# Political Information and Network Effects *

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**Abstract**

Why do political campaigns so often yield unexpected results? We address this question by separately estimating the direct effect of a campaign on targeted voters and the indirect effect on others in the same social environment. Partnering with a local NGO during Argentina's 2023 presidential election, we randomized the distribution of leaflets providing an expert assessment of the likely consequences of certain proposals by the outsider candidate Javier Milei. Exploiting Argentina's unique sub-precinct election reporting system, we show that the campaign reduced Milei's support among directly treated voters, as expected, but increased his support among untreated voters in treated precincts, producing a backfiring, net-positive effect for Milei. A pre-registered replication confirmed these opposite-signed effects. Using theory and a survey experiment, we show that the minority of voters who disbelieved the campaign were more motivated to discuss it with peers, convincing them to support Milei. This mobilization effect appears especially likely when campaigns criticize outsider candidates. Our results highlight how campaigns aimed at anti-elite candidates can unintentionally mobilize support for them.

**Keywords:** Political Information Campaign, Elections, Argentina, Milei, Network Effects, Spillovers

**JEL Classification:** P00, C93, D72

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# 1 Introduction

Political campaigns aim to influence voters' beliefs and motivations, which in turn are expected to shape their electoral choices. Although such campaigns are often tested in focus groups before being scaled up, many prove ineffective and some even backfire (e.g., Hewitt et al., 2024). Practitioners themselves are often wrong in predicting which messages will be persuasive on average (Broockman et al., 2024). Why are political campaign results so difficult to predict even in the absence of strategic responses by political opponents? We address this question by considering two political phenomena in combination: (1) campaigns generate both direct and indirect effects, and (2) different individuals may respond differently to the same political messages. We show that the direct and indirect effects can—and, in our context, do—have opposite signs, provide evidence for the mechanism underlying this divergence, and by doing so, shed new light on the functioning of political campaigns.

Direct effects of campaigns arise when voters are influenced by personal exposure to campaign messages. Indirect effects, in contrast, occur when directly exposed voters convince others who are not directly exposed to the campaign to change their voting behavior. Despite broad recognition that social networks play an important role in political persuasion and in the diffusion of political information (e.g., Ames et al., 2016; Arias et al., 2019; Cruz et al., 2020; Satyanath et al., 2017; Duarte et al., forthcoming), the empirical literature that separately estimates the direct and indirect effects of political campaigns aimed at changing voting behavior or boosting voter mobilization remains relatively small, despite its recent growth (Fafchamps and Vicente, 2013; Fafchamps et al., 2020; Grácio and Vicente, 2021; Marx et al., 2021; Bhandari et al., 2023; Enríquez et al., 2024; Blattman et al., 2024). The main challenge is that electoral outcomes are typically observed at overly aggregated levels, making it difficult to disentangle the two effects. Indirect effects are likely strongest within the most granular units used to measure electoral outcomes, i.e., electoral precincts. Consequently, existing studies have relied on geographical spillovers, which are ex-

pected to be limited relative to social spillovers among individuals within the same geographical unit, or on surveys requiring large amounts of sensitive data about social networks that are not always feasible, and about electoral choices that are not as reliable as official election results. In this paper, we address these measurement challenges in identifying direct and indirect effects of a political campaign, improving upon prior work by leveraging a unique feature of elections in Argentina: election results are reported for several groups of voters separately at the sub-precinct level.

We partnered with a local NGO that distributed two leaflets during the election campaign of the first round of the 2023 Argentine presidential election. Each leaflet presented factual and truthful information criticizing a distinct policy proposal from the outsider candidate Javier Milei, who ultimately won the election. One leaflet opposed replacing the public secondary school system with vouchers for private schools, while the other opposed abolishing the Central Bank and dollarizing the economy. Both leaflets included fact-checking by independent experts who argued that each proposal, if implemented, would hurt the poor: the first by substantially harming the education system, the second by causing significant devaluation and inflation.[1]

We designed an experimental intervention that allowed us to separately identify the campaign's effects on directly exposed voters and on unexposed voters living in the same electoral precinct, i.e., at the most granular level. This was made possible by three unique defining characteristics of Argentina's electoral system. First, as mentioned above, official election results in Argentina are reported at the sub-precinct *mesa* level ("mesa" is Spanish for "table"), with a typical precinct containing about eight mesas. Voters are assigned to mesas alphabetically by the first letter of their last name, making this assignment effectively random. Second, and crucial for campaign targeting,

[1] At that time, Argentina did not have sufficient dollar reserves to convert pesos at the official rate. Dollarization would therefore require a transition to a permanently higher dollar-peso exchange rate and a correspondingly higher price level. During this transition, inflation would be higher than under the status quo. Milei's unexpected victory in the PASO primary therefore set off a market reaction that led to a sell-off of pesos and a rapid depreciation of the currency. Once the transition was complete, dollarization would stabilize the exchange rate and bring inflation down toward U.S. levels, but experts and markets correctly anticipated that the short-run effect would be a major devaluation and a temporary spike in inflation.

political parties are legally required to publish membership lists with names and addresses, which uniquely identify the precinct and mesa where they are expected to vote. As our partner NGO had access to these lists, we were able to design a campaign that targeted members of *Partido Justicialista* (PJ) within specific mesas. Third, Argentina holds a mandatory primary (PASO) to determine eligible candidates for the general election. These primary results allowed us to identify pairs of mesas with comparable voter preferences, as measured by the PASO primary results and the share of voters on the party member lists, which were randomized into treatment and control.

The experimental intervention focused on Salta Province, one of the poorest in Argentina, where Javier Milei won his highest vote share (49%) in the PASO primary of August 2023. We first randomized all rural precincts into three groups: pure control precincts, where no mesa was treated; potential education-treatment precincts, where voters in some mesas could receive a leaflet focused on education; and potential dollarization-treatment precincts, where voters in some mesas could receive a leaflet focused on dollarization. For each mesa in potentially treated precincts, we then identified a comparable mesa in pure control precincts within the same department. Next, we randomly assigned treatment intensity in the potential treatment precincts, that is, the share of mesas to be treated, and randomly selected mesas for treatment among those with comparable pairs in control precincts. Due to budget constraints, only a subset of potentially treated precincts ultimately received treatment. The first intervention took place between the PASO and the first election round. A total of 5,000 education and 5,000 dollarization leaflets were mailed by the NGO to voters from party lists in mesas that we randomly assigned to treatment.

We developed a model showing that this design allows us to separately estimate the direct and indirect effects of the campaign within an electoral precinct. In particular, we derive the following closed-form relationship, which we take directly to the data: in a regression of mesa-level vote share on the share of voters treated in the mesa and the share of voters treated in the precinct, controlling for available addresses and fixed effects for comparable mesa pairs in treated and control precincts, the coefficient on the share treated in the mesa estimates the direct effect,

while the coefficient on the share treated in the precinct estimates the indirect effect. Intuitively, we compare voting results between pairs of mesas: treated mesas within treated precincts versus their counterparts in untreated precincts, keeping the share of voters treated in the precinct constant, to identify the direct effect; and untreated mesas in treated precincts versus their counterparts in untreated precincts, depending on the precinct's treatment intensity, to identify the indirect effect. The NGO sent leaflets to all available addresses within treated mesas. In all empirical exercises, we control for the available addresses of party members to account for the fact that voters treated within mesas had distinct political preferences.

We found that the education leaflet campaign worked as intended for those directly treated: every 100 mailed leaflets reduced Milei's electoral support by about 20 votes among voters directly exposed to the campaign. However, we found that the indirect effect backfired: every 100 mailed leaflets increased support for Milei by about 30 votes among voters who were not directly exposed but lived in the same precinct as those who received the leaflets. As a result, the education leaflet campaign was counterproductive on average, adding 10 votes in favor of Milei for every 100 leaflets sent. The dollarization treatment had no effect, either direct or indirect. Ex post, it became clear that due to the peso devaluation triggered by Milei's unexpected victory in the PASO primary, voters were already very familiar with the arguments presented in the dollarization leaflet.

To ensure that the opposite-sign result of the direct and indirect effects of the education leaflet campaign is real and replicable, we pre-registered it and conducted another intervention between the first round and the runoff election. In this experiment, a random sample of previously pure control precincts was reassigned to treatment, while all other aspects of the design were kept exactly the same. Another 5,000 education leaflets were distributed in this second experiment by our partner NGO, motivated by the aim of explaining the expert opinion about this Milei's policy proposal to voters. The results of the runoff experiment confirmed the opposite signs of the direct and indirect effects. We also used the runoff election to show that the results of the first-round experiment persisted, as both direct and indirect effects continued to be observed in the runoff

vote. In addition, we conducted a Monte Carlo placebo exercise, which showed that the results are extremely unlikely to be due to random chance.

We account for this seemingly surprising opposite-sign result by emphasizing heterogeneity in voter responses to information. A growing literature documents that people can react very differently to the same political information, depending on their priors and the context in which they receive it: some may be persuaded, others remain unaffected, and still others may be dissuaded or even outraged (e.g., Kendall et al., 2015; Adena et al., 2015; Barrera et al., 2020; Enríquez et al., 2024; Cruz et al., 2024; Jo et al., 2025; Fuller et al., 2025). Heterogeneity in voter responses to campaigns may also imply that individuals who react differently have varying propensities to discuss the campaign with others. If the majority are persuaded by campaign messages and finds them reasonable and convincing—and therefore emotionally neutral—they may feel little inclination to discuss them within their social networks. By contrast, the minority who are outraged by the campaign may be strongly motivated to explain their reaction to peers.[2] When this occurs, the average direct and indirect effects could have opposite signs. We construct an example within our theoretical framework to illustrate this possibility.

In order to test this mechanism, we conducted a survey in 2025 among a representative sample of rural Salta residents. Because the survey took place after the election, when Milei was already an established incumbent who had substantially moderated his actual policies compared to his radical campaign promises, we opted for a vignette experiment. Voters were asked to imagine themselves in an election prior to 2023, in which a traditional party candidate faced an outsider promising anti-corruption measures and change. They were presented with a campaign leaflet criticizing one of the outsider's proposals as harmful to the poor, citing expert opinion. To generate exogenous variation, respondents born on even dates were instructed to imagine believing the leaflet and therefore becoming less inclined to support the outsider, while those born on odd dates

[2]Social psychologists emphasize the role of emotions in persuasion (Redlawsk, 2006; Webster and Albertson, 2022).

were instructed to imagine rejecting it and becoming more inclined to support the outsider. We then examined whether and why respondents would discuss the leaflet with peers. Consistent with our mechanism, we found that those asked to imagine disbelieving the leaflet reported intending to discuss the campaign with a larger number of friends and neighbors. Moreover, they were more likely to state that the reason for such discussions was their motivation to convince others to vote like them, namely, for the outsider candidate. The survey results also suggest that this mechanism is particularly likely to operate when a campaign criticizes an outsider. Respondents randomized into imagining not believing the leaflet were more likely to report that they would have reacted differently had the leaflet criticized a mainstream candidate, indicating that voters perceive campaigns against outsiders as distinct from those against incumbents. A possible interpretation is that criticism of mainstream candidates is viewed by voters as common and expected (e.g., Guriev et al., 2021, 2025), whereas criticism of outsider candidates may be perceived as an attempt by corrupt elites to protect their position (Guriev and Papaioannou, 2022).

Our primary contribution is to two strands of literature: (1) the literature on spillover effects of information campaigns (e.g., Fafchamps and Vicente, 2013; Jaime Torres and Carlsson, 2018; Fafchamps et al., 2020; Grácio and Vicente, 2021; Marx et al., 2021; Enríquez et al., 2024), and (2) the literature on heterogeneity in voter responses to political information (e.g., Kendall et al., 2015; Adena et al., 2015; Barrera et al., 2020; Enríquez et al., 2024; Cruz et al., 2024; Jo et al., 2025; Fuller et al., 2025). We bring together the approaches of these two strands to provide new and complementary insights.

Our contributions are fourfold. First, on the methodological side, we present a theoretically grounded and intuitive approach to estimating direct and indirect effects within the same precinct by exploiting variation in the shares of treated voters in the mesa and in the precinct, respectively. Second, we provide robust and replicable evidence that spillover effects from directly treated voters to others in the same precinct can be sizable and statistically significant. Third, we show that direct and indirect effects need not be—and, in our case of a campaign criticizing a political outsider, are

not—of the same sign. To the best of our knowledge, this is the first paper to demonstrate that the direct and spillover effects of a political information campaign can have opposing signs. Fourth, we demonstrate that heterogeneity in responses to information campaigns extends to heterogeneity in voters' propensity to discuss politics within their social networks. In particular, voters who disagree with a campaign may be more motivated to counteract it than those who agree are motivated to spread its message.[3] Our survey experiment provides suggestive evidence that this dynamic may be a common feature of information campaigns against outsider candidates, possibly due to backlash against experts associated with the status quo. Taken together, our findings—that indirect effects can be large, of opposite sign to direct effects, and potentially dominant—help explain why political practitioners who test campaigns in focus groups often obtain unexpected results in the field. More broadly, our results shed new light on the effects of political campaigns and their role in the diffusion of political information.

More generally, our work is related to the literatures on the effects of exposing people to political information and information campaigns (e.g., Kendall et al., 2015; Kalla and Broockman, 2018; Pons, 2018; Barrera et al., 2020; Nyhan, 2021; Arias et al., 2022; Galasso et al., 2023; Ajzenman and Durante, 2023; Le Pennec and Pons, 2023), and on persuasion in politics (see, e.g., Gerber et al., 2009; DellaVigna and Kaplan, 2007; Enikolopov et al., 2011; DellaVigna and Gentzkow, 2010; DellaVigna et al., 2014). Our analysis identifies a novel mechanism that helps explain why anti-elite outsider candidates are difficult to defeat, thereby linking our results to the literature on the electoral success of anti-elite politicians across many countries (see Guriev and Papaioannou, 2022, for a survey). Key contributions to this literature include Dornbusch and Edwards (1990); Acemoglu et al. (2013); Algan et al. (2017); Allcott and Gentzkow (2017); Edwards (2019); Bursztyn et al. (2020); Rodrik (2021); Guriev et al. (2021); Danieli et al. (2022); Guiso et al. (2024); Galasso et al. (2024).

[3]Related literatures consider the role of emotions in reactions to online messages (see, for instance Brady et al., 2017; Rathje et al., 2021; Beknazar-Yuzbashev et al., 2025) and positive vs. negative campaigning (e.g., Polborn and Yi, 2006; Lau et al., 2007; Bernhardt and Ghosh, 2020).

The remainder of the paper is organized as follows. Section 2 provides background on Argentine politics and the 2023 presidential election. Section 3 outlines the theoretical framework. Section 4 describes the experimental design and data sources. Section 5 presents the main experimental results and discusses their internal validity. Section 6 reports the survey findings, offering evidence on the underlying mechanism. In Section 7, we discuss external validity. Finally, Section 8 concludes.

# 2 Background: Presidential election in Argentina in 2023

Argentina is a presidential republic in which the president serves as both the head of state and the head of government. During the twentieth century, the country oscillated between periods of military rule and the rise of populist governments associated with Juan Perón and his political legacy. Since the restoration of democracy in 1983, the presidency has largely been held by individuals from either the Peronist party or from non-Peronist center-right parties.

The Peronist Party (officially the Justicialist Party, *Partido Justicialista*) is among the most enduring political forces in Argentina. Founded by Juan Perón and his second wife Eva, it emphasizes social justice, a strong welfare state, and state intervention in the economy to promote national independence. In the last two decades, a more left-leaning current of Peronism, known as Kirchnerism, has gained prominence. Led by former presidents Néstor and Cristina Kirchner, this faction expanded social welfare programs and increased state control over key industries, pushing Peronism further to the left and reshaping Argentina's political landscape. Historically, opposition to Peronism has come from center-right parties under different banners. Since 1983 until 2023, two non-Peronist presidents have been elected: Raúl Alfonsín of the Radical Civic Union (1983–1989) and Mauricio Macri, founder of the Republican Proposal party (2015–2019).

Argentina's current electoral system has been in place since 2009, when the PASO system (*Primarias Abiertas, Simultáneas y Obligatorias*, or "Simultaneous and Mandatory Open Primaries")

was introduced during the first term of President Cristina Kirchner. These primaries, held roughly two months before the general election, determine which parties qualify to compete and which candidates will represent them. Voters select a single candidate within their preferred party, and only parties receiving more than 1.5% of the national vote advance to the general election. The party nominee must be the candidate who secured the most votes in the primary. In the general election, a candidate can win the presidency outright by receiving either 45% of the vote or at least 40% with a ten-point lead over the nearest rival. Otherwise, a runoff is held between the top two contenders. Voting is compulsory, with limited exceptions such as illness or being abroad. In practice, turnout has varied in the 70–85% range. Although nonparticipation carries a nominal fine of 100 pesos (about 20 U.S. cents in 2023), enforcement is rare.

Javier Milei's swift political rise and eventual victory in the 2023 presidential election were widely regarded as unexpected. Campaigning as an anti-establishment outsider with radical libertarian views, he proposed dollarizing Argentina's economy, dismantling the Central Bank, reducing public spending by the equivalent of 15% of GDP, eliminating entire ministries and implementing large-scale public sector layoffs, and introducing a voucher-based model for education. His surprise first-place finish in the August 2023 PASO primaries unsettled financial markets: the government devalued the peso by 18% the following day, while the currency lost nearly 30% of its value in the parallel markets.

Milei's main competitors in the 2023 election came from both the Peronist and center-right camps. On the left, Sergio Massa served as the incumbent minister of the economy under President Alberto Fernández; on the center-right, Patricia Bullrich had been minister of security under Mauricio Macri (2015–2019). In the PASO primaries, each emerged as the leading candidate in their respective coalitions. Milei won with 29.86% of the vote, while Massa came second with 21.43% and Bullrich third with 16.81%. As coalitions, Juntos por el Cambio (Bullrich's bloc) received about 28%, and Unión por la Patria (Massa's bloc) secured approximately 27.3%. Also advancing to the October first round were Juan Schiaretti (3.7%) and Myriam Bregman (2.6%) as

minor-coalition candidates.

Between the PASO primaries (often considered "round zero") and the first round of voting on October 22, 2023, both Milei and Massa concentrated their attacks on Bullrich, who was widely seen as the most competitive candidate in a potential runoff against either of them. This strategy worked to Massa's advantage: in the first round, he finished first with 36.8% of the vote, followed by Milei with 29.9%. Bullrich received 23.8% and was eliminated from the race.

For the runoff, the center-right coalition Juntos por el Cambio decided to support Milei. Leading party figures, including Patricia Bullrich and former president Mauricio Macri, campaigned on his behalf in exchange for assurances of policy moderation and a commitment to include center-right politicians in his administration. On November 19, 2023, Milei decisively defeated Massa in the runoff, winning 55.7% of the vote.

A key feature of Argentina's electoral system, especially relevant for our analysis, is that all election results, including those for presidential contests, are reported at the level of sub-precincts (known as *mesas*, or "tables"). Voters are assigned to mesas exogenously: within each precinct, they are grouped by the alphabetical order of their last names. Each mesa can accommodate up to 350 registered voters; when this limit is exceeded, additional mesas are created. Certain categories of voters, such as those requiring special assistance (e.g., individuals with disabilities), may be assigned to specially designated mesas equipped to meet their needs.

# 3 Conceptual framework

Our aim is to distinguish between the direct and indirect effects of a political information campaign. We define the direct effects as the impact of campaign leaflets on the voting behavior of individuals who receive them, relative to voters who neither receive leaflets nor have indirect exposure through their social networks. The indirect (spillover) effects are defined as the impact on voters who do not receive leaflets themselves but live in close proximity (within the same precinct) to those who

do, thereby being exposed through social interactions. In both cases, the relevant comparison is with voters who have neither direct nor indirect exposure to the campaign.

Since individual-level voting data are unavailable, we rely on mesa-level results to estimate these effects. To fix ideas, we introduce a simple probabilistic voting model. First, we define voter preferences and derive the equilibrium vote shares in the absence of any political campaigns. Consider a precinct $p$ populated by a continuum of voters with unit mass, indexed by $i$. These voters belong to $G$ groups, indexed by $g \in \{1, \ldots, G\}$, each characterized by distinct policy preferences $U_g$. The share of voters in each group $g$ is $n_g$.

