I'd Be Surprisingly Good for You: Political Information and Network Effects*

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March 2025

Abstract

The rise of political polarization has sparked research on heterogeneity in voter responses to election campaign messages. The same political information campaign can persuade some voters while dissuading others. Beyond direct effects, campaigns have indirect effects as exposed voters share messages with peers. The net impact of a campaign depends on whether voters who react positively or negatively are more vocal within their social networks. Leveraging unique features of Argentina's electoral system, we conducted a randomized experiment during the 2023 presidential election, varying both direct and indirect exposure to a campaign. Partnering with a local NGO, we sent fact-checking leaflets on Javier Milei's policy proposals to different subsets of voters before both the first round and the runoff. The leaflet campaign reduced Milei's support among recipients (direct effect) but increased it among their unexposed neighbors (indirect effect), with the latter effect dominating. The interventions in the first round and the runoff had consistent results, and the effects of the first intervention persisted through the runoff. The opposite-sign direct and indirect effects suggest that voters disagreeing with the leaflet message were more likely to discuss it with their neighbors. The results highlight a novel social-network mechanism through which information campaigns can backfire.

Keywords: Political Information Campaign, Elections, Argentina, Milei, Network Effects
JEL Classification: C93, D72, P00

*We are grateful to seminar participants at UC-Berkeley, Stanford GSB, Universidad Torcuato Di Tella and participants of Political Economy conference in Princeton and Political Economy of Development and Conflict conference in Barcelona (UPF) for many helpful comments. The study is approved by the Institutional Review Board of Paris School of Economics, #2023-028. The experiments are pre-registered as AEARCTR-0012305 (first round of election) and AEARCTR-0012504 (runoff).

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1 Introduction

Political campaigns are run to influence voters' beliefs, which in turn are expected to influence electoral choices. Voters receive information from politicians, the media, or friends and update their views on what is right and wrong for themselves and for the country. Voters also share this information and their newly updated beliefs with other voters. Therefore, political campaigns can have both "direct" and "indirect" effects: the effects on those directly exposed to the political information campaign and the spillover effects on voters who receive information from those directly exposed. Usually, it is very hard to differentiate between direct and indirect effects. In fact, in most countries, the most disaggregated level of observation of the electoral outcome is an electoral precinct that includes both those directly targeted by the campaign and their peers.[1]

In this paper, we manage to identify the direct and indirect effects of a political information campaign separately using subprecinct-level electoral data from the 2023 Argentinian presidential election. This election cycle—primaries, first round, and runoff—saw the rise and eventual victory of a political outsider, Javier Milei. The first important feature of the Argentinian electoral system that we use is that, unlike most elections worldwide, where results are reported at the precinct level, Argentinian elections are reported at the level of the sub-precinct (*mesa*, Spanish for "table") — resulting in multiple observations *within a precinct*. Second, the assignment of voters to these mesas is predetermined (essentially by the first letter of their last name), which allows a political campaign to directly treat some mesas but not others. Third, Argentinian political parties are required by law to publish lists of their individual members with their addresses, which uniquely identify the precinct and mesa where they are expected to vote. This allowed an NGO we worked with to ensure that voters on the list in certain mesas were contacted and voters in other mesas were not. We then compared the election results across (i) treated mesas, (ii) untreated mesas

[1] Some notable extant work, in particular Enríquez et al. (2024), documents the existence of spillover effects by comparing treated and neighboring untreated precincts. This, of course, underestimates the peer effects if much social interaction occurs between close neighbors within a precinct.

within precincts where other mesas were treated, and (iii) "pure control" mesas in precincts left untreated. These comparisons allowed us to measure both the direct and indirect effects of the political information campaign.

Our paper's contribution is four-fold. First, we present a theoretically grounded and intuitive way to measure direct and indirect effects by using the shares of treated voters in the mesa and in the precinct, respectively. Second, we document that spillover effects exist and are statistically significant. Third, we find that the direct and indirect effects need not be of the same sign. While our campaign had the predictable direct effect, it backfired in the aggregate because of the indirect effect, which had the opposite sign. We argue that this may be a common feature of information campaigns run against new politicians. Fourth, the results from our experiment in the first round of the election persist in the runoff; a separate experiment run during the runoff replicates these results. Taken together, our findings imply that the indirect effects of political campaigns may be significant in magnitude and not necessarily aligned in direction with the direct effects, possibly overshadowing them. This has immediate implications both for studying political campaigns and for conducting them in practice.

Our study focuses on Salta Province, one of Argentina's poorest provinces, where Javier Milei received his highest vote share (49%) in the primary (*PASO*) election in August 2023. With the help of an NGO, we designed two information campaigns aiming to provide truthful information about Milei's campaign proposals, one on abolition of the Central Bank and dollarizing the economy, and the other on replacing public funding for secondary schools with vouchers. The campaigns provided fact-checking by independent experts, who argued that the first policy would cause significant devaluation and inflation, while the second would significantly harm the educational system. The information campaign mailed ten thousand leaflets with fact-checking to individuals in randomly selected mesas and precincts in the first round and five thousand in the runoff. In the first round, the inflation treatment had no significant effect on voting. In contrast, the impact of the education treatment was highly significant, with every 100 mailed leaflets decreasing Milei's tally

by about 20 votes through the direct effect but *increasing* it by about 30 votes through the indirect effect. The total effect of the information on Milei's vote was therefore positive, with every 100 leaflets adding 10 votes in favor of Milei. The runoff experiment focused on the education treatment, leading to similar, though somewhat attenuated, results. There was no significant effect on turnout in either round.

The opposite signs of the direct and indirect effects may be explained by the heterogeneity of voters. On average, the information campaign had the intended direct effect, with some voters believing the information provided and deciding to vote against Milei. Other voters ignored the information, did not believe it, or took it into account but trusted Milei more. The opposite sign of the indirect effect suggests that the latter group—the one that rejected the information provided—turned out to be more vocal and effective in convincing their neighbors to take their side, possibly due to a strong backlash against experts criticizing their candidate.

Literature Our study contributes to several strands of literature. First and foremost, our paper focuses on estimating the spillover effects of a political campaign. The closest paper is Enríquez et al. (2024), which also documents spillover effects, but there are major differences. Set in Mexico, the experiment in Enríquez et al. (2024) varies the extent of treatment for voters living in a particular precinct to manipulate the indirect exposure of people living in neighboring precincts to the treatment. Their main findings are that this indirect effect exists, and that it is nonlinear: when increasing the share of treated voters in a treated precinct from 20% to 80%, the authors see a less than a fourfold change in the indirect effect; in other words, there is evidence of nonlinearity and satiation. These results highlight the importance of spillover effects, but the paper is likely to underestimate their magnitude considerably, as, presumably, most social interactions happen between close neighbors who likely vote in the same precinct. In addition, the paper does not consider the possibility of treated voters being subjected to the indirect effect from other treated voters. In contrast, our paper focuses on spillover effects between voters living in close proximity

(in the same precinct) and documents spillover effects large enough to dominate the direct effects. Most importantly, we see that the spillover effect does not necessarily take the same sign as the direct effect; to the best of our knowledge, ours is the first paper to establish this possibility. We do not study (and in fact, we assume away in our theoretical framework) nonlinearities, as our interventions are quite small for such effects to be meaningful.

In another related paper, Blattman et al. (2024) study the direct and spillover effects of an anti-vote-buying campaign in Uganda. The experiment randomly split nine hundred parishes into two-thirds, where at least one polling station was treated, and one-third control parishes. The authors then compared the effects on self-reported preferences and electoral outcomes in treated polling stations, untreated polling stations in treated parishes, and polling stations in control parishes. The treatments involve dropping leaflets and conducting village meetings. In survey data, the authors find significant differences in anti-incumbent attitudes between treated and untreated stations within treated parishes compared to control parishes; both effects have the same sign and similar magnitudes. However, in the electoral data, neither effect is significantly different from zero. The authors only find a significant effect of "saturation" (the share of treated stations within the whole parish). This is similar to what we call a "direct effect" in our paper, but this exercise does not allow for identifying a spillover effect. Thus, while Blattman et al. (2024) is similar in spirit to our study, we explore more granular data (mesas within a polling station) and manage to pin down spillover effects on electoral outcomes.

More generally, our paper is part of a growing literature on the effects of information campaigns on turnout and election outcomes. Kalla and Broockman (2018) provide an overview of several dozen experiments on campaign contact and advertising and suggest that a typical effect is very small, barring some cases with exceptionally unpopular candidates (see also Pons (2018)). Gerber et al. (2009) provide evidence of newspaper subscription effects on voting for Democratic candidates in gubernatorial elections in Virginia. Kendall et al. (2015) contrast different messages in a phone campaign and find that the candidate benefited from staying on a single message

rather than presenting several messages at once. Arias et al. (2022) study the effect of malfeasance revelations in Mexican elections. Galasso et al. (2023) examine the implications of negative campaigning, whereas Galasso et al. (2024) provide evidence that an anti-populist campaign in Italy, while effective against existing populists, paved the way for an even more radical party (Brothers of Italy). Ajzenman and Durante (2023) documents the existence of last-minute voting decisions, as evidenced by the quality of infrastructure (school buildings) in 2015 elections in Buenos Aires.

Our paper also contributes to the understanding of the recent rise and electoral success of anti-elite politicians in many countries, as well as the limited effect of efforts to counter their disinformation and misinformation with fact-checked expert information. This literature goes back to Dornbusch and Edwards (1990) on macroeconomic populism; see also Edwards (2019) for a recent discussion of the causes and effects of Latin American populist movements. Guriev and Papaioannou (2022) provide a recent overview of the literature and generalized view of populism and anti-elitist and anti-pluralist political movements; see also Rodrik (2021). Acemoglu et al. (2013) show, theoretically, how politicians may engage in extreme rhetoric or policy choices to signal that they are not captured by elite or corporate interests. Algan et al. (2017) and Guiso et al. (2024) provide empirical evidence on the role of rising unemployment and economic insecurity after the Great Recession in the rise of populist parties in Europe. Danieli et al. (2022) complements this finding by relating the rise of right-wing populism in Europe to shifting voter priorities. Allcott and Gentzkow (2017) and Bursztyn et al. (2020) study the 2016 presidential election in the U.S.; the former suggests only a limited effect of Facebook ads in the campaign, whereas the latter documents how Trump's campaign and eventual victory increased public (but not private) support for anti-immigrant policies.

Finally, our paper is also part of a large literature on persuasion in politics. DellaVigna and Gentzkow (2010) provides a general discussion and suggests ways to measure the persuasion effect. DellaVigna and Kaplan (2007) document the effect of Fox News on voting behavior in the U.S., while Enikolopov et al. (2011) document the effect of NTV, an independent media outlet in

Russia in the 2000s. Guriev et al. (2021) shows how the penetration of mobile broadband internet made voters more informed and increased government accountability. Several important papers suggest the existence of unintended or spillover effects, including DellaVigna et al. (2014) on the effect of Serbian radio on the Croatian population and Satyanath et al. (2017), which documents how Nazi ideology spread through preexisting social clubs in the Weimar Republic. Nyhan (2020) discusses misinformation and misperceptions in the modern era, and Barrera et al. (2020) shows that presenting factual information does not necessarily correct misperceptions. In this line, Nyhan (2021) suggests that at least, even if factual information fails to correct misperceptions, it does not appear to "backfire" and exacerbate them.

The rest of the paper is organized as follows. Section 2 describes Argentinian politics and the 2023 presidential election. Section 3 introduces the conceptual framework. In Section 4, we describe our experimental design and the data sources. Section 5 presents the main results, and Section 6 discusses their robustness and implications. Section 7 concludes.

2 Presidential election in Argentina in 2023

Argentina is a presidential republic where the president serves as both the head of state and the chief executive. In the 20th century, Argentina experienced both military dictators and juntas in power on the one hand and populist left-wing candidates, associated with Juan Perón and his political legacy, on the other. Over the last forty years, since the fall of the last military junta in 1983, presidents have come from either the Peronist or center-right parties.

The Peronist Party (also known as the Justicialist Party, *Partido Justicialista*) is one of the most enduring political movements in Argentina. Founded by Juan Perón and his second wife, Eva, it is broadly centered on social justice, a welfare state, and state intervention in the economy to achieve economic independence. Over the last two decades, a more left-wing variation of Peronism, known as Kirchnerism, has risen in prominence, emphasizing policies such as expanded social welfare

programs and increased state control over key industries. This faction, led by former presidents Néstor and Cristina Kirchner, has shaped Argentina's political landscape by pushing Peronism further to the left. Historically, opposition to Peronism has been represented by center-right parties under various names. Between Argentina's return to democracy in 1983 and 2023, there were two center-right presidents: Raúl Alfonsín of the Radical Civic Union from 1983 to 1989 and Mauricio Macri, who founded the Republican Proposal party, from 2015 to 2019.

