunately, the survey did not ask for the vaccination status and thus we rely on the 70% take-up rate from sections 4.1 and 5.1 to discuss instrumental variables estimates. We also include a flexible vector of controls $f(x_i)$ which include age fixed effects, individual-level controls (gender, education), and the number of covid cases in the region. The error term ϵ_{it} is clustered by municipality, and we employ weighted least squares with survey weights.

Panel A in Table 7 presents estimates of equation (4) without individual fixed effects for comparison. Panel B in the same table shows estimates with individual fixed effects. Columns 1 and 2 show that when individuals were eligible for the vaccine, their concentration improved. If we considered a take-up rate of 70% for the vaccine, then individuals who were fully vaccinated reported feeling overwhelmed 12 percentage points less than those not vaccinated. Moreover, vaccination leads to a 15 percentage points decrease in the probability of feeling not able to focus, a large decrease from a sample average of 40%. We do not find evidence of sleeping problems varying systematically with vaccines (column 3) and, consistent with the evidence from section 5.1, individuals fully vaccinated report significantly lower feelings of depression (columns 4-6). The results are similar if we replace the dependent variable by the 1-4 ordered response (Table A.13)

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 8 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 8 show that more vaccination leads to more political participation in the presidential and congress elections, but the point estimate is 30% smaller than in May. As the incumbent president was not a candidate in the presidential election, we propose two measures: (1) deviations from the political center derived from vote shares and an order of the seven candidates in a unidimensional left-right spectrum, and (2) vote shares of right-wing candidates as proxy for candidates from parties which were politically closer to the incumbent President. We find little evidence of impacts on the former but some evidence of higher preferences for challengers (left-wing) when vaccination rates were higher. Yet the magnitude of the coefficient is significantly smaller than in the May election. Studying preferences for incumbents in the Congress Election, we again fail to find evidence for vaccines tilting voters towards challengers (column 6). In all, we confirm the existence of attenuated impacts in the November election in terms of turnout and null or attenuated effects in terms of vaccines increasing preferences for challengers.

5.4 Information acquisition as measured by invalid votes

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

Given the lack of data measuring how informed voters were, we can only test this hypothesis using proxies of information. We argue that less information about candidates could have been associated with more invalid or null votes.[23] Therefore, we repeat our main estimation strategy but replace the dependent variable by the percentage of invalid (blank or null) votes in the corresponding election. The results in Table A.14 suggest that vaccines did not affect the amount of information about candidates as vaccination rates are unrelated to the percentage of invalid votes in local elections. Overall, the sign of coefficients is unstable across elections and the point estimates are relatively small when compared to the impact of vaccines on incumbents. We conclude that information acquisition is unlikely to be the main explanation behind our findings.

6 Conclusion

The causal impact of vaccination campaigns on elections has been elusive to estimate given the political factors driving the implementation of these policies. We exploited eligibility rules and other appealing characteristics of the Chilean context to show that increases in vaccination rates are causally associated with more political participation and empower outsiders by decreasing the votes of incumbents, irrespective of their party affiliation. We combined surveys, administrative data, and a replication in another election to provide suggestive evidence of psychological factors related to anxiety as one potential mechanism to explain the higher preferences for challengers.

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

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 lower 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 changes in risk preferences. However, more work needs to be done in order to pin down the range of mechanisms through which anxiety-changing policies can affect the political equilibrium.

