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Urbanization Patterns, Information Diffusion, and Female Voting in Rural Paraguay

Alberto Chong Georgia State University and Universidad del Pacífico
Gianmarco León-Ciliotta Universitat Pompeu Fabra, Barcelona GSE, IPEG, and CEPR
Vivian Roza Inter-American Development Bank (IADB)
Martín Valdivia Group of Analysis for Development (GRADE)
Gabriela Vega Inter-American Development Bank (IADB)

Abstract: *We use a field experiment to evaluate the impact of two informational get-out-the-vote campaigns to boost female electoral participation in rural areas of Paraguay. We find that public rallies had a small and insignificant effect either on registration or voter turnout in the 2013 presidential elections. Households that received door-to-door canvassing treatment were 4.6 percentage points more likely to vote. Experimental variation on the intensity of the treatment at the locality level allows us to estimate spillover effects, which are present in localities that are geographically more concentrated, which may favor social interactions and diffusion of information. Reinforcement effects on the already treated population are twice as large as diffusion effects on the untreated. Our results underscore the importance of taking into account urbanization patterns when designing informational campaigns.*

Replication Materials: The data, code, and any additional materials required to replicate all analyses in this article are available on the *American Journal of Political Science* Dataverse within the Harvard Dataverse Network, at: https://doi.org/10.7910/DVN/KPJUFL.

While the role of social interactions as a vehicle to boost the impact of information campaigns is not a new one, the evidence on whether information spreads through social networks and is able to generate behavioral changes is rather mixed (Fafchamps, Vicente, and Vaz 2018; Giné and Mansuri 2018; Sinclair, Green, and McConnell 2012) Understanding how social interactions help spread information and generate behavioral change is important, as it provides insights on the relevance of social networks in the design of public policies.

In this article, we present findings of a field experiment on the effects of two get-out-the-vote (GOTV) campaigns targeted to women in rural Paraguay. Using individual-level administrative voting data, survey information, and satellite images, we explicitly explore the role of urbanization patterns in mediating the effects of these campaigns on registration and turnout. Prior to the 2013 presidential elections, we randomly assigned rural localities and provided information related to registration and the importance of voting using either public rallies or personalized door-to-door campaigns (D2D). The experiment was designed to estimate spillover effects by randomly varying the intensity of the D2D treatment and tracking a random sample of untreated women in treatment localities. We find that neither intervention leads to increases in voter registration, but whereas public rallies showed small and insignificant effects on voting, face-to-face interactions significantly increased turnout among treated women. Furthermore, we find evidence of spillover effects that lead to higher turnout in localities with urbanization patterns that appear to favor social interactions and information diffusion. These spillover effects are more important for treated women

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(reinforcement effect) than for untreated women (diffusion effects.) Our findings are robust to the inclusion of locality and individual-level controls, as well as to the differential attrition rates that especially affect the contacted group, as we provide bounds to our estimates that are consistent with our main qualitative results.

Public rallies are a relatively inexpensive way to reach large audiences, and while somewhat impersonal, they are an appealing option for politicians and are widely used in political campaigns (either with or without the presence of politicians themselves), more generally in information campaigns. Interestingly, despite their popularity, very few studies have assessed their impact. On the other hand, door-to-door campaigns, while more capital and labor intensive, may be more effective in dense, urban areas in industrial countries, likely explained by the fact that they involve closer human contact and face-to-face interactions and the possibility that they generate information spillovers. The trade-off between a mobilization campaign that involves a more impersonal approach, which allows higher reach at a relatively lower cost, and one that is a more personal and interactive one, but has less coverage and is more expensive, is at the core of our research and sheds light on the conditions affecting mobilization efforts' effectiveness. We evaluate the impact of these two types of interventions side by side, in the same context, as a means to evaluate which one provides higher returns on investment in terms of generating the desired behavioral change.

Our research focuses on poor, rural areas of Paraguay, where access to information is scarce and people heavily rely on social networks for information diffusion. We focus on women, as the most basic way to increase representation, namely, through electoral registration and voting, is often overlooked. In Paraguay, women hold only 18% of executive posts, and although in the 2013 presidential elections 52% of registered voters were men, the gender gap was as large as 10 percentage points in some areas of the country (ONU Mujeres 2015).

A crucial feature of Paraguay is that it shows a particularity in its rural urbanization patterns. On the one hand, some rural localities follow a concentric layout, with an agglomeration of houses and agricultural land in the outskirts of town (henceforth, *nonlinear locality*). On the other hand, a significant number of rural towns in the country show an alternative and distinct geographical layout that follows a straightforward linear configuration, with houses distributed along a single road, and with most of them having a plot of agricultural land on the back. We call this configuration *linear locality*. Unlike the former, these localities do not have any obvious public gathering spaces, which may be less conducive to social interactions. Some historical evidence suggests that the linear configuration of towns was loosely planned by Alfredo Stroessner, a decades-long dictator, to attend to the need to expand the agricultural frontier during the 1950s while minimizing the existence of gathering places, making it harder for communities to organize protests (Hetherington 2011).

We explicitly consider the role of geographical restrictions to social interactions in evaluating the information spillover effects of our intervention. Our experiment generates exogenous variation in the intensity with which each locality is treated, allowing us to test for the prevalence of spillovers and their magnitude, for localities with linear and nonlinear layouts. It is reasonable to expect that the frequency of interactions is limited by the geographical layout of households within a community: When the distance between households is shorter, and there is a town center, people may interact more frequently and naturally.

The way in which messages are delivered is also expected to have different impacts in terms of their spillovers: A home visit with a more personalized and direct contact can help build a credibility bond between the messenger and the receiver, translating into further involvement by the individual, as well as an additional sense of commitment, contributing to the internalization of the message, and thus being more likely to act accordingly and disseminate it. Likewise, neighbors or other members of the social network can reinforce a message, and this can enhance the effects of any information treatment. This applies to people not contacted through the intervention, as well as those who were contacted.

Our article provides four key findings. First, neither public rallies nor door-to-door treatments impact registration. Second, door-to-door campaigns that disseminate the information in a conversational manner increase turnout by 4.6 percentage points, whereas impersonal messages disseminated in public rallies yield small and statistically insignificant effects on turnout. Third, spillover effects from door-to-door campaigns are only relevant in localities where the geographical distribution of households facilitates social interactions; in places without a natural gathering location, these effects are diluted. In particular, the intensity with which a village is treated affects turnout only in localities with a geographical layout that facilitates social interactions (nonlinear), and not in linear localities. While intuitive, to our knowledge, this is the first time that this has been studied. Finally, we find that information diffusion not only affects the behavior of untreated women in treated localities: Having a higher intensity of the treatment reinforces the effects on treated women, and these effects are

twice as large as diffusion effects in promoting political participation. In the conclusion, we provide a back-of-the-envelope calculation of the relative cost-effectiveness of these interventions.

Related Literature

While there is a large and still growing literature that uses field experiments to understand voter behavior, a limited number of studies focus on ways to increase voter registration.[1] We know, for instance, that impersonal messages are less likely to affect the likelihood of registration. Bennion and Nickerson (2011) and Nickerson (2007), for instance, find no effects of e-mails on voter registration. However, face-to-face interactions seem to affect registration rates. Nickerson (2015) uses a door-to-door campaign and shows that voter registration increases by 4.4% in the United States. Bracconiere, Dormagen, and Pons (2018) randomize two different door-to-door registration campaigns in France and find an increase in registration of 2.4 and 4.7 percentage points, respectively.

Gerber and Green (2005) examine the impact and effectiveness of door-to-door canvassing, telephone calls, direct mail, and other campaign tactics and show that more direct interactions are usually more effective at increasing turnout; they have larger implementation costs though. There is evidence of a positive and significant effect of canvassing on turnout (Green, McGrath, and Aronow 2013.) Pons (2018) carried out a large-scale experiment to mobilize voters in France and shows that personal interactions with the canvassers were effective at mobilizing voters.