The current electoral system in Argentina has been in place since 2009. At that time, the PASO election (*Primarias Abiertas, Simultáneas y Obligatorias* or "Simultaneous and Mandatory Open Primaries") was introduced for the 2009 national elections during the first term of Cristina Kirchner. These primaries take place around two months before the general election. Their purpose is to define the list of parties eligible to run in the general election and the list of politicians representing each party. Each citizen votes for a single candidate within their preferred party. The parties that receive more than 1.5% of votes in PASO then nominate their candidates for the first round of elections, and these candidates must be the ones who received the most votes within the party in the primary. In the first round of elections, the candidate who receives the most votes wins the presidency outright if they receive either 45% of the vote or 40% with a 10-point lead over the runner-up. Otherwise, a runoff is held between the top two candidates, and the one with the most votes wins. Voting is mandatory with few exceptions, such as those traveling abroad or who are sick, but the actual turnout has been close to 82% in the 1990s and around 75% in the last decade; the fine for nonparticipation is 100 pesos (roughly 20 U.S. cents at the time of the 2023 elections) and is rarely enforced in practice.

The political rise and victory of Javier Milei, who positioned himself as an anti-establishment figure with radical libertarian views, was unexpected. During his campaign, Milei's policy proposals included, among other things, the dollarization of the Argentine economy, the dismantling of the Central Bank, a reduction in government spending by 15% of GDP, the restructuring of the government with the removal of entire ministries and massive layoffs of public sector workers, and

a reform of the education system. His rise in popularity was seen by many as a worrying signal: financial markets responded negatively to his first-place finish in the August 2023 PASO primaries, with the Argentine peso depreciating overnight by 30%.

Milei's main competitors in the 2023 elections came from both the Peronist and center-right parties. On the left, Sergio Massa was the incumbent Minister of Economy under then-President Alberto Fernández; on the center-right, Patricia Bullrich was the former Minister of Security from 2015 to 2019 in Mauricio Macri's administration. These candidates emerged as champions of their respective parties in the PASO election. In this primary election, Milei received the most votes (almost 30%); Massa came second with 21% (with his party's total vote share at 27%), and Bullrich came third with 17% (whereas her party as a whole received 28%). These candidates, along with Juan Schiaretti and Myriam Bregman (both of whom received less than 4% of the votes), advanced to the first round of the election.

Between the PASO (which we will refer to as the primaries or round zero) and the first round, which was held on October 22, 2023, both Milei and Massa focused on attacking Bullrich, presumably because she was regarded as the likely winner in a head-on contest with either of them. As a result, Massa improved his standing substantially, coming in first with 37%; he was followed by Milei, who received 30%, while Bullrich received 24% and thus failed to advance to the runoff.

For the runoff, the center-right party decided to support Milei. Politicians from this party, including Bullrich herself and former President Macri, campaigned for Milei in exchange for policy moderation and promises to include center-right politicians in his government. As a result, on November 19, 2023, Milei defeated Massa in the runoff, obtaining 56% of the vote, while Massa received only 44%.

An important feature of Argentina's electoral system is that election results (including those for presidential elections) are reported at the level of sub-precincts (so called *mesas*, or tables) and that voters are exogenously assigned to mesas. Essentially, within a precinct, voters are grouped into mesas based on their last names. The maximum number of voters per mesa is 350; if this number

is exceeded, new mesas are added. Certain voters, such as those who require special assistance (e.g., people with disabilities), may be directed to mesas specifically equipped to accommodate their needs.

3 Conceptual framework

The purpose of our study is to distinguish between the direct and indirect effects of a political information campaign. We define direct effects as the impact of campaign leaflets on the voting behavior of voters who receive the leaflets compared to those who do not receive them. The indirect (spillover) effects are the impact on voters who do not receive the campaign leaflets but live close (i.e., in the same precinct) to those who do. In both cases, we compare the affected voters to individuals who have no direct or indirect exposure to the campaign.

Our experiment allows us to distinguish between direct and indirect effects thanks to Argentina reporting its election results by individual mesas (sub-precincts). In the treated precincts, we sent leaflets to some mesas (treated mesas) but not others (untreated mesas). We then compare the voting outcomes in the treated mesas to those in the untreated mesas within the treated precincts. These represent direct effects; by definition, the direct effect is proportional to the number of treated individuals per mesa. In contrast, the magnitude of the spillover effect depends on the share of treated individuals in the entire precinct but not on the mesas where they cast their votes. One could argue that this effect need not be linear: it could be concave if the same untreated individual is contacted by several treated individuals and the extra contacts are redundant, or it could be convex if several friends sharing the same message create a cumulative effect. However, our interventions are small enough that these nonlinearities are unlikely to be pronounced, so we expect the indirect effects to be proportional to the share of treated individuals in the precinct.

We formalize this intuition with a simple probabilistic voting model. Consider a precinct p populated by a continuum of citizens with unit mass, indexed by i. These citizens belong to G

groups indexed by $g \in \{1,\ldots,G\}$; the group of citizen i is denoted by $g(i)$. The share of citizens in each group g is n_g. When voting in elections, each citizen votes in one of M mesas, denoted $m(i)$; we assume that the share of citizens in mesa m is l^m. Crucially, each mesa has the same distribution of groups; in other words, the split of citizens into groups is orthogonal to their split into mesas.

Two candidates, A and B, run for office. The political preferences of people in each group are similar; their expected utilities from electing the candidates are U_g^A and U_g^B, respectively. We consider a probabilistic voting model where citizen i votes for candidate A if and only if

$$U_g(i)^A > U_g(i)^B + \varepsilon_i.$$

The last variable, ε_i, reflects taste shocks orthogonal to political preferences. As is standard in the literature, we assume that ε_i of citizen i is distributed uniformly on $[-K_g, K_g]$. This value is independent of the group and mesa the citizen is in, as well as ε_j of other citizens. The share of votes for candidate A among voters in group g in 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 a similar expression weighted by group size (note that it does not depend on the mesa explicitly, as all mesas are identical):

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

Adding direct effects of the political information campaign Let us now suppose that we treat share τ^m of voters in mesa m with some information about candidate A, and that of these, share α_g are from group g (if all groups were equally likely to be treated we would have $\alpha_g = n_g$). Denote the fraction of group g members that we treat by $\tau_g^m = \frac{\alpha_g}{n_g}\tau^m$. Suppose that the expected utility from electing candidate A is increased by r_g (which can be either positive or negative, and we assume it

to be the same within a group but possibly different across groups). If so, the share of voters from group g in mesa m voting for 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 in mesa m is

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

Now, the last term may be rewritten as

$$\sum_g \frac{\alpha_g}{2K_g} r_g \tau^m.$$

In other words, the direct effect of a political campaign is proportional to the share of voters treated in a mesa τ^m, with the coefficient on τ^m depending on which groups are relatively more likely to be reached by the treatment (α_g), how much information impacts their benefit from voting for a candidate (r_g), and how malleable this group was in the first place (K_g).

Adding spillover effects To model spillover effects, let us introduce probabilities η_{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 increases the utility of the voter in group g from voting for candidate A on average by s_{hg}, which may be positive or negative.[2] 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

[2]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.

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 τ^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

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

and the total vote share 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

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

Here, only the second term depends on mesa (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, τ^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 get information from, and how likely voters in these other groups were to be treated. Note that the total effect depends both on the different groups' reaction to political information r_g as well as network structure η_{hg} and voters' capacity to persuade each other s_{hg}.

Equation (1) can be taken directly to the data. Indeed, if we regress Milei's vote share in a given mesa on τ^m and τ^p, the respective coefficients provide estimates of the magnitudes of direct and indirect effects of our treatment.

Discussion The probabilistic voting model suggests that to capture the direct and indirect effects of a political campaign, we need to use, respectively, the share of individuals treated in a given

mesa and the share of individuals treated in the whole precinct. This provides an intuitive way to link the intensity of the campaign to its effects, and it allows us to identify these effects separately whenever these shares, even though correlated, are not collinear. In our case, varying the share of treated mesas achieves this goal.

In the model above, we allowed voters to be heterogeneous and to belong to different groups. These groups may capture a range of characteristics, including demographics and life status, political preferences, openness to receiving political information from our leaflets and willingness to open the envelopes in the first place, and willingness to discuss politics with members of other groups. The probabilistic voting model allows us to incorporate these groups without affecting tractability. For our approach to be valid, the important assumption 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 a precinct that are related to voting decisions. The fact that our treatment reaches a nonrandom sample of voters (for example, those we reach have a history of being associated with the PJ party and are more interested in opening the envelope with political information) does not change the validity of our approach. However, as always, one should be cautious about generalizing our results to different settings. See also Section 6 for a discussion of external validity.

Our approach assumes additivity of the effects. Indeed, individuals treated directly are still subject to indirect effects. We find this plausible: an individual receives a myriad of signals, and having received our leaflet does not render them immune to persuasion by friends, for example, and it is plausible that these small pieces of information add up without an explicit interaction term. One could, of course, defend a different approach, and argue that the leaflet pushes the voter to decide one way or another so that they are unaffected by indirect effects. Introducing these intricacies would complicate the model significantly and would also make the estimates sensitive to the assumed functional form. Nevertheless, we are confident that our approach is valid. First, the share of voters we treated is small, between 5% and 23%, so the nonlinear effects are likely

negligible because they are second-order. Second, we can also estimate indirect effects by only looking at untreated mesas in treated or untreated precincts. This approach reduces power but only considers untreated individuals; the results are similar.

We do not explicitly model how individuals who are indirectly affected by our treatment could further influence other individuals (tertiary effects). This does not mean we rule them out; instead, one should think of our estimates of the indirect effect as the sum of all spillover effects from our treated individuals to the population as a whole. This is easy to see mathematically if effects are additive or if nonlinearities are small. In what follows, when talking about indirect effects, we mean all the consequences of treated individuals' communication with others, including further communication down the road.

4 Experimental design and data

4.1 Experimental design

In August 2023, when Javier Milei unexpectedly won the PASO election (mandatory primaries) with about 30% of the vote, we decided to use this setting to experimentally study how factual information about the consequences of Milei's proposals would affect people's propensity to vote for him. The treatment came in the form of leaflets sent to voters in sealed envelopes that included a warning stating that by opening the envelope they agreed to receive political information (along with a WhatsApp number to contact in case they had questions; WhatsApp is the most common means of communication in Argentina). The factual information that we presented to voters came from reputable sources that were fully referenced in the leaflets sent to voters (i.e., no information was presented as our opinion). To improve the chance that at least some treatment would succeed in changing the voters' opinions, we decided to implement two different treatments explaining the consequences of two different policy proposals articulated by Milei. One set of leaflets warned that

Milei's policies would only make inflation worse (the "inflation" treatment), and the other warned that the abolition of free public education and its replacement with vouchers would make outcomes worse (the "education" treatment). See Appendix Figures A1-A4 for the leaflets (they were folded before being placed in the envelope, so the outside picture contains the front on the right and the back on the left).

For the experiment, we selected Salta Province, because Milei received the highest vote share in this province during the PASO election (see Figure A5). He received almost 50% vote share in PASO; he went on to perform well in Salta in the first round with 40% and won 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). Its capital city, also named Salta, is home to 43% of the province's residents and is the 7th most populous in the country. Along with neighboring Formosa and Jujuy, the province is one of the poorest in the country, with a regional GDP per capita about 20% below the national average.

To reach voters in Salta, we took advantage of the fact that in Argentina, political parties are required to publish lists of their members. As the largest political party in Argentina is the Justicialist Party (*Partido Justicialista*, henceforth PJ), we used its list. Out of about 1 million registered voters in Salta, 100,000 are members of the PJ, about 60% of whom reside outside the Salta's capital city. We worked with an NGO to reach these voters.

Specifically, the treatment was as follows. There are 23 departments (*departamentos*) in Salta Province, including the capital city. We excluded Salta's capital city from the study right away, since in a densely populated city our information campaign was more likely to encounter contamination across precinct boundaries. Another reason for this exclusion was that voters were more likely to be exposed to other political advertising, making our campaign less noticeable. In each department, there are about 10 precincts, and each precinct contains several *mesas*, with voters distributed within a precinct among mesas in alphabetical order. We excluded mesas where fewer than 5% of voters were PJ members according to the list because, even if these mesas were chosen

to be treated, we would not have been able to reach enough voters for the treatment to be effective. In the end, our sample contained 1,273 mesas in 209 precincts across 22 departments.