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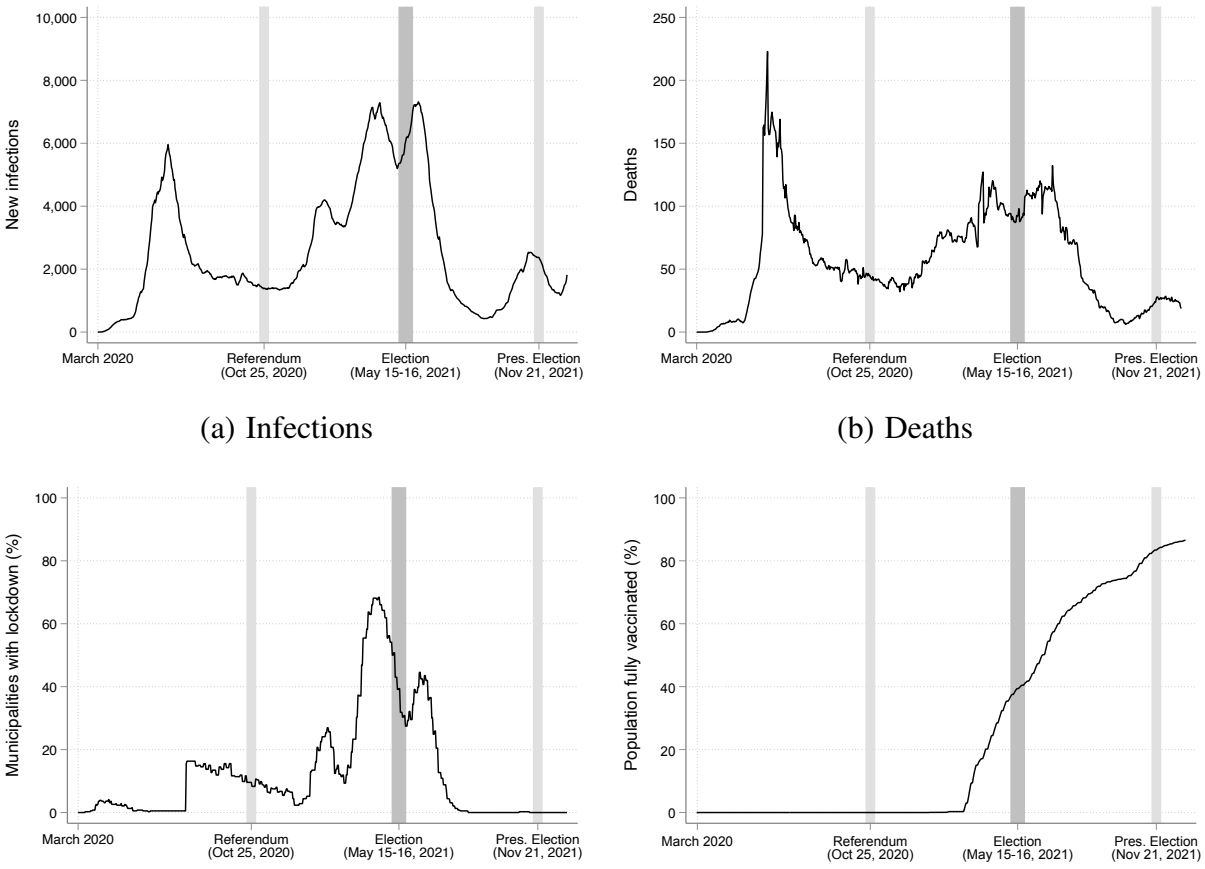
Figure 1: Pandemic and vaccination during study period