When voting in elections, each voter is assigned to one of $M$ mesas, denoted $m(i)$. We assume that the share of voters in mesa $m$ is $l^m$. Importantly, all mesas have the same distribution of groups. In other words, the assignment of voters to mesas is orthogonal to their policy preferences, which determine their allocation to groups $g(i)$.

Two candidates, $A$ and $B$, distinguished by their policy proposals, compete for office. The expected utilities from the proposals of candidates $A$ and $B$ for all voters in group $g$ are denoted $U_g^A$ and $U_g^B$, respectively. Voters make their electoral choices based on these policy preferences as well as on an idiosyncratic taste shock that is unrelated to policies. Specifically, voter $i$ in group $g$ votes for candidate $A$ if and only if

$$U_g^A > U_g^B + \varepsilon_i,$$

where $\varepsilon_i$ reflects a taste shock orthogonal to political preferences. As is standard in the literature, we assume that $\varepsilon_i$ for voter $i$ in group $g$ is i.i.d. and distributed uniformly on $[-K_g, K_g]$. The density $\frac{1}{2K_g}$ captures how sensitive voters in group $g$ are to changes in policy priorities. Importantly, $\varepsilon_i$ is independent of the mesa assignment of voter $i$ as well as of the shocks $\varepsilon_j$ experienced by other voters.

The share of votes for candidate $A$ among voters in group $g$ within mesa $m$ is therefore

$$Y_g^m = \frac{1}{2} + \frac{1}{2K_g}\left(U_g^A - U_g^B\right),$$

whereas the total vote share of candidate $A$ in mesa $m$ is the sum of the votes across groups weighted by their group size:

$$Y^m = \frac{1}{2} + \sum_g \frac{n_g}{2K_g}\left(U_g^A - U_g^B\right).$$

Note that, absent any intervention, the vote share does not depend explicitly on the mesa, since all mesas are identical by construction. This expression thus represents the baseline equilibrium vote share for candidate $A$ in the absence of a political information campaign.

**Adding direct effects of the political information campaign.** Let us now suppose that we treat a share $\tau^m$ of voters in mesa $m$ with some information about candidate $A$, and that voters in different groups are reached by the campaign with different probabilities. In particular, among voters treated in mesa $m$, share $\alpha_g$ are from group $g$. (If all groups of voters were equally likely to be treated, we would have $\alpha_g = n_g$). Denote the fraction of group $g$ members that are treated in mesa $m$ by $\tau_g^m = \frac{\alpha_g}{n_g}\tau^m$.

Suppose that the expected utility from electing candidate $A$ increases by $r_g$ for voters in group $g$ (which may be positive or negative). We assume that this shift is identical for all voters within a group but may vary across groups. In this case, the share of voters from group $g$ in mesa $m$ who support candidate $A$ is

$$Y_g^m = \frac{1}{2} + \frac{1}{2K_g}\left(U_g^A - U_g^B\right) + \frac{1}{2K_g} r_g \tau_g^m,$$

and the total share of votes for $A$ in mesa $m$ is

$$Y^m = \frac{1}{2} + \sum_g \frac{n_g}{2K_g}\left(U_g^A - U_g^B\right) + \sum_g \frac{n_g}{2K_g} r_g \tau_g^m.$$

The last term can be rewritten as

$$\left(\sum_g \frac{\alpha_g}{2K_g} r_g\right) \tau^m.$$

Thus, the direct effect of a political campaign is proportional to the share of treated voters in a mesa, $\tau^m$. The coefficient on $\tau^m$ depends on three factors: which groups are relatively more likely to be reached by the campaign ($\alpha_g$), how strongly the campaign shifts their expected benefit from voting for candidate $A$ ($r_g$), and how responsive each group is to policy differences in the first place ($K_g$).

**Adding spillover effects.** To model spillover effects of the campaign to untreated voters in the same precinct, let us introduce probabilities $\eta_{hg}$ that a given member of group $g$ interacts with some (randomly chosen) member of group $h$ in a given time period, and suppose that *if* that member in group $h$ was treated with the information treatment, this interaction changes the utility of the voter in group $g$ from voting for candidate $A$ by $s_{hg}$ on average, which may be positive or negative.[4] Importantly, these numbers are the same regardless of the mesas in which these voters vote; as long as the sender was subject to treatment, the receiver's utility from voting for candidate $A$ changes by the specified amount. Denote the share of treated members of group $g$ across all mesas by $\tau_g = \sum_m l^m \tau_g^m$; then the total share of treated voters in the whole precinct, which we denote by $\tau^p$, satisfies $\tau_g = \sum_m l^m \tau_g^m = \frac{\alpha_g}{n_g} \sum_m l^m \tau^m = \frac{\alpha_g}{n_g} \tau^p$ for each group $g$. Now the share of voters from group $g$ in mesa $m$ voting for candidate $A$ may be expressed as

$$Y_g^m = \frac{1}{2} + \frac{1}{2K_g}\left(U_g^A - U_g^B\right) + \frac{1}{2K_g} r_g \tau_g^m + \frac{1}{2K_g} \sum_h \eta_{hg} s_{hg} \tau_h,$$

[4]The simplest case would be $s_{hg} = r_g$, but we consider a more general case where the effect may also depend on the receiver of the original message $h$ who communicates to $g$.

and the total vote share of $A$ in this mesa is

$$Y^m = \frac{1}{2} + \sum_g \frac{n_g}{2K_g}\left(U_g^A - U_g^B\right) + \sum_g \frac{n_g}{2K_g} r_g \tau_g^m + \sum_g \frac{n_g}{2K_g} \sum_h \eta_{hg} s_{hg} \tau_h.$$

As before, this may be rewritten as

$$Y^m = \frac{1}{2} + \sum_g \frac{n_g}{2K_g}\left(U_g^A - U_g^B\right) + \left(\sum_g \frac{\alpha_g}{2K_g} r_g\right)\tau^m + \left(\sum_{g,h} \frac{n_g}{2K_g}\frac{\alpha_h}{n_h}\eta_{hg} s_{hg}\right)\tau^p. \tag{1}$$

Here, only the second term depends on mesa $m$, and it is proportional to the share of voters treated there. The last term is proportional to the share of voters treated in the entire precinct, $\tau^p$, with the coefficient of proportionality depending on the extent to which different groups of voters are receptive to new information, which other groups they interact with, and how likely these groups were to be treated. Note that the total effect depends both on the groups' direct reaction to political information, $r_g$, and on the network structure, $\eta_{hg}$, together with the inter-group persuasion parameters, $s_{hg}$. As the first term depends only on the candidates' positions, it is constant for national elections.

Equation (1) thus separates the *direct effect*, operating at the mesa level, from the *indirect effect*, operating at the precinct level. Note that everyone within the same precinct is subject to the same indirect effect, either through exposure to across-mesa or within-mesa spillovers. Therefore, once we control for the share of voters treated at the precinct level, the remaining between-mesa variation identifies the direct mesa-level effect even in the presence of intra-mesa spillovers.

This equation can be taken directly to the data. In particular, if we regress Milei's vote share in a given mesa on $\tau^m$ and $\tau^p$, the respective coefficients identify the magnitudes of the direct and indirect effects of our treatment, provided that $\tau^m$ and $\tau^p$ vary exogenously and separately and that the shares of voters potentially reachable by the campaign ($\alpha_g$) are properly controlled for.

**How can this framework be applied to the experimental setting?** The key assumption of the model is that the distribution of groups across treated and untreated mesas is the same, i.e., that the division of a precinct into mesas is orthogonal to other characteristics of individuals living in the precinct that are related to voting decisions, such as their policy preferences. In our setting, this is a reasonable assumption because voters are assigned to mesas alphabetically.

Equation (1) suggests that, in order to capture the direct and indirect effects of a political campaign, we should use the share of individuals treated in a given mesa and the share of individuals treated in the entire precinct, respectively. This provides an intuitive way to link campaign intensity to its effects and allows us to identify these effects separately whenever the two shares are not too highly correlated. In the experimental design described in detail in the next section, we create an independent component of variation between the shares of voters treated in a mesa and in its precinct by randomly varying precinct-level treatment intensity, i.e., the share of treated mesas within the precinct.

In the model above, we allowed voters to be heterogeneous and to belong to different groups. We also allowed the probability of different groups being treated within a mesa to vary. These groups may capture a range of characteristics, including demographics, socio-economic status, political preferences, openness to receiving political information from our leaflets, willingness to open the envelopes in the first place, and willingness to discuss politics with members of other groups. The model incorporates these groups without affecting tractability. These features of the model are crucial for its applicability, as they reflect our experimental setting. Since the NGO had access only to the personal information of party members, and since voters with different levels of political interest vary in their propensity to open an envelope marked as containing political information, our treatment inevitably reached a nonrandom subset of voters within randomly chosen mesas. Yet, as the model shows, this does not compromise the validity of our approach, provided that we control for the share of voters with available addresses, namely, registered party members.

Our approach assumes additivity and linearity of the effects. Indeed, individuals treated directly

are still subject to indirect effects. We consider this plausible: an individual receives a myriad of signals, and receiving our leaflet does not make them immune to persuasion by friends, for instance. It is reasonable to expect these small pieces of information to accumulate without requiring explicit interaction terms. One could, of course, adopt a different view and argue that the leaflet pushes a voter to decide one way or another, thereby making them unaffected by indirect effects. However, introducing such intricacies would substantially complicate the model and render the estimates sensitive to assumptions about the functional form.

Nevertheless, we have strong reasons to believe that our approach is valid. First, the share of voters per mesa that were treated is small (between 5% and 23%), so any nonlinearities are likely negligible, as they would be second-order. Second, we also estimate indirect effects by focusing only on untreated individuals, comparing untreated mesas in treated and untreated precincts; the results are similar. One might argue that indirect effects need not be linear: they could be concave if repeated contacts from multiple treated individuals are redundant, or convex if reinforcement from several friends produces a cumulative effect. Yet, given the modest scale of our interventions, such nonlinearities are also unlikely to be pronounced. We therefore expect indirect effects to be approximately proportional to the share of treated individuals in the precinct.

We do not explicitly model how individuals who are indirectly affected by treatment might, in turn, influence others (i.e., higher-order spillover effects). This does not mean such effects are excluded; rather, our estimates of the indirect effect should be understood as capturing the sum of all spillovers from treated individuals to the broader population. Mathematically, this interpretation is straightforward if effects are additive or if nonlinearities are small. Throughout, when we refer to indirect effects, we mean all consequences of treated individuals' communication with others, including any further downstream communication.

**Example with positive direct and negative indirect effects.** Our model allows us to identify situations where the direct effect of a political campaign is positive while the indirect effect is

negative.

Consider the simplest case of two groups. For example, group 1 consists of the voters who are positively predisposed to the mainstream candidate $A$; group 2 consists of the voters who are negatively predisposed to $A$ and positively predisposed to $B$, who is a political outsider. The shares of voters in these groups are $n_1 > 0.5$ and $n_2 = 1 - n_1$, respectively. Let us assume that the two groups have the same density, namely, the same within-group dispersion of taste shocks toward the candidates orthogonal to policy preferences, $K_1 = K_2 = K$.

Suppose also, for simplicity, that both groups are equally likely to be treated within each treated mesa: $\alpha_1 = n_1$ and $\alpha_2 = n_2$. Suppose the political campaign criticizes $B$, and voters from group 1 react to the campaign's message positively by increasing their support for the mainstream candidate $A$. In contrast, group 2 voters react negatively to the campaign. This means that $r_1 > 0$ and $r_2 < 0$. For simplicity, let us assume that the absolute magnitudes of the treatment effects are the same: $r_1 = -r_2 = r$.

After the campaign reaches the voters, they socialize. Let us make the extreme assumption that voters from different groups do not talk to each other; that is, there is extreme social segregation or extreme homophily: $s_{12} = s_{21} = 0$. However, voters do interact with each other within the same groups and, through these interactions, they influence each other. Again, assume that in group 1, these interactions increase support for candidate $A$, and in group 2 the effect is negative: $s_{11} > 0$ and $s_{22} < 0$. Similarly, let us assume that the absolute magnitudes are the same: $s_{11} = -s_{22} = s$.

Equation (1) implies that, in this example, the campaign's direct effect per unit of treatment is equal to

$$\sum_g \frac{\alpha_g}{2K_g} r_g = \frac{n_1 r_1 + n_2 r_2}{2K} = \frac{r(n_1 - n_2)}{2K},$$

while the indirect effect is

$$\sum_{g,h} \frac{n_g}{2K_g} \frac{\alpha_h}{n_h} \eta_{hg} s_{hg} = \frac{n_1 \eta_{11} s_{11} + n_2 \eta_{22} s_{22}}{2K} = \frac{s(n_1 \eta_{11} - n_2 \eta_{22})}{2K}.$$

The situation where the direct effect is positive and the indirect effect is negative therefore arises if and only if the first group is larger than the second group but individuals in the second group interact with far more voters than those from the first group:

$$1 < \frac{n_1}{n_2} < \frac{\eta_{22}}{\eta_{11}}.$$

In this simple example, as we only have two groups, the left-hand-side inequality is tantamount to the campaign having an intended effect on a majority of voters ($n_1 > 1/2$). The right-hand-side inequality implies that the minority voters for whom the campaign backfires talk to so many others that their mobilization compensates for their group's small size.

# 4 Experimental design, data, and balance

In this section, we describe our experimental design and the additional information we relied on for this design.

## 4.1 Experimental design

In August 2023, Javier Milei's unexpected victory in the PASO mandatory primaries prompted various political actors and nonpartisan NGOs to inform voters about the potential consequences of his unconventional and potentially harmful policy proposals. We used this setting to experimentally study the effects of a political information campaign based on factual evidence about these proposals. To this end, we partnered with an NGO that distributed leaflets to voters and designed the intervention in Salta, one of the most remote and poorest provinces in Argentina, where campaigning was limited. This context allowed us to measure rigorously both the direct and indirect effects of the campaign.

The treatment consisted of leaflets mailed to voters in sealed envelopes. Each envelope con-

tained a disclaimer stating that, by opening it, recipients consented to receive political information. The envelopes also provided a WhatsApp contact number for inquiries, as WhatsApp is the most common channel of communication in Argentina. The information presented in the leaflets was drawn from reputable sources and fully referenced; no material was expressed as an opinion. Two different leaflets addressed the consequences of two distinct policy proposals advanced by Milei. The first treatment warned that abolishing free public education and replacing it with a voucher system would lead to worse educational outcomes (the "education" treatment). The second treatment warned that Milei's proposed macroeconomic policy would exacerbate inflation (the "dollarization" treatment). The leaflets and their English translations are presented in Appendix Figures A1–A4. Each leaflet consisted of a single sheet of paper, folded before being placed in the envelope, thereby producing four printed half-pages.

For the experiment, we selected Salta Province, as it is relatively remote, with limited prior campaigning efforts and therefore a lower risk of contamination from other campaigns. In addition, Milei obtained the highest vote share in this province during the PASO primary (see Appendix Figure A5). In Salta, Milei secured 49.39% of the vote in the PASO, 40% in the first round, and 58% in the runoff. Salta is located in the northwest of the country. Its population is 1,440,672 (ranked 7th), and its area is 155,488 sq. km (ranked 6th). The capital city, also named Salta, is home to 43% of the province's residents and is the 7th most populous city in Argentina. Along with neighboring Formosa and Jujuy, the province is among the poorest in the country, with a regional GDP per capita approximately 20% below the national average.

To reach voters in Salta, the campaign relied on the fact that in Argentina political parties are required to publish membership lists. As the largest political party in Argentina is the Justicialist Party (*Partido Justicialista*, henceforth PJ), our partner NGO used its list to mail leaflets to voters in mesas that we had randomized for treatment. Out of approximately 1 million registered voters in Salta, 100,000 are members of the PJ, roughly 60% of whom reside outside the provincial capital. In Section 5.6, we discuss how targeting PJ voters affects our results.

Specifically, the treatment was implemented as follows. Salta Province is divided into 23 departments (*departamentos*), including the capital city. We excluded the capital from the study at the outset, since in a densely populated urban area our information campaign was more likely to spill over across precinct boundaries. Another reason for this exclusion was that voters in the capital were more exposed to competing political advertising, which could have diluted the visibility of our intervention. A typical department contains roughly 10 precincts, and each precinct is subdivided into several *mesas*, with voters assigned alphabetically across mesas. We excluded mesas in which fewer than 5% of registered voters were PJ members according to the party list. In such mesas, even if selected for treatment, the campaign would not have reached a sufficient number of eligible voters for the effect to be detectable at the mesa level. After applying these restrictions, the final sample comprised 1,273 mesas in 209 precincts across 22 departments. Appendix Figure A6 displays a map of Salta Province showing department boundaries, the locations of all 209 precincts, and the distribution of economic development levels by decile.

Within each of the 22 departments, we randomly assigned precincts to one of three groups. Approximately 50% were allocated to the *pure control* group, where no mailings were sent during the first-round experiment. About 25% were assigned to the *potential education treatment* group, and another 25% to the *potential dollarization treatment* group. This design ensured that no precinct received two different types of leaflets.

For each mesa in each precinct in the two potential treatment groups, we selected a matched mesa from the control group precincts to minimize differences using the following lexicographic norm:

$$\text{floor}(\textit{Milei\_share} \times 100) \times 10000 + \text{floor}(\textit{Turnout} \times 100) \times 100 + \text{floor}(\textit{PJ\_share} \times 100), \quad (2)$$

where *Milei_share* is the share of votes for Milei, *Turnout* is voter turnout as a proportion of registered voters, and *PJ_share* is the share of PJ members in the mesa, that is, the share of available

addresses. The floor function rounds each value down to the nearest integer.

In the baseline specification, we retained only matched pairs where the difference in Milei's vote share between treatment and control mesas was no greater than 2.5%. Because the matching procedure did not require unique control mesas, some treated mesas were paired with the same control mesa; thus, we refer to them as matched sets rather than matched pairs.

After that step, the "treatment" precincts were randomly divided into two categories: *high-intensity* treatment precincts, where we aimed to treat approximately two-thirds of mesas (or the closest feasible rational number), and *low-intensity* treatment precincts, where we aimed to treat roughly one-third of mesas. Within each precinct, the specific mesas to be treated were randomly selected among those eligible. The high–low split was determined so that the total number of leaflets would be close to 5,000. Since the printing office processed orders in batches of 5,000, targeting this number minimized the average cost per leaflet. In all treated mesas, the NGO sent leaflets to all voters with available addresses (i.e., PJ members listed in the party registry).

As detailed below, after the publication of the first-round election results, we estimated the direct and indirect effects of both campaigns. The results showed no effect of the dollarization treatment and opposite-sign effects of the education treatment. To verify whether the education treatment results were genuine and replicable, we pre-registered them and repeated the experiment before the runoff, using exactly the same design. For the runoff, we focused on the 50% of precincts that had been pure controls in the first round. These precincts were again divided into pure controls and potential education treatments, following the same procedure as in the first round. The only key difference was that for matching treated and control mesas (Equation (2)), instead of the PASO results (Milei vote share and turnout), we used the respective results of the first round election. This yielded an experiment comparable in scope to the education treatment in the first round, with 5,000 leaflets distributed.

In total, across the two experiments, 15,000 leaflets were mailed: 10,000 in the first round—5,000 with the education treatment in 165 mesas and 5,000 with the dollarization treatment in 163

mesas—and 5,000 in the runoff with the education treatment in 154 mesas.

To the best of our knowledge, all envelopes were mailed as planned, even though, for the second experiment, the NGO had very limited time to print and distribute leaflets between the two rounds of elections. The timing of delivery, however, remains uncertain. Although, based on standard transit times for Argentine mail, all envelopes should have arrived at least 3–4 days before the election (with some shipped even earlier), concerns remain about whether voters in remote areas actually received them on time, or even whether delivery was attempted at all. While some envelopes were returned as undeliverable, we received no feedback from the most remote locations, raising doubts about the reliability of the mail service there. It should be noted that some returned envelopes were expected in all departments, as the party address lists used by the NGO are not regularly updated.