Within each of the 22 departments, we randomly split precincts into three categories. About 50% went into the *pure control* group, where we did not do any mailings during the first round. About 25% went into the *potential inflation treatment* group, and another 25% went into the *potential education treatment* group. With this design, we ensured that we never distributed different leaflets within the same precinct. Then, for each mesa from the two potential treatment groups, we selected a matched mesa from the control group to minimize the difference according to the following lexicographic norm:

$$\text{floor}(\text{Milei_share} * 100) * 10000 + \text{floor}(\text{Turnout} * 100) * 100 + \text{floor}(\text{PJ_share} * 100),$$

where Milei_share is the share of votes for Milei, Turnout is voter turnout as a share of registered voters, and PJ_share is the share of PJ members in the mesa. The floor function rounds each value down to the nearest integer. In what follows, we only consider mesas (and their matches) that were sufficiently close, so that the difference in Milei votes between the treatment and control mesas within the pair was no more than 2.5%. There was no requirement that different treatment mesas be matched to different control ones.

After that, the "treatment" precincts were randomly assigned between "high" treatment, where we aimed to treat two-thirds of mesas (or the closest rational number possible) and "low" treatment precincts, where we aimed to treat one-third of mesas. The exact mesas to be treated were randomly chosen among those eligible for treatment. The split between high and low treatment was chosen so that the number of leaflets to be sent was close to 5,000; the printing office took orders in batches of 5,000, so aiming for this number minimized the average cost of a leaflet. In all cases, when a mesa was chosen to be treated, leaflets were sent to all available addresses.

After the first round of elections, the results (see below) quickly revealed that our inflation

treatment had no effect (for reasons we discuss), whereas the education treatment produced interesting and statistically significant effects on the vote. We decided to replicate the education treatment in the runoff by utilizing the 50% of precincts that were pure controls in the first round (we did not have time to design any new treatment and barely had enough time to have leaflets printed and sent). These precincts were split into pure controls and potential education treatments, following the same approach as in the first round; the only significant difference was that we used the outcome of the first round as a basis. This resulted in an experiment comparable in scope to the education experiment in the first round, with 5,000 leaflets sent. In total, 15,000 leaflets were mailed: 10,000 in the first round (5,000 with the inflation treatment and 5,000 with the education treatment) and 5,000 in the runoff with the education treatment.

To the best of our knowledge, all envelopes were sent as planned. The same is not necessarily true about delivery: while according to typical transit times for Argentine mail all envelopes should have arrived at least 3–4 days before the election (and some were shipped earlier), there are concerns about whether voters in some remote locations received them on time, or even if attempts were made to deliver the envelopes. Notably, while some envelopes were returned as undeliverable, we never heard back from the most remote places, which made us question the reliability of the mail service there. In the seminal study of the quality of mail services around the world Chong et al. (2014), Argentina ranked 84th, with 6 out of 10 fictitious letters returned to sender (5 within the statutorily required 90 days). For these reasons, we excluded the three departments that are more than five hours' driving distance driving distance from the Salta's capital city: our understanding is that mail is delivered by car, and there is no way to guarantee that large and unexpected batches of mail were delivered on time to places where a round trip would take more than a whole workday. Figure A6 presents the map of Salta province with the distribution of economic development of its departments; Figures A7, A8, and A9 show precincts with education treatment in the first round, inflation treatment, and education treatment in the second round, respectively.

4.2 Data

We use mesa-level official election data from the Argentine government websites.[3] These data included the total number of registered voters, turnout, the number of valid votes, and the vote shares of all parties and candidates. We also used socioeconomic data from the 2022 census.[4] The driving time between the capital of the province (the city of Salta) and the capitals of the departments was obtained using Google Maps. Individual addresses of PJ members were published on the PJ website and were downloaded by our partner NGO, who also administered the mailing of the leaflets.

4.3 Balance

In all three experiments, our randomization has generally worked on both the extensive margin (the probability that a mesa is treated) and the intensive margin (the share of treated voters in a mesa, provided the mesa is treated).

Figure A10 presents the differences in pre-treatment characteristics between treated and non-treated mesas in the first-round education experiment. These differences are insignificant. Figure A11 presents the same estimates for the inflation treatment. There are marginally significant differences in the longitude and latitude of mesas' locations. The magnitudes of these differences are small. In our regressions below, we always include a specification that controls for longitude and latitude. Figure A12 shows the estimates for the runoff education experiment. Here again, most differences are insignificant, except for the marginally significant difference in longitude; the magnitude also small.

Figures A13-A15 present the balance of the intensive margins. Here, we regress each pre-

[3]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 data were accessed on March 12, 2024.

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

treatment characteristic on the share of treated voters in a mesa and in a precinct controlling for the shares of available addresses either linearly (Panel A in Figure) or using dummies for deciles of shares of available addresses in the mesa and the precinct (Panel B Figures A13 and A15). Again, for most pre-treatment variables the coefficients are not significantly different from zero. The only exceptions are the marginally significant coefficients on longitude, latitude, and Bullrich's vote share in PASO. In the regressions below, we always include a specification that controls for all three.

5 Results

5.1 First round experiment

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

$$V_{pm} = \alpha + \beta \times \tau_m + \gamma \times \tau_p + \delta \times X_m + \phi_{\text{pair}} + \varepsilon_{pm}, \tag{2}$$

Here, V_{pm} is Milei's vote share in precinct p and mesa m; τ_m and τ_p are the shares of treated individuals in the mesa and in the precinct, respectively; X_m is a set of mesa-level controls; ϕ_{pair} are matched set fixed effects (which, by construction, are the same for a control mesa and all treated mesa matched with this control mesa). Note that these fixed effects subsume department fixed effects, as all matched sets were chosen within departments. In all regressions, we control for shares of available addresses at the mesa and precinct levels, as these directly affect the share of voters treated, and we want to separate the effects of our treatment from these characteristics of mesas; we call these "minimal set of controls."

Table 1 presents the main results for the education treatment. Column (1) documents the results with the minimal set of controls. In column (2), we add Milei's vote share in the PASO election (there is minimal variation between mesas within a pair because the matched sets were chosen so

that their Milei vote in the PASO election are similar). In column (3), we further add the votes shares of Bullrich and Massa parties in the PASO round, as well as turnout (note that since mesas affect voting decisions only, the only variables that vary at the mesa level are related to election outcomes). In column (4) we add controls for latitude and longitude; we also control for the share of available addresses in a flexible way by using dummies for deciles of the share of available addresses rather than a linear term.

In all cases, the direct effect is negative and significant at the 10% level, whereas the indirect effect is positive and significant at 10% level (5% in the three out of four specifications). Their magnitudes are about -0.2 for the direct effect and +0.3 for the indirect one. To put it in perspective, sending 10 leaflets in a mesa resulted in 2 fewer votes for Milei in that mesa but 3 more votes for Milei scattered around the precinct, with a net benefit of about 1 vote for Milei for every 10 leaflets we sent.[5]

Our experimental setting allows us to estimate direct and indirect effects separately. To estimate the direct effect only, we control for precinct fixed effects; this allows us to compare treated and untreated mesas within each precinct, which differ only by the direct effect, whereas the indirect effect is the same for the entire precinct (and is therefore collinear with precinct fixed effects). If we do so, we are unable to also control for pair fixed effects as before. Table 2 reports the results. In column (1), we report the results for the specification with the minimal set of controls, in column (2), we add controls for the PASO vote shares of Milei, Bullrich's and Massa's parties, as well as turnout. In column (3), we expand the sample from the matched sets to the full sample. In all cases, the estimates of the direct effect in Table 2 are statistically significant at the 1% level and are very similar in magnitude to what we see in Table 1.

[5]Indeed, imagine a precinct with 1,000 voters divided into 10 mesas with 100 voters each. Suppose that we sent leaflets to 10 voters in one of the mesas. In that mesa, the share of treated voters went up by 10%, and then the direct effect decreased Milei's vote in that mesa by 2%, or 2 votes. Across the whole precinct, our intervention treated $10/1,000$, or 1% of voters, which means that in every mesa, the indirect effect results in 0.3%, or 0.3 more votes for Milei, on average. Summing these effects over the 10 mesas, we get 3 more votes for Milei due to the indirect effect. The total (direct plus indirect) effect is 1 additional vote in favor of Milei.

To estimate indirect effects separately, we run the same specification as in Table 1, but restrict the sample to mesas that were not treated directly. In these mesas, the direct effect is zero by definition; we therefore estimate only the indirect effect. Columns (4)-(6) in Table 2 show the results with different sets of controls. In all cases, the estimated indirect effect is positive and statistically significant; its magnitude, approximately 0.4, is somewhat higher than in Table 1, but statistically indistinguishable from it. These separate estimates of direct and indirect effects are important because they confirm that the different signs of the direct and indirect effects in our main results are real, rather than an artifact of, for example, multicollinearity.

Figure 1 presents binscatter plots for the results in Table 2 (columns (1) and (4)), showing that the results are not driven by outliers.

Figure 2 presents the distribution of Milei's vote shares across mesas with different treatment intensities. In the upper panel, we show that the direct effects shift the distribution of Milei vote shares to the left (relative to the untreated mesas), while the indirect effects shift the distribution to the right. In the lower panel, we compare the indirectly treated mesas (in treated precincts) with higher and lower intensities of the indirect effects. The distribution of Milei's vote shares in mesas where the intensity of the indirect effect is above the median is shifted to the right relative to mesas where the intensity of the indirect effect is below the median.

5.2 Persistence

The runoff election between Javier Milei and Sergio Massa allows us to use its results as another set of observations. Since some of our control precincts were treated in the runoff experiment, when reporting the results, we remove those precincts (as well as any mesas that were paired with mesas from these precincts) from the sample; the results would largely be unchanged if we did not do that.

Table 3 reports both direct and indirect effects, similarly to Table 1. One can see that the direct

effect in the runoff has a very similar magnitude (and the same sign), whereas the indirect effects are, if anything, larger (and again have the same sign). Separate estimations of direct and indirect effects, which we present in Table 4 confirm these observations (the specifications are similar to those in Table 2 from the first round).

These results suggest that the persuasive effect of our education treatment persisted well into the runoff, which took place four weeks later. Interestingly, while the direct effect stayed practically the same, the indirect effect grew larger in magnitude. This is consistent with voters who were treated directly having had extra time between the first round and the runoff to exert spillover effects on their peers (essentially persuading them to vote for Milei).

We can also pool the observations from the first round and the runoff; this results in similar and more precisely estimated results.

5.3 Runoff experiment

The results for the runoff experiment are reported in Table 5. The results for the direct effect are smaller in magnitude and less statistically significant compared to the first round experiment, while the results for the indirect effect are similar to those in the first round. These results are confirmed when we estimate direct effects and indirect effects separately, in Table 6. This is are not surprising, as by the time of the runoff, Milei was no longer a novice and unknown politician, and our leaflets carried less new information for voters. Importantly, the finding that the direct and indirect effects have opposite signs still holds; in other words, the results of our education treatment successfully replicate. Interestingly, the indirect effects are quite similar in magnitude to those in the first-round experiment, so while the leaflets were less effective in the runoff, the persuasive power was retained in peer-to-peer interactions.

Figure 3 presents binscatter plots for the separate estimations of direct and indirect effects (Table 6, columns (1) and (5)) showing again that the results are not driven by outliers.

5.4 Additional results

While the education treatment has significant effects on vote shares in both the first and second-round experiments, the inflation treatment does not seem to have any effects. As shown in Table A1, neither the direct effect nor the indirect effect is statistically significant in any specification, and this does not change when we look at the direct or indirect effects separately (Table A2). We believe that this may be driven by the fact that Milei's dollarization policy proposal was widely discussed during the campaign and covered by the media; therefore, the leaflets did not provide their recipients with new information. We consider the inflation experiment to be a null result and do not investigate or discuss it further.

We have also studied the impact of our experiments on turnout in the first and second round. Table A3 presents the main specification for the education treatment in the first round. Table A4 shows separate estimations for direct and indirect effects. Table A5 and A6 provide similar estimates for the runoff experiment. In neither case do we find any significant effects of our treatments on turnout; both direct and indirect effects are null.