On the other hand, additional evidence from outside the United States shows that the effect of GOTV campaigns on turnout displays great variation in terms of magnitude For example, consider Guan and Green (2006), John and Brannan (2008), and Giné and Mansuri 2018 for China, the United Kingdom, and Pakistan, respectively, and for Europe, Bhatti et al. (2016) present a survey of the literature, which shows small or even zero effects. It is therefore not clear that these types of campaigns are effective in mobilizing voters.

The fact that conversational messages are more likely to change people's minds and increase the likelihood of participating in elections suggests that closer interactions are key in the success of GOTV campaigns, and spillover effects can be relevant. Recent studies show that social pressure has larger effects on turnout than the direct effects on contacted individuals (Della Vigna et al. 2017; Gerber, Green, and Larimer 2008).

Despite the popularity of public rallies, there are few studies on its effects. Addonizio, Green, and Glaser (2007) estimate the effect of festivals held at polling sites and find that they significantly increase turnout. Green and McClelland (2017) find that these festivals may increase turnout in a cost-effective manner. Surprisingly, in developing countries, where these rallies are more common, to our knowledge, no studies have evaluated their effects on political participation.[2] This is particularly interesting because these strategies involve human interactions (as in other GOTV campaigns), but they are not face-to-face or customized.

Different studies have found mixed evidence that spillover effects outside the household exist. Nickerson (2008) presents the first evidence of spillover effects within the household. Uncontacted people in households that were contacted by canvassers were more likely to vote than those in uncontacted households: 60% of the propensity to vote induced by the experimental treatment is passed on to other members of the household.

Sinclair, Green, and McConnell (2012), using a multilevel GOTV experiment designed to measure within-household spillovers and spillovers within 9-digit zip codes in Chicago, found evidence for the former, but regardless of the intensity of the treatment, there were no spillovers within zip code. Fafchamps, Vicente, and Vaz (2018) show that information about the candidates in the 2009 Mozambican election diffuses through kinship networks and chatting, but evidence on spillovers encouraging voting is mixed. In the closest study to ours, Giné and Mansuri (2018) assess the direct impact of a door-to-door voter awareness campaign on female participation, candidate choice, and vote shares in Pakistan. Treated women were 12 percentage points more likely to vote, and to choose a different candidate than their husbands.

The literature studying the determinants of the development of linear and nonlinear settlements, while barely studied in economics (Nijkamp and Reggiani 1993),

has been present in related fields for a long time. In fact, it has been studied in architecture (Shadar 2016), geography (Jiang and Miao 2014), and urbanism (De Landa 2000). For instance, Shadar (2016) argues that the existence of linear settlements defined as normally small to medium-sized towns or a group of buildings that are formed in a long line with no obvious center and narrow

[1] See Gerber and Green (2000, 2001, 2012), Michelson and Nickerson (2011), Pande (2011), León (2017), Bhatti et al. (2016), Gerber, Green, and Nickerson (2003), Gerber, Green, and Shachar (2003), Gerber, Green, and Larimer (2008), Arceneaux and Nickerson (2009), Gerber and Rogers (2009), and Chong et al. (2014).

[2] In Fujiwara and Wantchekon (2013), public rallies are the status quo, and the authors do not evaluate the effects of rallies themselves.

shape may have been driven by settlements built along a route, which predated the settlement. Typically, such towns are populated along a single street with houses on either side of the road. Many examples may be found in countries around the world, from Mileham in England, to Victoria in Hong Kong (Shelton, Karakiewicz, and Kvan 2011), and even Brooklyn in the United States, where city and state agencies formed a nonprofit corporation to draft detailed plans for a linear city of schools, residences, commercial sites, and transportation facilities in central Brooklyn that would encompass a 6-mile arc (Roberts 1967).[3,4] Researchers from different disciplines have long argued that the origins of linearities or nonlinearities in the formation of towns may not be necessarily linked to different observable characteristics.[5]

Institutional Background

Paraguay is a presidential representative democracy, where the president and National Congress are elected simultaneously every 5 years. In the elections of March 2013, over 2,000 candidates were running for president and vice president, for a seat in the two chambers of Congress and in the MERCOSUR Parliament (PARLASUR), and for departmental governors. The president (and vice president) and governors are elected under plurality rule, whereas the election of representatives follows closed-list proportional representation. The elections came up after the impeachment of Fernando Lugo, a former Catholic bishop who was elected in 2008 as the first president not belonging to the Colorado party. In this election, Horacio Cortés was elected president with 45% of the vote.

Historically, Paraguay has been a two-party country, but smaller political parties have gained popularity in the past 15 years. Political parties tend not to be ideological, and therefore political campaigns are typically built around the presidential candidate (Rizova 2007). Political campaigns are conducted using mass media, and the ones with a larger coverage area are radio and TV.[6] In rural settings, most candidates do not have direct contact with voters, but rather, they rely on local political brokers. These political operators act as intermediaries between the candidate and voters and are typically local leaders with a certain degree of credibility and legitimacy, who are either longtime party members or specifically hired as political campaign operators (see Finan and Schechter 2012, and the references within).

The role of political brokers in rural Paraguay is twofold: They work to secure party loyalty and to obtain the votes of swing voters. They do this by engaging in personal contact with voters, often offering personal favors, as well as heading public rallies where they expose the politician's electoral program and promises. As such, the treatments provided in our experiment are motivated by observations of regular campaign activities in the setting in which we work.

Registering to vote is a costly activity. While automatic registration has recently been implemented for people who turn 18 after December 30, 2015, at the moment of our fieldwork, anyone who wanted to be eligible to vote had to register in the Registro Cívico Permanente.[7] To register, people have to go in person to an office of the Tribunal Supremo de Justicia Electoral (TSJE) and show their national identification (ID) card with a verified address.[8] While in some cases this can be a relatively easy process, travel costs are not negligible, especially for people living in rural areas.[9] On the other hand, voting itself is much more accessible to the registered population. Schools or other public buildings are used as polling stations, so many more localities have their own.[10] This implies voters in more isolated localities will still have to

[3]See http://www.visionofbritain.org.uk/place/3159, (accessed October 2, 2017).

[4]Curtis (2013) describes some of the main reasons underlying the emergence of concentrated settlements in medieval western Europe and provides examples of linear settlements created around roads and rivers. Similar evidence is provided in Nierlich (1959) for five specific case studies: Egypt, the Great Lakes settlement, Siberia, Venezuela, and the Mississippi River Basin.

[5]See, for example, Nijkamp and Reggiani (1993) and Allen (1983).

[6]Recently, the independence of Paraguayan media has been severely questioned. In the elections of 2013, Reporters without Borders (2013) noted that TV channels had refused to air political campaigns of specific parties. This led to lower confidence in mass media advertising, and instead greater reliance on personal interactions.

[7]See https://tsje.gov.py/inscripciones-2017.html (accessed February 1, 2018).

[8]In cases when the person does not have a national ID card, the process becomes much more cumbersome since it involves getting a new ID card, showing proof of identity, and so on. Ninety-nine percent of our survey respondents had a national ID card.

[9]At the time of our fieldwork, there were 22 offices in Caaguazú and 21 in San Pedro, and the large majority of them are located in district capitals. See https://tsje.gov.py/sedes/ (accessed February 1, 2018).

[10]In 2013, there were 67 voting locations in Caaguazú and 76 in San Pedro. Polling stations in small districts are typically located in the district capital, but in larger ones, they are spread throughout. See https://tsje.gov.py/e2013/consulta-al-padron.html (accessed February 1, 2018).

travel to issue a vote, but not as far as when they need to register, which is a one-time event.