In a global study on the quality of mail services (Chong et al., 2014), Argentina ranked 84th out of 159 countries. For these reasons, we excluded the three departments located more than five hours' driving distance from Salta's capital city from our analysis. Our understanding is that mail is delivered by car, and it is unlikely that large, unexpected batches of mail were reliably delivered to places requiring more than a full workday for a round trip. These remote departments include 12 mesas with the education treatment and 13 mesas with the dollarization treatment in the first-round experiment and 10 mesas with the education treatment in the runoff experiment.

Appendix Figures A7, A8, and A9 show maps of precincts with treated and control mesas for the education and dollarization treatments in the first round and for the education treatment in the runoff, respectively, along with the excluded remote departments.

## 4.2 Data

We use mesa-level official election data from Argentine government websites.[5] These data include the total number of registered voters, voter turnout, the number of valid votes, and the vote shares of all parties and candidates.

We also use socioeconomic data from the 2022 census.[6] The driving time between the provincial capital (the city of Salta) and the departmental capitals was obtained using Google Maps. Individual names and addresses of PJ members were published on the PJ website and downloaded by our partner NGO, which also administered the leaflet mailing. The NGO shared with us the total number of available addresses by mesa, which we use as a control variable in our analysis. All variables used in the analysis of the experiments are summarized in Appendix Table A1 for the full sample of all mesas.[7]

## 4.3 Balance

In all experiments, we randomized precincts into treatment and control, and then randomized which mesas to treat within treated precincts. To assess whether randomization was successful, we regress each pre-treatment observable separately on a dummy indicating whether a mesa was treated, including fixed effects for matched mesa sets in treatment–control precincts and clustering standard errors at the precinct level.

The pre-treatment observables include mesa-level vote shares for all candidates and turnout

[5]The PASO data are available at: https://www.argentina.gob.ar/sites/default/files/dine-resultados/2023-PROVISORIOS_PASO.zip. The first-round and second-round data are available at: https://www.argentina.gob.ar/sites/default/files/2023_generales_1.zip. All election data were last accessed on March 12, 2024.

[6]https://censo.gob.ar/index.php/datos_definitivos_salta/, accessed March 12, 2024.

[7]Some mesas are missing from the official election results. Under the National Electoral Code (Law No. 19.945, Articles 112–114) (https://www.argentina.gob.ar/normativa/nacional/ley-19945-19442/actualizacion, accessed October 10, 2025), certain mesas are excluded because they were annulled due to procedural irregularities, not opened on election day, or had their results contested or impugned and therefore withheld pending judicial review. Consequently, the number of observations for the first-round and runoff election results is slightly lower than in the PASO round in Appendix Table A1.

in the PASO primary, the shares of available addresses at both the mesa and precinct levels, the number of mesas in a precinct, mesa size (measured as the total number of registered voters), latitude, and longitude. For the runoff experiment, we also include first-round vote shares and turnout.

Overall, randomization appears to have worked. We find only two significant coefficients in 34 regressions, which is consistent with random variation. Results are presented in the Appendix in the form of coefficient plots using standardized pre-treatment observables. Appendix Figure A10 reports the differences in pre-treatment characteristics between treated and non-treated mesas in the first-round education experiment; Figure A11 presents the same estimates for the dollarization treatment; and Figure A12 shows the estimates for the runoff education experiment. Only longitude appears significant in some specifications. In our baseline analysis, we therefore include controls for both longitude and latitude.

We refer to these tests as extensive-margin balance tests because they assess whether treated mesas differ systematically from control mesas. Our main empirical exercise, however, is more demanding. As implied by our theoretical framework, the key explanatory variables are the shares of treated voters at the mesa and precinct levels. These are defined as the product of the randomized mesa treatment status and the share of available voter addresses, which in turn depend on the political preferences of the electorate, since they are drawn from party lists. For this exercise to be meaningful, we must control for the share of available addresses at the mesa level. Balance on the intensive margin requires that pre-treatment observables are uncorrelated with the shares of treated voters in both the mesa and the precinct, conditional on the shares of available addresses at both levels.

Appendix Figure A13 reports the coefficient plots for the intensive-margin balance tests for the first-round education experiment. In Panel A, we regress standardized pre-treatment observables on the shares of voters treated in the mesa and in the precinct, controlling for the shares of available addresses at both levels and including mesa matched-set fixed effects, with standard errors clus-

tered at the precinct level. In all but one case, there is no statistically significant relationship. The exception is the PASO vote share for Bullrich, which is significantly negatively correlated with the share of voters treated at the precinct level in the first-round education experiment. However, Panel B shows that when we control more flexibly for the shares of available addresses—by including fixed effects for each decile of the share of available addresses at both the mesa and precinct levels—the intensive-margin balance is restored: pre-treatment characteristics are no longer correlated with our main explanatory variables. This test defines our baseline specification, in which we flexibly control for the shares of available addresses.

Appendix Figure A14 reports the intensive-margin balance tests for the first-round dollarization experiment. We find no evidence of imbalance, except for coefficients on geographic coordinates, which are small in magnitude but marginally significant; these are controlled for in the main analysis.

Finally, Appendix Figure A15 presents the intensive-margin balance tests for the runoff education experiment. Panel A shows several important imbalances in pre-treatment voting results, particularly for Bullrich and Massa vote shares in the first round of the election. To address these, we apply the entropy balancing procedure developed by Hainmueller (2012). Panel B reports the same balance tests after applying the entropy balancing weights. These yield no significant relationships, with the exception of longitude. Accordingly, in our baseline specification for this experiment, we control for geographic coordinates and use entropy balancing weights.

Importantly, we also show below that, for all experiments, the results are largely robust across specifications and not sensitive to the inclusion or exclusion of controls for pre-treatment election results and other pre-treatment observables.

# 5 Main results

In this section, we present the results of our main experiments.

## 5.1 Estimation models

Guided by Equation (1), we estimate the following regression model:

$$V_{pm} = \beta \times \tau_m + \gamma \times \tau_p + \delta \times X_m + \phi_{\text{pair}} + \varepsilon_{pm}, \tag{3}$$

where $p$ and $m$ index precincts and mesas, respectively. $V_{pm}$ denotes Milei's vote share in mesa $m$ of precinct $p$. As in our model, $\tau_m$ and $\tau_p$ represent the shares of treated individuals in the mesa and in the precinct, respectively. $X_{pm}$ is a set of controls. $\phi_{\text{pair}}$ are matched-set fixed effects, which link each treated mesa to its corresponding control mesa. The standard errors are clustered at the precinct level. Typically, this is a one-to-one relationship, but not always, as some mesas serve as controls for multiple treated mesas. Note that these fixed effects subsume department fixed effects, since all matched sets were chosen within the same department. In all regressions, we control for the shares of available addresses at the mesa and precinct levels. These directly affect the share of voters treated, and we aim to isolate the causal effect of our treatments from these characteristics of mesas. We refer to this as our "minimal set of controls." We also routinely present results with additional controls and, where needed, entropy weights to ensure balance (see Section 4.3 for details).

The coefficients of interest are $\beta$ and $\gamma$. The parameter $\beta$ captures the direct effect of treatment, that is, how exposure to leaflets among voters in a treated mesa affects Milei's vote share in that mesa. The parameter $\gamma$ captures the indirect (or spillover) effect, that is, how treatment of other mesas in the same precinct influences Milei's vote share in an untreated mesa.

We have derived Equation (3) from theory. However, one potential concern is that the shares of treated voters in the mesa and the precinct are correlated. To ensure that this correlation is not creating spurious artificial statistically significant opposite-sign estimates due to potential multi-collinearity, we use an alternative strategy separately estimating direct and indirect effects, which

our experimental design allows us to do.

To estimate the indirect effect independently of the direct effect, we use the same specification as in Equation (3) but restrict the sample to mesas that were not treated directly. In these mesas, the direct treatment does not vary as $\tau_m$ is zero by definition, so the parameter $\beta$ is not identified, but $\tau_p$ varies exogenously, and therefore, the coefficient $\gamma$ identifies the indirect effect.

Conversely, to estimate the direct effect separately, we exploit the fact that all mesas in the same precinct are subject to the same indirect effect (since the indirect effect depends only on the propensity of groups to interact, and group division is orthogonal to the division into mesas). Including precinct fixed effects allows us to compare treated and untreated mesas within the same precinct. Within the same precinct, mesas differ only in their exposure to the direct treatment, while their exposure to the indirect treatment is identical; therefore, the difference in outcomes can be attributed to the direct effect, as the choice of which mesas to treat was randomized. Note, however, that this comparison requires dropping the matched-set fixed effects $\phi_{\text{pair}}$: by construction, mesas within matched sets belong to different (treated and control) precincts, and thus matched-set fixed effects cannot be included alongside precinct fixed effects.

## 5.2 First-round education experiment

Table 1 reports estimates of Equation (3) for the education treatment in the first-round experiment. Column (1) includes only the minimal set of controls. Column (2) adds Milei's vote share in the PASO election. Column (3) further includes the PASO vote shares of Bullrich and Massa, as well as PASO turnout. Finally, Column (4) adds controls for latitude and longitude (which are constant within precincts) and incorporates a more flexible specification of the shares of available addresses by including dummies for their deciles at both the mesa and precinct levels.

Irrespective of the specification, the direct effect is negative and significant at the 10% level, while the indirect effect is positive and significant at the 5% level in three of the four specifications

and at the 10% level in the remaining one. The magnitude of the direct effect is about $-0.2$, and the indirect effect is about $+0.3$. In substantive terms, sending 100 leaflets to voters in a mesa reduced Milei's support in that mesa by about 20 votes but increased his support in untreated mesas within the same precinct by about 30 votes, yielding a net gain of roughly 10 votes per 100 leaflets distributed.[8]

Table 2 reports the results from the estimation of the direct and indirect effects separately. The first three columns estimate the direct effect (by adding precinct fixed effects and dropping matched-set fixed effects), while the last three columns estimate the indirect effect (by restricting the sample to untreated mesas). Column (1) presents estimates from the specification with the minimal set of controls and precinct fixed effects. Column (2) adds controls for the PASO primary voting results. Both columns (1) and (2) restrict the sample to mesas included in the matched sets. Column (3) expands the sample to the full set of mesas. The identifying variation comes from the randomization of mesas into treatment within precincts. The results are highly robust across specifications and samples. The estimated direct effects are very similar in magnitude to those reported in Table 1 and are statistically significant at the 1% level. Moreover, precision appears to improve relative to Table 1 due to lower correlation among covariates.

The next three columns estimate the indirect effect separately from the direct effect. Column (4) presents results for the most basic version of Equation (3), using the subsample of untreated mesas in the matched sets. Column (5) adds controls for PASO primary results and precinct coordinates. Column (6) extends the sample to the full set of untreated mesas, replacing matched-set fixed effects with department fixed effects. Once again, the results are highly robust. Irrespective of the specification, we find a significant backfiring indirect effect of the campaign. The point es-

[8]To illustrate the results, consider a precinct with 1,000 voters divided into 10 mesas of 100 voters each. Suppose we sent leaflets to 10 voters in one mesa. In that mesa, the share of treated voters rises by 10%, and the direct effect reduces Milei's vote share by 2%, or 2 votes. Across the precinct, 10 of 1,000 voters (1%) are treated. The indirect effect then increases Milei's support in each mesa by 0.3%, or 0.3 votes. Summing over the 10 mesas, the indirect effect adds 3 votes for Milei. Combining the direct and indirect effects, the total net effect of the intervention is +1 vote for Milei.

timates in this specification are somewhat larger than those reported in Table 1, ranging from 0.36 to 0.44. However, these point estimates fall well within the 95% confidence intervals of the earlier estimates, and vice versa. These separate estimates of direct and indirect effects confirm that the opposite signs of the two effects in the estimation of Equation (3) are not driven by multicollinearity.

To illustrate the results, Figure 1 presents binscatter plots corresponding to Columns (1) and (4) of Table 2. The figure confirms that the findings are not driven by outliers. Figure 2 presents the raw data, showing the distribution of Milei's vote share across mesas with different treatment intensities. The upper panel demonstrates that the direct effect shifts part of the distribution of Milei's vote share to the left relative to untreated mesas. This highlights both the negative average direct effect and the heterogeneity in its magnitude: the distribution of Milei's vote share in treated mesas is wider than that in untreated mesas. The same panel also shows that the indirect effect shifts the distribution of Milei's vote share to the right. The lower panel compares indirectly treated mesas (untreated mesas in treated precincts) with different intensities of the indirect effect. The distribution of Milei's vote share in mesas where the intensity of the indirect effect is above the median is shifted to the right relative to those where the intensity is below the median.

Together, these visualizations confirm evidence of a negative (intended) direct effect and a positive (backfiring) indirect effect of the education campaign.

## 5.3 Persistence of first-round education experiment

The runoff election between Javier Milei and Sergio Massa, held four weeks after the first round, provides an additional, longer-term outcome for the first-round experiment. Because some control precincts in the first round were treated during the runoff, we exclude those precincts and any mesas paired with them from the persistence analysis. The results, however, remain largely unchanged if these precincts are retained.

Tables 3 and 4 present the persistence results using the same specifications as in Tables 1 and 2, respectively, but with the outcome variable coming from the runoff rather than the first round election. Across all samples and specifications, the effects of the first-round education experiment persist into the runoff. Specifically, we observe a negative and significant direct effect and a positive and significant indirect effect of the first-round education campaign on Milei's vote share in the runoff election. Moreover, the direct effect in the runoff is very similar in magnitude to that observed in the first round, while the indirect effect is, if anything, larger. This pattern is consistent with directly treated voters having had additional time between the first round and the runoff to generate spillover effects on their peers, persuading them to support Milei.[9]

## 5.4 Runoff education experiment

The results of the runoff experiment are reported in Table 5, where direct and indirect effects are estimated jointly, and in Table 6, where they are estimated separately. These tables present the same specifications as in the first-round experiment, as well as specifications with entropy balancing weights to ensure balance in pre-treatment observables. The signs of both direct and indirect effects mirror those in the first round, though magnitudes are somewhat attenuated, which is unsurprising given that voters had become more familiar with Milei's policy proposals between the two elections. By the runoff, Milei was no longer an unknown politician, and the informational content of our leaflets diminished.

The indirect effect is very robust and remains statistically significant across all specifications. The direct effect is statistically significant when estimated separately (Columns (1) to (4) of Table 6), as this specification gives consistently higher precision due to lower covariate correlation. However, when estimated together, it essentially disappears in the most demanding joint specification (Columns (3) and (6) of Table 5), while remaining significant in the more basic specification

[9] If we pool observations from the first round and the runoff, the results are similar but estimated with greater precision.

(Columns (1) and (4) of Table 5). Most importantly, the finding that direct and indirect effects have opposite signs continues to hold, replicating the first-round results. The magnitude of the indirect effect (about 0.2–0.3) is less attenuated than that of the direct effect (about 0.075 in our preferred specification, Column (4) of Table 6). Thus, while the leaflets themselves were less persuasive on average in the runoff, their backfiring influence remained relatively strong in peer-to-peer interactions.

Figure 3 displays the results of the runoff experiment using binscatter plots based on the separate estimations of the direct and indirect effects (the figure corresponds to Columns (1) and (5) of Table 6).

## 5.5 Null results: Dollarization experiment and effects on turnout

While the education leaflet treatment had significant and largely replicable effects on Milei's vote share, the dollarization leaflet treatment—also sent to 5,000 voters—appears to have had no effect. As shown in Tables A2 and A3, neither the direct nor the indirect effect is statistically significant in any specification. A plausible explanation for this null result is that Milei's dollarization proposal was already widely discussed during the campaign and covered extensively by the media, leaving the leaflets with little new information to convey.

We also examine the impact of each experiment on turnout in both the first and second rounds. Table A4 reports the main specification for the education treatment in the first round, while Table A5 presents separate estimates of the direct and indirect effects. Tables A6 and A7 provide analogous estimates for the runoff. In all cases, we find no significant effects of the treatments on turnout: both the direct and indirect effects are statistically indistinguishable from zero.

## 5.6 Placebo validation exercise and internal validity

To ensure that our estimates of direct and indirect effects are not driven by mechanical features of the design, we conduct a placebo validation exercise. Specifically, we use the same code that generated the treated mesas and their paired controls, but instead create placebo treatment sets by assigning mesas to treatment using different randomization seeds. We then estimate direct and indirect effects for these fictitious treatments using Equation (3) with the real data. For both direct and indirect effects, the resulting placebo-effect distributions are centered at zero, with the actual estimated coefficients and their t-statistics lying at the tails of these distributions. We present the results of this placebo exercise in Figure 4 for the first-round and runoff education treatments and in Appendix Figure A16 – for the dollarization treatment. Overall, the placebo validation confirms that our main findings are not artifacts of mechanical relationships in the data, but reflect real and systematic effects of the education treatment observed in the data. In addition, any spurious mechanical patterns would be expected to manifest in the dollarization treatment as well, yet we find no such effects there.

It is possible that our experimental design does not perfectly separate direct and indirect effects, since our leaflets could have been read by household members registered to vote at a different mesa (in Spanish-speaking countries, spouses often have different last names). If we treat such within-household sharing as part of the indirect effect, this poses no problem, as it involves individuals who were not the intended recipients. Even if we instead classified it as a direct effect, our estimated indirect effect would then combine this household spillover with the genuine peer-to-peer spillover arising from political discussion and persuasion. Because the two effects have opposite signs in our estimates, this implies that the true spillover effect is, in fact, positive and even larger in magnitude. In other words, this potential concern, if anything, strengthens our interpretation.

It is also conceivable that people who share the same last name are much more likely to talk—including about politics—than those who do not. Under Spanish naming conventions, typical

examples would include siblings or a father and child. This would violate our key assumption that the allocation to mesas is orthogonal to the allocation to groups, and in particular, orthogonal to political preferences. If this is the case, we may be ascribing part of the indirect effect to the direct effect. However, this would not change our estimates of the indirect effect derived from untreated mesas only (as in Columns (4)–(6) in Table 2). Since the indirect effect is positive, this would imply that the true direct effect is even more negative than estimated. Thus, this consideration also does not invalidate our results.

Another potential concern is that treated voters in our experiments had PJ party affiliation at some point in their lives, as this is how they appeared on the party list, while untreated voters did not necessarily have such affiliation, raising the possibility that their reactions to the same information would differ, even if treated voters did not act strategically in persuading others. This could occur, for example, due to different priors leading to more polarized posteriors, as in (Acemoglu et al., 2016). However, polarization alone is not consistent with our results. If PJ members had simply become more convinced to oppose Milei and support Massa, while non-PJ members reacted in the opposite way, their voting choices would have remained the same. The only channel through which this could have affected outcomes is turnout, yet we find no treatment effect on turnout. Moreover, we always control for PJ membership at both the mesa and precinct levels, ensuring that comparisons are made across similar mesas in similar environments. Finally, not all PJ affiliates supported Massa, the left-wing candidate, over Milei. Milei's support came from across the political spectrum, including former PJ supporters. Thus, differences in party affiliation are very unlikely to account for our main findings.

Overall, the body of evidence strongly indicates that the opposite-sign results for the direct and indirect effects of the education campaign are genuine and internally valid. The findings are robust across estimation methods (joint versus separate estimation of direct and indirect effects), persist into the runoff, and are replicated in a follow-up runoff experiment featuring the same pattern with the opposite signs for the direct and indirect effects. Across all analyses, the direct and indirect

effects consistently move in opposite directions. In the next section, we explore the mechanism behind these opposite-sign effects.

# 6 Vignette survey evidence on the mechanism

The simple theoretical example discussed in Section 3 suggests that the positive direct and negative indirect effects of a political campaign may arise when the minority of voters not persuaded by campaign leaflets are more effective at persuading others to vote for Milei than the majority who were persuaded. The most plausible explanation for this asymmetry is backlash: voters who remained unconvinced by the leaflet but were strong Milei supporters may have been so outraged by the campaign against him that they actively sought to persuade their neighbors and friends to support him. Such motivated voters can be highly effective, often more so than any direct persuasive effect of a leaflet campaign, because they know their social networks well, can identify who is persuadable, and understand which arguments are likely to resonate with each individual.