6 Discussion

To the best of our knowledge, this is the first paper to demonstrate that the direct and spillover effects of a political information campaign may have opposing signs. In our experiment, while the direct effect had the intended sign, the indirect effect counteracted it and was of greater magnitude, resulting in an overall opposite impact. We use a novel approach that takes advantage of the unique Argentine system of running and reporting elections. The Argentine electoral system allows us to obtain multiple observations within a precinct and to pin down direct and indirect effects.

There are several reasons to believe that our results are robust. First, we address the natural concern that regressing vote outcomes on the shares of treated individuals at the mesa and precinct

levels could result in multicollinearity, which could artificially produce large (and statistically significant) coefficients of opposite signs. Indeed, we obtain very similar estimates when we either capture the direct effect only by introducing precinct fixed effects or capture the indirect effect only by examining untreated mesas. In all settings, Milei got a smaller share of votes in treated mesas compared to untreated mesas in the same precincts, whereas among untreated mesas, those in precincts with many treated voters saw Milei receive a higher share of votes. This provides immediate evidence that the direct effect is negative and the indirect one is positive in our experiment.

Second, we find the same pattern in three settings: the main first-round experiment, the first-round experiment with runoff results on the left-hand side (persistence), and the runoff experiment. In other words, the patterns that we observed in the first experiment persisted in the runoff, and they were also replicated in the second experiment. In all three analyses, the direct and indirect effects consistently move in opposite directions.

Third, we carry out a placebo exercise. We take the code that was used to generate treated mesas and pair them with control ones, and then generate placebo sets of "mesas to treat" using different randomization seeds. We then estimate the direct and indirect effects of these fictitious treatments using real data. For both direct and indirect effects, the distributions are clearly centered at zero, and the actual estimated coefficients are at the tails of these distributions (see Figure A16).[6]

Fourth, it is possible that our experiment does not perfectly separate the direct and indirect effects, because our leaflets could be read by household members who vote in a different mesa (in Spanish-speaking countries, it is typical for spouses to have different last names). One way to think about it is that it does not pose a problem if we interpret letting other people read one's mail as part of the indirect effect, as it affects people who are not the intended recipients of the information. Even if we interpret it as a direct effect, this means that our measure of the indirect effect is a combination of the direct effect on household members with a different last name and the genuine spillover effect arising from political conversation and persuasion. Given that the two

[6]Figure A17 presents the placebo results for the inflation experiment.

effects have different signs in our estimate, this would imply that the spillover effect is positive and even greater in magnitude. In other words, this possibility would, if anything, work in our favor.

Fifth, we address the concerns of internal and external validity. Our treated individuals had PJ party affiliation in the past, while untreated voters might or might not have, which makes it possible that their reactions to the same information differ (for example, if they have different priors, their posteriors could be more polarized, as e.g. in Acemoglu et al. (2016)) even if treated voters do not act strategically in persuading others (for instance, if the leaflets we sent were shared among everyone in the neighborhood). However, polarization alone is not consistent with our results. Indeed, if PJ members just became more convinced to oppose Milei and support Massa, and non-PJ members had the opposite reaction, they would have voted the same way. The only way to affect the election outcome would be through an increase in their likelihood of voting, but we do not observe any effect on turnout. Furthermore, we always control for PJ membership at the mesa level, so we are comparing mesas that are similar in this regard. It is also not true that all PJ party affiliates supported Massa (the left-wing candidate) over Milei: Milei's support came from all sides of the political spectrum.

Thus, there are reasons to believe that our findings are real in this setting. This means that people who were not persuaded by our leaflets were more likely to persuade others to vote for Milei than those whom we were able to persuade were likely to go and persuade others. In our view, the most likely explanation for this asymmetry is backlash: people who were not persuaded by the information we provided but who happened to be strong Milei supporters were so outraged by the campaign against him that they went out of their way to persuade neighbors and friends to support him. It is probably not surprising that they could be highly effective at this (and more effective than our direct campaign), as they knew their neighbors and friends well, knew who was probably persuadable, and knew which arguments were likely to be effective in each individual case. Unfortunately, for both logistical and privacy reasons, it is impossible for us to see the exact motivation behind their efforts to persuade peers to support Milei, nor is it possible to see how it

was done (what arguments were used, whether the leaflet was shown, or whether its content was discussed, etc.). This is an important question for future research on spillover effects.

It may well be the case that our results do not necessarily hold in all other settings: for some information campaigns even the direct effect may be null; other campaigns may have the intended direct effect with a null spillover effect, or the indirect effect might reinforce the direct one. However, there are reasons to believe that in some important settings we could observe similar patterns. In our case, Milei was a politician who had been largely unknown to a majority of voters; his policy proposals were novel as well, and their merit was difficult to judge. In such a setting, voters could be open to new, expert information about Milei and his proposals, which could contribute to the magnitude of the effects both in this and in similar settings. Like many politicians in other countries running anti-establishment campaigns, he was likely despised by some voters and lionized by others, which could contribute to the outrage and the backfiring effect. All of this suggests that it would be natural to expect a similar backfiring effect in comparable settings—for example, in the case of Donald Trump in the 2016 U.S. elections Of course, the scope of the intervention to achieve a similar magnitude of effects would have to be very different. We were likely helped by the fact that in Salta Province, especially in its suburban and rural parts, voters are not typically inundated with political leaflets. Drawing anyone's attention to such a campaign in a U.S. swing state such as Pennsylvania would be much more difficult, but a properly scaled campaign could still produce similar results.

The above arguments imply that our findings may have profound implications for political campaigns, especially those involving presenting information or fighting misinformation through fact-checks and expert opinions. Our results show that even information that voters find persuasive—and that fares well, for example, in focus groups—does not guarantee the intended effect on elections due to spillover effects from recipients who are not persuaded. This may explain why correcting misperceptions or dispelling misinformation with credible evidence or expert opinion may work well in a controlled setting but has been notoriously difficult in real elections with candidates

running on anti-establishment platforms. Thus, taking into account the possibility of backfiring through spillover effects is extremely important in such settings.

7 Conclusion

In this paper, we use a unique system of reporting election results in Argentina to study both the direct and indirect effects of a political information campaign. Our main finding is that indirect effects of such a campaign are not necessarily pale shadows of the direct effects; in contrast, they may be both large in magnitude and opposite in sign. For academic work on elections, this result implies the necessity of either explicitly incorporating indirect effects into the analysis or providing a convincing explanation for ruling them out. Our results caution against dismissing spillover effects simply because they are assumed to be small.

Our findings also have practical implications for designing and running election campaigns. We demonstrate that political information campaigns may backfire, and the backfiring effect need not be borne by the direct recipients of information. This means, for example, that focus groups can be misleading if one cannot anticipate how this information will be passed on to other voters.

By highlighting the prominence of spillover effects in elections, our work suggests important directions for future research. One question is understanding the conditions under which backfiring is possible or likely to happen. The backfiring effect is probably a feature of information campaigns against anti-elite politicians, but it may be more general than that. Second, finding a setting that allows for a deeper investigation——potentially through interviews——could provide valuable insight into why the indirect effect operates in the opposite directions. Lastly, understanding how significant spillover effects of information campaigns influence polarization, echo chambers, and policies designed to mitigate polarization and combat misinformation remains an important avenue for research. These questions offer ample opportunities for theoretical, empirical, and experimental work.

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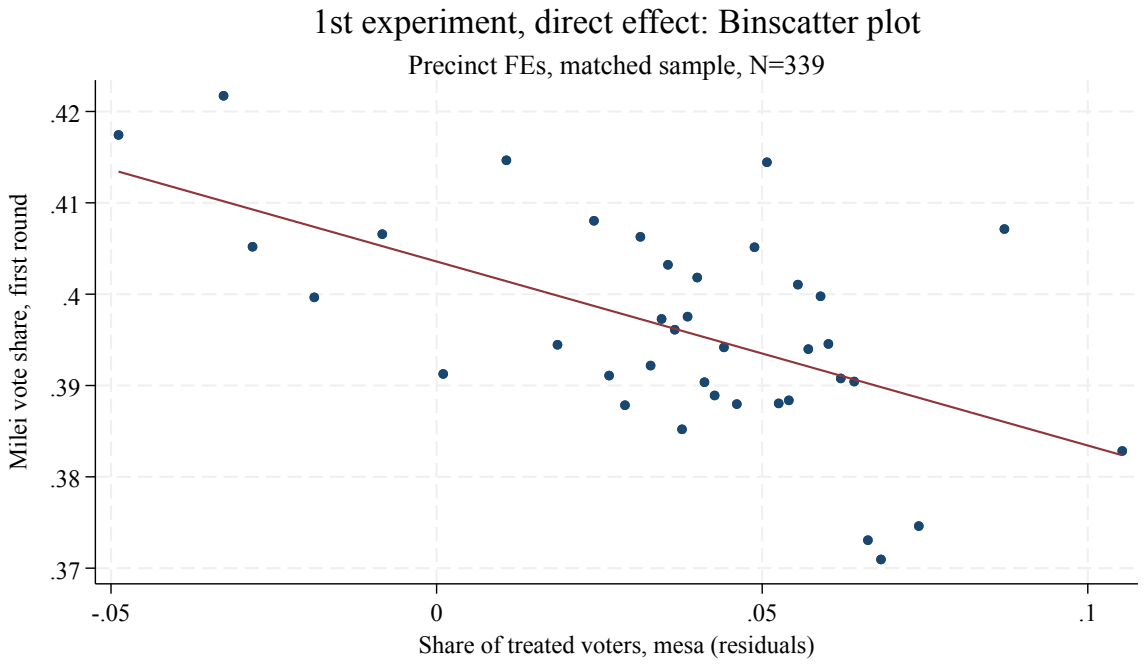
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Figure 1: Illustration of the main effects in the first experiment



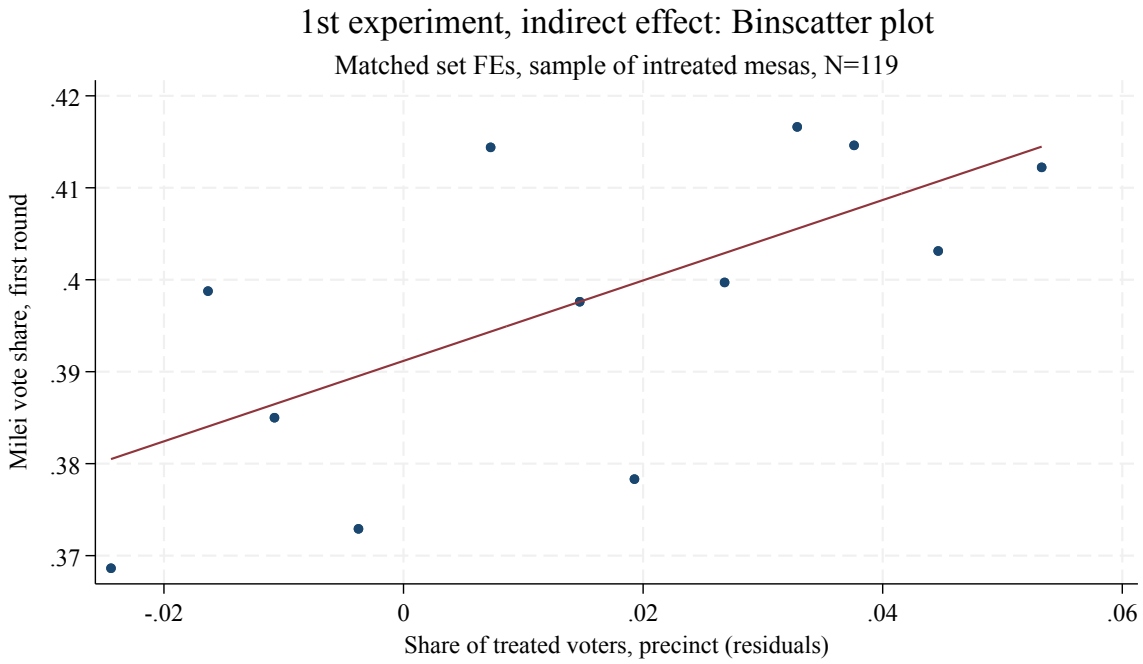


Figure 2: Distribution of Milei vote shares across mesas with different treatment intensities

