Urbanization Patterns, Information Diffusion and Female Voting in Rural Paraguay

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ABSTRACT

We use a field experiment to evaluate the impact of two informational get-out-the-vote (GOTV) campaigns to boost female electoral participation in rural areas of Paraguay. We find that public rallies have a small and insignificant effect either on registration or voter turnout in the 2013 presidential elections. Households that received door-to-door canvasing treatment (D2D) are 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 to the already treated population are twice as large as diffusion effects to the untreated. Our results underscore the importance of taking into account urbanization patterns when designing informational campaigns.

1.   INTRODUCTION

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 (Sinclair et al 2012, Fafchamps et al. 2018, Gine and Mansuri 2017.) 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 paper 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 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 campaign (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 while public rallies show small and insignificant effects on voting, face-to-face interactions significantly increased turnout among treated women. Furthermore, we find evidence of spillover effects that leads 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 (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 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 it involves 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 means to evaluating 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 percent of executive posts, and while in the 2013 presidential elections 52 percent of registered voters were men, such gender gap was as large as ten 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 "non-linear 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 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 non-linear 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 are 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 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 both, people not contacted by the intervention, as well as those who were contacted.

Our paper 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 facilitate social interactions, while 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 facilitate social interactions ("non-linear"), 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 conclusions, we provide a back of the envelope calculation of the relative cost effectiveness of these interventions.

2. 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 percent in the US. Bracconiere et al (2017) 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.

Green and Gerber (2004) 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 et. al. 2013.) Pons (2017) 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 US shows that the effect of GOTV campaigns on turnout displays great variation in terms of magnitude (see e.g. Guan and Green 2006, John and Brannan 2008, Gine and Mansuri 2017 for China, the UK and Pakistan), and for European, Bhatti et. al. 2016 presents 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 mind 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 those contacted at individuals (Dellavigna, et al, 2017; Gerber et al 2008).

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

Despite the popularity of public rallies, there are few studies on its effects. Addonizio, et al (2007) estimate the effect of festivals held at polling sites and find that they significantly increase turnout. Green and McCleland (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 onto other members of the household.

Sinclair et al (2012), using a multilevel GOTV experiment designed to measure within household and within 9-digit zip code spillovers in Chicago found evidence for the former, but regardless of the intensity of the treatment, there were no spillovers within zip code. Fafchamps et al. (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, Gine and Mansuri (2017) 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 non-linear 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 group of buildings that is formed in a long line with no obvious center and narrow shape may have been driven by settlements built

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

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, et al. 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, and transportation facilities in central Brooklyn, which would encompass a six mile arc (Roberts, 1967). [3],[4] Researchers from different disciplines have long argued on the origins of linearities or non-linearities in formation of towns may not be necessarily linked to different observable characteristics.[5]

3. INSTITUTIONAL BACKGROUND

Paraguay is a presidential representative democracy, where the president and National Congress are elected simultaneously, every five years. In the elections of March 2013, over 2,000 candidates were running for president and vice-president, 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, while the election of representatives follow a 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 percent 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 are the radio and TV.[6] In rural settings, most candidates do not have a

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[3] See: http://www.visionofbritain.org.uk/place/3159, last accessed on October 2nd, 2017.

[4] Curtis (2013) describes some of the main reasons underlying the emergence of concentrated settlements in medieval western Europe, and provide 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 e.g. Nijkamp and Reggiani (1993) and Allen (1984).

[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

direct contact with voters, but rather, they rely on local political brokers. These political operators act as middlemen between the candidate and voters and are typically local leaders with a certain degree of credibility and legitimacy, who are either long time 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 where we work.

Registering to vote is a costly activity. While automatic registration has recently been implemented for people who turn 18 after December 30th 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 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 that many more localities have their own.[10] This implies voters in more isolated localities will still have to travel to issue a vote, but not as far away as when they need to register, which is a one-time event.

4. EXPERIMENTAL DESIGN AND DATA

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parties. This led to lower confidence in mass media advertising, and rather relying more in personal interactions.