Experimental Design and Data

Our intervention took place between August 2012 and March 2013, before the April 2013 presidential elections in Paraguay. We designed an experiment to estimate the causal effect of public rallies and door-to-door campaigns on voter registration and turnout in the 2013 elections. Our experimental design also generated exogenous variation in the intensity of the treatment at the locality level to allow us to estimate the spillover effects of the door-to-door campaign. We limited our intervention to two departments that have traditionally shown high gender inequities in electoral registration in rural Paraguay, Caaguazú and San Pedro. Coincidentally, these are among the poorest departments in the country.[11,12]

Randomization and Intervention

Using locality-level data from the 2002 population census (the latest available at the time), the randomization of localities was done so that samples were balanced in population by age, sex, occupational activity, access to electricity and treated water, and the proportion of people with birth certificates and national identification cards. Each of the 300 localities sampled was assigned with an equal probability to the public rally campaign, door-to-door campaign, or control. Door-to-door localities were in turn randomly selected to be treated with different intensities (30, 40, or 50% of households in the locality). The locality-level randomization was done prior to any intervention.

We developed the GOTV campaigns with CIRD, a local nongovernmental organization with an extensive background of fieldwork across rural Paraguay. Researchers and CIRD coordinated carefully the messages to be included and the design of the material. CIRD then hired and trained local facilitators to implement the different campaigns. We conducted two rounds of the campaigns. In the first round, which took place one month prior to the voter registration deadline of October 31, 2012, we provided information, explained steps, raised awareness, and encouraged registration and voting.

In localities assigned to the D2D campaign, the fieldwork team first drew a map of the locality and estimated the total number of households, and from there, the team supervisor decided on the total number of randomly selected households to be visited to deliver the treatment (e.g., if the locality had 200 households and it was assigned to be treated with an intensity of 40%, 80 households were to be visited). Each household selected to be treated was approached up to three times, and the contact rate at this stage was extremely high, at 97%, significantly larger than that observed in other experiments (e.g., the contact rate was 41% in Arceneaux, Gerber, and Green 2006). The treatment protocol established a starting point in the village, and the team of enumerators counted the doors to knock depending on the proportion of households to be sampled. Two members of the canvassing team (one female, one male, wearing the Paraguayan national football team jersey to signal no party alliances) approached each of the randomly selected households and asked to speak to all women in the household 18 years or older. Canvassers were fluent in both Spanish and Guaraní to accommodate the respondents, but most of the contacts were done in Jopará, a colloquial form that combines both. Following a script and utilizing the campaign material, they delivered the main campaign messages, provided women with the campaign fliers, and made themselves available to answer questions. At this round, no data were collected from the canvassed women (beyond what was needed for the follow-up).

In localities assigned to the public rally treatment, the campaign was announced at least a week in advance utilizing various channels of communication, such as banners, posters, presentations at churches, and mobile billboards with the campaign slogan. The canvassing team was composed of a female and a male canvasser wearing the Paraguayan football team jersey. Public rallies were usually scheduled after Sunday church services, since in our preliminary fieldwork this was identified as a natural gathering place for the majority of the population. At the beginning of the rally, a standardized audio spot was played with a megaphone announcing the event as people were exiting mass, asking people to gather to receive the information to be offered by the canvassing team. The team members orally presented the campaign material to the group and distributed fliers with information on the steps needed for registration/voting and highlighting the importance of voting (presenters were bilingual and mostly spoke in Jopará). Our public rallies are somewhat in between large campaign rallies described in Fujiwara and Wantchekon (2013) and

[11]In the two provinces where we intervened, San Pedro and Caaguazú, only 44.9 and 45.9% of the registered population in 2012 were women, respectively (Tribunal de Justicia Suprema Electoral 2013).

[12]Eligible communities were rural, with 250 to 2,500 individuals and with at least one primary school. These criteria translated into 724 eligible localities in the two provinces, of which we randomly selected 300 to allocate across the control and two treatment groups.

megaphone information dissemination, as used in Arias et al. (2018). Importantly, both the script with the orally disseminated message and the fliers were exactly the same as in the door-to-door campaign. After the speech, the team was available to answer specific questions. In this first round of treatment, nobody in the control group was contacted. In Appendix B in the supporting information, we provide examples of the campaign fliers and pictures that illustrate the way the public rallies were conducted. No information was collected from rally attendants.

The second round of the treatment was intended to reinforce the message about the importance of participating in elections. On top of this, we provided useful information about the steps to be followed to issue a vote. This campaign took place in March 2013, one month prior to the presidential election on April 21, 2013. During this round of treatment, we followed the same format as in the first round of treatment: In public rally localities, we announced the meeting with a week's notice, and it took place after Sunday church services; in door-to-door localities, we aimed at reaching again all households contacted in 2012, and the follow-up rate was high (84%).

Data Collection and Sampling

Our individual-level survey was planned along the second round of the intervention. We interviewed an average of 20 women in each locality. In the public rally and control groups, households were chosen randomly; that is, this was the first contact with women in these localities.

In door-to-door localities, the survey sampling was stratified between households that had been contacted twice (treated households in D2D localities; henceforth, D2DT) and uncontacted households (control households in D2D localities; henceforth, D2DC). Within each stratum, about 10 households were randomly chosen, and one eligible woman was randomly administered a survey. Note that for D2DT women, the sample was drawn from those who had been already been contacted during the registration campaign. In this group, the survey was applied just before the treatment. The number of surveys in D2D localities was always lower than the number of households assigned to treatment, so the survey was conducted for a subsample of treated women; that is, in all cases, the number of surveys administered to D2DT women was less than 30% of the households in all localities.

The survey included an informed consent, which asked women permission to access their voting records, and the questionnaire asked about their dwelling, socioeconomic characteristics, political preferences and participation, and related information.[13]

The classification of localities into linear and nonlinear was done based on satellite images. Using Google Earth, two reviewers working independently completed the coding. In the unusual cases when differences between reviewers arose, the principal investigators weighed in to reach an agreement on how to classify the locality. The few borderline cases are not critical to our results, as their exclusion or switch does not change our findings. Maps 1 and 2 provide examples of the maps and satellite images used, and more examples can be found in Appendix C in the supporting information.

Empirical Strategy

The empirical strategy follows directly from the experimental design. To estimate the causal effect of receiving the different types of campaigns on registration and voting, we compare women in localities where we held public rallies, those in the door-to-door localities, and the control group. More precisely, we estimate the following regression equation:

$$y_{ij} = \alpha + \beta_1 PublicRally_j + \beta_2 D2D_{ij} + \gamma y_{ij}^{t-1} + \rho X_{ij} + \varepsilon_{ij,} \quad (1)$$

where y_{ij} represents an outcome variable (being registered to vote or voting in the 2013 election for woman i in locality j), $PublicRally_j$ is an indicator for whether locality j received the public rally campaign treatment, and $D2D_{ij}$ indicates whether woman i lives in D2D locality j. X_{ij} is a vector of individual- and locality-level controls, including a set of dummies indicating the department where the locality is.[14] Efficiency gains can be achieved by including the lagged value of the dependent variable; thus, we

[13]The full survey applied is available from the authors.

[14]Individual controls are age, years of education, only speaks Guarani, born in the same locality, formally employed, married, has children and number of children, HH asset index, and number of members in the HH. Locality covariates are population, percentage female, percent 0–14 years of age, percent 15–64 years of age, percent age 65+, percent illiterate, percent 6–14 attending school, percent with access to electricity, percent with access to running water, percent with access to sewage, percent with cell phone, percent with landline, percent rural, percentage of women employed, percentage of men employed, number of occupied houses in village, distance to voting center, and percent with access to trash collection. When finding missing values for controls, we impute a zero and control for a dummy for imputation. This imputation is balanced across treatment groups.

include y_{ij}^{t-1} in our preferred specifications. Finally, ε_{ij} is the error term. The treatment assignment was done by locality, and, thus, we cluster our standard errors at this level.