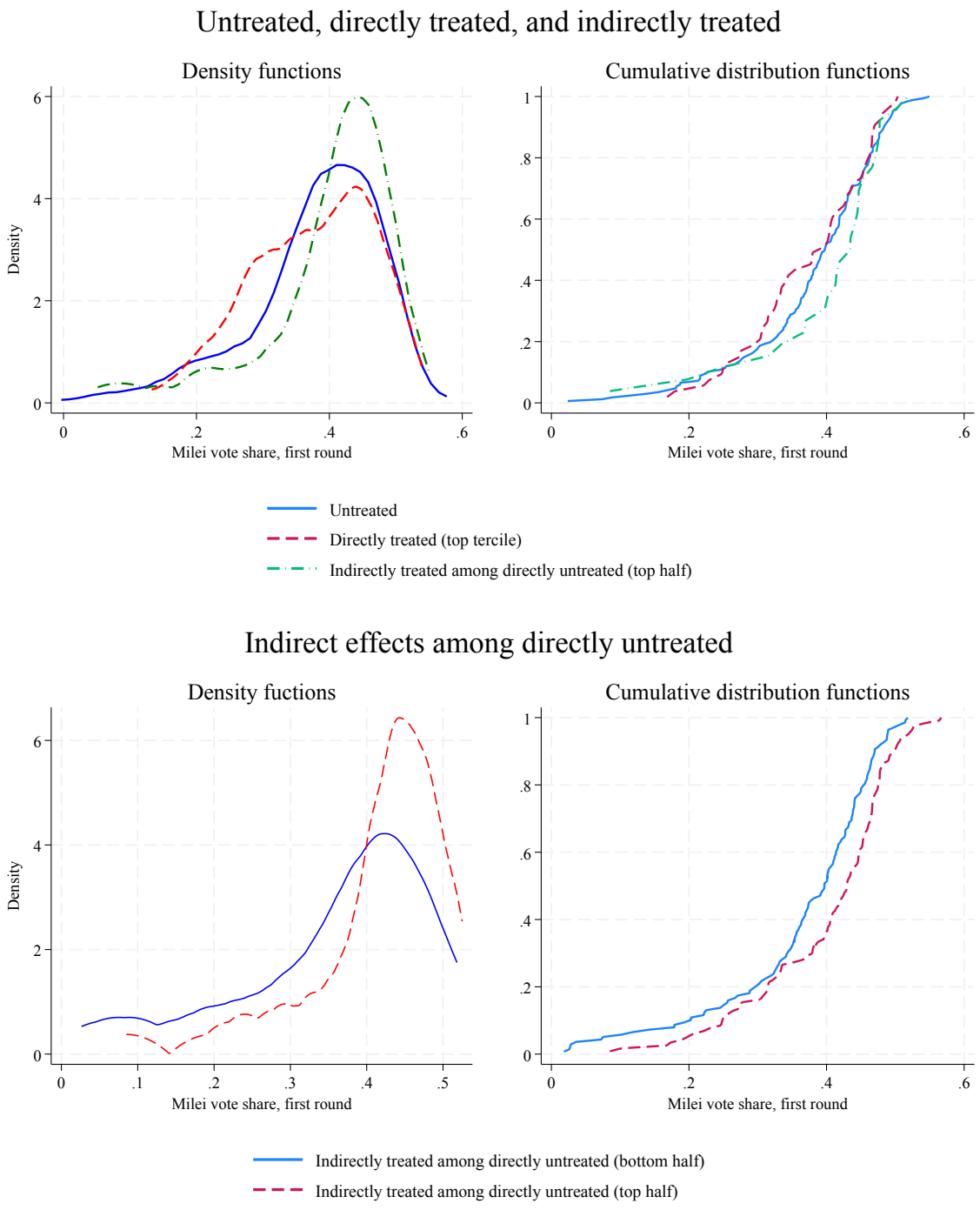
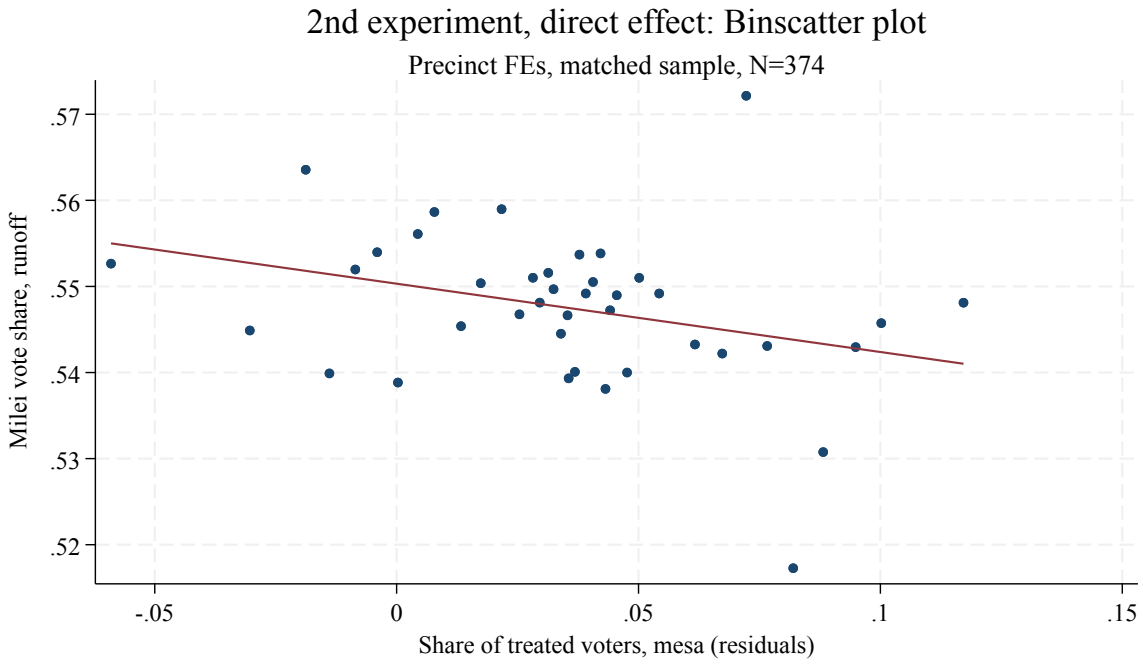


Figure 3: Illustration of the main effects in the second experiment



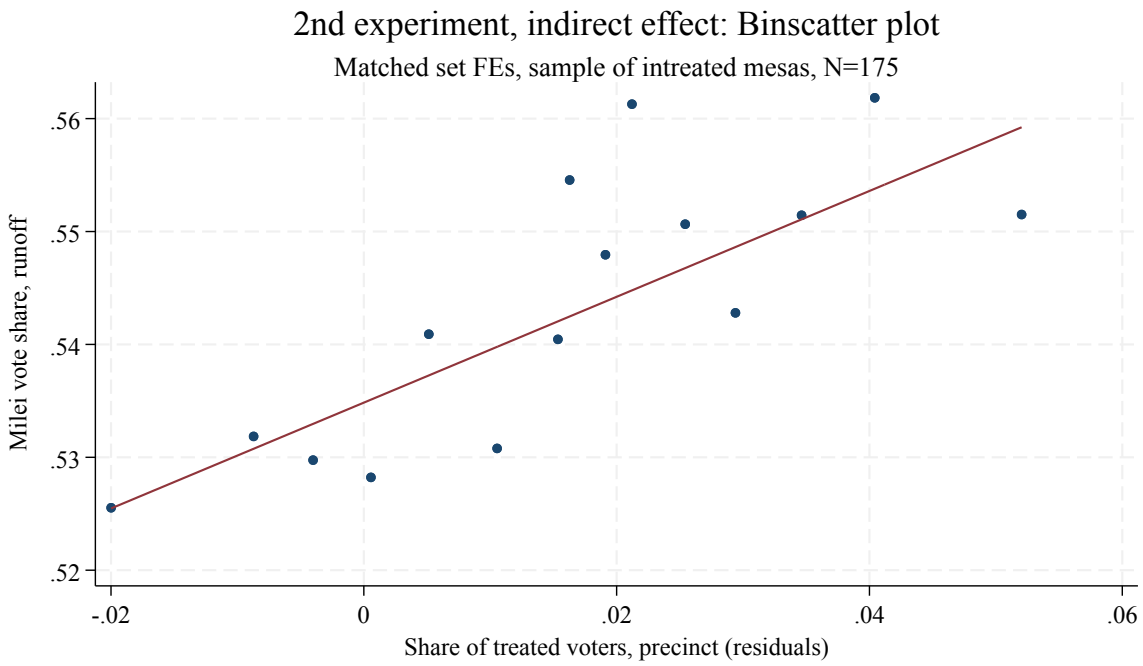


Table 1: First experiment, education treatment, main results

	(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
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 inflation 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
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 inflation 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							
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 inflation 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.

Appendix Figures

Figure A1: Leaflet: Education treatment, outside

Original:



Translation:



Figure A2: Leaflet: Education treatment, inside

Original:



Translation:

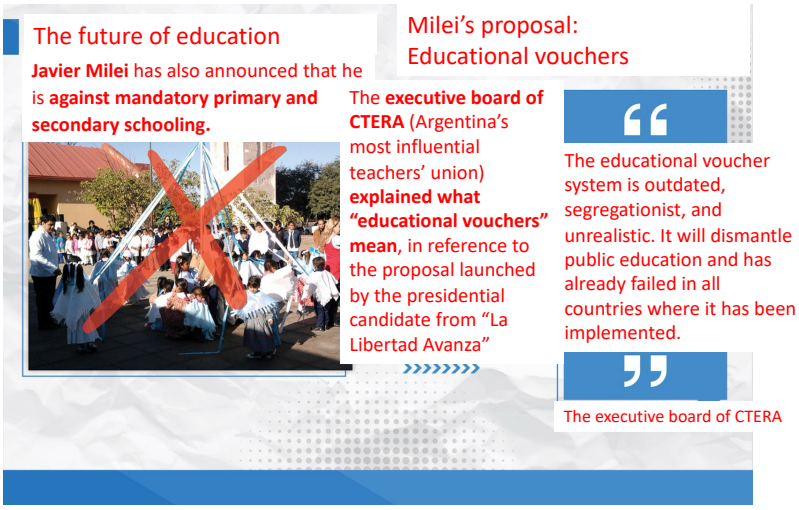


Figure A3: Leaflet: Inflation treatment, outside

Original:



Translation:



Figure A4: Leaflet: Inflation treatment, inside

Original:



Translation:

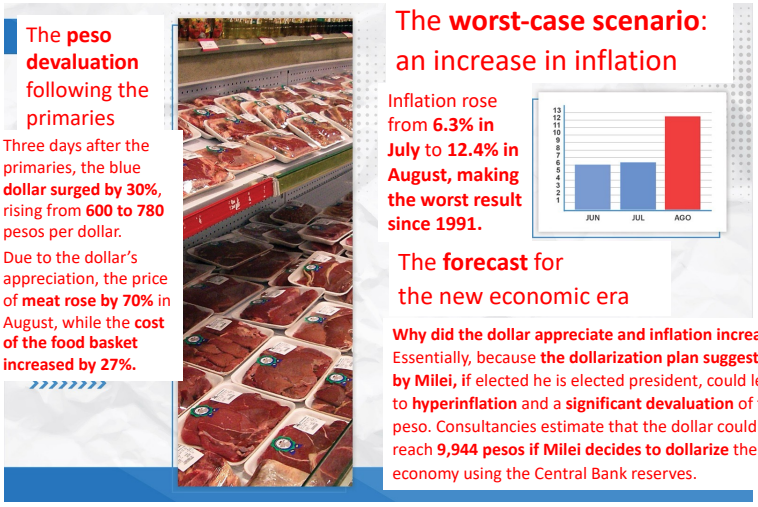


Figure A5: Map of Argentina, results of primary elections (PASO) vote by province