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

[8] In cases when the person does not have a national ID, the process becomes much more cumbersome, since it involves getting a new identity card, showing proof of identity, etc. 99 percent of respondents to our survey had a national ID card.

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

[10] In 2013, there were 67 voting locations in Caaguazu and 76 in San Pedro. Polling stations is 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 1st 2018.

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]

4.1. 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 percent of households in the locality). The locality level randomization was done prior to any intervention.

We developed the GOTV campaigns with CIRD, a local NGO with 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 31st, 2012, we provided information, explained steps, raised awareness, and encouraged registration and voting.

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[11] In the two provinces where we intervened, San Pedro and Caaguazú, only 44.9 and 45.9 percent of the registered population in 2012 were women, respectively (TSJE, 2013).
[12] Eligible communities were rural, with 250 to 2500 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 the two treatment groups.

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 percent, 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 percent, significantly larger than those observed in other experiments, eg. in Arceneaux et al (2006), where the contact rate was of 41 percent. 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 was collected from the canvased 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 male canvasser using 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 megaphone information dissemination, as used in Arias et. al. (2018). Importantly, both the script with the orally disseminated message as well as 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, we provide examples of the campaign fliers, and pictures that illustrate the way the public rallies were conducted. No information was collected from attendants to the rally.

The second round of the treatment was intended to reinforce the message on 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 of April 21st 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 percent).

4.2. 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, i.e. this was the first contact with women in these localities.

In door-to-door localities, the survey sampling was stratified between those households that had been contacted twice (treated households in D2D localities, henceforth DTDT), and uncontacted (control households in D2D localities, henceforth D2DC). Within each strata, about 10 households were randomly chosen, and one eligible women was randomly administered a survey. Note that for D2DT women, the sample was done from those who had been already 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, i.e. in all cases, the number of surveys to D2DT women was less than 30 percent 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 the dwelling, socio-economic characteristics, political preferences and participation, and related information.[13]

The classification of localities into linear and non-linear was done based on satellite images. Using Google Earth, two reviewers working independently did the coding. In the unusual cases when differences between reviewers arose, the PIs 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 do 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.

5. 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:

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

where y_{ij} represent 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; $D2D_{ij}$ 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

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[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, % of female, % 0-14 years of age, % 15-64, years of age, % age 65+, % of illiterate, % 6-14 attending to school, % with access to electricity, % with access to running water, % with access to sewage, % with cellphone, % with landline, rural, % of women employed, % of men employed, number of occupied houses in village, distance to voting center and % with access to trash

achieved by including the lagged value of the dependent variable, thus we 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, public rally and D2D localities, the effects of registration should be interpreted as exposure to one round of treatment, while 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 intention-to-treat (ITT) effect. We focus on the ITT to avoid concerns about potential self-selection into treatment, for instance those who attend to 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 (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 separately estimate equation (1) for linear and non-linear localities. Note that we do not claim that 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 percent of households in the locality were directly contacted. Due to the

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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.

[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.

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 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 use 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:

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

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 one percent 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 your neighbors ("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 ("reinforcement effect"). We estimate these effects separately for women in linear and non-linear localities.

5.1. DESCRIPTIVE STATISTICS

Tables A.1-A.4 in Appendix A we 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

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[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.

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 one 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 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 D2DT 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 percent). On average, about 78 percent of the dwellings had electricity, but only 23 percent had running water within the household. Finally, 94 percent of the population was registered at birth, but only 55 percent 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 percent of our original sample. Second, in the second round of treatment, when the survey was implemented, we were able to reach 94 percent of households originally contacted (5,621 out of 5,987 women,) which disproportionally affects D2DT women. Third, out of the 5,621 women that 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 4,922 women, only. 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 Sub Sections 5.2 and 5.3.

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[17] To our knowledge, only Leon (2017) and us 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.

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 non-linear localities is in the level of education and household wealth index, with women in former scoring significantly better on both variables (see Table A.2.) Table A.4. show the baseline differences at the locality level. Linear localities are slightly smaller in population and area, but are *more* densely populated (mainly because they are slightly smaller.) On the other hand, non-linear localities seem to be *poorer*, with a lower coverage of electricity, water, and phone lines. Overall, the data in Table 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.