(a) Infections

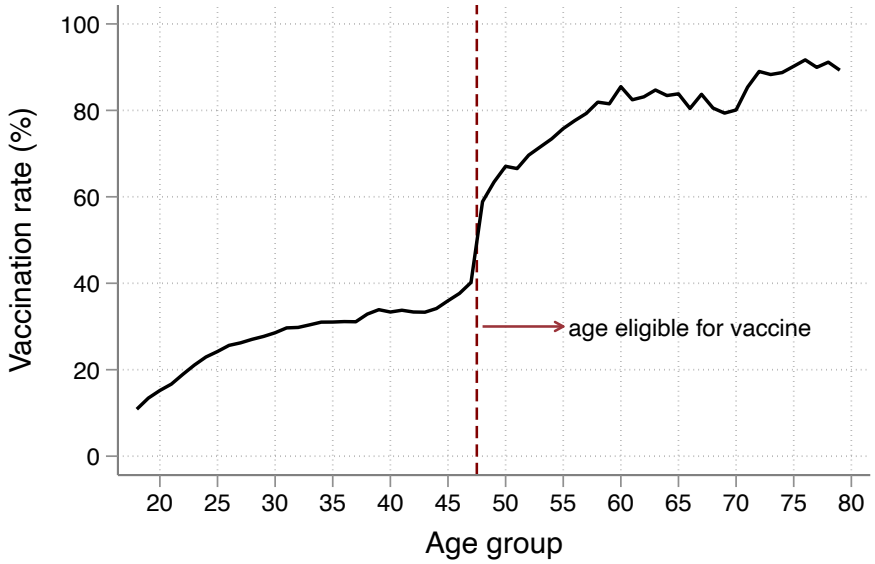
(b) Deaths

(c) Lockdowns

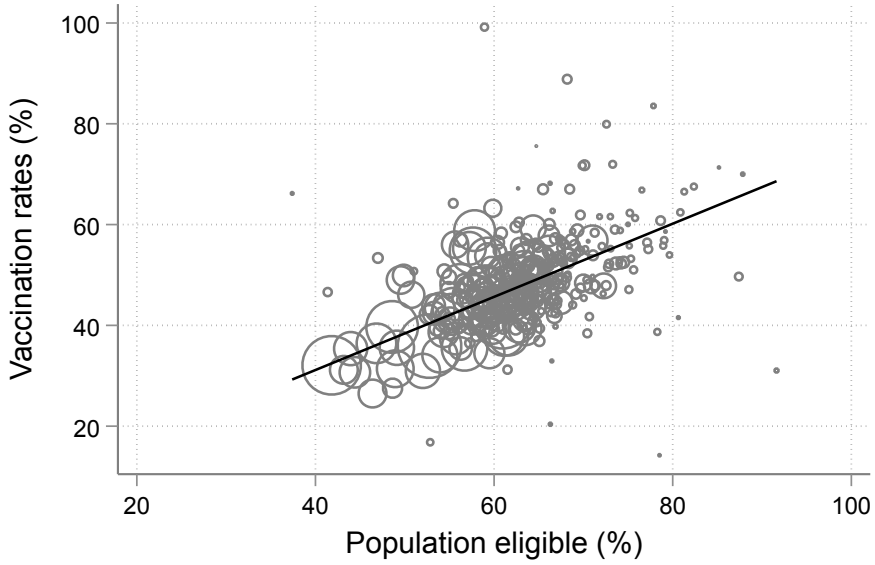
(d) Share fully vaccinated

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



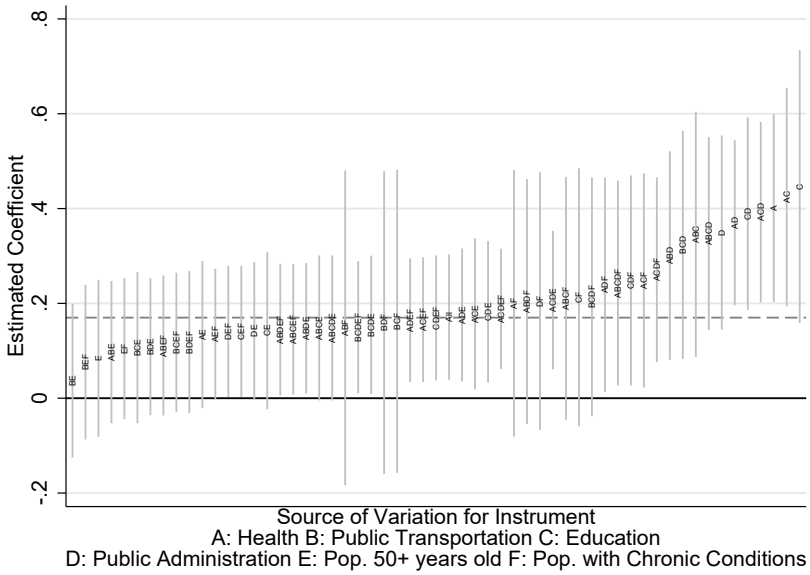
(a) Vaccination rate by election day



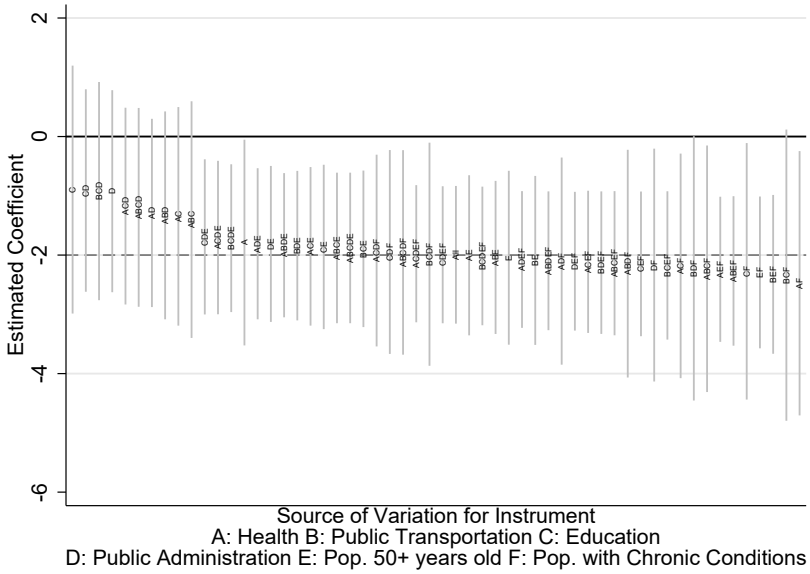
(b) First-stage

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

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

Notes: The inclusion of vote share for incumbents (both major and councilors) was not originally specified in our pre-analysis plan. Column 1 reports the mean and standard deviation for 19 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 cases in adult pop.	-5.809**				
	(2.555)				
Share of active cases in adult pop. (logs)		-10.201***			
		(3.232)			
Active cases (logs)			-2.219***		
			(0.380)		
Intensity of lockdown before election				-0.277***	
				(0.070)	
Lockdown on election day					-3.092***
					(0.812)
Observations	1,029	1,029	1,029	686	686
Municipality fixed effects	X	X	X	X	X
Election fixed effects	X	X	X	X	X
R-squared	0.782	0.784	0.792	0.801	0.798

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

Table 5: Vaccination, partisanship, and incumbents

	Dependent variable: Vote share							
	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	0.4	0.16	-0.35	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: Vaccines and anxiety in high-frequency surveys (n.p.s)

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

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

Table 7: Vaccine-eligibility and mental health in surveys (n.p.s)

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

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

Table 8: 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

List of Figures

List of Tables

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.