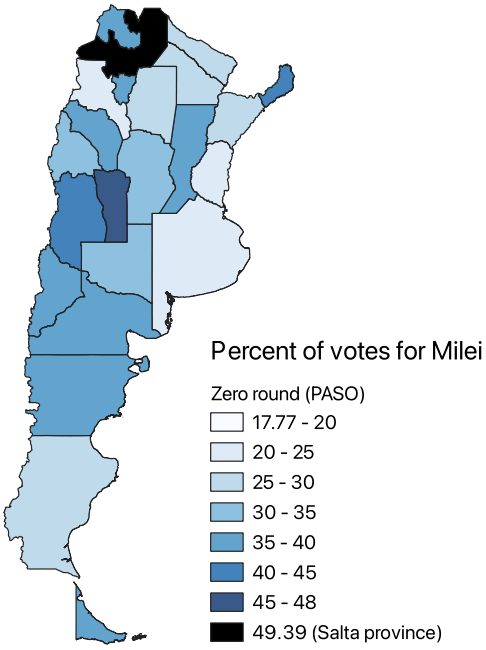


Figure A6: Map of Salta province, economic development of departments

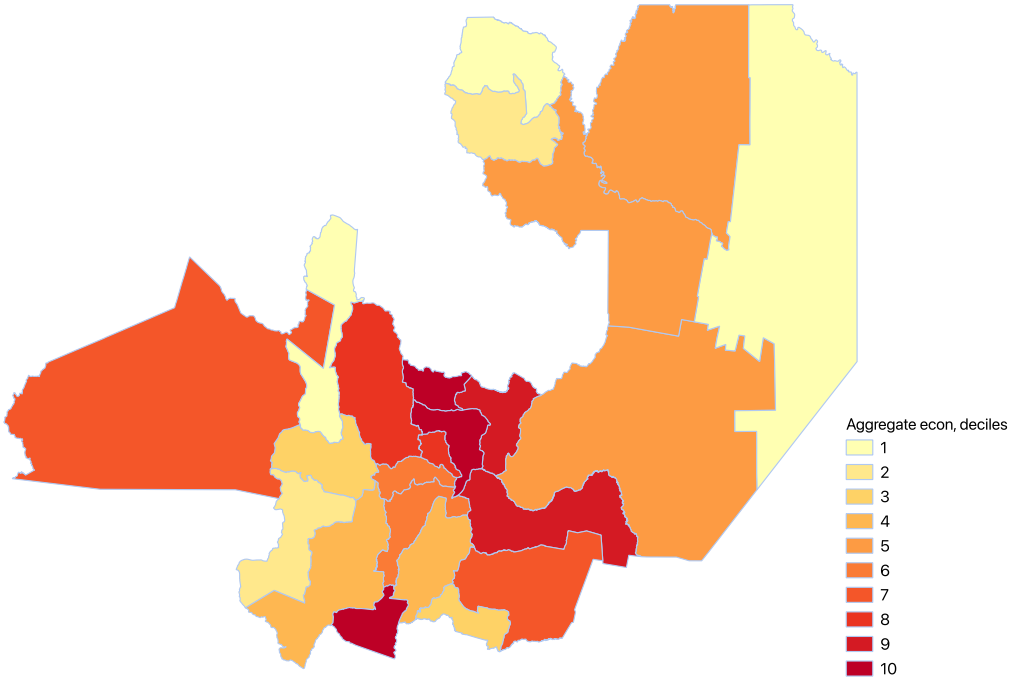


Figure A7: Map of precincts with education treatment, 1st experiment

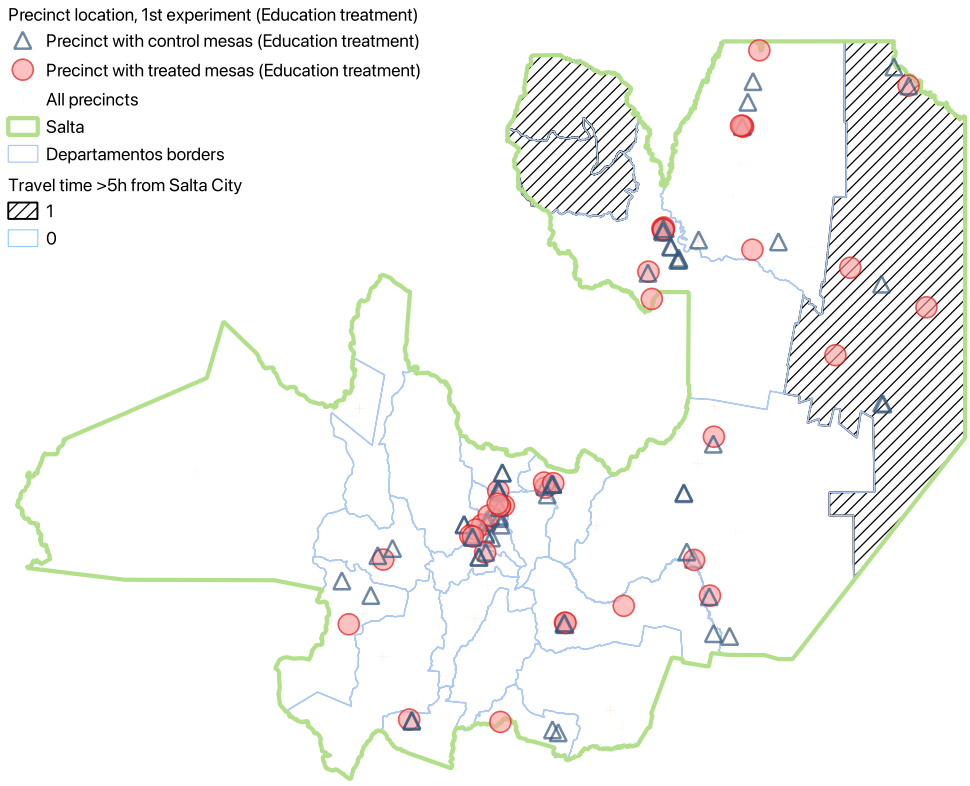


Figure A8: Map of precincts with inflation treatment, 1st experiment

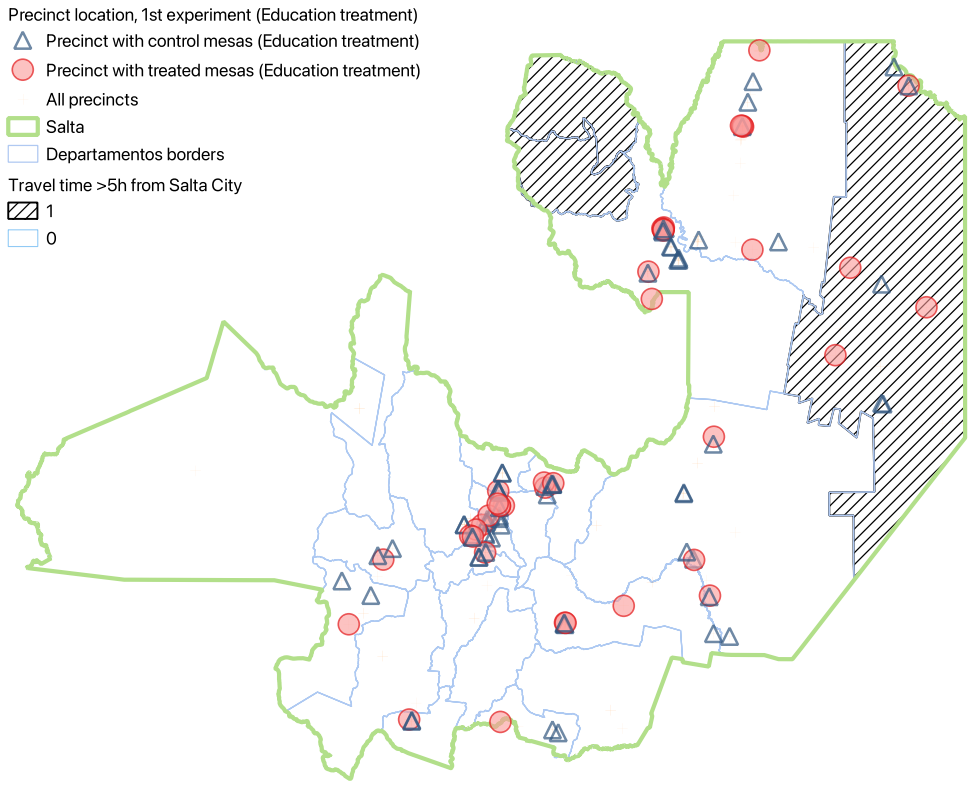


Figure A9: Map of precincts with education treatment, 2nd experiment

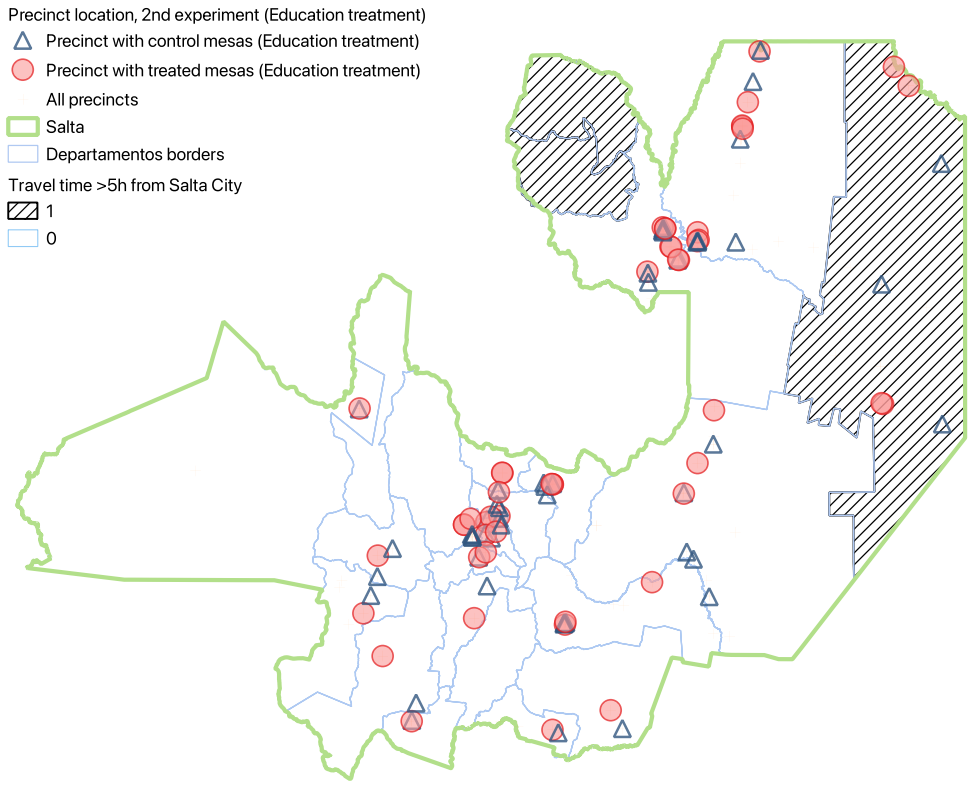
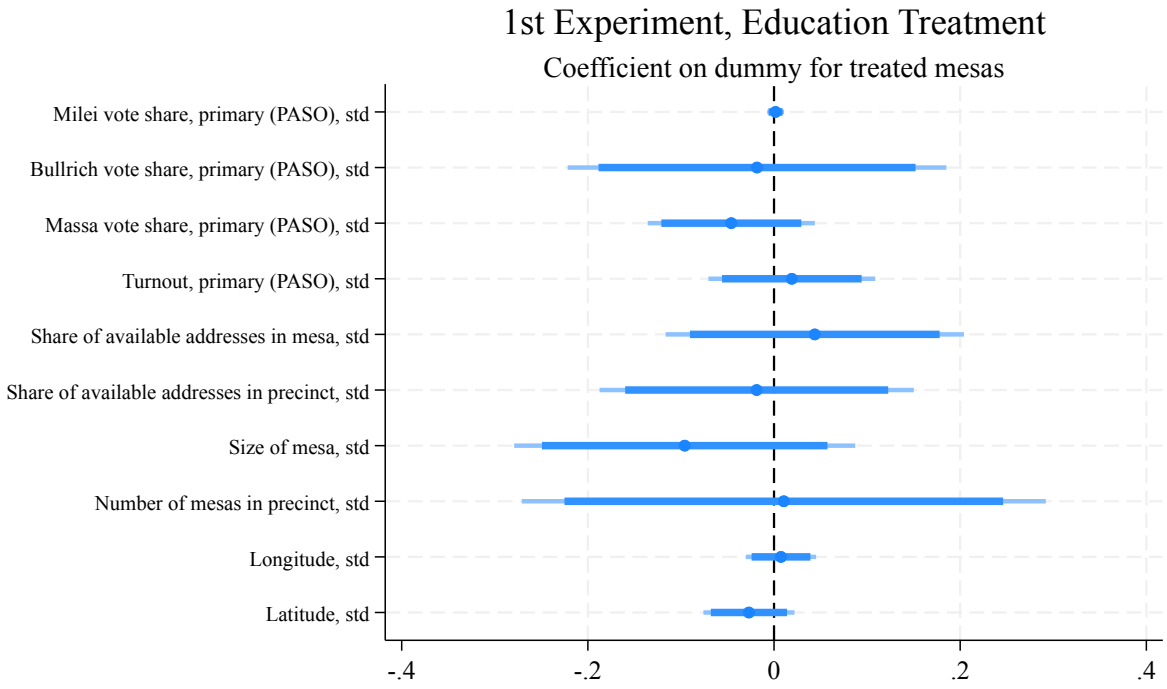
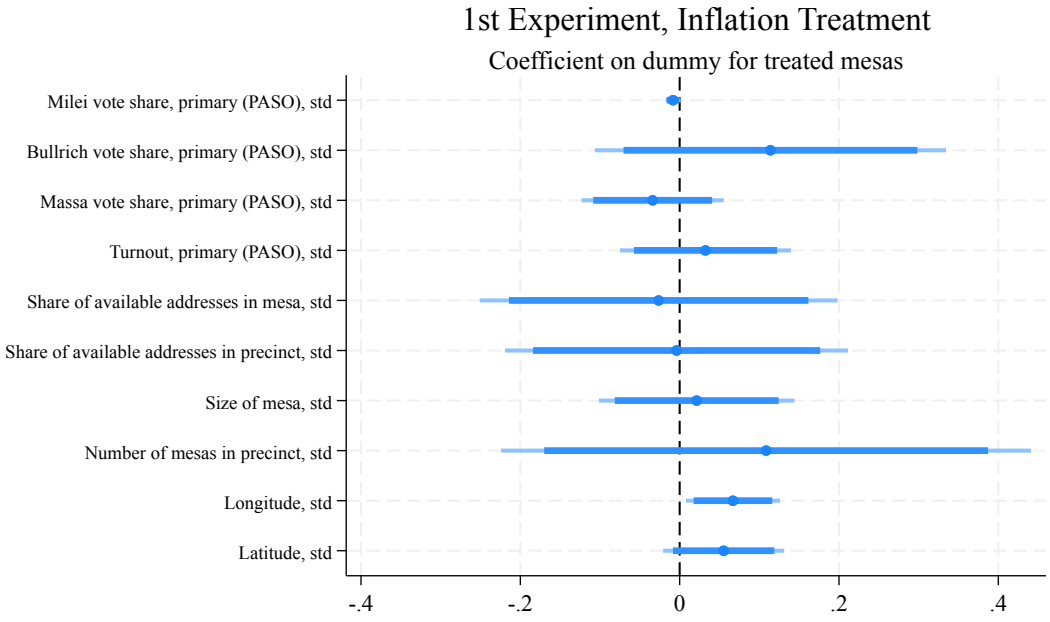


