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Social interactions and female voting in rural Paraguay: The role of urbanization patterns on the effectiveness of GOVT campaigns

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Gender and Diversity Division

August 2018

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**SOCIAL INTERACTIONS AND FEMALE VOTING IN RURAL PARAGUAY:
THE ROLE OF URBANIZATION PATTERNS ON THE EFFECTIVENESS OF GOTV CAMPAIGNS**

Alberto Chong, Gianmarco León, Vivian Roza, Martín Valdivia, and Gabriela Vega*

August 2018

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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 Paraguay. We find that public campaigns had no effect either on the probability of registration, or on voter turnout in the 2013 presidential election. However, households that received door-to-door (D2D) treatment were four percentage points more likely to vote. Experimental variation on the intensity of the treatment at the village level allows us to estimate spillover effects, which are present in localities that are geographically more concentrated, and thus may favor social interactions. The effect of reinforcement of the message to the already treated population is twice as large as the diffusion to the untreated. Our results underscore the importance of taking into account urbanization patterns when designing informational campaigns.

Keywords: Voter Behavior, Electoral Politics, Urbanization, Spillover Effects, Paraguay

JEL Classification Codes: O10, D72 , O53, D71

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1. INTRODUCTION

The role of social interactions as a vehicle to boost the impact of information campaigns or technology adoption, while an intriguing concept is not a new one. Governments and NGOs alike rely on information spillovers when designing different types of education, health, microfinance and political campaigns. However, in the case of the latter, evidence on whether information spreads through social networks and generates behavioral changes is mixed (Sinclair et al 2012, Fafchamps et al. 2015, Gine and Mansuri 2017). Understanding the conditions under which social interactions allow information to spread and generate changes in behavior is not only important from the policy perspective, to increase women's political participation as a way to enfranchise them and improve the provision of public goods that benefit them, but also because it allows us to get insights into the formation and relevance of social networks.

We apply a field experiment and use individual level administrative voting data, survey information, and satellite images to study the direct and indirect effects of two distinct get-out-the-vote (GOTV) campaigns targeted to women in rural Paraguay, and evaluate their impacts explicitly taking into account different urbanization patterns that may facilitate social interactions. Before the 2013 presidential elections, we randomly assigned rural villages either to be exposed to massive rallies that disseminated information about registration and voting, or to receive the same information in a 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 both treated and untreated households in D2D localities. Neither intervention led to increases in voter registration, but while massive campaigns had no effect on voting, face-to-face interactions significantly increased turnout among treated women. Furthermore, we find evidence of information spillovers that lead to higher turnout, but these are only present in localities with urbanization patterns that appear to favor social interactions. We find that these spillover effects are also more important for treated women (reinforcement effect) than for untreated women (diffusion effects).

Mass public campaigns are typically perceived as a very cost-effective way to generate political mobilization to support particular actions. The relatively low cost and their capacity to reach large audiences make them an attractive option for both politicians

and policy makers.¹ Door-to-door campaigns are more expensive and intensive in human resources and coordination efforts, however, they have proven to be very effective in mobilizing voters in urban areas in developed countries, although there is less evidence of these interventions in rural areas in the developing world. This distinction is relevant because of the limited access to information, but still heavy reliance on social networks for information acquisition. It is unclear which type of intervention provides the larger returns on investment in terms of generating the desired behavioral change for a larger share of the population.

We focus on rural Paraguay, which shows a geographic particularity in its urbanization patterns. The typical rural Paraguayan locality follows one of two clearly identifiable geographic configurations. On the one hand, it follows 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 also show an alternative and distinct geographical layout that follows a very straightforward linear configuration, with houses distributed along a single road, and with most of them having a plot of agricultural land at 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.² Some historical evidence suggests that the linear distribution of towns was loosely planned during the colonization process organized by the authoritarian regime of Alfredo Stroessner, to expand the agricultural frontier in Caaguazú and San Pedro during the period that went from the sixties to the eighties (Galeano, 2011). The implementation of this urbanization strategy was linked to the desire to populate border areas with neighboring countries and the pampas (Hetherington, 2011). Importantly, the difference between linear and non-linear localities is not driven by differences in rurality, or population density, but rather by the geographic layout³.

¹ We know of only one relevant mass campaign that used experimental methods to measure effectiveness. It was carried out in the US and measured the impact of a street sign campaign on voter mobilization (Panagopoulos 2009). It produced higher turnout rates and was not focused on women.

² The layouts are illustrated in Figure 1 using satellite photographs. Interestingly, as will be shown in Tables 1 and A.1, linear and non-linear localities are similar along a number of observable characteristics of the locality and of individuals living in these localities.

³ 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.

We explicitly take into account the role of geographical restrictions on social interactions in evaluating the information spillover effects of our intervention.⁴ Our experiment generates exogenous variation in the intensity with which each locality is treated, allowing us to test for the prevalence of spillover effects and their magnitude for localities with linear and non-linear layouts described above. 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 center of town, people 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 that produces a more personalized and direct contact can help build a credibility bond between the messenger and the receiver, which may translate in further involvement by the individual, as well as an additional sense of commitment, which may contribute to internalizing the message and thus more likely to act accordingly and disseminate it.