In both the public rally and D2D localities, the effects of registration should be interpreted as exposure to one round of treatment, whereas when we use turnout as the outcome, the treatment effects reflect a compounded treatment composed of two visits.

In Regression (1), β_1 represents the causal effect of living in a locality that received the public rally treatment, that is, the intent-to-treat (ITT) effect. We focus on the ITT effect to avoid concerns about potential self-selection into treatment; for instance, those who attend the rallies or who pay more attention to the information provided by the canvasser are likely to also be more interested in politics.[15] β_2 is the average treatment effect of living in a locality assigned to the door-to-door treatment. We run the regression in Equation (1) with two separate samples to estimate the direct effect and the spillovers of the door-to-door treatment. For the estimation of the direct effect of the door-to-door treatment, we restrict the sample to women contacted by one of our canvassers (D2DT). For the estimation of the spillovers, we restrict the sample to uncontacted women in treated localities (D2DC).

We hypothesize that the spillover effects will only be present in localities with a geographical distribution that facilitates social interactions and information diffusion, and therefore we separately estimate Equation (1) for linear and nonlinear localities. Note that we do not claim there is a causal relationship between the geographic layout of a locality and turnout, since the geographical distribution of houses in a locality is not randomly assigned. Rather, *within* each type of locality, the effect of the treatment can be interpreted as causal.

We investigate the intensive margin of the spillover effects by exploiting exogenous variation in the intensity of the treatment in each locality assigned to the door-to-door treatment. Each D2D locality was assigned to one of three intensity treatments: 30, 40, or 50% of households in the locality were directly contacted. Due to the small (and not necessarily divisible) number of households, attrition between the two treatment rounds, and imperfect compliance with the assignment, the actual proportion of households treated does not always coincide with the assignment. One might be concerned that either imperfect compliance or attrition could be correlated with the outcome of interest, or about measurement error, and therefore we employ an instrumental variable strategy in which we use the three dummies of treatment assignment as an instrument for the actual proportion of households treated in the locality. The main regression equation used to test whether information spillovers affect voting behavior is

$$y_{ij} = \alpha + \beta_1 PublicRally_j + \beta_2 PercTreated_{ij} + \gamma y_{ij}^{t-1} + \rho X_{ij} + \gamma Z_j + \varepsilon_{ij}, \quad (2)$$

where $PercTreated_{ij}$ is the ratio of the number of treated households divided by the total number of households in the locality, instrumented by three dummies representing treatment assignment to different intensities of the treatment.[16] β_2 represents the spillover effect: the effect of increasing the proportion of treated households by 1% for the average treated or untreated household, depending on the sample used. Again, we estimate the effects separately for contacted and uncontacted women in D2D localities, which allows us to identify two distinct types of spillovers. On the one hand, uncontacted women can be affected by the diffusion of information from treated women, that is, the effect of being exposed to the campaign information only through their neighbors (i.e., diffusion effect). On the other hand, the effect of being directly contacted can also be reinforced by other members of the community who were treated or those who were not contacted but heard about the campaign contents (i.e., reinforcement effect). We estimate these effects separately for women in linear and nonlinear localities.

Descriptive Statistics

In Appendix A in the supporting information, Tables A.1–A.4 show descriptive statistics using individual survey and administrative locality-level data. The sample we study is composed of 4,033 women who are, on average, 42 years old, with about 6 years of schooling, and with low rates of participation in the labor market. The registration rate just before intervention was about 83%, although less than 56% of adult women had voted in the previous presidential election (2010). There are some small differences between women in the different groups. In particular, women in D2D communities are 1 year older, have 0.23 more children, are more likely to only speak Guarani, and are more likely to be registered to vote in 2012. However, note that all the differences observed for the D2D group are led by contacted women (D2DT). To account for these

[15] Arguably, from a policy perspective (cost-effectiveness), the ITT effects are the relevant parameters to consider, rather than the treatment-on-the-treated estimates.

[16] Results are unchanged if we use as an instrument a single variable, which takes the value of the proportion of the population in the locality that was intended to be treated.

imbalances, we focus our attention on regressions that include individual-level controls. Importantly, there is a statistically significant difference in the baseline registration levels between women in the D2D treatment and the control. This reinforces the importance of controlling for the lagged dependent variable in our registration regressions.

The randomization balance was done using the latest available census, and the descriptive statistics at the locality level in Table A.3 show that despite the fact that we were not able to reach 14 localities due to bad weather or poor road conditions, balance was still achieved. Localities in the study had around 686 inhabitants in 2002. Women had low levels of participation in the labor force (9%). On average, about 78% of the dwellings had electricity, but only 23% had running water within the household. Finally, 94% of the population was registered at birth, but only 55% had ID cards.

The imbalance in individual-level characteristics in treatment groups is due to three main sources of attrition. First, as mentioned above, we were not able to reach 14 localities for the second round of treatment. This represents 4.2% of our original sample. Second, in the second round of treatment, when the survey was implemented, we were able to reach 94% of households originally contacted (5,621 out of 5,987 women), which disproportionately affects D2DT women. Third, out of the 5,621 women whom we interviewed, we were able to get a successful match between the ID number collected in the survey with the administrative data from the TSJE for only 4,922 women. Furthermore, the TSJE data had missing values for our main outcomes, which left us with 4,033 observations. This is shown in Figure 1.[17] We discuss the implications of attrition for the interpretation of our estimates in the Results and Robustness Checks subsections.

One key feature of our study is that we highlight the differences in the impact of GOTV campaigns between localities with different geographical configurations. The main difference between women living in linear and nonlinear localities is in the level of education and household wealth index, with women in the former scoring significantly better on both variables (see Table A.2). Table A.4 in the supporting information shows the baseline differences at the locality level. Linear localities are slightly smaller in population and area, but they are *more* densely populated (mainly because they are slightly smaller.) On the other hand, nonlinear localities seem to be *poorer*, with a lower coverage of electricity, water, and phone lines. Overall, the data in Tables A.2 and A.4 show that the linearity of the locality reflects a pattern of special distribution of households, and it is not the case that these are more densely populated and richer (actually, it is the opposite). Registration and turnout rates are not statistically different between these types of localities.

[17]To our knowledge, only León (2017) and the current research are turnout studies in developing countries that rely on survey information merged with administrative data for the main outcome variable. Attrition is a cost to doing this.