Figure A10: Balance, extensive margin: 1st Experiment, Education Treatment
Average difference between treated and non-treated mesas in pre-treatment characteristics



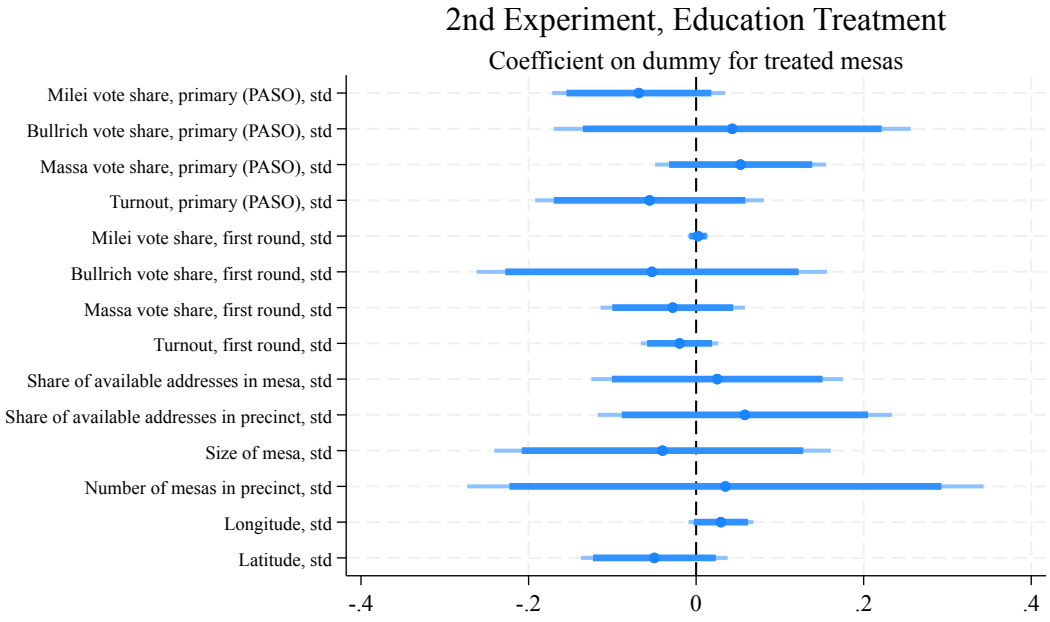
Note: The figure presents the coefficients from a bivariate regression, in which standardized pre-treatment characteristics are regressed on the dummy for treated mesa.

Figure A11: Balance, extensive margin: 1st Experiment, Inflation 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.

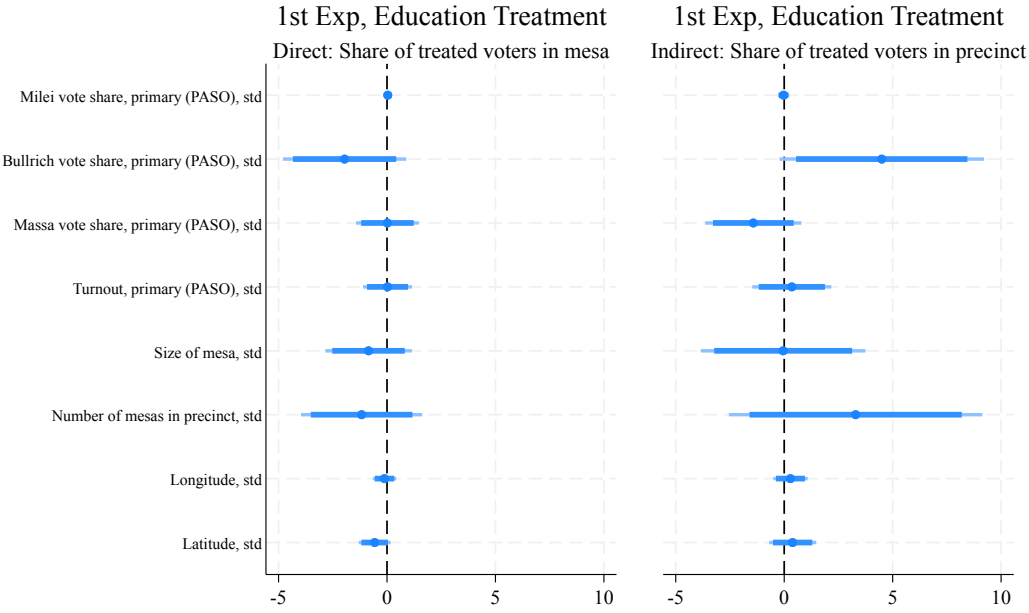
Figure A12: Balance, extensive margin: 2nd Experiment, Education Treatment
Average difference between treated and non-treated mesas in pre-treatment characteristics



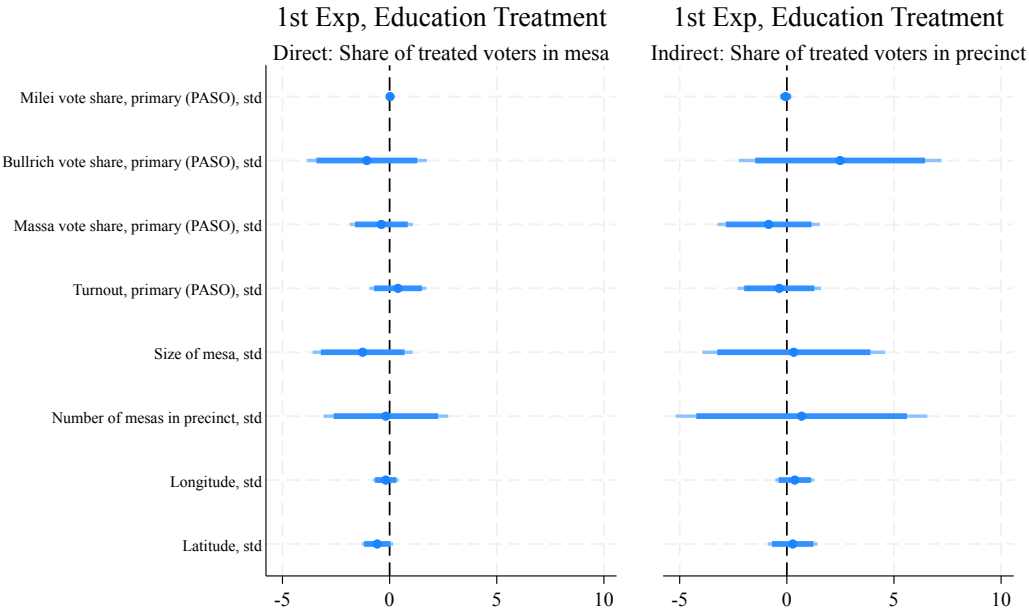
Note: The figure presents the coefficients from a bivariate regression, in which standardized pre-treatment characteristics are regressed on the dummy for treated mesa.

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

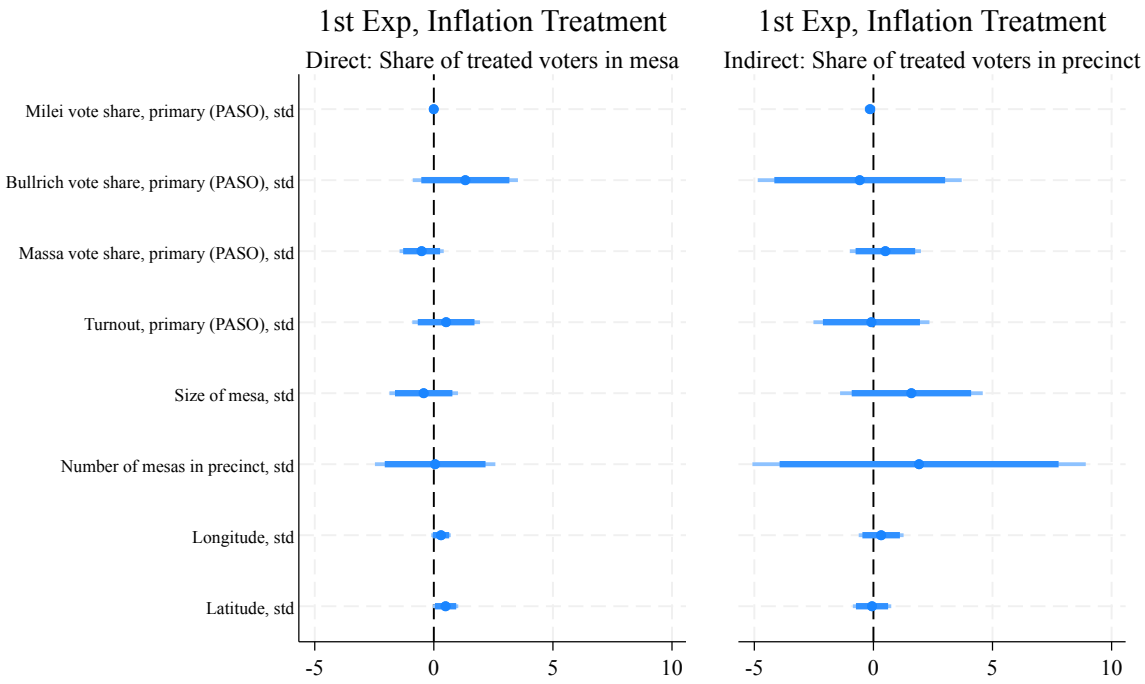


Panel B: Deciles FEs for available addresses



Note: The figure presents the main coefficients of the placebo estimation of equation 2, 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.