5.2. 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 canvasing 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 percent, and registering to vote is quite a costly action. This is unlike in developed countries, where registration is much less costly, and in these cases different studies have found significant effects of similar door-to-door campaigns (Nickerson 2015, Bracconiere et. al. 2017).

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 percent, a relatively high proportion compared to the one observed in developed democracies, like the US 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 (as in e.g. Green and Gerber 2000, Gerber et al 2003, Pons 2017).[18] On the other hand, for women in D2D localities who were randomly selected not to be contacted

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[18] Gerber and Green (2002) conducted an experiment testing whether contacting voters by phone affects voting, and found a small, negative but insignificant effect, while 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 affect turnout (Nickerson 2008; Arceneaux 2006; Arceneaux and Nickerson 2006) 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.

by the campaign ("Uncontacted"), we see a small (1.4 p.p.) and statistically insignificant effect.[19]

Finally, living in a locality where public rallies were held led to a small (1.5 p.p.) and insignificant effect on the probability of turning out to vote in Election Day. Very few studies test interventions similar to ours. Arias et. al (2018) find null effects of information disseminated through mobile loud speakers in Mexico. Addonizio, et al (2007) and Green and McCleland (2017) are the closest ones, and they find that in the US, the effect of festivals held at polling sites increase turnout significantly and are a cost effective way of doing so. Our treatment is not strictly comparable to the ones analyzed in Addonizio, et al (2007) and Green and McCleland (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 (through e.g. rallies) or messages distributed indirectly.

5.3. ROBUSTNESS CHECKS

The main concern with the validity of our experimental results arises from the relatively high attrition rate, and the potential for differential attrition to be correlated with unobservables that could bias our estimates of the direct effects of the D2DT.[20] Between the three different sources of attrition described in Section 5.1, we end up considering in our final analysis sample about 65 percent of sampled observations (see Figure 1 for details). If the fact that we were (i) not able to reach some localities (4.2 percent), (ii) we were not able to re-contact women who were treated in the first round (5.8 percent), or (iii) not able to match the survey data with the administrative information or we do not have a full information on the outcomes (25.4 percent) were correlated with turnout, the main estimates shown in the tables above would be biased.

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[19] Following Nickerson (2008), this implies that 35 percent 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 over interpret this result. When a single regression with the pooled sample, and find that the ratio of the coefficients is indistinguishable from zero.

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

To alleviate these concerns, in Table A.5. 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 if 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), while 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 D2DT 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 non-random 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 of is done such that we eliminate those with the best and worst values of the outcome variable, in this case, either women that voted or who didn't vote. The advantage of using Lee bounds is that it does not rely on any assumptions about the selection mechanism, but does 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

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[21] Note that in these regressions, we can't 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 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 on the new text is likely to hold in this case. Typically, Lee bounds are much tighter that Manski bounds (which in many practical applications ends up being uninformative).

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 bounds for these unconditional treatment effects. For the Public Rally and D2D-Uncontacted, the upper and lower bounds are very close to zero and are statistically insignificant, while for the D2D-Contacted, the treatment effects go from 3.1 to 10 percentage points. The upper bound is statistically different from zero, while the lower bound has a p-value of 0.16. 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 0.1 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 D2DT 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 (ie. that there is some measurement error and *both* the unobserved and observed variation can explain up to 90 percent of the variation in the dependent variable), we find a delta of 1.14. Conversely, if we assume that the maximum R-squared is of 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 one, hence this sensitivity analysis confirm 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 show that the observations that we consider in our estimation sample are mostly balanced across the treatment groups.

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[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 dichotomic covariates to define cells within which the trimming procedure is applied. Given that most of our controls are continuous, we can't use the for tightening the bounds. Instead, we present the conservative option of the computation of the unconditional bounds.

Second, the regression analysis in Table 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.