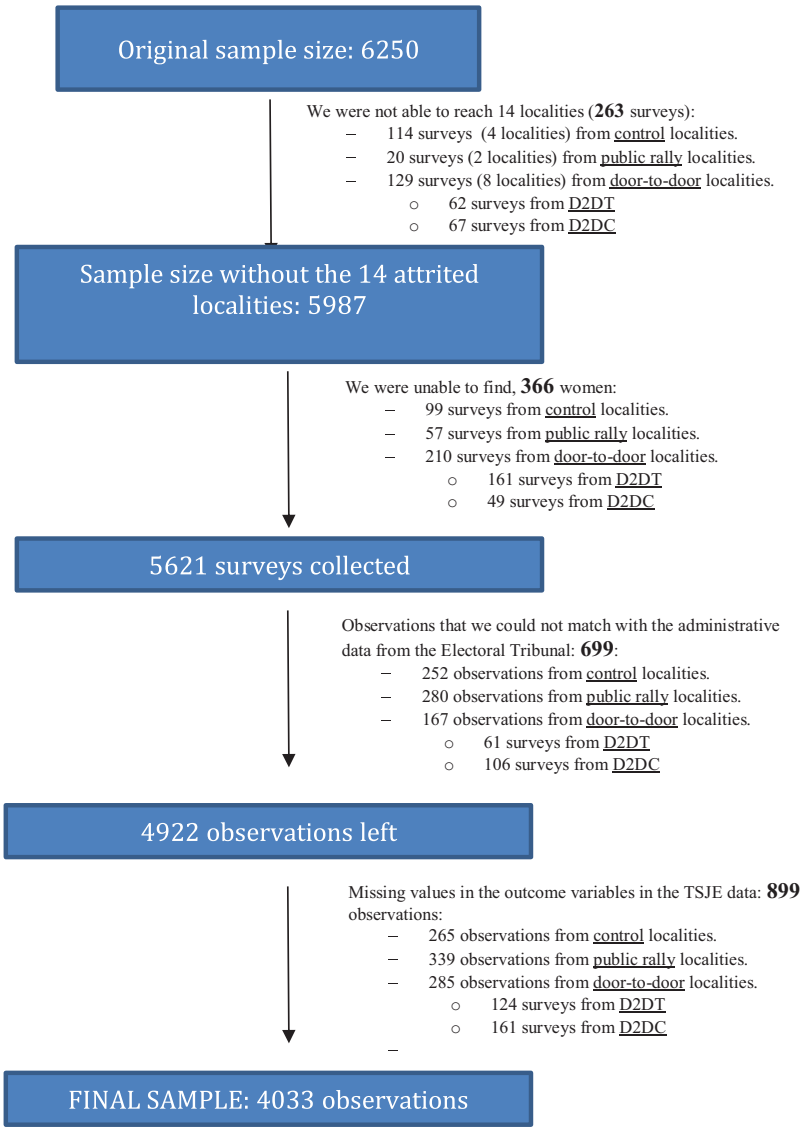
Results

Table 1 shows the effects of the different treatments on registration (Panel A) and turnout (Panel B). Columns 1–4 (Contacted) show the results of the estimation of the effects of the public rally treatment, as well as the direct effects of canvassing by excluding untreated women in D2D localities from the sample. On the other hand, columns 5–8 (Uncontacted) estimate the effect of the public rallies and the spillover effect of being untreated in a D2D locality. We do this by excluding from the estimation sample all the treated women in D2D localities. In each set of results, we gradually include controls at the individual level and locality level, department fixed effects, and the lag of the dependent variable.

We first evaluate the effects of being exposed to the first round of the campaign on registration, which are shown in Panel A of Table 1. The first thing to note in our regressions is that the point estimates do not change much when including controls and fixed effects (columns 1–3), which is reassuring. However, the inclusion of the lagged dependent variable reduces significantly the magnitude and statistical significance of the direct effect of the D2D treatment for contacted women. This is not surprising, as we had already reported an imbalance in the baseline registration rates for this group (Table A.1). Overall, after including the relevant controls, we see that public rallies had no effect on registration (columns 4 and 8). Our estimates allow us to reject effect sizes as small as 2.1 percentage points at conventional levels. Being directly or indirectly exposed to the door-to-door campaign does not affect registration either (first row, columns 4 and 8, respectively.) All the coefficients in these regressions are very close to zero and have tight standard errors. We can safely reject that direct contact increases the probability of registration by more than 2 percentage points, and the analog for uncontacted women allows us to reject effect sizes of more than 0.7 percentage points.

The lack of an effect can potentially be explained by the fact that registration was already high among Paraguayan women at the moment when we started our intervention, at around 88%, and registering to vote is quite a costly action. This is unlike in developed

FIGURE 1 Sample and Sources of Attrition



countries, where registration is much less costly, and in these cases, different studies have found significant effects of similar door-to-door campaigns (Bracconiere, Dormagen, and Pons 2018; Nickerson 2015).

Panel B of Table 1 shows the results of our turnout regressions. Again, note that the inclusion of controls (even the ones that show an imbalance in Table A.1) does not affect the magnitude of the coefficients in a meaningful way. The coefficients estimated for D2D and public rallies are very stable across specifications. We find that raising awareness about the importance of voting and informing women about the steps that have to be followed to vote affected the probability of voting if the message was delivered in a personalized way (column 4). These women were 4.6 percentage points more likely to vote. It is worth noting that the turnout rate in the control group was 75%, a relatively high proportion compared to the one observed in developed democracies like the United States or France. Therefore, the scope for increasing turnout was lower than other places where GOTV campaigns have been conducted; still, we find a comparable effect of our treatment (e.g., as in Gerber and Green 2000; Gerber, Green, and Shachar 2003; Pons 2018).[18]

[18]Green and Gerber (2002) conducted an experiment testing whether contacting voters by phone affects voting and found a small, negative, but insignificant effect, whereas Gerber and Green (2000) find that personal contact increases turnout significantly, directed mail slightly increases participation, and phone calls do not affect turnout. More recently, other researchers have shown that personalized messages delivered over the phone can actually

Map 1 Example of Linear and Nonlinear Localities



Note: The upper map illustrates localities in the district of Caaguazú. The lower map shows the linear localities of Calle 6 Tacua Cora and Calle 8 Tacua Cora; the nonlinear locality shown is Asentamiento 3 de mayo.

Map 2 Distribution of Households in the Linear and Nonlinear Localities



Note: Panel A shows the linear localities of Calle 6 Tacua Cora and Calle 8 Tacua Cora; in Panel B, the nonlinear locality shown is Asentamiento 3 de mayo.

On the other hand, for women in D2D localities who were randomly selected not to be contacted by the campaign (*Uncontacted*), we see a small (1.4 percentage points) and statistically insignificant effect.[19]

affect turnout (Arceneaux 2007; Arceneaux and Nickerson 2009; Nickerson 2008), but these effects are relatively small. These results are in line with Mann and Klofstad (2015), who show that the quality of the calls is also relevant in determining their effect on turnout.

[19]Following Nickerson (2008), this implies that 35% of the direct effect of the direct treatment was passed on to untreated women. However, the coefficient on the latter is not statistically significant, so we should not overinterpret this result. When a single regres-

Table 1 Effect of GOTV Campaigns on Registration and Turnout

	Panel A: Effect of the Treatment on Registration							
	Dependent Variable: Registered to Vote							
	Contacted Households				Uncontacted Households			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
D2D	0.035*	0.027	0.031*	0.004	0.009	0.002	0.009	–0.011
	(0.015)	(0.014)	(0.015)	(0.008)	(0.016)	(0.016)	(0.016)	(0.009)
Public Rallies	0.011	0.005	0.008	0.005	0.011	0.007	0.009	0.006
	(0.013)	(0.012)	(0.012)	(0.008)	(0.013)	(0.013)	(0.013)	(0.008)
Mean of Dep. Var. for Control Group	0.878	0.878	0.878	0.878	0.878	0.878	0.878	0.878
Observations	3,350	3,350	3,350	3,350	3,434	3,434	3,434	3,434
R-squared	0.002	0.048	0.049	0.688	0.000	0.045	0.046	0.707
	Panel B: Effect of the Treatment on Turnout							
	Dependent Variable: Voted in Presidential Elections of 2013							
	Contacted Households				Uncontacted Households			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
D2D	0.046*	0.036	0.044	0.046*	–0.009	–0.009	0.001	0.014
	(0.023)	(0.022)	(0.023)	(0.020)	(0.024)	(0.022)	(0.022)	(0.019)
Public Rallies	0.005	0.008	0.013	0.015	0.005	0.011	0.014	0.016
	(0.021)	(0.020)	(0.020)	(0.016)	(0.021)	(0.020)	(0.020)	(0.016)
Mean of Dep. Var. for Control Group	0.741	0.741	0.741	0.741	0.741	0.741	0.741	0.741
Observations	3,350	3,350	3,350	3,350	3,434	3,434	3,434	3,434
R-squared	0.001	0.064	0.066	0.208	0.000	0.071	0.072	0.211
Individual Covariates		Yes	Yes	Yes		Yes	Yes	Yes
Locality-Level Covariates		Yes	Yes	Yes		Yes	Yes	Yes
Department Fixed Effects			Yes	Yes			Yes	Yes
Lagged Dependent Variable				Yes				Yes

Note: Individual- and locality-level covariates included are detailed in the main text (note 10). Columns 1–4 exclude from the sample untreated women in D2D localities, whereas columns 5–8 exclude treated women in D2D localities. Standard errors are in parentheses, clustered at the locality level.
*significant at the .05 level, **significant at the .01 level.