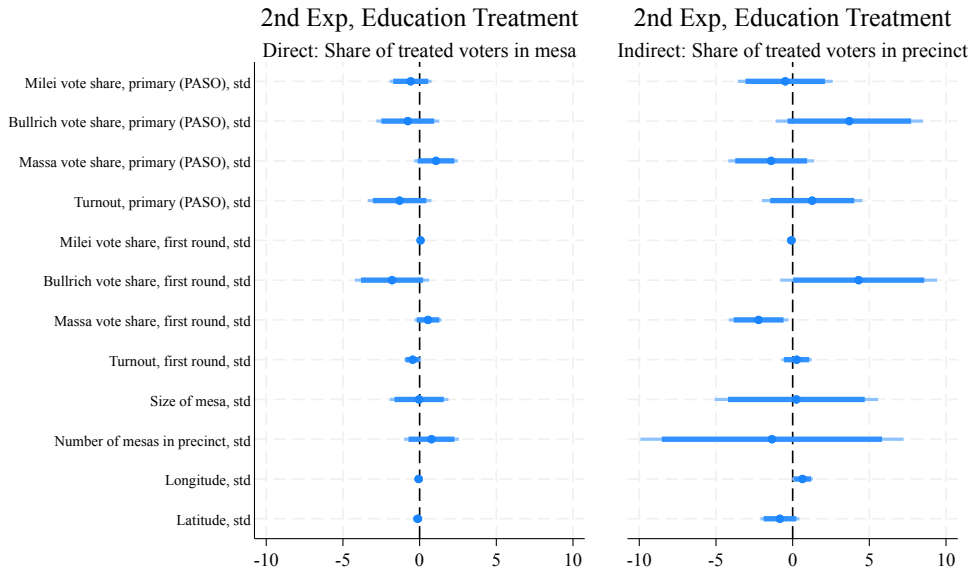
Figure A14: Balance, intensive margin: 1st Experiment, Inflation Treatment
Direct and indirect effects of treatment on pre-treatment characteristics



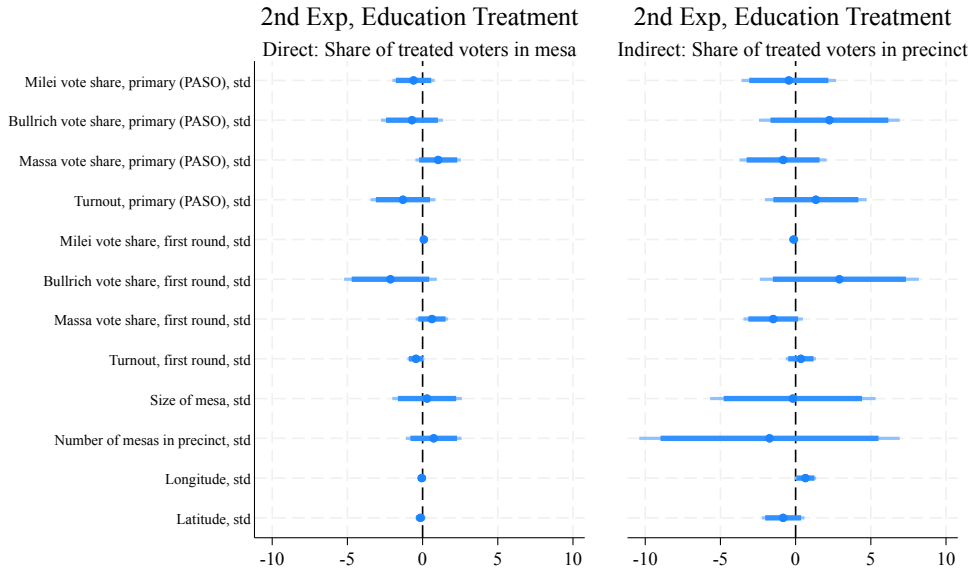
Note: The figure presents the main coefficients of the placebo estimation of equation 2, in which standardized pre-treatment characteristics are considered as outcomes and the main regressors are the share of treated voters in mesa and in precinct controlling only for the shares of available addresses 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 2, 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 education treatment, 1000 alternative randomizations

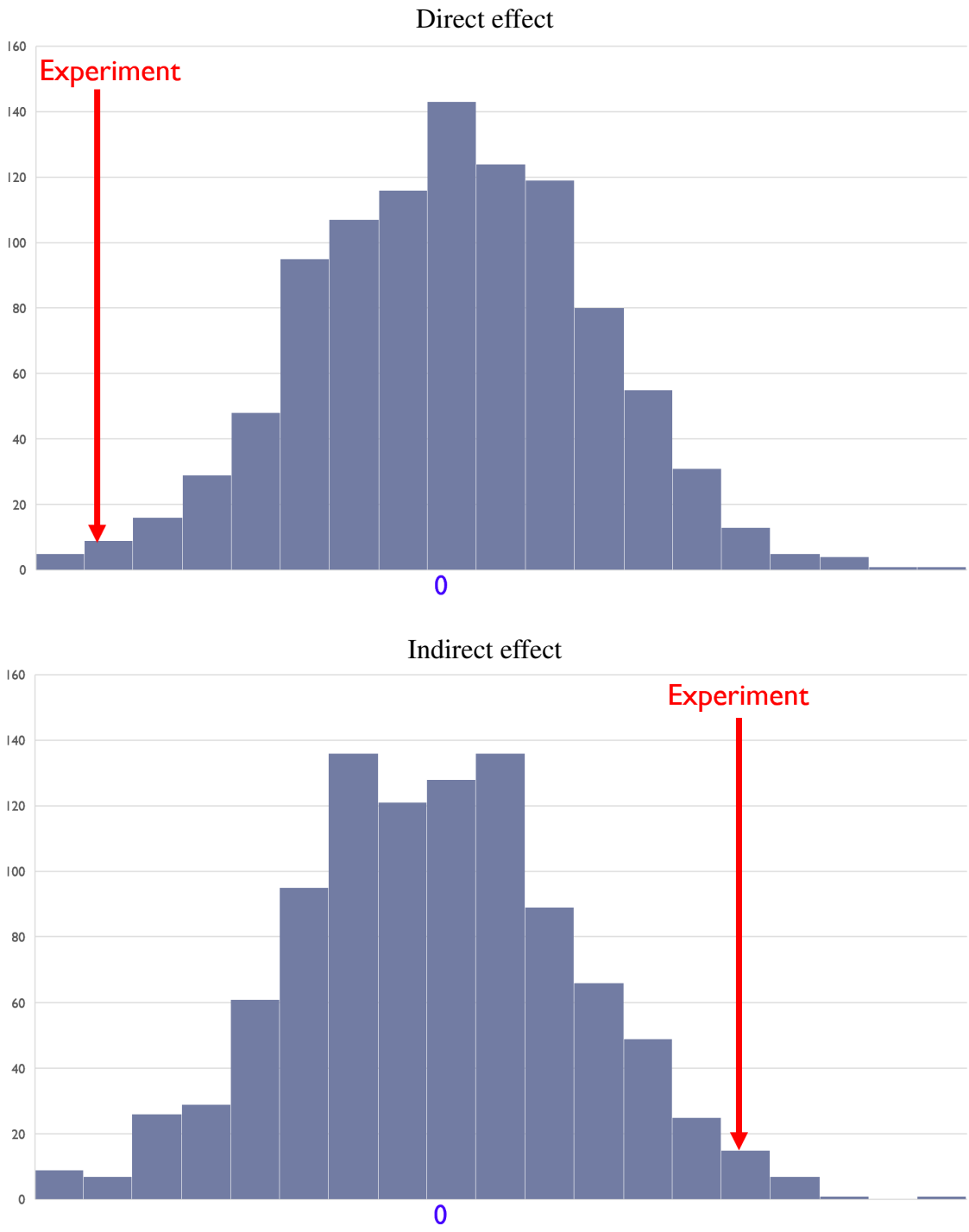
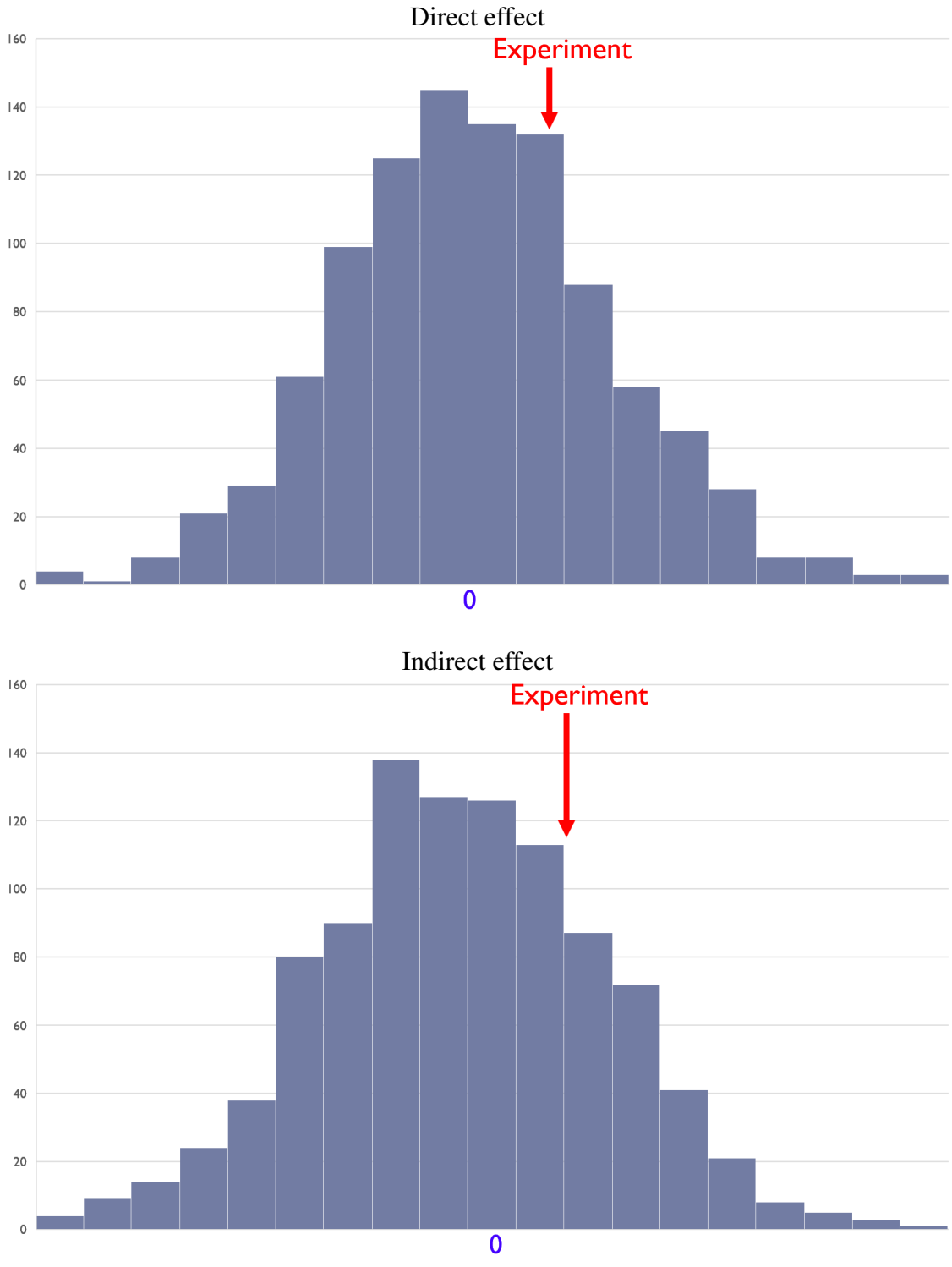


Figure A17: Placebo, first round inflation treatment, 1000 alternative randomizations



Appendix Tables

Table A1: First experiment, inflation 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, **inflation treatment**)	(0.061)	(0.061)	(0.065)	(0.068)
Share of treated voters, precinct	0.080	0.087	0.111	0.145
(Indirect effect, **inflation 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 A2: First experiment, inflation 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, **inflation treatment**			Indirect effect, **inflation treatment**		
Sample:	Matched sets		Full	Matched sets		Full
Share of treated voters, mesa	–0.017	0.015	0.047			
(Direct effect, **inflation treatment**)	(0.051)	(0.046)	(0.049)			
Share of treated voters, precinct				0.102	0.056	0.001
(Indirect effect, **inflation 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 inflation treatment.

Table A3: 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 A4: 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 inflation treatment. In columns, 4, 5, and 6, to estimate the indirect effect, we exclude mesas that received education treatment.

Table A5: Second experiment, education treatment, turnout as outcome

	(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	–0.040	–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 A6: Second experiment, education treatment, estimating direct and indirect effects separately, turnout as outcome

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	Milei Vote Share, Runoff							
Sample:	Matched sets, full							
Weights:	No weights		Entropy balancing		No weights		Entropy balancing	
	Direct effect				Indirect effect			
Share of treated voters, mesa (Direct effect)	–0.065	–0.062	–0.059	–0.056				
	(0.047)	(0.046)	(0.049)	(0.049)				
Share of treated voters, precinct (Indirect effect)					–0.114	–0.075	–0.095	–0.063
					(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 inflation 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.