6. 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 are 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, while 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 present these results, where we use our specification from columns (4) and (8) from Table 1, and split the sample between linear and non-linear localities.

First, the treatment effects of both the door-to-door and public rally intervention on registration rates is a very precisely estimated zero in both, linear and non-linear localities, as shown in 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 non-linear localities.[25]

The effect of the door-to-door treatment on directly contacted women is explained by women living in non-linear localities. These women are 7.6 percentage points more likely to vote, a 10 percent 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

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[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 non-linear localities, and therefore we are able to make causal statements within linear and within non-linear localities.

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

difference between the effect for linear and non-linear localities is significant at the 5 percent level.[26] For uncontacted women in D2D localities, we see that the treatment effects are slightly larger in non-linear localities, and the difference between both effects is significant at the 10 percent level. Given that it is nearly impossible to randomize the geographical distribution of a locality, and absent a good instrument, we don't claim that the effects from Table 3 are causal, but rather that *within* linear or non-linear 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.

If the treatment is similar in both linear and non-linear 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 your 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 massage 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 and Fafchamps et al. 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 to test for the presence of reinforcement and diffusion effects. This interpretation relies on the assumption that the average woman in a low intensity treatment locality is 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 2SLS strategy described above, i.e. 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

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[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.

contacted women in door-to-door localities treated with different intensities to women in the pure control group and on public rally localities. This allows us to estimate the reinforcement and diffusion effects, i.e. 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 1.2 percentage points higher turnout rates.[27] When we split the sample between linear and non-linear localities, we see that again the effect is entirely driven by those in non-linear 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 percent 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 your 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 non-linear localities. The intensity of the treatment determines how much does the effect diffuses among non-treated 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 encourage behavioral changes.

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[27] The first stage regressions are shown Table A.9. 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 OLS version of this table in appendix A.10. As expected, these results are qualitatively similar, but measurement error and imperfect compliance with the experimental protocol lead to attenuation bias.

7. CONCLUSIONS

GOTV campaigns are widely used around the world, and the evidence available present mixed results in terms of their effectiveness for fostering electoral participation. In this paper, 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 also consider registration, which is a large barrier for electoral participation, in particular for groups of the population that have low representation, like for example, 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 effect 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 non-linear localities, which we hypothesize that 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 non-linear localities we find suggestive evidence that uncontacted women are more likely to participate in elections. Not only this, but the experimental variation allow us to estimate the effects of the intensity of the campaign on treated women, and we find that reinforcement effects are particularly large in non-linear 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 (compare 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 do 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: while for the public rallies each team of two people spent 1 days in a locality (2 working days overall), in the average D2D locality (where we treated 35.6 percent of households) two teams of 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

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[28] In public rally localities, a team did 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 mass, 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 average locality, where 40 percent of households were visited, the treatment was completed in 1 day, and it implied one round trip per person.

campaigns, the effect of the later 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 2 times larger than the effect of the public rallies (the results from these regressions are in Table A.11). We find that with our average treatment effects, the public rally 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 can't reject that the ratio of the effectiveness of these interventions is different from zero. On the other hand, when we consider only non-linear 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 should consider the geographical constraints that affect the frequency of social interactions, and therefore could limit the extent of spillovers effects. This point is bolstered by the evidence from our cost-effectiveness analysis, which suggests that door-to-door canvasing may be more cost effective in non-linear 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 the one of D2DC (weighting by the population treated in the average locality, i.e. 0.356 and 0.64, respectively), minus 2 times the public rally coefficient equals zero (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 0. 84 (the test for whether $0.356*D2DT + 0.644*D2DC – 2*PR > 0$, fails to reject the null with a p-value of 0.40). In non-linear localities, where we see lager direct and indirect effects for the door-to-door campaign, the difference is 0.13 with a p-value of 0.73 (the test for whether $0.356*D2DT + 0.644*D2DC – 2*PR > 0$, fails to reject the null with a p-value of 0.37).