Finally, living in a locality where public rallies were held led to a small (1.5 percentage points) and insignificant effect on the probability of turning out to vote on Election Day. Very few studies test interventions similar to ours. Arias et al. (2018) find null effects of information disseminated through mobile loudspeakers in Mexico. Addonizio, Green, and Glaser (2007) and Green and McClelland (2017) are the closest ones, and they find that in the United States, the effect of festivals held at polling sites increases turnout significantly, and they are a cost-effective way of doing so. Our treatment is not strictly comparable to the ones analyzed in Addonizio, Green, and Glaser (2007) and Green and McClelland (2017) since our rallies took place before the election, rather than on Election Day.

All in all, our findings are consistent with previous evidence in that more personalized and directed treatments (door-to-door visits) are much more effective than impersonal messages delivered to a broad public (e.g., through rallies) or messages distributed indirectly.

Robustness Checks

The main concern with the validity of our experimental results arises from the relatively high attrition rate, as well as the potential for differential attrition to be correlated

sion is run with the pooled sample, we find that the ratio of the coefficients is indistinguishable from zero.

Table 2 Lee (2009) Bounds for Treatment Effects

	Baseline Specification	Lower Bound	Upper Bound
Public Rallies	0.005	–0.009	0.010
	(0.017)	(0.023)	(0.018)
D2D-Uncontacted	–0.009	–0.010	–0.004
	(0.021)	(0.022)	(0.028)
D2D-Contacted	0.046*	0.031	0.100**
	(0.021)	(0.022)	(0.030)

Note: Results in the baseline specification come from bivariate OLS regressions of turnout on the specified treatment, comparing each treatment group only with the control. The lower and upper bounds are computed using the procedure outlined in Lee (2009). Standard errors are in parentheses.
*significant at the .05 level, **significant at the .01 level.

with unobservables that could bias our estimates of the direct effects of the D2DT.[20] Between the three different sources of attrition described in the Descriptive Statistics subsection, we end up considering in our final analysis sample about 65% of sampled observations (see Figure 1 for details). If the fact that we were not able to (a) reach some localities (4.2%), (b) recontact women who were treated in the first round (5.8%), or (c) match the survey data with the administrative information or the fact that we do not have full information on the outcomes (25.4%) were correlated with turnout, the main estimates shown in the tables above would be biased.

To alleviate these concerns, in Table A.5 in the supporting information, we show that, after controlling for locality-level covariates that we include in our main regression and department fixed effects, missing information from the 14 localities that we were not able to reach is uncorrelated with the treatment status of the locality.

Similarly, in Table A.6, we use our individual-level data to investigate whether the treatment status is correlated with attrition. In this case, we consider the 6,250 observations that we initially *planned to survey*. The results show that there is *less* attrition for women in the public rally treatment arm (compared to the control group), whereas there is no differential attrition for women in the D2D group. When looking into this group, however, we see that in the D2DT group, there is a higher rate of differential attrition, and it is statistically significant.[21] This is expected since women in the D2D treatment arm were contacted twice, and, therefore, the chances of finding them for a second contact at the moment of the survey were lower.

To show that the selective attrition does not significantly affect our estimates, in Table 2, we provide the upper and lower bounds for our estimates using the procedure outlined in Lee (2009.) This procedure computes treatment effect bounds for samples with nonrandom sample attrition. The bounds are computed under extreme assumptions on the missing information in the observed data. To do this, we trim observations from the sample with lower attrition, so that both have the same proportional missing observations, and the trimming is done such that we eliminate those with the best and worst values of the outcome variable—in this case, either women who voted or who did not vote. The advantage of using Lee bounds is that they do not rely on any assumptions about the selection mechanism, but they do assume monotonicity, which effectively means that assignment to treatment can only affect attrition in one direction.[22]

We run the bounding exercise in a conservative way, making *unconditional* comparisons between each of the three treatment groups and the control separately without using any controls to tighten the bounds. Column 1 shows the bivariate and unconditional treatment effects for each of the treatments, as compared to the control group.[23] Columns 2 and 3 show the upper and lower

[20]Recall that no differential effects are found in the control and D2DC samples, so that the estimation of the spillovers effects is not threatened by this feature.

[21]Note that in these regressions, we cannot use any controls since, by definition, they are missing.

[22]In our case, there is no reason to believe that being able to find someone for the survey or to match the survey and administrative data is correlated with treatment assignment. On the other hand, given that women in D2D localities were contacted directly twice, it is less likely that we were able to find them; hence, the monotonicity assumption is a plausible one. The reason why we chose to use this bounding method is that it relies only on the assumption of monotonicity, which, as explained in the new text, is likely to hold in this case. Typically, Lee bounds are much tighter than Manski bounds (which in many practical applications end up being uninformative).

[23]The procedure to compute the Lee (2009) bounds does not allow for the inclusion of controls; rather, it allows for tightening the bounds by using dichotomous covariates to define cells within

bounds for these unconditional treatment effects. For Public Rally and D2D-Uncontacted, the upper and lower bounds are very close to zero and are statistically insignificant, whereas for D2D-Contacted, the treatment effects go from 3.1 to 10 percentage points. The upper bound is statistically different from zero, whereas the lower bound has a p-value of .16. Note that the mean of the dependent variable in this context is high, around 0.75, and therefore the assumption taken for the computation of the lower bound (that all the differential attrition abstained) is quite extreme. Therefore, the fact that the estimated lower bound has a p-value slightly larger than .1 should not be a concern. All in all, as expected, the differential attrition for the D2DT group is larger, and, therefore, the estimated bounds for this group are wider.

In an additional robustness check for the significance of the effect of the D2D treatment on turnout, we run a sensitivity analysis following Oster (2017) and Altonji et al. (2005). In this analysis, we estimate the proportion of the unobserved to observed variation needed to drive our estimated effect to zero. Assuming a maximum R-squared of 0.9 (i.e., that there is some measurement error and *both* the unobserved and observed variation can explain up to 90% of the variation in the dependent variable), we find a delta of 1.14. Conversely, if we assume that the maximum R-squared is 1, the estimated delta is 1.002. The rule of thumb suggested in Oster (2017) for this statistic is that the results are not sensitive if the ratio of the unobserved to observed variation needed to nullify the result is greater than 1; hence, this sensitivity analysis confirms those from the Lee (2009) bounds.

Overall, three pieces of evidence suggest that attrition does not seem to generate major biases in our estimation. First, Tables A.1 and A.4 in the supporting information show that the observations that we consider in our estimation sample are mostly balanced across the treatment groups. Second, the regression analysis in Tables A.5 and A.6 show that, after controlling for the relevant covariates, the treatment status at the locality level is uncorrelated with the probability of attrition, but this is not the case at the individual level, where there is some differential attrition (especially in the D2DT group), and hence the need to empirically test whether this affects our estimated effects. The Lee (2009) bounds in Table 2 demonstrate that this selective attrition does not affect our qualitative results, and these results are confirmed by additional sensitivity analysis.

which the trimming procedure is applied. Given that most of our controls are continuous, we cannot use them for tightening the bounds. Instead, we present the conservative option of the computation of the unconditional bounds.