Due to logistical constraints——specifically, limited time and resources——we were unable to collect evidence on the underlying mechanism during the 2023 presidential elections. However, we later gathered information that sheds light on the mechanism. In particular, to better understand the mechanism behind the opposite signs of the direct and indirect effects of the leaflet campaign, we conducted a brief, five-minute in-person survey of a representative sample of 2,021 rural residents in Salta in August 2025.[10]

By August 2025, Milei had been in office for 20 months, so we implemented a vignette design. Respondents were asked to imagine themselves at a point in the past when Alberto Fernández was still president. In this scenario, two candidates compete: one from a traditional, long-established party, and another a political outsider who seeks to fight corruption and bring change. The election

[10]IRB approval was obtained from the Paris School of Economics (certificate no. 2025-040). The survey was administered by the professional Argentinian polling company Datamática.

is close, and the outsider has a real chance of winning. Respondents were told to imagine receiving a colorful campaign leaflet that criticizes one of the outsider candidate's proposals, claiming—according to specified experts—that its implementation could harm the poor. Respondents born on even dates of the month were asked to suppose that they found the leaflet reasonable, believed its message, and therefore felt less inclined to vote for the outsider. Those born on odd dates of the month were asked to suppose that they did not believe the leaflet, viewed it as an establishment attack on the outsider candidate, and, if anything, became more inclined to support the outsider.

We use this randomly assigned attitude, whether respondents were asked to imagine believing or disbelieving the leaflet, as the main treatment variable and relate it to outcomes measuring whether and why they would discuss the leaflet with peers, controlling for socio-demographic characteristics and voting behavior in the 2023 presidential election. Our main hypothesis is that respondents asked to imagine disbelieving the leaflet should be more likely to talk to their peers about it than those asked to imagine believing it. We find evidence consistent with this hypothesis. Online Appendix A provides the full survey instrument and summary statistics for the key variables. We regress different outcomes on a dummy variable indicating whether the respondent was asked to imagine not believing the leaflet criticizing the outsider candidate, controlling for education, income, gender, age, municipality of residence, and voting behavior in the 2023 election, as well as fixed effects for the first letter of the respondent's last name, which serves as a proxy for ethnic and cultural background. We use robust standard errors as the baseline because randomization in the vignette survey is at the individual level.[11]

Panel A of Table 7 reports the main results. There is no significant relationship between whether respondents were asked to imagine not believing the leaflet and whether they report wanting to discuss the leaflet with their peers. This was the first question we asked after presenting the hypothetical scenario. The question used a 5-point Likert scale, but there is no statistically signifi-

[11]The results are similar and estimated more precisely when standard errors are clustered at the precinct level, as in the main experiment.

cant relationship between the treatment dummy and this variable, regardless of how the responses are coded. Column (1) of Panel A in Table 7 presents the results for the dummy corresponding to the highest (fifth) category: "definitely would discuss the leaflet." The coefficient is positive, consistent with our explanation of the main experiment's results, but not statistically significant.

The second question asked how many people respondents would discuss the leaflet with. We find that those asked to imagine not believing the leaflet are 4 percentage points more likely to choose the maximum possible response: "with more than six people." This relationship is statistically significant (see Column (2) of the same Panel). If we impute the number of people using the lower bound of each category (to be conservative) or interact this number with the dummy indicating the intention to discuss with peers (i.e., the fourth and fifth categories of the respective question), we also find that imagining not believing the leaflet is a positive and statistically significant predictor of the number of peers with whom the respondent intends to discuss the campaign (see Columns (3) and (4)).

In Column (1) of Panel B of the table, we relate the treatment to the answers to the question about why respondents would talk to their peers about this campaign. Among the different reasons—(1) to convince others to vote like them, (2) because they like talking about politics, (3) to learn what others think about the leaflet, (4) because they believe traditional politicians attack the new candidate, and (5) because they want to warn others about the risks of supporting the new candidate—the only answer that is statistically significantly related to the treatment is the first one. Specifically, respondents asked to imagine not believing the leaflet are significantly more likely to state that they would talk to others about this campaign because they want to convince others to vote like them.

We also asked respondents whether they would behave the same way if the leaflet criticized the mainstream incumbent politician rather than the outsider candidate. We find that those randomized into imagining not believing leaflet are more likely to state that they would behave differently in that case (see Column (2) of Panel B of Table 7), highlighting that the mechanisms underlying the

direct and indirect effects may differ for outsider and mainstream candidates.[12] We further asked how respondents assessed the sender of the leaflets, but found no significant association with the treatment (Columns (3) and (4)). The point estimates of all survey results are relatively small, but given that this is a vignette survey, the responses are necessarily measured with error, which would bias the estimates toward zero.

Overall, the survey evidence supports our interpretation of the mechanism behind the main experimental results. People who disagree with the political campaign, particularly when it targets an outsider candidate, are more likely to mobilize in support of that candidate among their peers. This dynamic can generate a negative indirect effect of the campaign, even if most people exposed to it are persuaded to shift their votes toward mainstream candidates, thereby yielding a positive direct effect.[13]

# 7 External validity discussion

Our results may not necessarily generalize to all contexts. In some information campaigns, even the direct effect may be null; in others, the direct effect may occur without any spillovers, or the indirect effect may reinforce the direct one. Nevertheless, there are reasons to believe that similar patterns might emerge in other important settings.

In our case, Javier Milei was largely unknown to most voters, his policy proposals were novel, and their merits were difficult to evaluate. In such circumstances, voters may be more receptive to expert information about the candidate, which could amplify both direct and indirect effects. At the same time, like many politicians leading anti-establishment campaigns elsewhere, Milei was

[12] Online Appendix Section A.1 presents further evidence from the survey on this issue.

[13] One might wonder whether experimenter-demand effects (EDE) could affect our results. However, it is unclear why EDE should differ between respondents asked to imagine believing the leaflet and those asked to imagine not believing it. In both cases, respondents may infer that the researchers expect them to consider how the leaflet could spark political discussions with peers. Moreover, if EDE operated differently across the two treatment groups, we would expect a similar pattern to appear when respondents evaluate a mainstream candidate. We do not observe such a pattern, which is reassuring.

polarizing—admired by some voters and despised by others. This polarization likely contributed to the backlash effect we observe. Our survey evidence confirms that such backlash effects are more likely when campaigns criticize outsider candidates.

Taken together, this suggests that similar backlash dynamics could arise in comparable contexts—for example, in the cases of Donald Trump in the 2016 U.S. election, Jair Bolsonaro in the 2018 Brazilian election, Andrés Manuel López Obrador in the 2018 Mexican election, or Nayib Bukele in the 2019 Salvadoran election. Each of these winners was an outsider candidate whose campaign polarized the electorate, creating fertile ground for the kind of backlash we observe in our setting.

Finally, it is important to note that the magnitude of the effects we observe is likely amplified by the campaign's design, which specifically targeted remote and rural areas. In Salta Province, particularly in its suburban and rural zones, voters are not typically inundated with political leaflets, making such outreach more salient. By contrast, attracting attention to a similar campaign in a politically saturated environment, such as a U.S. swing state like Pennsylvania, would be more challenging. To obtain comparable results in a saturated environment, the campaign would probably need to operate at a much larger scale or intensity.

# 8 Conclusion

We demonstrate that the direct and spillover effects of a political information campaign can—and in our setting, do—have opposite signs. A campaign criticizing Milei's education policy proposal had the intended direct effect on average, whereas the indirect effect, operating through social interactions among the unconvinced minority within the same precinct, countervailed it and was of greater magnitude, thus resulting in an overall backfiring impact. We are able to identify these effects using a novel approach that leverages the unique Argentine system of reporting election results, which provides multiple observations within a precinct and allows us to disentangle direct from indirect effects at a very granular level. We also provide supporting evidence for the

underlying mechanism through a vignette survey.

Our main finding challenges the common assumption in the literature that the indirect effects of political information campaigns are small and operate in the same direction as direct effects. We show instead that spillovers can be both large in magnitude and opposite in sign. This insight implies that empirical analyses of electoral persuasion should either explicitly incorporate indirect effects or provide a convincing justification for excluding them. Our results caution against dismissing spillover effects simply because they are assumed to be small.

The findings also have important practical implications for political campaigns, particularly those that seek to inform voters or combat misinformation through fact-checks and expert opinions. Political information campaigns may backfire, and the backlash need not come from individuals directly exposed to the campaign. This suggests that focus groups and pilot studies, which typically measure only direct persuasion, may offer misleading guidance if the diffusion of information through social networks is ignored. Our results help explain why political practitioners often struggle to anticipate the outcomes of their own campaigns, and why efforts to correct misinformation or shape perceptions—though effective in controlled environments—can fail or even backfire in real elections, especially when targeting anti-establishment candidates.

Finally, by highlighting the prominence and complexity of spillover effects in elections, our work points to promising directions for future research. Understanding the conditions under which backlash arises—such as when campaigns target anti-elite outsiders or polarizing candidates—is a crucial next step. More broadly, examining how these indirect effects contribute to polarization, the formation of echo chambers, and the (in)effectiveness of interventions designed to mitigate misinformation constitutes an important avenue for theoretical, empirical, and experimental research.

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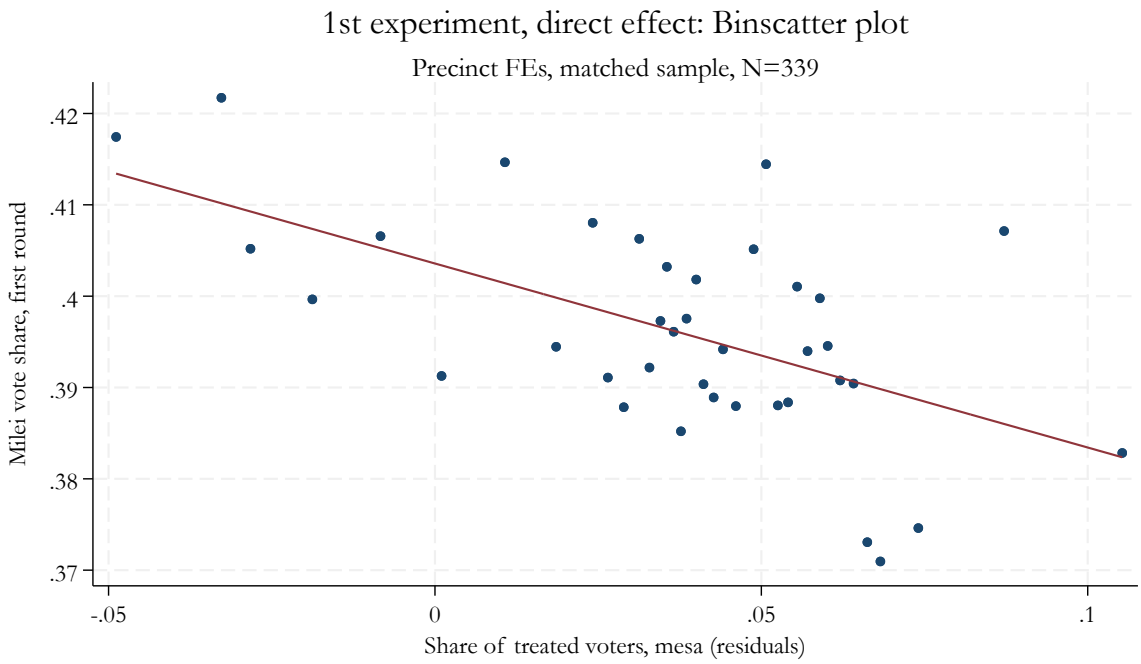
Figure 1: Illustration of the main effects in the first experiment



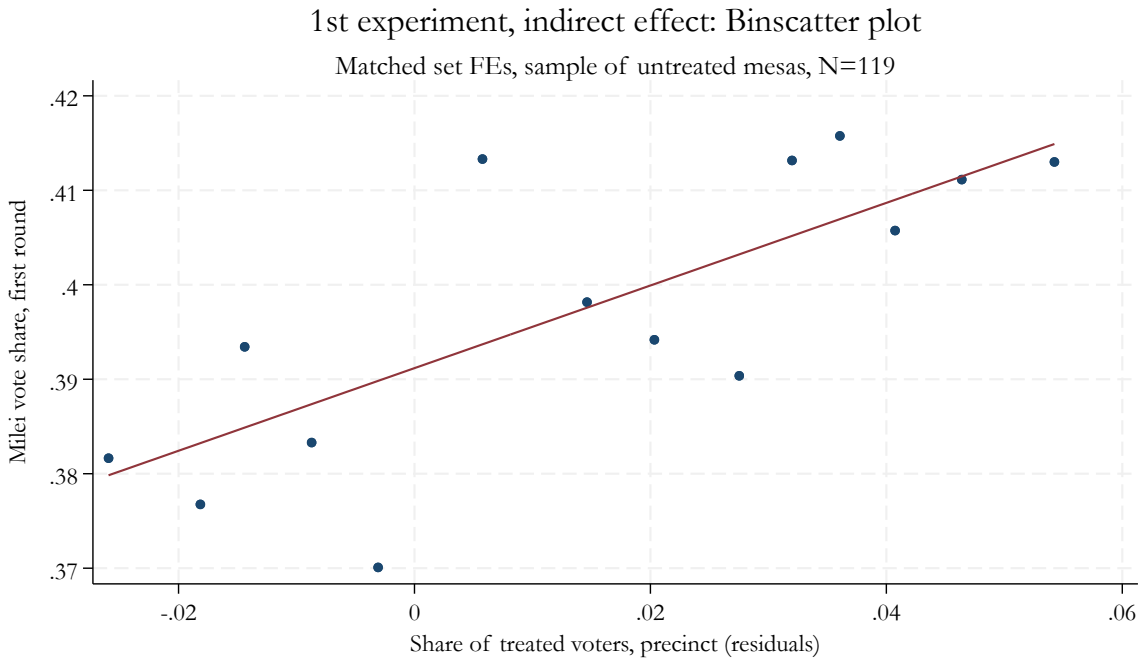



Figure 2: Distribution of Milei vote shares across mesas with different treatment intensities

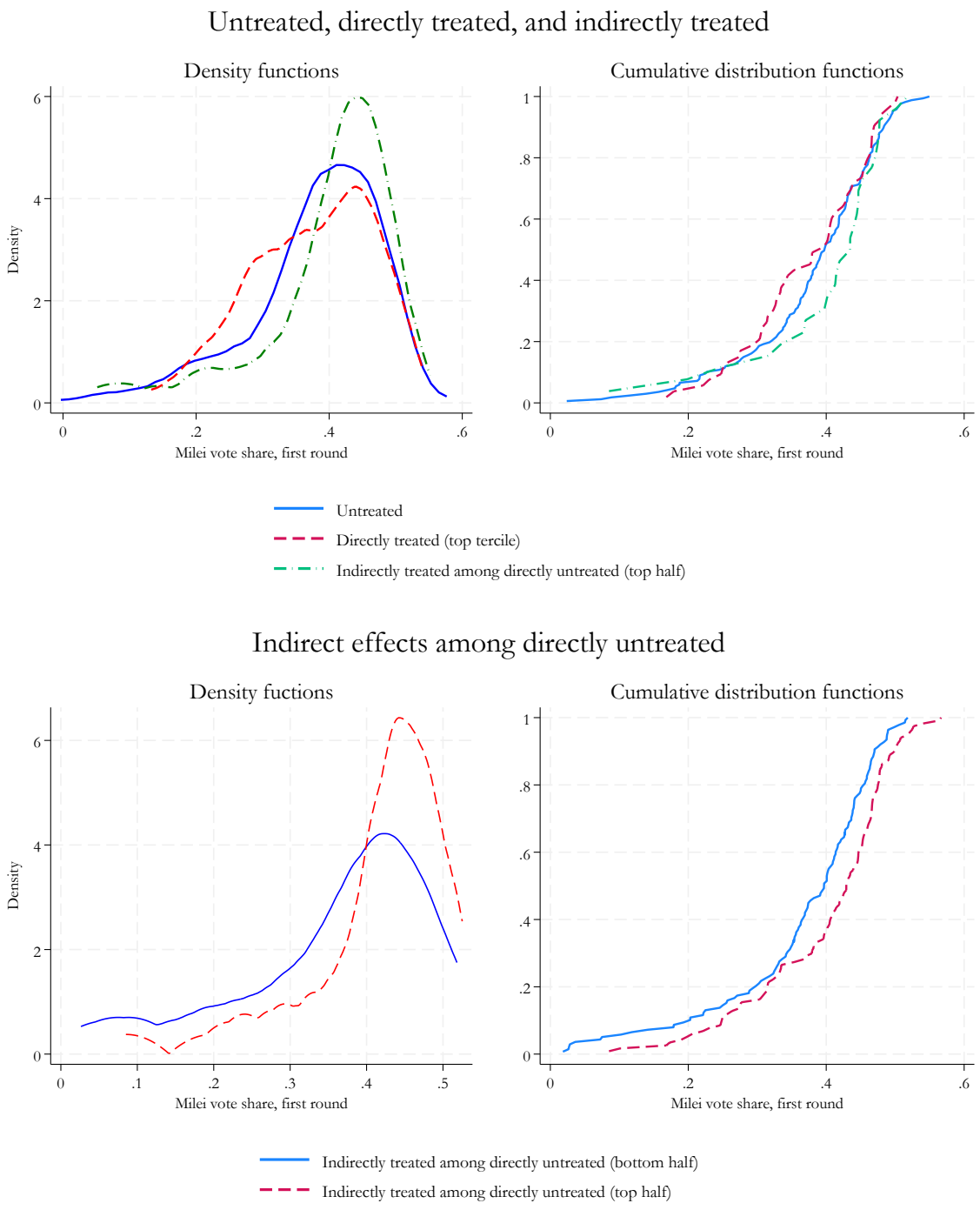


Figure 3: Illustration of the main effects in the second experiment

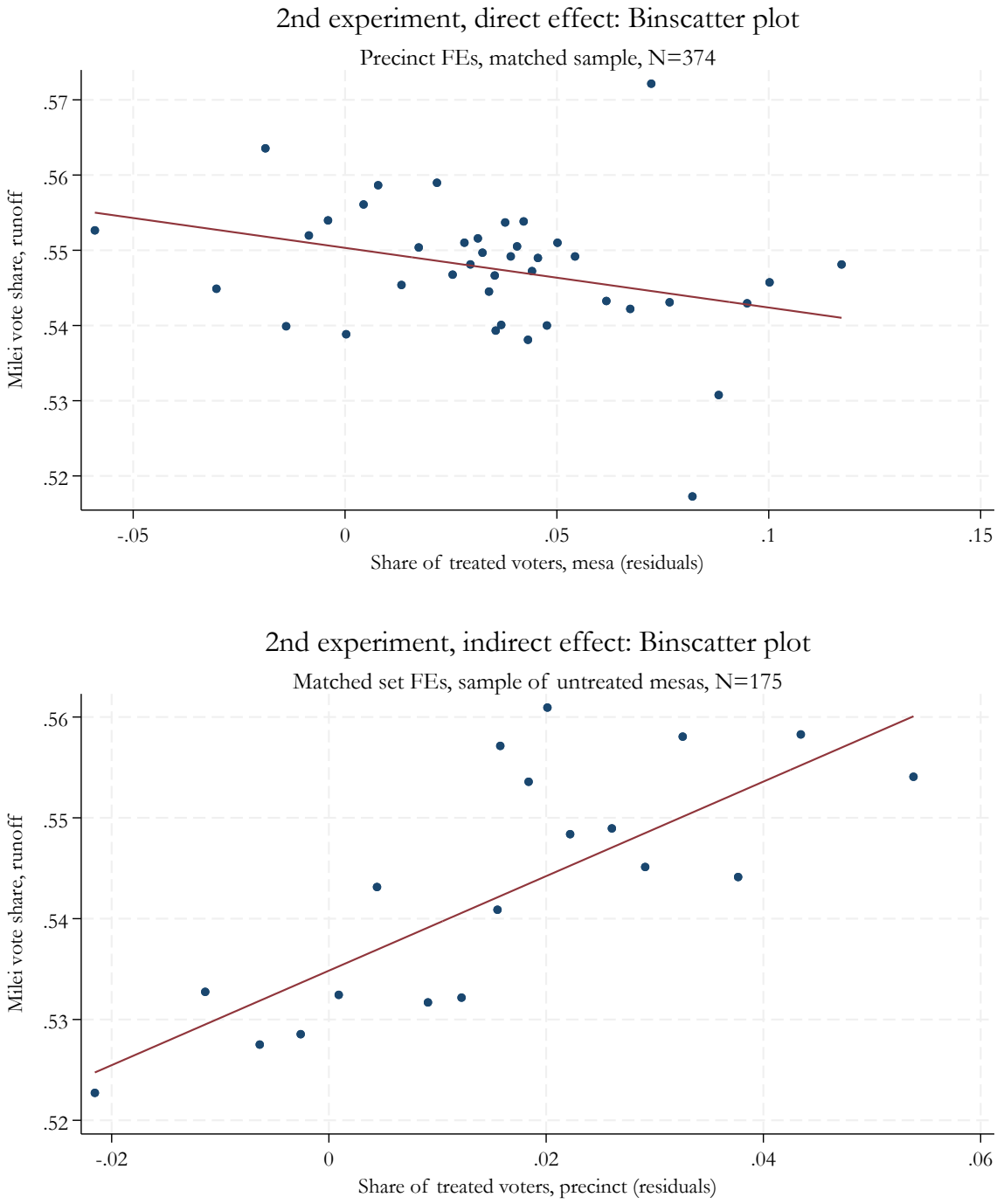


Figure 4: Placebo, first-round and runoff education treatment, 1000 alternative randomizations