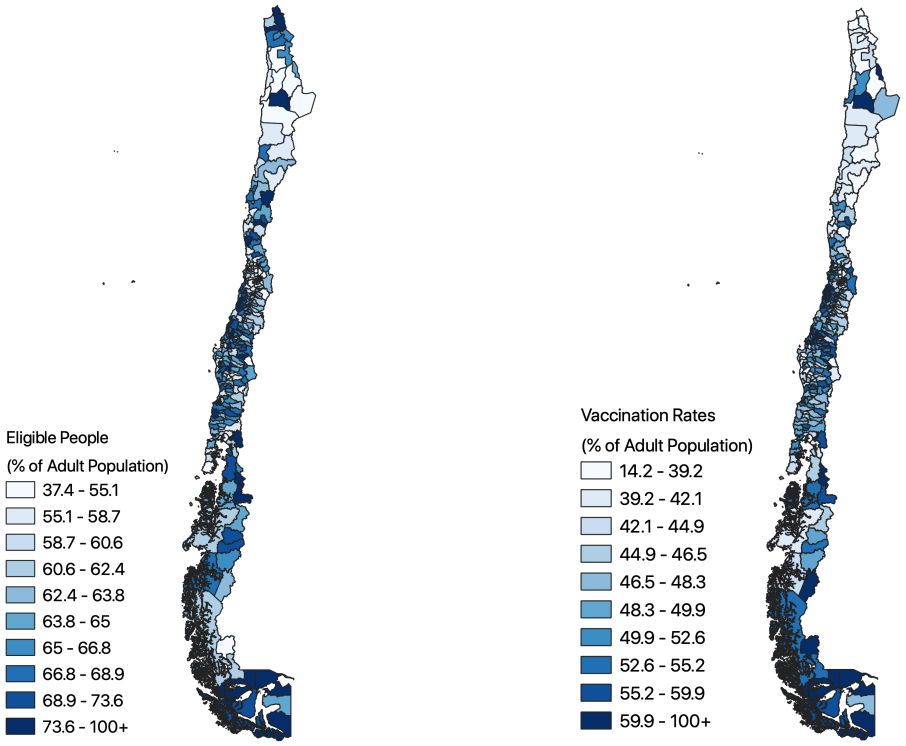
A.3 Regional Governors Election

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.

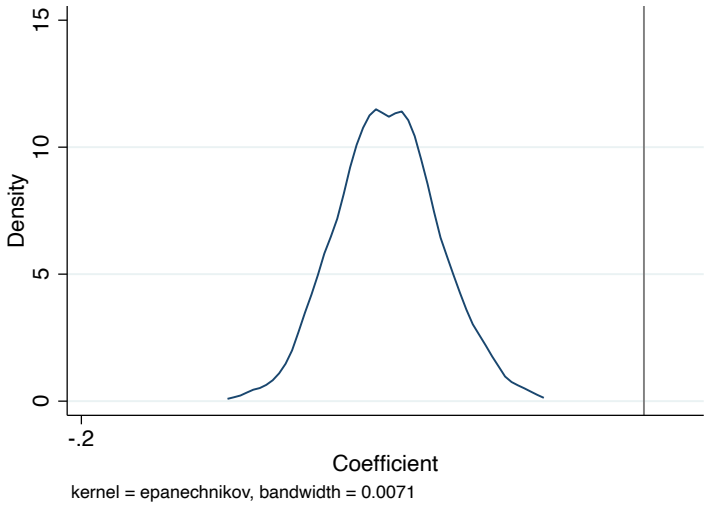
Figure A.1: Geographic Distribution of Eligible and Vaccinated People



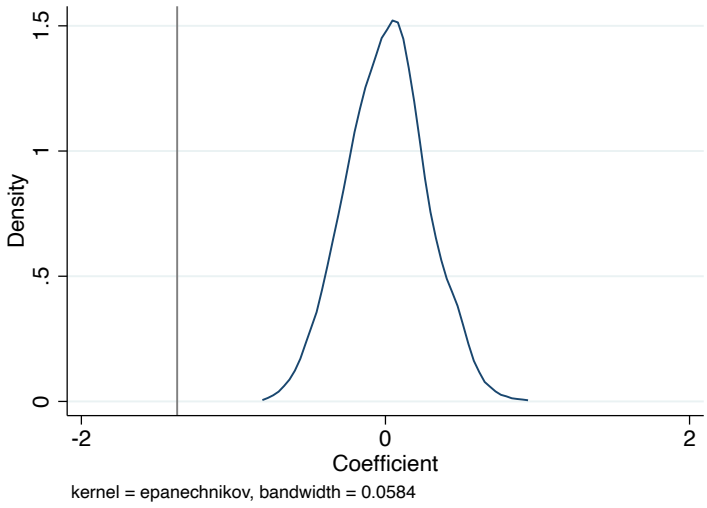
(a) Share of Eligible People (deciles) (b) Share of Vaccinated People (deciles)

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

Figure A.2: Randomization inference



(a) Effect of vaccines on political participation



(b) Effect of vaccines on vote share for incumbents

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

Table A.1: Descriptive statistics from the 2017 Census

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

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

Table A.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 and before the election

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

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

Table A.4: Vaccination and political participation when controlling non-linearly for unbalances

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

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

Table A.5: 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.6: 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.7: 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.8: 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.9: Partisanship in local councilors elections

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

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

Table A.10: Number of competitors per election

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

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

Table A.11: Partisanship in governors election

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

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

Table A.12: Summary statistics mental health measures

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

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

Table A.13: Eligibility and mental health in surveys, ordered responses (n.p.s)

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

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

Table A.14: Blank and null votes per election

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

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