Urbanization Patterns, Turnout, and Spillover Effects

We hypothesize that the effects of our treatments should be stronger in localities that have an urbanization pattern that is conducive to social interactions and information dissemination. As mentioned above, in the Paraguayan case, there are interesting heterogeneities in the geographic layout of localities—in particular, some of them are set up as a long street, whereas others have a more concentric shape. In the latter, social interactions are more likely to take place.[24] In this section, we test whether the main effect estimated in the previous section is larger in these localities. Table 3 presents these results, where we use our specification from columns 4 and 8 from Table 1 and split the sample between linear and nonlinear localities.

First, the treatment effects of both the door-to-door and public rally intervention on registration rates str a very precisely estimated 0 in both linear and nonlinear localities, as shown in Table 3, Panel A. Second, Panel B shows the effects of both treatments on turnout. We find that the effect of public rallies is small and statistically insignificant in both linear and nonlinear localities.[25]

The effect of the door-to-door treatment on directly contacted women is explained by women living in nonlinear localities. These women are 7.6 percentage points more likely to vote, a 10% increase, compared to the control group. For those in linear localities, we see a very precise zero effect of the treatment (see columns 1–3). The difference between the effect for linear and nonlinear localities is significant at the 5% level.[26] For uncontacted women in D2D localities, we see that the treatment effects are slightly larger in nonlinear localities, and the difference between both effects is significant at the 10% level. Given that it is nearly impossible to randomize the geographical distribution of a locality, and absent a good instrument, we do not claim that the effects from Table 3

[24]Our treatment assignment, even though not stratified by the geographical configuration of the localities (since this dimension of heterogeneity was not in our original analysis plan), is balanced within linear and nonlinear localities, and therefore we are able to make causal statements within linear and within nonlinear localities.

[25]The results from independent comparisons between each of the treatment groups and the control, for the full sample, and for linear and nonlinear localities are shown in Table A.7 in the supporting information. The qualitative results hold, but statistical significance is reduced.

[26]We compute the p-value for the difference in the coefficients by pooling the data and interacting our variable of interest with the linear dummy. We report the p-value of the difference between the coefficients.

Table 3 Effect of GOTV Campaigns on Registration and Turnout, by Linearity of the Locality

	Panel A: Effect of the Treatment on Registration							
	Dependent Variable: Registered to Vote							
	Contacted Households			Uncontacted Households			p-value	
	Full Sample	Linear	Nonlinear	Full Sample	Linear	Nonlinear		
	(1)	(2)	(3)	(4)	(5)	(6)	(3)-(2)	(6)-(5)
D2D	0.004	–0.002	0.005	–0.011	–0.021	–0.014	.697	.695
	(0.008)	(0.017)	(0.010)	(0.009)	(0.015)	(0.010)		
Public Rallies	0.005	–0.006	0.009	0.006	–0.012	0.009		
	(0.008)	(0.013)	(0.010)	(0.008)	(0.013)	(0.010)		
Mean of Dep. Var. for Control Group	0.878	0.884	0.876	0.878	0.884	0.876		
Observations	3,350	1,122	2,228	3,434	1,154	2,280		
R-squared	0.688	0.683	0.693	0.707	0.697	0.717		
	Panel B: Effect of the Treatment on Turnout							
	Dependent Variable: Voted in Presidential Elections of 2013							
	Contacted Households			Uncontacted Households			p-value	
	Full Sample	Linear	Nonlinear	Full Sample	Linear	Nonlinear		
	(1)	(2)	(3)	(4)	(5)	(6)	(3)-(2)	(6)-(5)
D2D	0.046*	–0.015	0.076**	0.014	–0.032	0.033	.018	.076
	(0.020)	(0.030)	(0.025)	(0.019)	(0.027)	(0.025)		
Public Rallies	0.015	–0.002	0.018	0.016	0.002	0.019		
	(0.016)	(0.027)	(0.022)	(0.016)	(0.027)	(0.022)		
Mean of Dep. Var. for Control Group	0.741	0.761	0.733	0.741	0.761	0.733		
Observations	3,350	1,122	2,228	3,434	1,154	2,280		
R-squared	0.208	0.195	0.228	0.211	0.205	0.228		

Note: All regressions follow the same specification as the one in column 4 in Table 1. Standard errors are in parentheses, clustered at the locality level.
*significant at the .05 level, **significant at the .01 level.

are causal, but rather that *within* linear or nonlinear communities, there are causal effects of the randomly assigned treatment. These results are robust to the inclusion of interactions between the treatments and other covariates at the locality or individual level that could be correlated with the linearity of the locality. Findings are shown in Table A.8 in the supporting information.

If the treatment is similar in both linear and nonlinear localities, how could it be that the effect is so much larger in the latter? Just listening to a message delivered directly might not be enough to change voting behavior, but rather, this message may have to be reinforced by the members of one's social network (either treated or untreated). Likewise, it might be that there is a need for a critical mass of social connections to convey a message for it to be effective in changing behavior. In other words, the effect of the treatment might be reinforced if more people are talking about the treatment, therefore increasing its effectiveness. Identifying these reinforcement and diffusion effects is not straightforward since usually it requires very detailed data on network connections (as in Fafchamps and Vicente 2013; Fafchamps, Vicente, and Vaz 2018). Alternatively, we can use the exogenous variation in the intensity with which each locality is treated, which implicitly generates exogenous variation in the number of treated connections each respondent has, allowing us to test for the presence of reinforcement and diffusion effects. This interpretation relies on the assumption that

Table 4 Estimating Reinforcement and Diffusion Effects (2SLS Estimates)

	Voted in Presidential Elections of 2013					
	Targeted Households			Untargeted Households		
	Full Sample	Linear	Nonlinear	Full Sample	Linear	Nonlinear
Public Rallies	0.016	0.000	0.020	0.018	0.006	0.020
	(0.016)	(0.025)	(0.022)	(0.016)	(0.025)	(0.022)
Percentage of Treated Households	0.124*	–0.040	0.231**	0.079	–0.056	0.124
	(0.056)	(0.069)	(0.074)	(0.060)	(0.062)	(0.085)
Mean of Dep. Var. for Control Group	0.741	0.761	0.733	0.741	0.761	0.733
F-test of Excluded Instruments	45.91	24.30	38.63	34.44	17.60	22.04
Observations	3,350	1,122	2,228	3,434	1,154	2,280
R-squared	0.208	0.196	0.228	0.212	0.206	0.230

Note: All regressions are 2SLS and include the same set of controls and fixed effects as in column 4 in Table 1. We instrument the percentage of treated households with the three treatment assignment dummies (30, 40, or 50%). The first-stage regressions are reported in Table A.6 in the supporting information. Standard errors are in parentheses, clustered at the locality level.
*significant at the .05 level, **significant at the .01 level.

the average woman in a low-intensity treatment locality is as equally connected as the average woman in a high-intensity treatment locality, which is plausible, given the randomization of treatment intensity.

In Table 4, we estimate the effect of the proportion of treated households on turnout using the two-stage least squares (2SLS) strategy described above that is, we instrument the proportion of households actually treated with the assigned one (30, 40, or 50%). Given the zero effect found for registration so far, here we only focus on turnout. We do this by comparing contacted women in door-to-door localities treated with different intensities to women in the pure control group and in public rally localities. This allows us to estimate the reinforcement and diffusion effects—the effect of the intensity of the treatment on contacted and uncontacted women in D2D localities, respectively.