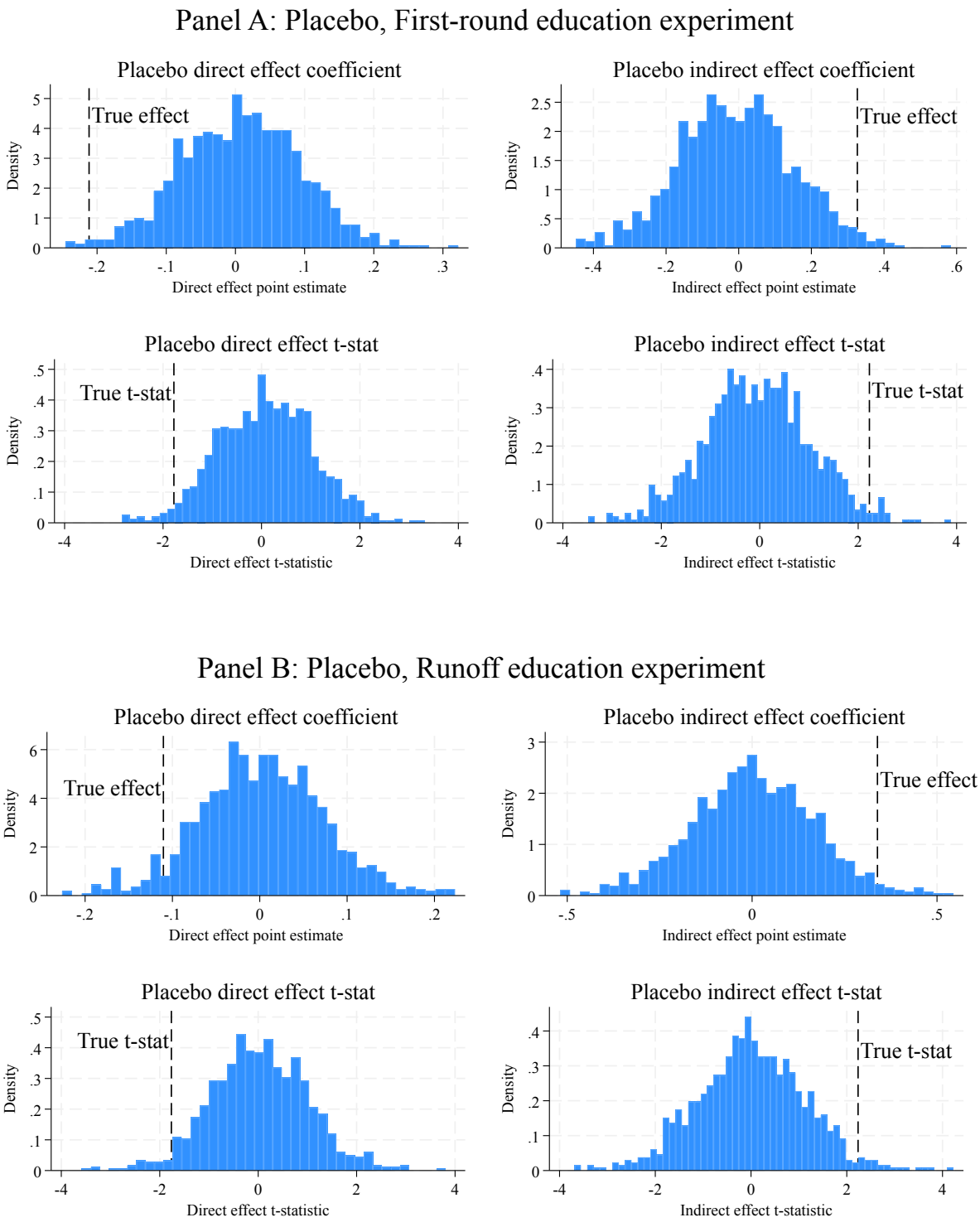


Table 1: First experiment, education treatment, main results

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Dependent variable: | Milei Vote Share, First Round | | | |
| Sample: | Matched sets | | | |
| Share of treated voters, mesa | –0.212* | –0.211* | –0.199* | –0.216* |
| (Direct effect) | (0.119) | (0.124) | (0.117) | (0.112) |
| Share of treated voters, precinct | 0.327** | 0.329** | 0.252* | 0.312** |
| (Indirect effect) | (0.147) | (0.150) | (0.148) | (0.136) |
| Milei vote share, primary (PASO) | | –0.720 | 0.574 | 0.789 |
| | | (0.835) | (0.760) | (0.700) |
| Bullrich party vote share, primary (PASO) | | | 0.129 | 0.080 |
| | | | (0.095) | (0.120) |
| Massa party vote share, primary (PASO) | | | –0.039 | –0.129 |
| | | | (0.072) | (0.112) |
| Turnout, primary (PASO) | | | 0.565*** | 0.550*** |
| | | | (0.096) | (0.083) |
| Mean, Dep. Var. | 0.396 | 0.396 | 0.396 | 0.396 |
| SD, Dep. Var. | 0.085 | 0.085 | 0.085 | 0.085 |
| R2 | 0.838 | 0.839 | 0.877 | 0.898 |
| Observations | 359 | 359 | 359 | 359 |
| Available addresses, mesa, precinct | ✓ | ✓ | ✓ | |
| Matched set FEs | ✓ | ✓ | ✓ | ✓ |
| Latitude, Longitude | | | | ✓ |
| Decile FEs for available addresses | | | | ✓ |

**Note:** *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at precinct level are in parentheses. Unit of observation is mesa.

Table 2: First experiment, education treatment, estimating direct and indirect effects separately

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Dependent variable: | Milei Vote Share, First Round | | | | | |
| | Direct effect | | | Indirect effect | | |
| Sample: | Matched sets | | Full | Matched sets | | Full |
| | | | | Untreated mesas | | |
| Share of treated voters, mesa (Direct effect) | –0.201*** (0.067) | –0.200*** (0.070) | –0.201*** (0.062) | | | |
| Share of treated voters, precinct (Indirect effect) | | | | 0.437*** (0.146) | 0.391** (0.172) | 0.359*** (0.116) |
| Milei vote share, primary (PASO) | | 0.519*** (0.094) | 0.278*** (0.098) | | –0.709 (1.268) | 0.400*** (0.091) |
| Bullrich party vote share, primary (PASO) | | 0.182* (0.094) | 0.018 (0.078) | | –0.354 (0.273) | 0.145* (0.081) |
| Massa party vote share, primary (PASO) | | 0.225*** (0.070) | 0.006 (0.089) | | –0.506* (0.267) | –0.016 (0.086) |
| Turnout, primary (PASO) | | 0.171* (0.089) | 0.166*** (0.048) | | 0.162 (0.168) | 0.260*** (0.044) |
| Mean, Dep. Var. | 0.399 | 0.399 | 0.392 | 0.401 | 0.401 | 0.392 |
| SD, Dep. Var. | 0.083 | 0.083 | 0.092 | 0.072 | 0.072 | 0.089 |
| R2 | 0.805 | 0.853 | 0.828 | 0.801 | 0.831 | 0.726 |
| Observations | 339 | 339 | 870 | 119 | 119 | 955 |
| Available addresses, mesa, precinct | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Precinct FEs | ✓ | ✓ | ✓ | | | |
| Matched set FEs | | | | ✓ | ✓ | |
| Department FEs | | | | | | ✓ |
| Latitude, Longitude | | | | | ✓ | ✓ |

**Note:** *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at precinct level are in parentheses. Unit of observation is mesa. In columns 1, 2, and 3, we exclude mesas that received dollarization treatment. In columns, 4, 5, and 6, to estimate the indirect effect, we exclude mesas that received education treatment.

Table 3: First experiment, education treatment, persistence of the effect

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Dependent variable: | Milei Vote Share, Runoff | | | |
| Sample: | Matched sets | | | |
| Share of treated voters, mesa | –0.252** | –0.243** | –0.186* | –0.200* |
| (Direct effect) | (0.105) | (0.115) | (0.110) | (0.105) |
| Share of treated voters, precinct | 0.513*** | 0.505*** | 0.315* | 0.360*** |
| (Indirect effect) | (0.171) | (0.174) | (0.160) | (0.134) |
| Milei vote share, primary (PASO) | | –1.948** | –0.214 | 0.192 |
| | | (0.943) | (0.848) | (0.744) |
| Bullrich party vote share, primary (PASO) | | | 0.532*** | 0.516*** |
| | | | (0.139) | (0.161) |
| Massa party vote share, primary (PASO) | | | –0.079 | –0.140 |
| | | | (0.116) | (0.135) |
| Turnout, primary (PASO) | | | 0.691*** | 0.673*** |
| | | | (0.112) | (0.111) |
| Mean, Dep. Var. | 0.551 | 0.551 | 0.551 | 0.551 |
| SD, Dep. Var. | 0.118 | 0.118 | 0.118 | 0.118 |
| R2 | 0.847 | 0.851 | 0.906 | 0.921 |
| Observations | 387 | 387 | 387 | 387 |
| Available addresses, mesa, precinct | ✓ | ✓ | ✓ | |
| Matched set FEs | ✓ | ✓ | ✓ | ✓ |
| Latitude, Longitude | | | | ✓ |
| Decile FEs for available addresses | | | | ✓ |

**Note:** *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at precinct level are in parentheses. Unit of observation is mesa.

Table 4: First experiment, education treatment, persistence, estimating direct and indirect effects separately

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Dependent variable: | Milei Vote Share, Runoff | | | | | |
| | Direct effect | | | Indirect effect | | |
| Sample: | Matched sets | | Full | Matched sets | | Full |
| | | | | Untreated mesas | | |
| Share of treated voters, mesa (Direct effect) | –0.205*** (0.054) | –0.205*** (0.057) | –0.193*** (0.061) | | | |
| Share of treated voters, precinct (Indirect effect) | | | | 0.661*** (0.229) | 0.517** (0.215) | 0.425*** (0.136) |
| Milei vote share, primary (PASO) | | 0.355** (0.137) | 0.189** (0.088) | | –1.736 (1.049) | 0.369*** (0.101) |
| Bullrich party vote share, primary (PASO) | | 0.399*** (0.114) | 0.274*** (0.079) | | –0.101 (0.289) | 0.574*** (0.098) |
| Massa party vote share, primary (PASO) | | 0.057 (0.090) | –0.095 (0.075) | | –0.744** (0.289) | –0.076 (0.094) |
| Turnout, primary (PASO) | | 0.331*** (0.115) | 0.280*** (0.052) | | 0.216 (0.197) | 0.408*** (0.048) |
| Mean, Dep. Var. | 0.558 | 0.558 | 0.543 | 0.563 | 0.563 | 0.541 |
| SD, Dep. Var. | 0.111 | 0.111 | 0.123 | 0.093 | 0.093 | 0.123 |
| R2 | 0.862 | 0.901 | 0.903 | 0.762 | 0.881 | 0.817 |
| Observations | 356 | 356 | 902 | 129 | 129 | 986 |
| Available addresses, mesa, precinct | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Precinct FEs | ✓ | ✓ | ✓ | | | |
| Matched set FEs | | | | ✓ | ✓ | |
| Department FEs | | | | | | ✓ |
| Latitude, Longitude | | | | | ✓ | ✓ |

**Note:** *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at precinct level are in parentheses. Unit of observation is mesa. In columns 1, 2, and 3, we exclude mesas that received dollarization treatment. In columns, 4, 5, and 6, to estimate the indirect effect, we exclude mesas that received education treatment.

Table 5: Second experiment, education treatment, main effects

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Dependent variable: | Milei Vote Share, Runoff | | | | | |
| Sample: | Matched sets, full | | | | | |
| Weights: | No weights | | | Entropy balancing weights | | |
| Share of treated voters, mesa | –0.110* | –0.106* | –0.019 | –0.107* | –0.101 | –0.019 |
| (Direct effect) | (0.063) | (0.062) | (0.058) | (0.064) | (0.061) | (0.059) |
| Share of treated voters, precinct | 0.339** | 0.343** | 0.225** | 0.260* | 0.264* | 0.189* |
| (Indirect effect) | (0.152) | (0.152) | (0.112) | (0.154) | (0.154) | (0.113) |
| Milei vote share, primary (PASO) | | 0.054 | –0.047 | | 0.071 | –0.026 |
| | | (0.048) | (0.089) | | (0.051) | (0.089) |
| Bullrich party vote share, primary (PASO) | | | 0.155 | | | 0.177 |
| | | | (0.123) | | | (0.124) |
| Massa party vote share, primary (PASO) | | | –0.236* | | | –0.218* |
| | | | (0.123) | | | (0.121) |
| Turnout, primary (PASO) | | | 0.197*** | | | 0.189*** |
| | | | (0.048) | | | (0.047) |
| Mean, Dep. Var. | 0.548 | 0.548 | 0.548 | 0.547 | 0.547 | 0.547 |
| SD, Dep. Var. | 0.117 | 0.117 | 0.117 | 0.118 | 0.118 | 0.118 |
| R2 | 0.916 | 0.917 | 0.944 | 0.918 | 0.919 | 0.946 |
| Observations | 390 | 390 | 390 | 390 | 390 | 390 |
| Available addresses, mesa, precinct | ✓ | ✓ | | ✓ | ✓ | |
| Matched set FEs | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Latitude, Longitude | | | ✓ | | | ✓ |
| Decile FEs for available addresses | | | ✓ | | | ✓ |

**Note:** *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at precinct level are in parentheses. Unit of observation is mesa.

Table 6: Second experiment, education treatment, estimating direct and indirect effects separately

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Dependent variable: | Milei Vote Share, Runoff | | | | | | | |
| Sample: | Matched sets, full | | | | Matched sets, untreated mesas | | | |
| Weights: | No weights | | Entropy balancing | | No weights | | Entropy balancing | |
| | Direct effect | | | | Indirect effect | | | |
| Share of treated voters, mesa (Direct effect) | –0.079** | –0.071* | –0.082** | –0.075** | | | | |
| | (0.039) | (0.036) | (0.039) | (0.036) | | | | |
| Share of treated voters, precinct (Indirect effect) | | | | | 0.469*** | 0.315** | 0.422** | 0.290* |
| | | | | | (0.175) | (0.150) | (0.183) | (0.152) |
| Milei vote share, first round | 0.801*** | 0.759*** | 0.783*** | 0.744*** | 0.464 | 0.108 | 0.453 | 0.074 |
| | (0.068) | (0.069) | (0.063) | (0.063) | (0.902) | (0.775) | (0.948) | (0.775) |
| Milei vote share, primary (PASO) | | 0.024 | | 0.035 | | 0.089 | | 0.113 |
| | | (0.068) | | (0.070) | | (0.125) | | (0.128) |
| Bullrich party vote share, primary (PASO) | | 0.178* | | 0.197** | | 0.322** | | 0.351** |
| | | (0.092) | | (0.090) | | (0.142) | | (0.144) |
| Massa party vote share, primary (PASO) | | –0.043 | | –0.034 | | –0.027 | | –0.012 |
| | | (0.069) | | (0.070) | | (0.122) | | (0.123) |
| Mean, Dep. Var. | 0.551 | 0.551 | 0.552 | 0.552 | 0.536 | 0.536 | 0.538 | 0.538 |
| SD, Dep. Var. | 0.116 | 0.116 | 0.116 | 0.116 | 0.117 | 0.117 | 0.117 | 0.117 |
| R2 | 0.916 | 0.922 | 0.917 | 0.924 | 0.928 | 0.944 | 0.928 | 0.946 |
| Observations | 374 | 374 | 374 | 374 | 175 | 175 | 175 | 175 |
| Available addresses, mesa, precinct | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Precinct FEs | ✓ | ✓ | ✓ | ✓ | | | | |
| Matched set FEs | | | | | ✓ | ✓ | ✓ | ✓ |
| Latitude, Longitude | | | | | | ✓ | | ✓ |

**Note:** *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at precinct level are in parentheses. Unit of observation is mesa. In columns 1 to 4, we exclude mesas that received dollarization treatment. In columns, 5 to 8, to estimate the indirect effect, we exclude mesas that received education treatment. All columns report results for the matched sets sample.

Table 7: Vignette Survey Evidence on the Mechanism

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| **Panel A: Main outcomes** | | | | |
| Dependent variables: | Would you discuss the leaflet with your peers and how many? | | | |
| | Very likely to discuss (max category) | With at least 6 people (max category) | Number of people | Number of people × Likely or very likely to discuss |
| Imagine: you do not believe the leaflet | 0.0247 | 0.0432** | 0.1844* | 0.2163** |
| | (0.0204) | (0.0183) | (0.0951) | (0.1020) |
| Mean, Dep. Var. | 0.351 | 0.222 | 2.360 | 2.035 |
| SD, Dep. Var. | 0.477 | 0.416 | 2.181 | 2.315 |
| R2 | 0.159 | 0.116 | 0.132 | 0.112 |
| Observations | 2021 | 2021 | 2021 | 2021 |
| Age, gender, income, education | ✓ | ✓ | ✓ | ✓ |
| Vote runoff and PASO 2023 | ✓ | ✓ | ✓ | ✓ |
| Municipality and first letter FEs | ✓ | ✓ | ✓ | ✓ |
| **Panel B: Auxiliary outcomes** | | | | |
| Dependent variables: | Discuss leaflet: | Behave differently | Opinion about the sender: | |
| | To convince others to vote like me | if leaflet about mainstream candidate | Bad: could be a lie | Good: useful information |
| Imagine: you do not believe the leaflet | 0.0259* | 0.0469** | 0.0109 | -0.0285 |
| | (0.0143) | (0.0204) | (0.0142) | (0.0192) |
| Mean, Dep. Var. | 0.110 | 0.336 | 0.117 | 0.258 |
| SD, Dep. Var. | 0.313 | 0.472 | 0.321 | 0.438 |
| R2 | 0.082 | 0.139 | 0.085 | 0.110 |
| Observations | 2021 | 2021 | 2021 | 2021 |
| Age, gender, income, education | ✓ | ✓ | ✓ | ✓ |
| Vote runoff and PASO 2023 | ✓ | ✓ | ✓ | ✓ |
| Municipality and first letter FEs | ✓ | ✓ | ✓ | ✓ |

**Note:** *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors. Unit of observation is Vignette Survey respondent.

# Online Appendix

## Political Information and Network Effects

by Georgy Egorov, Sergei Guriev, Maxim Mironov, and Ekaterina Zhuravskaya

Figure A1: Leaflet: Education treatment, two outside half-pages

Panel A: Original.


Panel B: Translation.

Figure A2: Leaflet: Education treatment, two inside half-pages

Panel A: Original.


Panel B: Translation.

Figure A3: Leaflet: Dollarization treatment, two outside half-pages

Panel A: Original.


Panel B: Translation.

Figure A4: Leaflet: dollarization treatment, two inside half-pages

Panel A: Original.


Panel B: Translation.