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

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Figure 1: Sample and Sources of attrition

```
┌─────────────────────────────────────┐
│     Original sample size: 6250       │
└─────────────────────────────────────┘
              │
              │   We were not able to reach 14 localities (263 surveys):
              │        –   114 surveys  (4 localities) from control localities.
              │        –   20 surveys (2 localities) from public rally localities.
              │        –   129 surveys (8 localities) from door-to-door localities.
              │                o   62 surveys from D2DT
              │                o   67 surveys from D2DC
              ▼
┌─────────────────────────────────────┐
│   Sample size without the 14 attrited │
│           localities: 5987            │
└─────────────────────────────────────┘
              │
              │   We were unable to find, 366 women:
              │        –   99 surveys from control localities.
              │        –   57 surveys from public rally localities.
              │        –   210 surveys from door-to-door localities.
              │                o   161 surveys from D2DT
              │                o   49 surveys from D2DC
              ▼
┌─────────────────────────────────────┐
│        5621 surveys collected        │
└─────────────────────────────────────┘
              │
              │   Observations that we could not match with the administrative
              │   data from the Electoral Tribunal: 699:
              │        –   252 observations from control localities.
              │        –   280 observations from public rally localities.
              │        –   167 observations from door-to-door localities.
              │                o   61 surveys from D2DT
              │                o   106 surveys from D2DC
              ▼
┌─────────────────────────────────────┐
│        4922 observations left        │
└─────────────────────────────────────┘
              │
              │   Missing values in the outcome variables in the TSJE data: 899
              │   observations:
              │        –   265 observations from control localities.
              │        –   339 observations from public rally localities.
              │        –   285 observations from door-to-door localities.
              │                o   124 surveys from D2DT
              │                o   161 surveys from D2DC
              │        –
              ▼
┌─────────────────────────────────────┐
│    FINAL SAMPLE: 4033 observations   │
└─────────────────────────────────────┘
```

Map 1: Example of Linear and Non-Linear Localities

Note: The map in the left illustrates localities in the district of Caaguazu. The map in the right shows the linear localities of Calle 6 Tacua Cora and Calle 8 Tacua Cora; the non-linear locality shown is Asentamiento 3 de mayo.

Map 2: Distribution of households in the Linear and Non-Linear localities

Panel A: Linear   **Panel B: Non-Linear**

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

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
# of obs.	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
# of obs.	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
Deparmtent Fixed effects			Yes	Yes			Yes	Yes
Lagged dependent variable				Yes				Yes

Note: Individual and locality level covariates included are detailed in the main text (footnote 10). Columns (1)-(4) exclude from the sample untreated women in D2D localities, while columns (5)-(8) exclude treated women in D2D localities. Standard errors in parenthesis clustered at the locality level. * significant at the 0.05 level ** significant at the 0.01 level.

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 in parenthesis. * significant at the 0.05 level ** significant at the 0.01 level.

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	Not Linear	Full Sample	Linear	Not Linear		
	(1)	(2)	(3)	(4)	(5)	(6)	(3)-(2)	(6)-(5)
D2D	0.004	-0.002	0.005	-0.011	-0.021	-0.014	0.697	0.695
	(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		
# of obs.	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	Not Linear	Full Sample	Linear	Not Linear		
	(1)	(2)	(3)	(4)	(5)	(6)	(3)-(2)	(6)-(5)
D2D	0.046*	-0.015	0.076**	0.014	-0.032	0.033	0.018	0.076
	(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		
# of obs.	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 in parenthesis clustered at the locality level.
* significant at the 0.05 level ** significant at the 0.01 level.

Table 4: Estimating Reinforcement and Diffusion Effects –2SLS Estimates

| | Voted in presidential elections of 2013 | | | | | |
| | Targeted Households | | | Untargeted Households | | |
	Full Sample	Linear	Not Linear	Full Sample	Linear	Not Linear
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)
% 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
# of obs.	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 % of Treated households with the three treatment assignment dummies (30, 40 or 50 percent). The first stage regressions are reported in Table A.6. Standard errors reported in parenthesis are clustered at the locality level. * significant at the 0.05 level ** significant at the 0.01 level.