The average reinforcement effect is large (column 1). An increase in the proportion of treated households of 10 percentage points leads to turnout rates 1.2 percentage points higher.[27] When we split the sample between linear and nonlinear localities, we see that again the effect is entirely driven by those in nonlinear localities. An increase in 10 percentage points in the proportion of treated households in a locality leads to an increase of 2.3 percentage points in the probability of voting for women directly treated. If we take into account that in the average door-to-door locality in our sample, we treated 35.6% of households, these estimates indicate that in the average locality, treated women were 8.2 percentage points more likely to vote, almost doubling the direct effect of the treatment shown in Table 1.

The diffusion effects of the treatment are estimated in columns 3–6. This is the effect of being indirectly exposed to the door-to-door treatment through one's network connections. We do this by comparing untreated women in door-to-door localities with those in pure control localities, as well as the ones in public rally localities. Again, there are relatively large but insignificant diffusion effects, but the average result masks substantial heterogeneity. All of the effect is coming from the effects in nonlinear localities. The intensity of the treatment determines how much the effect diffuses among nontreated women. Increasing the proportion of treated households in the locality by 10 percentage points leads uncontacted women to vote 1.2 percentage points more often, but the effects are noisy.

Compared to the effects of the direct treatment, both the diffusion and reinforcement effects are large, highlighting the importance of social interactions in the diffusion of information that encourages behavioral changes.

[27] The first-stage regressions are shown Table A.9 in the supporting information. The predictive power of our instruments is large, and the F-stat associated with the excluded instruments is between 17 and 45, well above the conventional standards. We report the ordinary least squares (OLS) version of this table in Table A.10. As expected, these results are qualitatively similar, but measurement error and imperfect compliance with the experimental protocol lead to attenuation bias.

Conclusions

GOTV campaigns are widely used around the world, and the evidence available presents mixed results in terms

of their effectiveness for fostering electoral participation. In this article, we present experimental results that contribute to the evidence on the effectiveness of information campaigns to foster electoral participation in several ways. First, we evaluate in the same setting two widely used information dissemination methods, namely, door-to-door campaigns and public rallies. Second, we not only study the effects of the campaign on turnout, but we also consider registration, which is a large barrier for electoral participation, in particular for groups of the population that have low representation, such as women. Third, we take seriously the information spillovers that take place in rural communities, where information is scarce and social interactions determine the extent to which information can disseminate through networks. We experimentally evaluate spillover effects and estimate the indirect effects of the treatment on uncontacted women, as well as the reinforcement on the treated population. Moreover, we explicitly consider in our study the role that the physical layout of the locality plays in mediating the effectiveness of these campaigns. Finally, we add to the literature by studying rural areas in a poor Latin American country, where gender gaps in registration are high, registration costs are significant, and there is a high incidence of patronage.

Using administrative data on registration and turnout, survey data, and satellite images, our findings show that public rallies do not affect registration: We are able to reject effect sizes as small as 2 percentage points. The results are robust to a number of specification checks and allow us to reject even very small effect sizes. On the other hand, the effects of public rallies on turnout are small and statistically insignificant. Door-to-door campaigns also have no effect on the probability that a woman in rural Paraguay registers to vote. We conjecture that this has to do with the relatively high baseline registration rates in our study population and the high registration costs.

Having received two visits from a canvasser (registration and turnout campaigns), however, does have a large average effect on the probability of voting. Women directly contacted by our canvassers are 4.6 percentage points more likely to vote. The use of administrative data and the sampling design generated a relatively high attrition rate in this group; however, our results are robust to the inclusion of controls at the individual and locality level, and the estimated bounds marginally reject that the treatment effect is different from zero. Importantly, these effects are entirely driven by women who live in nonlinear localities, which we hypothesize are more conducive to social interactions. For this specific group, directly treated women are 7.6 percentage points more likely to vote.

Spillovers to uncontacted women in villages where we did the door-to-door campaign are small and indistinguishable from zero, on average. However, digging deeper in the data, in nonlinear localities, we find suggestive evidence that uncontacted women are more likely to participate in elections. Not only this, but the experimental variation allows us to estimate the effects of the intensity of the campaign on treated women, and we find that reinforcement effects are particularly large in nonlinear localities.

The cost-effectiveness of these interventions is of paramount importance for policy design. While public rallies may be a cheaper way of reaching a larger share of the population (compared to direct, face-to-face contacts), our estimates show that they generate a small and statistically insignificant effect on turnout. Still, we can use the data on the costs of each intervention to perform a back-of-the-envelope calculation of the relative cost-effectiveness of these treatments. Following Dhaliwal et al. (2011), we abstract from the costs of designing the campaign and the associated materials and defining the messages. In both interventions, we gave away the same amount of campaign materials, and the cost of transport was similar. The difference in the cost between the two campaigns is in the time that each field team spent in the average locality: Whereas for the public rallies each team of two people spent 1 day in a locality (2 working days overall), in the average D2D locality (where we treated 35.6% of households), two teams of two people each took 1 day to complete the treatment (4 working days overall).[28] In sum, the door-to-door campaign was twice as expensive than the public rallies, and hence for the public rallies to be more cost-effective than the door-to-door campaigns, the effect of the latter has to be twice as large as the former. Using this information and the results of a regression similar to columns 4 and 8 in Panel B of Table 1, but including at the same time the contacted and uncontacted households (so we can estimate the effect of D2DT, D2DC, and public rallies simultaneously), we test whether the sum of the *direct and indirect* effects of the door-to-door campaign is two times larger than the effect of the public rallies (the results from these regressions are in Table A.11 in the supporting information). We find that with our average treatment effects, the public rally

[28]In public rally localities, a team made a first visit to the locality and spent half a day putting out posters announcing the campaign and promoting it using megaphones. The day of the rally, they went to the locality again, gave the public speech, and stayed for some additional time to answer any questions. All these activities took 1 day in total, and two round trips per person. In the door-to-door campaign, each team of two people completed 14 interventions per day (each took between 15 and 20 minutes). In the average locality, where 40% of households were visited, the treatment was completed in 1 day, and it implied one round trip per person.

campaigns are marginally more cost-effective; however, given the noisiness of the estimates for the indirect effect of the door-to-door and public rallies treatments, we cannot reject that the ratio of the effectiveness of these interventions is different from zero. On the other hand, when we consider only nonlinear localities, our findings suggest that door-to-door campaigns are marginally more cost-effective interventions, but again, the result is noisy.[29,30]

Overall, our results suggest that the design of GOTV campaigns should consider the geographical constraints that affect the frequency of social interactions and, therefore, could limit the extent of spillover effects. This point is bolstered by the evidence from our cost-effectiveness analysis, which suggests that door-to-door canvassing may be more cost-effective in nonlinear localities. While ours is one of the first studies to find evidence on this issue, it is clear that more research is needed to understand the mechanisms through which geographical layouts may play a role in the design of informational campaigns.

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[29]We test whether the coefficient associated with D2DT plus that of D2DC (weighting by the population treated in the average locality, i.e., 0.356 and 0.64, respectively), minus two times the public rally coefficient equals 0 (i.e., $0.356*D2DT + 0.644*D2DC - 2*PR = 0$). In the full sample, the difference is –0.007 with a p-value of .84 (the test for whether $0.356*D2DT + 0.644*D2DC - 2*PR > 0$ fails to reject the null with a p-value of .40). In nonlinear localities, where we see larger direct and indirect effects for the door-to-door campaign, the difference is 0.13 with a p-value of .73 (the test for whether $0.356*D2DT + 0.644*D2DC - 2*PR > 0$ fails to reject the null with a p-value of.37).

[30]Public rallies could have also affected men's likelihood to vote, but as long as we assume that the effect is similar, this should not affect our relative cost-effectiveness calculation.

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Supporting Information

Additional supporting information may be found online in the Supporting Information section at the end of the article.

Supporting Information
Appendix A: Tables A.1–A.11
Appendix B: Campaign Materials
Appendix C: Examples of Linear and Nonlinear Localities