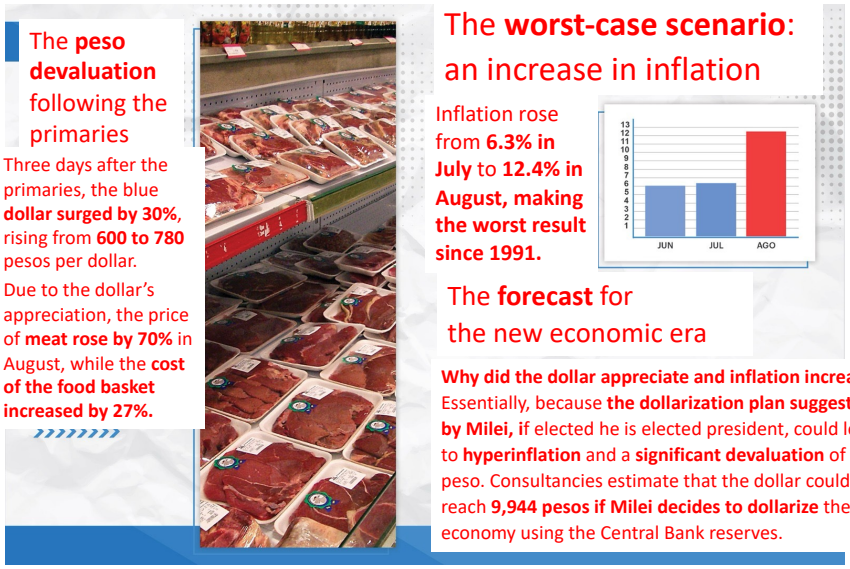


Figure A5: Map of Argentina, results of primary elections (PASO) vote by province

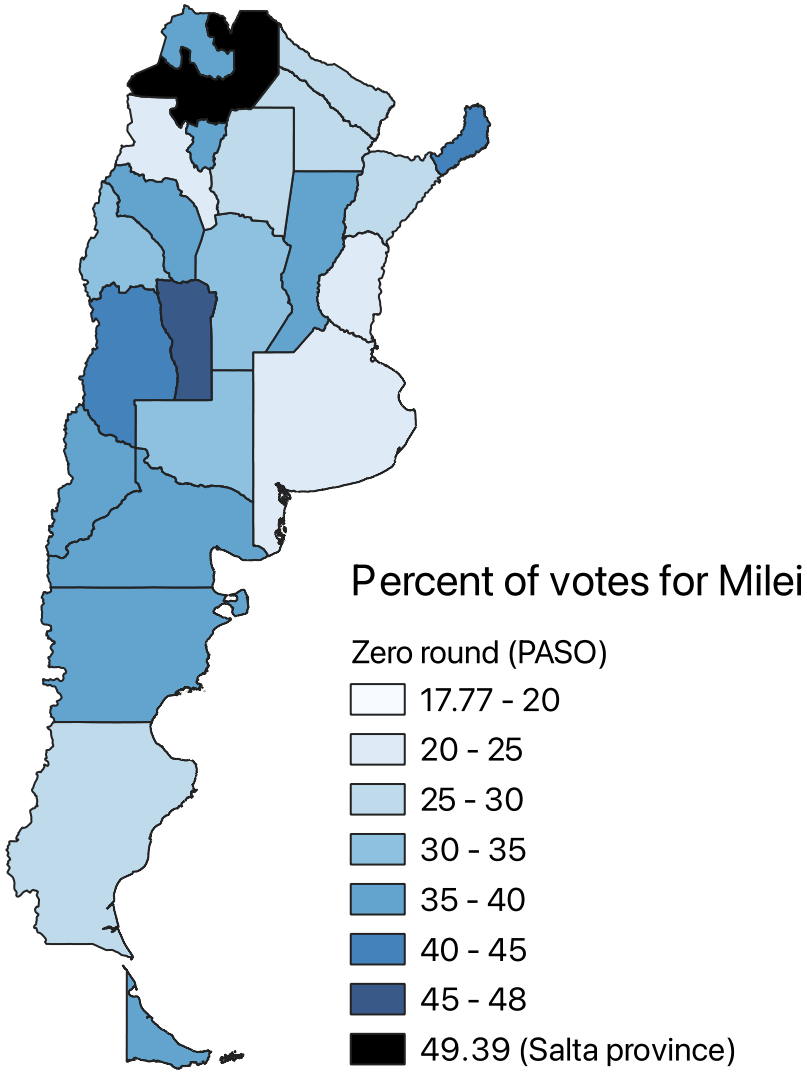


Figure A6: Map of Salta province, economic development by departments, and location of all electoral precincts

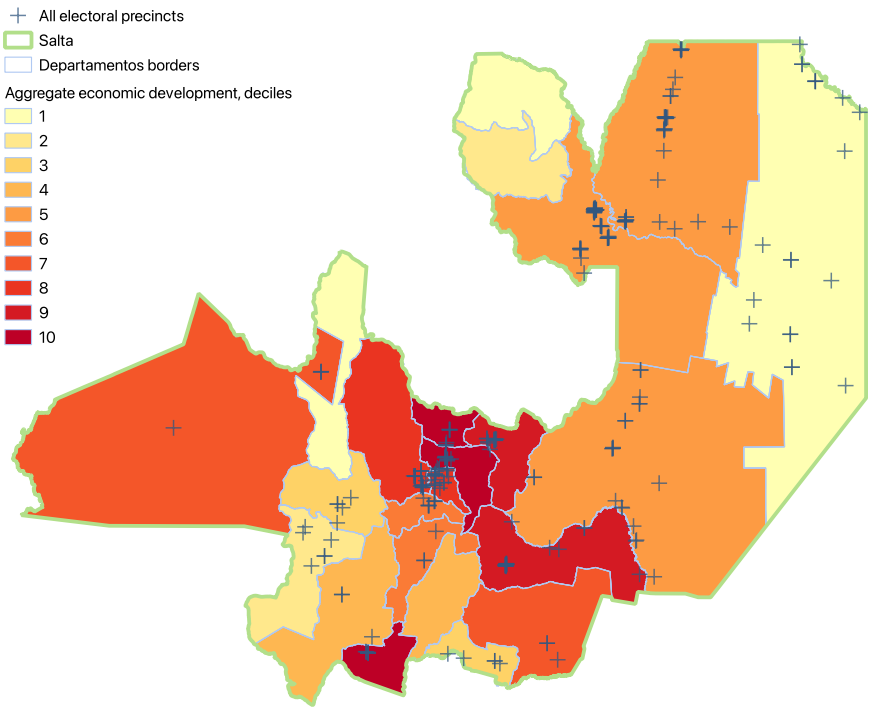


Figure A7: Map of precincts with education treatment, 1st round experiment

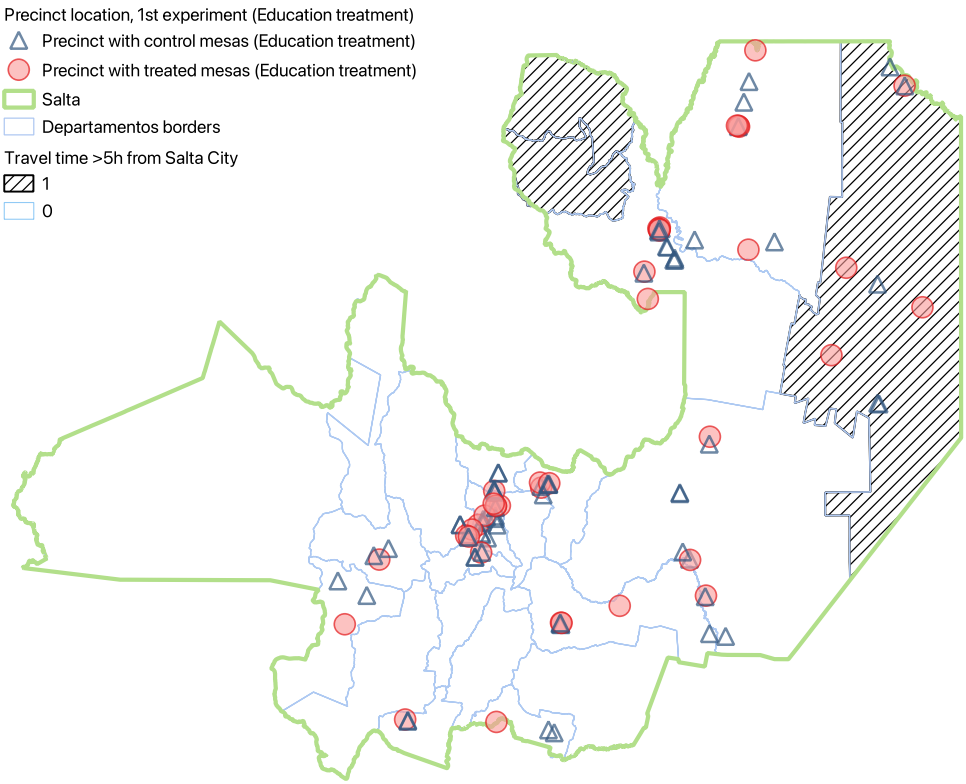


Figure A8: Map of precincts with dollarization treatment, 1st round experiment

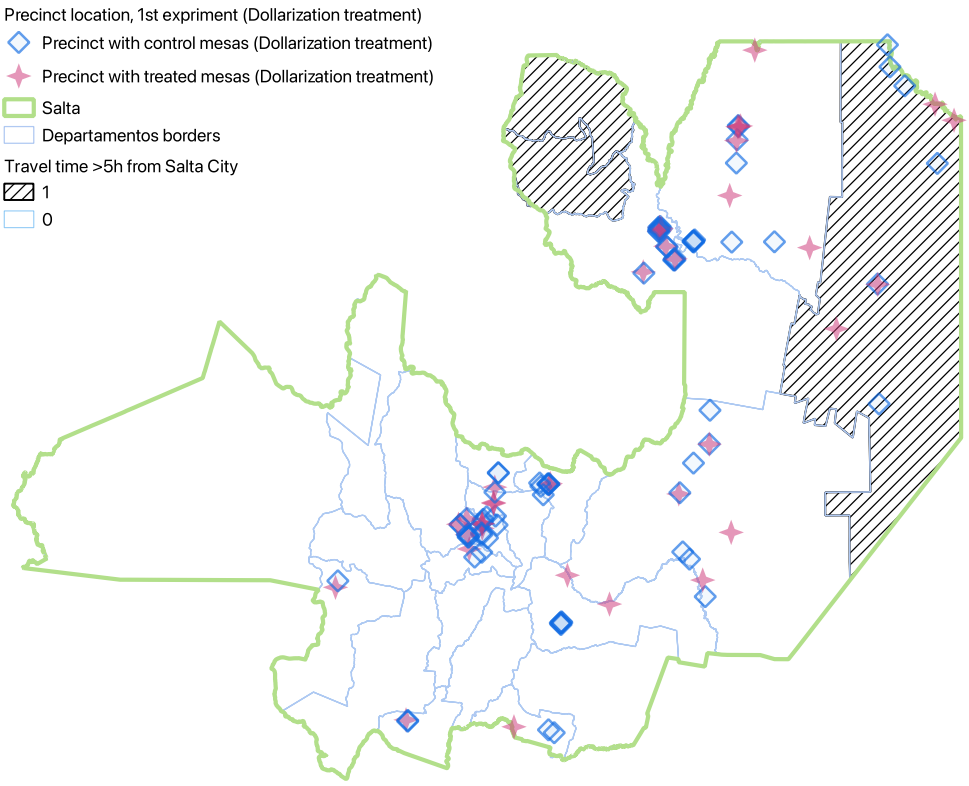


Figure A9: Map of precincts with education treatment, Runoff experiment

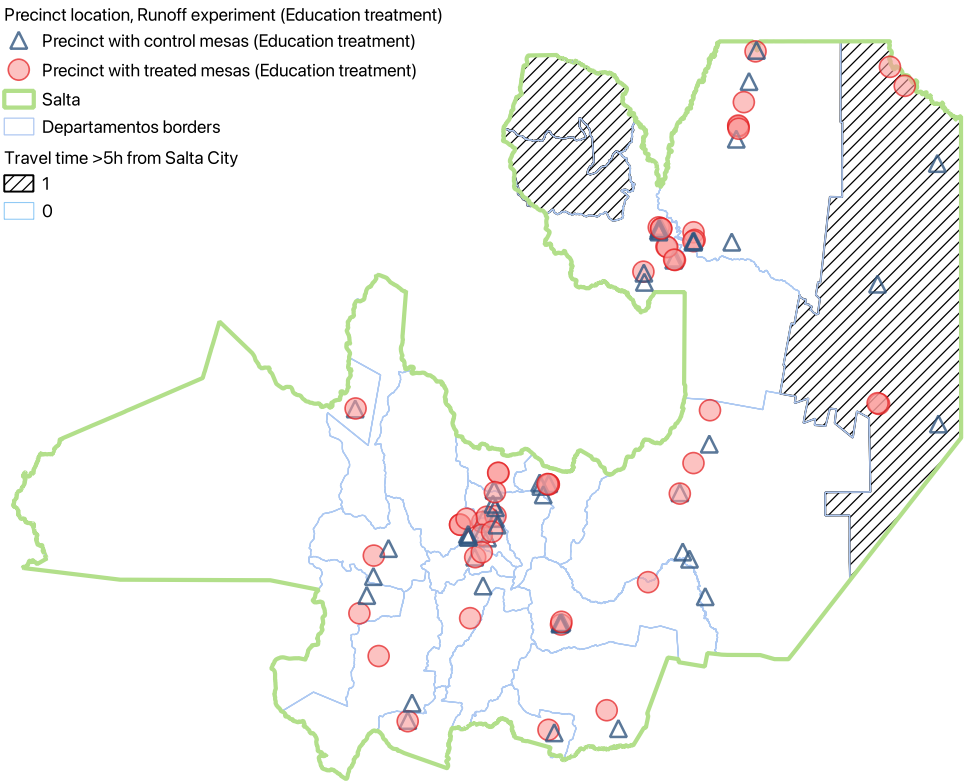
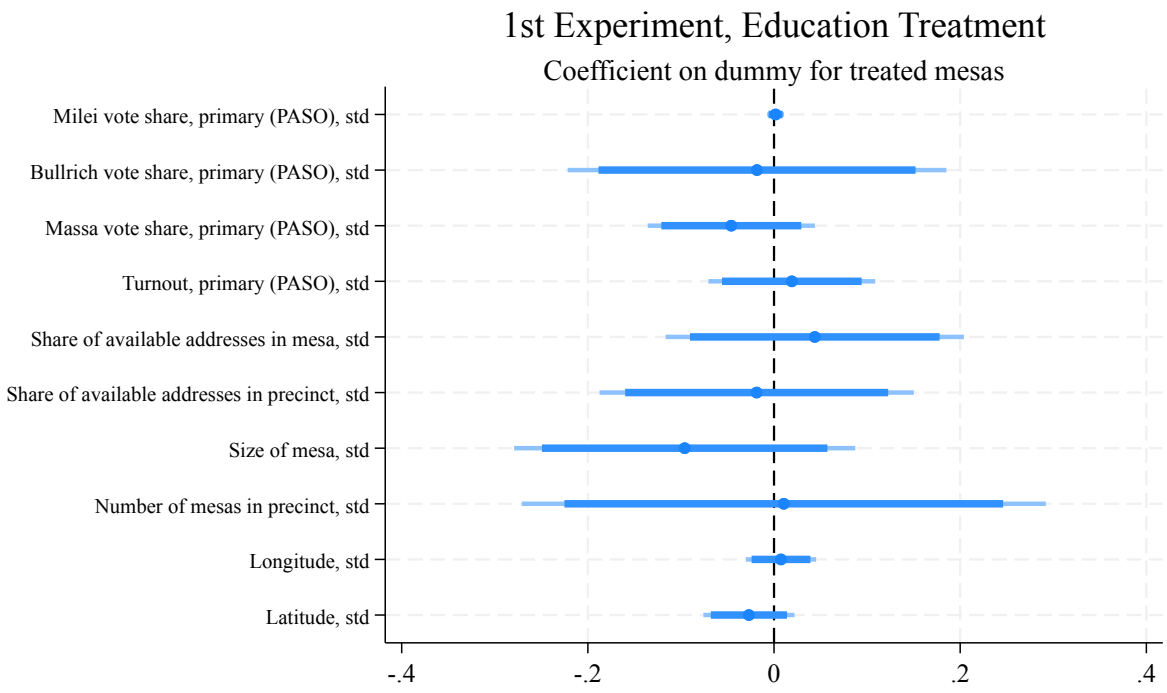
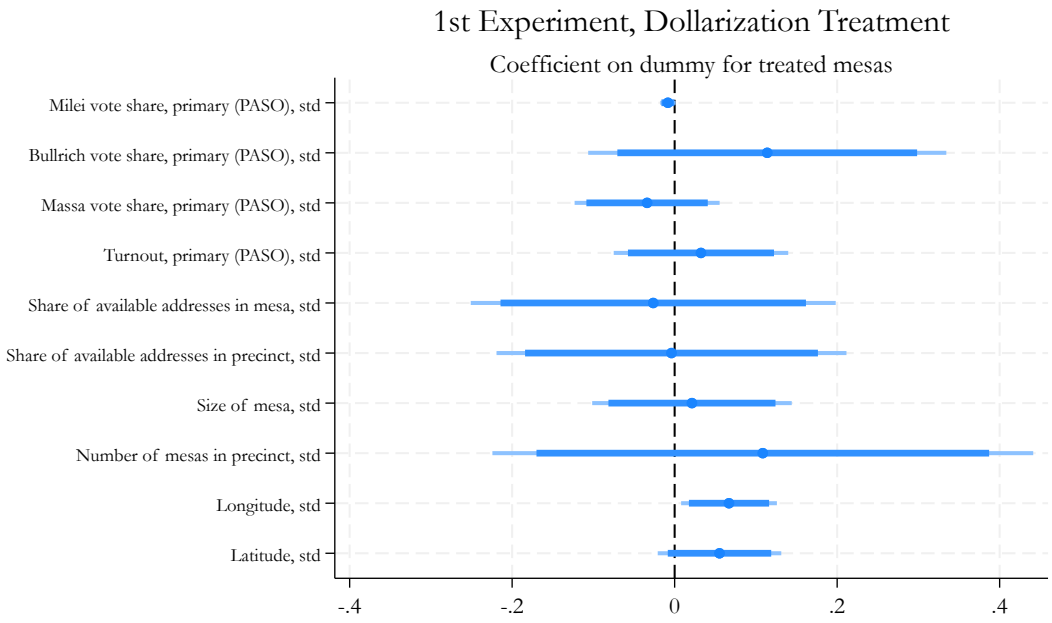


Figure A10: Balance, extensive margin: 1st Experiment, Education Treatment
Average difference between treated and non-treated mesas in pre-treatment characteristics



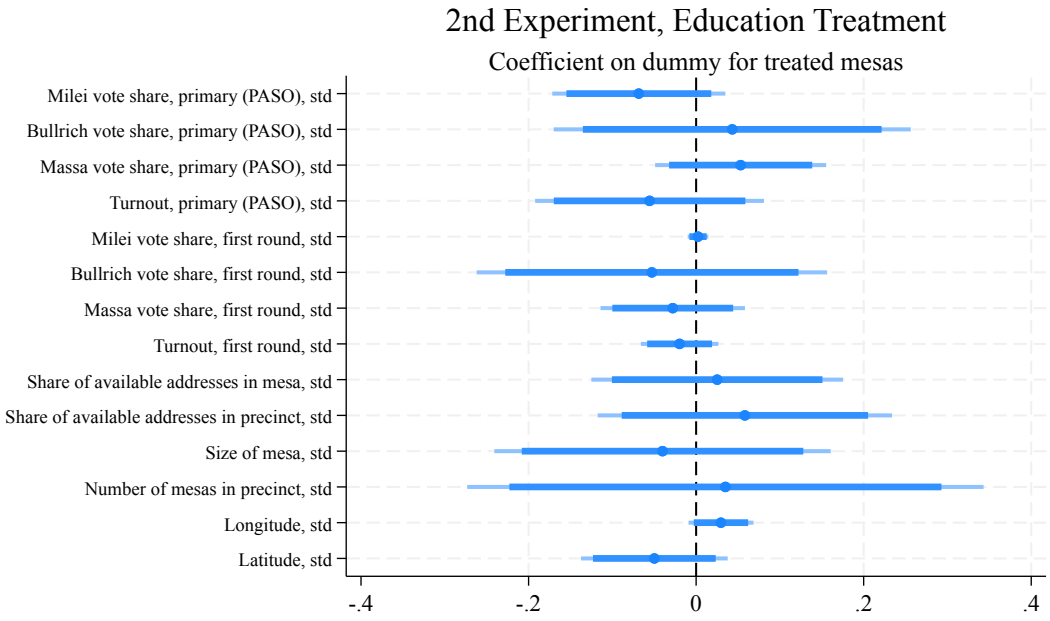

Note: The figure presents the coefficients from a bivariate regression, in which standardized pre-treatment characteristics are regressed on the dummy for treated mesa.

Figure A11: Balance, extensive margin: 1st Experiment, Dollarization Treatment
Average difference between treated and non-treated mesas in pre-treatment characteristics




Note: The figure presents the coefficients from a bivariate regression, in which standardized pre-treatment characteristics are regressed on the dummy for treated mesa.

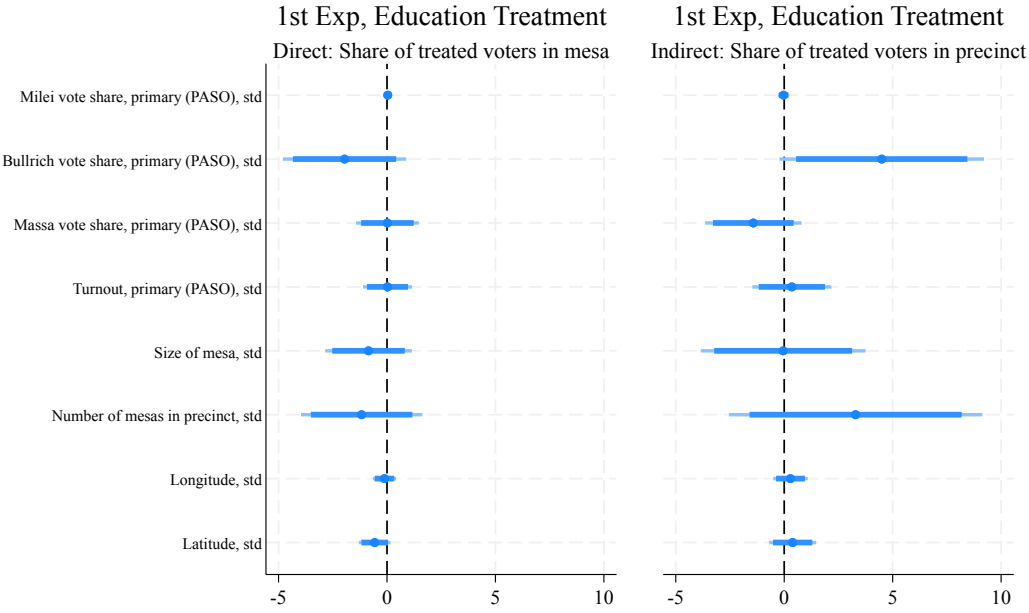
Figure A12: Balance, extensive margin: 2nd Experiment, Education Treatment
Average difference between treated and non-treated mesas in pre-treatment characteristics



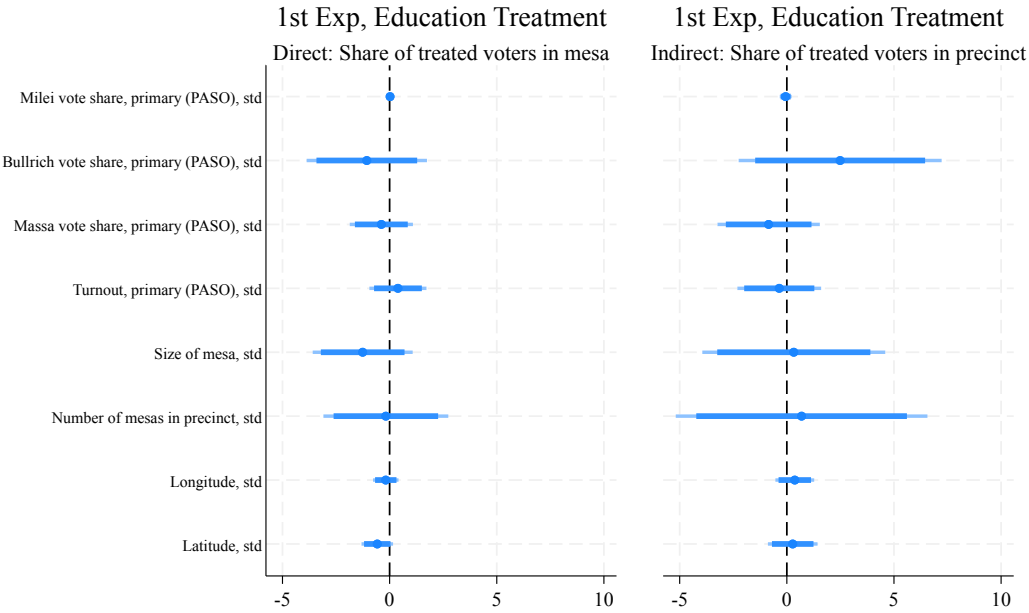

Note: The figure presents the coefficients from a bivariate regression, in which standardized pre-treatment characteristics are regressed on the dummy for treated mesa.

Figure A13: Balance, intensive margin: 1st Experiment, Education Treatment
Direct and indirect effects of treatment on pre-treatment characteristics

Panel A: Linear controls for available addresses

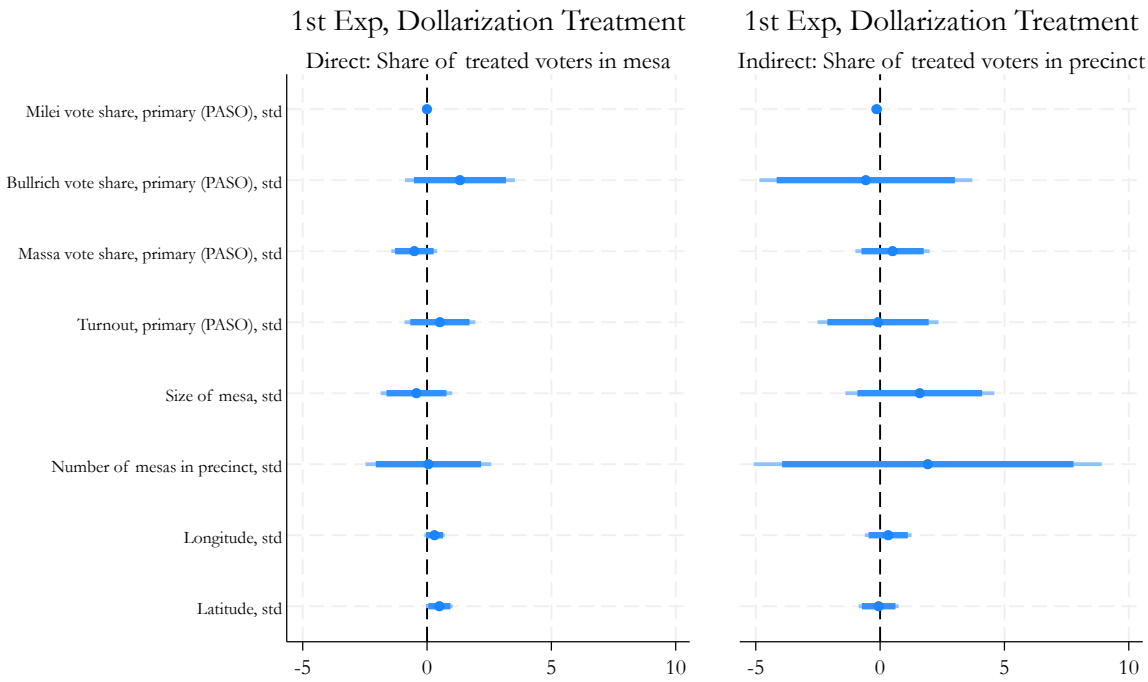



Panel B: FEs for each decile of available addresses




Note: The figure presents the main coefficients of the placebo estimation of equation (3), in which standardized pre-treatment characteristics are considered as outcomes and the main regressors are the share of treated voters in mesa and in precinct controlling linearly for the shares of available addresses in mesa and precinct in Panel A and for dummies for each decile in the shares of available addresses in mesa and precinct in Panel B.

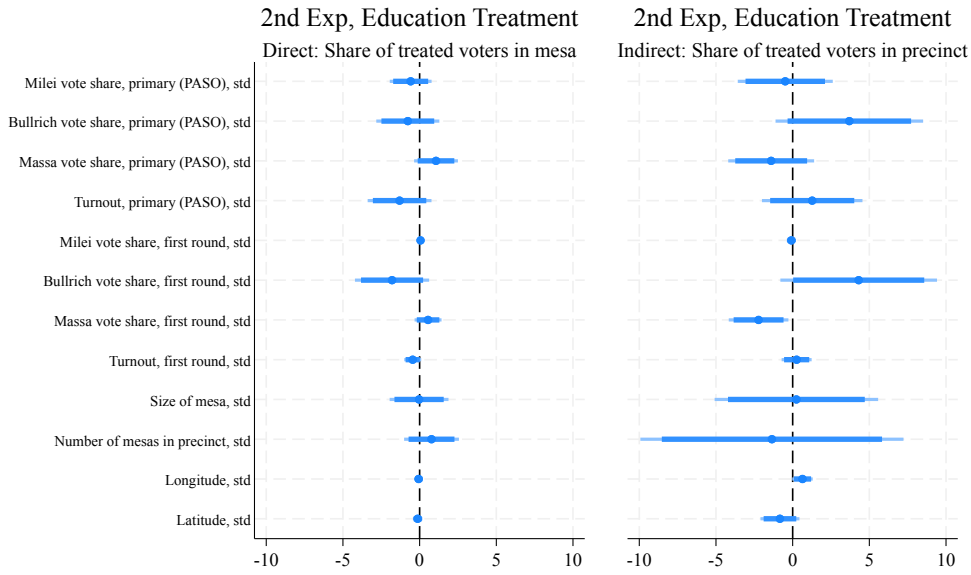
Figure A14: Balance, intensive margin: 1st Experiment, Dollarization Treatment
Direct and indirect effects of treatment on pre-treatment characteristics



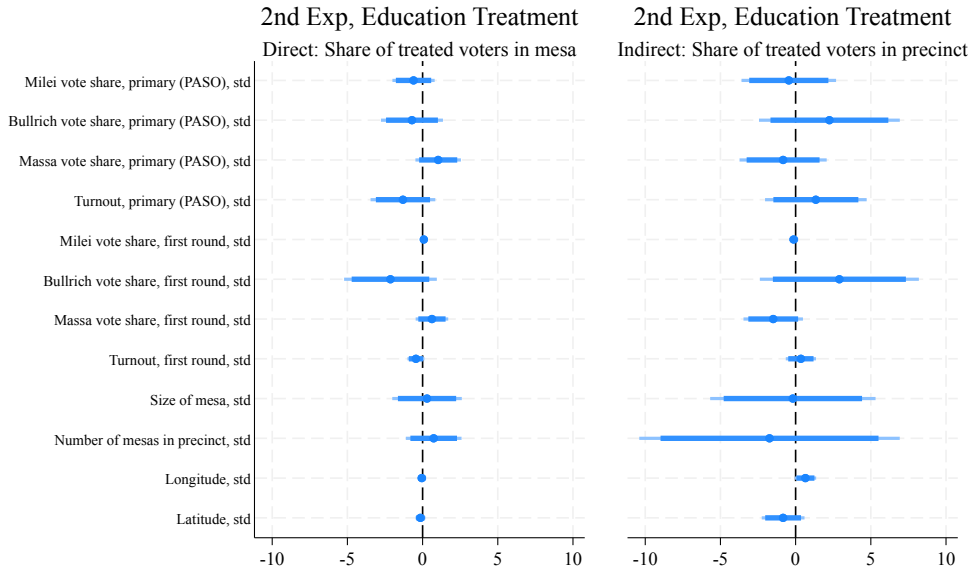

Note: The figure presents the main coefficients of the placebo estimation of equation (3), in which standardized pre-treatment characteristics are considered as outcomes and the main regressors are the share of treated voters in mesa and in precinct controlling only for the shares of available addresses in mesa and precinct.

Figure A15: Balance, intensive margin: 2nd Experiment, Education Treatment
Direct and indirect effects of treatment on pre-treatment characteristics

Panel A: No weights




Panel B: Entropy balancing weights




Note: The figure presents the main coefficients of the placebo estimation of equation (3), in which standardized pre-treatment characteristics are considered as outcomes and the main regressors are the share of treated voters in mesa and in precinct controlling for the shares of available addresses in mesa and precinct. Panel A presents the results without weights, Panel B presents the results with entropy balancing weights.

Figure A16: Placebo, first round dollarization treatment, 1000 alternative randomizations

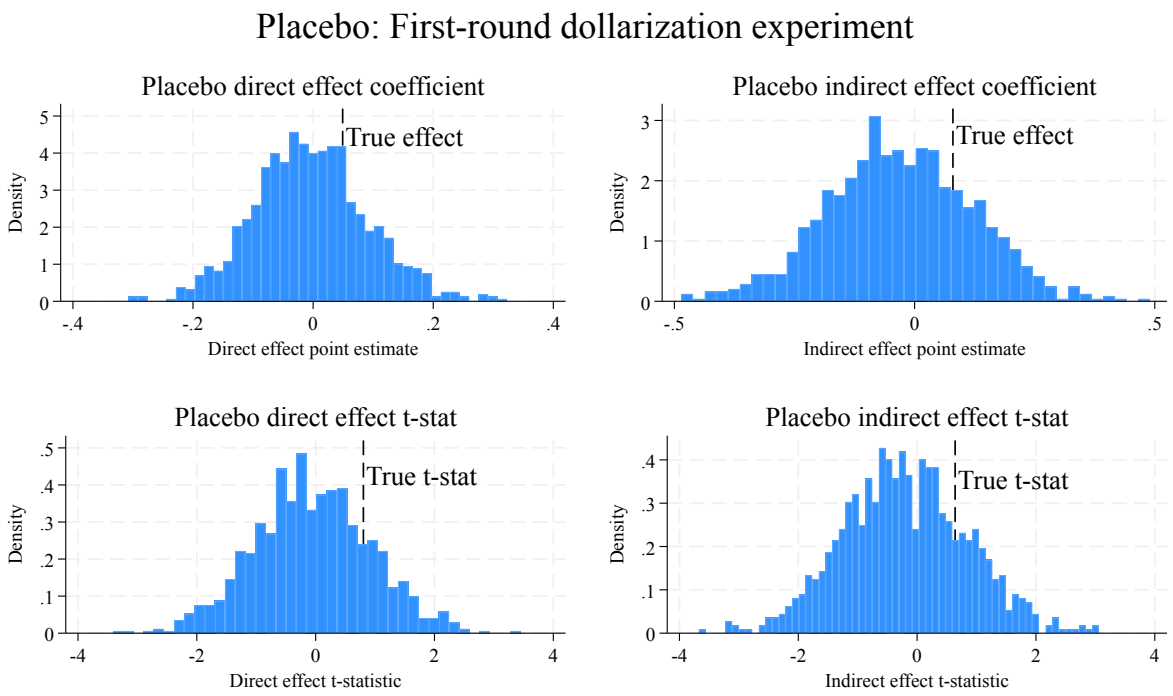


Table A1: Summary Statistics, Mesa-level, Main Experiments

| | Obs | Mean | SD | Min | Max |
|---|---|---|---|---|---|
| Education treatment, First round: Share of treated voters, mesa | 1273 | 0.012 | 0.032 | 0.000 | 0.180 |
| Education treatment, First round: Share of treated voters, precinct | 1273 | 0.011 | 0.023 | 0.000 | 0.100 |
| Education treatment, Runoff: Share of treated voters, mesa | 1273 | 0.012 | 0.034 | 0.000 | 0.230 |
| Education treatment, Runoff: Share of treated voters, precinct | 1273 | 0.010 | 0.022 | 0.000 | 0.102 |
| Dollarization treatment, First round: Share of treated voters, mesa | 1273 | 0.012 | 0.033 | 0.000 | 0.274 |
| Dollarization treatment, First round: Share of treated voters, precinct | 1273 | 0.011 | 0.024 | 0.000 | 0.136 |
| Share of available addresses, mesa | 1273 | 0.097 | 0.035 | 0.051 | 0.282 |
| Share of available addresses, precinct | 1273 | 0.088 | 0.035 | 0.004 | 0.250 |
| Number of mesas in precinct | 1273 | 8.943 | 3.311 | 2 | 14 |
| Size of mesa | 1273 | 335 | 24 | 183 | 359 |
| Distance from Salta to departmental capital in driving hours | 1273 | 2.042 | 1.666 | 0.000 | 7.000 |
| Milei vote share, primary (PASO) | 1273 | 0.494 | 0.135 | 0.000 | 0.951 |
| Bullrich party vote share, primary (PASO) | 1273 | 0.145 | 0.064 | 0.000 | 0.347 |
| Massa party vote share, primary (PASO) | 1273 | 0.304 | 0.171 | 0.000 | 0.967 |
| Turnout, primary (PASO) | 1273 | 0.611 | 0.109 | 0.060 | 0.919 |
| Milei vote share, first round | 1213 | 0.380 | 0.103 | 0.000 | 0.608 |
| Bullrich vote share, first round | 1213 | 0.116 | 0.053 | 0.000 | 0.582 |
| Massa vote share, first round | 1213 | 0.441 | 0.151 | 0.054 | 0.992 |
| Turnout, first round | 1213 | 0.732 | 0.063 | 0.407 | 0.944 |
| Milei percent, second round | 1247 | 0.522 | 0.144 | 0.000 | 0.784 |
| Massa percent, second round | 1247 | 0.478 | 0.144 | 0.216 | 1.000 |
| Turnout, second round | 1247 | 0.733 | 0.061 | 0.330 | 0.865 |

Table A2: First experiment, dollarization treatment, Milei vote share as outcome

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Dependent variable: | Milei Vote Share, First Round | | | |
| Sample: | Matched sets | | | |
| Share of treated voters, mesa | 0.049 | 0.048 | 0.002 | –0.065 |
| (Direct effect, **dollarization treatment**) | (0.061) | (0.061) | (0.065) | (0.068) |
| Share of treated voters, precinct | 0.080 | 0.087 | 0.111 | 0.145 |
| (Indirect effect, **dollarization treatment**) | (0.125) | (0.126) | (0.118) | (0.146) |
| Milei vote share, primary (PASO) | | 0.367 | 1.118 | 1.265* |
| | | (0.786) | (0.766) | (0.755) |
| Bullrich party vote share, primary (PASO) | | | 0.259** | 0.153* |
| | | | (0.122) | (0.088) |
| Massa party vote share, primary (PASO) | | | 0.035 | –0.102 |
| | | | (0.151) | (0.129) |
| Turnout, primary (PASO) | | | 0.335*** | 0.335*** |
| | | | (0.092) | (0.082) |
| Mean, Dep. Var. | 0.393 | 0.393 | 0.393 | 0.393 |
| SD, Dep. Var. | 0.079 | 0.079 | 0.079 | 0.079 |
| R2 | 0.833 | 0.833 | 0.856 | 0.878 |
| Observations | 384 | 384 | 384 | 384 |
| Available addresses, mesa, precinct | ✓ | ✓ | ✓ | |
| Matched set FEs | ✓ | ✓ | ✓ | ✓ |
| Latitude, Longitude | | | | ✓ |
| Decile FEs for available addresses | | | | ✓ |

**Note:** *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at precinct level are in parentheses. Unit of observation is mesa.

Table A3: First experiment, dollarization treatment, estimating direct and indirect effects separately, Milei vote share as outcome

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Dependent variable: | Milei Vote Share, First Round | | | | | |
| | Direct effect, **dollarization treatment** | | | Indirect effect, **dollarization treatment** | | |
| Sample: | Matched sets | | Full | Matched sets | | Full |
| Share of treated voters, mesa | –0.017 | 0.015 | 0.047 | | | |
| (Direct effect, **dollarization treatment**) | (0.051) | (0.046) | (0.049) | | | |
| Share of treated voters, precinct | | | | 0.102 | 0.056 | 0.001 |
| (Indirect effect, **dollarization treatment**) | | | | (0.166) | (0.155) | (0.094) |
| Milei vote share, primary (PASO) | | 0.396*** | 0.261** | | –0.060 | 0.339*** |
| | | (0.107) | (0.119) | | (1.450) | (0.083) |
| Bullrich party vote share, primary (PASO) | | 0.045 | –0.014 | | 0.300 | 0.099 |
| | | (0.112) | (0.092) | | (0.311) | (0.077) |
| Massa party vote share, primary (PASO) | | 0.012 | –0.058 | | –0.012 | –0.070 |
| | | (0.106) | (0.108) | | (0.377) | (0.074) |
| Turnout, primary (PASO) | | 0.315*** | 0.203*** | | 0.453*** | 0.285*** |
| | | (0.062) | (0.053) | | (0.127) | (0.048) |
| Mean, Dep. Var. | 0.392 | 0.392 | 0.392 | 0.400 | 0.400 | 0.392 |
| SD, Dep. Var. | 0.078 | 0.078 | 0.087 | 0.079 | 0.079 | 0.091 |
| R2 | 0.752 | 0.809 | 0.805 | 0.831 | 0.879 | 0.731 |
| Observations | 362 | 362 | 887 | 153 | 153 | 959 |
| Available addresses, mesa, precinct | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Precinct FEs | ✓ | ✓ | ✓ | | | |
| Matched set FEs | | | | ✓ | ✓ | |
| Department FEs | | | | | | ✓ |
| Latitude, Longitude | | | | | ✓ | ✓ |

**Note:** *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at precinct level are in parentheses. Unit of observation is mesa. In columns 1, 2, and 3, we exclude mesas that received education treatment. In columns, 4, 5, and 6, to estimate the indirect effect, we exclude mesas that received dollarization treatment.

Table A4: First experiment, education treatment, turnout as outcome

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Dependent variable: | Turnout, First Round | | | |
| Sample: | Matched sets | | | |
| Share of treated voters, mesa | 0.063 | 0.072 | 0.061 | –0.011 |
| (Direct effect, education treatment) | (0.241) | (0.239) | (0.240) | (0.213) |
| Share of treated voters, precinct | 0.153 | 0.145 | 0.105 | 0.153 |
| (Indirect effect, education treatment) | (0.398) | (0.397) | (0.418) | (0.466) |
| Milei vote share, primary (PASO) | | –1.976 | –1.227 | –2.266 |
| | | (2.276) | (2.485) | (2.638) |
| Bullrich party vote share, primary (PASO) | | | –0.061 | –0.102 |
| | | | (0.273) | (0.348) |
| Massa party vote share, primary (PASO) | | | –0.192 | –0.184 |
| | | | (0.221) | (0.345) |
| Turnout, primary (PASO) | | | 0.365 | 0.203 |
| | | | (0.227) | (0.272) |
| Mean, Dep. Var. | 0.711 | 0.711 | 0.711 | 0.711 |
| SD, Dep. Var. | 0.155 | 0.155 | 0.155 | 0.155 |
| R2 | 0.469 | 0.471 | 0.476 | 0.508 |
| Observations | 389 | 389 | 389 | 389 |
| Available addresses, mesa, precinct | ✓ | ✓ | ✓ | |
| Matched set FEs | ✓ | ✓ | ✓ | ✓ |
| Latitude, Longitude | | | | ✓ |
| Decile FEs for available addresses | | | | ✓ |

**Note:** *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at precinct level are in parentheses. Unit of observation is mesa.

Table A5: First experiment, education treatment, estimating direct and indirect effects separately, turnout as outcome

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Dependent variable: | Turnout, First Round | | | | | |
| | Direct effect | | | Indirect effect | | |
| Sample: | Matched sets | | Full | Matched sets | | Full |
| Share of treated voters, mesa | 0.091 | 0.059 | –0.090 | | | |
| (Direct effect, education treatment) | (0.271) | (0.258) | (0.227) | | | |
| Share of treated voters, precinct | | | | –0.176 | –0.104 | 0.051 |
| (Indirect effect, education treatment) | | | | (0.391) | (0.434) | (0.389) |
| Milei vote share, primary (PASO) | | –0.390 | 0.068 | | –4.833 | –0.056 |
| | | (0.309) | (0.270) | | (4.196) | (0.194) |
| Bullrich party vote share, primary (PASO) | | –0.354 | –0.132 | | –1.453 | –0.122 |
| | | (0.224) | (0.219) | | (0.972) | (0.179) |
| Massa party vote share, primary (PASO) | | –0.286 | –0.054 | | –1.336 | –0.150 |
| | | (0.229) | (0.246) | | (0.840) | (0.187) |
| Turnout, primary (PASO) | | 0.241 | 0.229** | | 0.102 | 0.337*** |
| | | (0.167) | (0.116) | | (0.309) | (0.097) |
| Mean, Dep. Var. | 0.713 | 0.713 | 0.712 | 0.711 | 0.711 | 0.713 |
| SD, Dep. Var. | 0.152 | 0.152 | 0.152 | 0.151 | 0.151 | 0.148 |
| R2 | 0.295 | 0.305 | 0.229 | 0.567 | 0.595 | 0.108 |
| Observations | 357 | 357 | 907 | 129 | 129 | 991 |
| Available addresses, mesa, precinct | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Precinct FEs | ✓ | ✓ | ✓ | | | |
| Matched set FEs | | | | ✓ | ✓ | |
| Department FEs | | | | | | ✓ |
| Latitude, Longitude | | | | | ✓ | ✓ |

**Note:** *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at precinct level are in parentheses. Unit of observation is mesa. In columns 1, 2, and 3, we exclude mesas that received dollarization treatment. In columns, 4, 5, and 6, to estimate the indirect effect, we exclude mesas that received education treatment.

Table A6: Second experiment, education treatment, turnout as outcome

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Dependent variable: | Turnout, Runoff | | | | | |
| Sample: | Matched sets, full | | | | | |
| Weights: | No weights | | | Entropy balancing weights | | |
| Share of treated voters, mesa | –0.040 | –0.038 | 0.021 | –0.034 | –0.033 | 0.026 |
| (Direct effect) | (0.050) | (0.050) | (0.045) | (0.049) | (0.049) | (0.045) |
| Share of treated voters, precinct | –0.085 | –0.084 | –0.122 | –0.078 | –0.077 | –0.114 |
| (Indirect effect) | (0.110) | (0.110) | (0.104) | (0.111) | (0.112) | (0.104) |
| Milei vote share, primary (PASO) | | 0.016 | –0.106 | | 0.014 | –0.110 |
| | | (0.029) | (0.091) | | (0.030) | (0.093) |
| Bullrich party vote share, primary (PASO) | | | –0.133 | | | –0.136 |
| | | | (0.112) | | | (0.116) |
| Massa party vote share, primary (PASO) | | | –0.201* | | | –0.200* |
| | | | (0.107) | | | (0.109) |
| Turnout, primary (PASO) | | | 0.275*** | | | 0.280*** |
| | | | (0.055) | | | (0.055) |
| Mean, Dep. Var. | 0.743 | 0.743 | 0.743 | 0.742 | 0.742 | 0.742 |
| SD, Dep. Var. | 0.056 | 0.056 | 0.056 | 0.055 | 0.055 | 0.055 |
| R2 | 0.776 | 0.776 | 0.832 | 0.773 | 0.774 | 0.834 |
| Observations | 390 | 390 | 390 | 390 | 390 | 390 |
| Available addresses, mesa, precinct | ✓ | ✓ | | ✓ | ✓ | |
| Matched set FEs | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Latitude, Longitude | | | ✓ | | | ✓ |
| Decile FEs for available addresses | | | ✓ | | | ✓ |

**Note:** *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at precinct level are in parentheses. Unit of observation is mesa.

Table A7: Second experiment, education treatment, estimating direct and indirect effects separately, turnout as outcome

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Dependent variable: | Turnout, Runoff | | | | | | | |
| Sample: | Matched sets, full | | | | | | | |
| Weights: | No weights | | Entropy balancing | | No weights | | Entropy balancing | |
| | Direct effect | | | | Indirect effect | | | |
| Share of treated voters, mesa | –0.065 | –0.062 | –0.059 | –0.056 | | | | |
| (Direct effect) | (0.047) | (0.046) | (0.049) | (0.049) | | | | |
| Share of treated voters, precinct | | | | | –0.114 | –0.075 | –0.095 | –0.063 |
| (Indirect effect) | | | | | (0.149) | (0.139) | (0.147) | (0.139) |
| Milei vote share, first round | 0.059 | 0.041 | 0.060 | 0.044 | –0.597 | –0.576 | –0.598 | –0.577 |
| | (0.042) | (0.037) | (0.039) | (0.034) | (0.490) | (0.535) | (0.494) | (0.537) |
| Milei vote share, primary (PASO) | | 0.054 | | 0.069 | | 0.199** | | 0.185* |
| | | (0.056) | | (0.058) | | (0.095) | | (0.094) |
| Bullrich party vote share, primary (PASO) | | –0.002 | | 0.014 | | 0.114 | | 0.100 |
| | | (0.094) | | (0.096) | | (0.106) | | (0.104) |
| Massa party vote share, primary (PASO) | | 0.021 | | 0.038 | | 0.202** | | 0.195** |
| | | (0.072) | | (0.075) | | (0.098) | | (0.094) |
| Mean, Dep. Var. | 0.744 | 0.744 | 0.743 | 0.743 | 0.740 | 0.740 | 0.739 | 0.739 |
| SD, Dep. Var. | 0.054 | 0.054 | 0.054 | 0.054 | 0.052 | 0.052 | 0.051 | 0.051 |
| R2 | 0.756 | 0.758 | 0.760 | 0.762 | 0.831 | 0.846 | 0.836 | 0.850 |
| Observations | 374 | 374 | 374 | 374 | 175 | 175 | 175 | 175 |
| Available addresses, mesa, precinct | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Precinct FEs | ✓ | ✓ | ✓ | ✓ | | | | |
| Matched set FEs. | | | | | ✓ | ✓ | ✓ | ✓ |
| Latitude, Longitude | | | | | | ✓ | | ✓ |

**Note:** *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at precinct level are in parentheses. Unit of observation is mesa. In columns 1 to 4, we exclude mesas that received dollarization treatment. In columns, 5 to 8, to estimate the indirect effect, we exclude mesas that received education treatment. All columns report results for the matched sets sample.

# A Survey Details

## Questionnaire (Translated into English)

## Introduction (read by interviewer)

Hello, how are you? My name is [NAME] and I am conducting a survey for a study from universities in Europe and the United States. We want to know what people think about political campaigns in Argentina. It is anonymous, nobody will know your name, and it lasts about 5 minutes. You can stop answering whenever you want.

Would you like to participate?

- Yes
- No $\rightarrow$ END

## Screening Question

Q0. Please indicate if the day of the month you were born is even or odd.

(For clarification: for example, if you were born on the 2nd, 4th, 6th, and so on up to the 30th, the answer is "even." If you were born on the 1st, 3rd, 5th, 7th, and so on up to the 31st, the answer is "odd.")

- A. I was born on an even day of the month
- B. I was born on an odd day of the month

## Vignette (Imaginary Scenario)

I am going to tell you a situation that is like an "imagination game." It is not real, it is just to know your opinion.
Imagine we are in an election and in Argentina, Alberto Fernández is still president.
There are two candidates:

- One from a traditional, long-standing party.
- The other is new, not a politician, and says he wants to fight against corruption and change things in the country. He is a non-traditional candidate.

The elections are close, and the non-traditional candidate has real chances of winning.
You receive a colorful leaflet (like a campaign flyer) criticizing a proposal from the new candidate. The leaflet says that if the new candidate implements the policy in that proposal, it may harm the poor, according to some experts.

*INTERVIEWER: Verify the response to Question 0. If the answer is A (the person was born on an even day), read Scenario A. If the answer is B (the person was born on an odd day), read Scenario B.*

**Scenario A (even birth date): Suppose you believe what the leaflet says, and because of that you feel less likely to vote for the new candidate.**

**Scenario B (odd birth date): Suppose you do not believe the leaflet and think it was made to attack the new candidate, and that makes you more likely to vote for him or does not change anything.**

## Block 1 – Opinions about the leaflet

Q1. Would you tell your friends or neighbors what the leaflet says? (Use a card with numbers 1 to 5 in colors or faces)

- Definitely would not tell anyone
- Maybe/probably would not tell anyone
- Don't know if I would tell someone or not
- Maybe/probably would tell someone
- Definitely would tell someone

Q2. How many people do you think you would tell?

- Nobody
- 1 or 2 people
- 3 to 5 people
- More than 6 people

Q3. What do you think about those who sent the leaflet? (choose the option that best represents you)

- It seems good, because they give information
- It seems good, they have the right to express their opinion
- It seems bad, it could be a lie
- It bothers me, it's wasted paper and a waste of time
- It doesn't change anything

Q4. If you were talking with someone about politics, would you mention the leaflet?

- Yes, and I would show the leaflet
- Yes, but I would only talk about it without showing it
- No

Q5. Why would you mention the leaflet in a conversation with neighbors or friends? (You may mark more than one)

- To convince others to vote like you
- Because you like talking about politics
- To know what others think about the leaflet
- Because you believe traditional politicians attack the new candidate
- Because you want to warn others about the risks of supporting the new candidate
- Other: _____ (Interviewer: please record the response)

Q6. If the leaflet were against a traditional politician, would you react the same?

- Yes
- No

## Block 2 – Opinion on Milei's education proposal

*INTERVIEWER READS:* President Milei wants to change how public schools are funded and give vouchers, as he proposed in his 2023 campaign. Some experts say that could be harmful for poor children.

Here is a leaflet presenting this argument. Please take a look. SHOW THE LEAFLET TO THE RESPONDENT. [This is the same leaflet as education treatment in the main experiment, but with references to 2023 election hidden.]

Q7. Knowing that, do you feel more or less likely to vote for Milei's party (La Libertad Avanza) in the October legislative elections?

- More likely
- Less likely
- Does not change your opinion
- Don't know

Q8. Do you think it is important to talk about this topic with neighbors or friends before the elections to help them decide their vote?

- Yes
- No

## Block 3 – About you

Q9. Are you. . . ?

- Man
- Woman
- Other / Prefer not to say

Q10. How old are you? (Record number)

Q11. What is the first letter of your last name? (Record letter or "prefer not to say")

Q12. What is the highest level of education you completed?

- Did not finish high school
- Finished high school
- Took a technical or professional course
- Went to university
- Completed a postgraduate degree

Q13. Who did you vote for in the August 2023 primaries (PASO)? (Use card with options)

Q14. And in the October 2023 elections?

Q15. And in the November 2023 runoff?

Q16. Do you remember where you voted? (e.g., School XX, near the plaza, etc.)

Q17. In which municipality do you currently live?

Q18. What is your approximate monthly income (if you want to say it)? (Use card with ranges in Argentine pesos)

Thank you very much for your participation!

## Summary statistics for the survey

Table A8: Summary Statistics of the Main Survey Variables

| | Obs | Mean | Std. Dev. | Min | Max |
|---|---|---|---|---|---|
| **Panel A. Treatment and Outcomes** | | | | | |
| Imagine: you do not believe the leaflet | 2,021 | 0.471 | 0.499 | 0 | 1 |
| Very likely to discuss with peers | 2,021 | 0.351 | 0.477 | 0 | 1 |
| Likely or very likely to discuss with peers | 2,021 | 0.612 | 0.487 | 0 | 1 |
| Will discuss with at least 6 people (max category) | 2,021 | 0.222 | 0.416 | 0 | 1 |
| Number of people (count, minimum of each range) | 2,021 | 2.360 | 2.181 | 0 | 6 |
| Number of people $\times$ Likely to discuss with peers | 2,021 | 2.035 | 2.315 | 0 | 6 |
| Will discuss to convince others to vote like me | 2,021 | 0.110 | 0.313 | 0 | 1 |
| Behave differently of leaflet about mainstream candidate | 2,021 | 0.336 | 0.472 | 0 | 1 |
| Negative attitude to senders (they lie) | 2,021 | 0.117 | 0.321 | 0 | 1 |
| Positive attitude to senders (they give info) | 2,021 | 0.258 | 0.438 | 0 | 1 |
| **Panel B. Main Controls** | | | | | |
| Male | 2,021 | 0.466 | 0.499 | 0 | 1 |
| Third gender | 2,021 | 0.027 | 0.163 | 0 | 1 |
| Education: High school | 2,021 | 0.529 | 0.499 | 0 | 1 |
| Education: Vocational school | 2,021 | 0.207 | 0.405 | 0 | 1 |
| Education: Has a university degree | 2,021 | 0.156 | 0.363 | 0 | 1 |
| Income category 1 | 2,021 | 0.132 | 0.338 | 0 | 1 |
| Income category 2 | 2,021 | 0.262 | 0.440 | 0 | 1 |
| Income category 3 | 2,021 | 0.072 | 0.258 | 0 | 1 |
| Income category 4 | 2,021 | 0.328 | 0.470 | 0 | 1 |
| Age | 2,021 | 35.195 | 12.902 | 18 | 91 |
| Voted Milei, runoff | 2,021 | 0.468 | 0.499 | 0 | 1 |
| Did not vote, runoff | 2,021 | 0.026 | 0.158 | 0 | 1 |
| Voted Milei, PASO | 2,021 | 0.358 | 0.480 | 0 | 1 |
| Voted Bullrich, PASO | 2,021 | 0.104 | 0.306 | 0 | 1 |
| Voted small candidates, PASO | 2,021 | 0.102 | 0.303 | 0 | 1 |

Notes: Table reports means, standard deviations, minima, and maxima for the treatment, outcomes, and main controls from the vignette survey.

## A.1 Showing the actual leaflet criticizing Milei education proposal to survey respondents

At the end of the outcomes block of the survey (see Block 2 of the Questionnaire), we presented respondents with the actual leaflet from the main 2023 experiment, which criticized Milei's education proposal, and asked them two questions. First, we asked whether seeing this leaflet made them more or less likely to vote for Milei's party (La Libertad Avanza) in the forthcoming October 2025 legislative elections, in which Milei is the established incumbent. Second, we asked whether respondents think it is important to talk about the topic of education with neighbors and friends before the elections to help them decide their vote (see questions 7 and 8 of the survey instrument). The purpose was to understand whether the negative reaction to a leaflet that campaigns against an incumbent, rather than against an outsider, triggers the same or a different indirect-effect reaction.

The results are presented in Online Appendix Table A9. In column 1, we verify that there is persistence in respondents' voting choices. Those who voted for Milei in 2023 are more likely to dismiss the leaflet's information and state that this leaflet makes them more likely to vote for Milei's party in the upcoming October 2025 elections, despite the criticism of his policy proposal. In column 2, we consider the dummy indicating that respondents considered it important to discuss the education policy, criticized in the leaflet, with peers as the dependent variable and regress it on the dummy that the leaflet backfired. We find a significant and negative association between these two variables. In addition, finding it important to discuss education policy is also negatively correlated with support for Milei in the 2023 elections.

Overall, these results suggest that a political campaign that criticizes the incumbent is seen differently by voters than a political campaign that criticizes the outsider candidate. People who disagree with campaigns against outsiders mobilize to talk to their peers (as we show in the main text of the paper, see Panel A of Table 7). In contrast, they simply dismiss the arguments against the incumbent and prefer not to discuss the criticized policies with peers. This is consistent with the results presented in Table 7, namely, that respondents who negatively react to a campaign leaflet against the outsider state that they would mobilize to campaign in his support but that they would have behaved differently if the leaflet concerned the mainstream candidate.

Table A9: Reaction to leaflet against incumbent (Milei in 2025) and the propensity to discuss leaflet policy with peers

| | (1) | (2) |
|---|---|---|
| Dependent variable: | Leaflet backfires: vote more for incumbent, 2025 | Important to discuss policy criticized in leaflet with peers |
| Voted for the winner (Milei), runoff 2023 | 0.0518*** | –0.1754*** |
| | (0.0156) | (0.0292) |
| Leaflet backfires: vote more for incumbent, 2025 | | –0.1094*** |
| | | (0.0397) |
| Mean, Dep. Var. | 0.092 | 0.669 |
| SD, Dep. Var. | 0.288 | 0.471 |
| R2 | 0.147 | 0.234 |
| Observations | 2021 | 2021 |
| Age, gender, income, education | ✓ | ✓ |
| Vote PASO 2023 | ✓ | ✓ |
| Municipality and first letter FEs | ✓ | ✓ |

**Note:** *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are in parentheses. Unit of observation is survey respondent.