Our paper shows evidence on four key findings. First, door-to-door canvassing has a direct effect on treated women, but massive public campaigns do not generate higher turnout. Second, 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, we find that 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. To our knowledge, the political economy implications of different urbanization patterns in both developed and developing countries have not been studied to this day, and we contribute to the literature by incorporating this dimension as one of the main determinants of how information spills over through social networks. Third, information dissemination affects the behavior of untreated women in treated localities, as the reinforcement effects on treated women is twice as large as diffusion effects in promoting political participation. Finally, we show that massive, impersonal messages are

⁴ Paraguay still has one of the lowest rates of female participation in leadership positions in the region. In the 2003 elections, 29.8 percent of candidates were female, and 12 percent were elected. In the 2008 elections the corresponding numbers were 33 percent and 16.2 percent, respectively. The scarcity of women in decision-making positions in public office might reflect the gender gaps in electoral participation. Even though at the national level 48.4 percent of the registered population are women, the average masks significant regional heterogeneities in the proportion of registered women.

not effective at generating changes in behavior among women in rural Paraguay, while door-to-door campaigns that disseminate the exact same information in a conversational manner increase turnout in 4.2 percentage points among contacted women. One advantage of door-to-door campaigns is that information flows within communities, and thus, it might also affect untreated women.

Section 2 discusses the current literature on field experiments and incentives to voting. Section 3 describes our data and experimental design. Section 4 presents our empirical strategy. Section 5 presents descriptive statistics and basic results that compare door-to-door canvassing with public campaigns, as well as robustness checks. Section 6 analyzes social interactions in the context of geographical restrictions, and Section 7 summarizes and concludes.

2. RECENT LITERATURE ON FIELD EXPERIMENTS AND VOTING

There is a growing literature that uses field experiments to understand voter behavior (Green 2013; Michelson and Nickerson 2011; Green et. al. 2013; Pande 2011; Leon 2017.)⁵ Evidence from randomized controlled trials shows that there is large variation in the magnitude of the estimated effects of campaign type. Green and Gerber (2004) analyze experiments conducted in real electoral settings, examining the impact and effectiveness of door-to-door canvassing, telephone calls, direct mail, and other campaign tactics, mostly in the US. They show that more direct interactions are usually more effective at increasing turnout, despite their larger implementation cost. Pons (2016) carried out a large-scale experiment to mobilize voters to vote for François Hollande in the 2012 French presidential elections. His results show that personal interactions with the canvassers were effective at mobilizing voters, and the campaign was responsible for about one fourth of the victory margin obtained. Our study directly tests in the same setting the impact of two commonly used campaigning mechanisms, and our findings echo the previous evidence.

The fact that conversational messages are more likely to change people's minds and increase the likelihood of participating in elections or candidate choice suggests that

⁵ See: 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; Gine and Mansuri 2017.

closer interactions are key in the success of GOTV campaigns. A series of recent studies have shown that campaigns that incorporate social pressure components have much larger effects on turnout than those targeted at individuals. For example, Dellavigna et al (2017) presents evidence that social image is an important determinant of voter turnout. Gerber et al (2006) use a large-scale field experiment in which they experimented with the social salience of one's vote and show that social pressure influences the decision to vote. People who thought that other household members or neighbors were likely to find out whether they voted or not were more likely to participate in elections. Similarly, Nickerson (2008) presents evidence that uncontacted people in households that were contacted by canvassers were more likely to vote than those in uncontacted households, which shows that there are large information spillovers within the household: 60% of the propensity to vote induced by the experimental treatment is passed onto other members of the household.

Different studies have found mixed evidence that spillover effects outside the household exist. 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, no within zip code spillovers were found. In a developing country setting, Fafchamps et al. (2015) show that information about the candidates in the 2009 Mozambican election diffuses through kinship networks and chatting. However, evidence on the effectiveness of information spillovers in 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. Their evidence shows that treated women are 12 percentage points more likely to vote, and to choose a different candidate than their husbands. Moreover, untreated women in treated clusters are also more likely to vote and to make an independent choice, showing large spillover effects. Our findings help reconcile the previous evidence by noting that informational spillovers should not be expected to exist in every context, but rather only in places where there are physical spaces that facilitate social interactions, and the geographical layout of the community is conducive to them.

3. EXPERIMENTAL DESIGN AND DATA

Our intervention took place between August 2012 and March 2013, just before the April 2013 presidential elections in Paraguay.⁶ We designed an experiment to estimate the causal effect of both public rallies and door-to-door campaigns on voter registration and turnout in the 2013 elections. The experimental design also generated exogenous variation in the intensity of the treatment at the locality level. We limited our intervention to two provinces that have traditionally shown high gender inequities in electoral registration and turnout in rural Paraguay: Caaguazú and San Pedro.^{7,8}

Using community-level data from the 2002 population census (the latest available), 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.⁹ Each of the 300 communities sampled was assigned to the public rally campaign, door-to-door campaign, or control. Additionally, within the door-to-door localities, we randomly selected them to be treated with different intensities (30, 40, or 50 percent of households).¹⁰

⁶ These were the sixth democratic presidential elections in the country since the end of the long-term Strossner's dictatorship in 1989, and Paraguayans were about to elect a new president, members of the lower and upper chambers of Congress, as well as regional governors, and their corresponding councils. In June 2012, Fernando Lugo, the first elected President from a party other than the Colorado one, was impeached. In these elections, there were eleven presidential candidates and the list headed by the Colorado party candidate, Horacio Cartes won the elections in the first round with over 45 percent of the votes.

⁷ In the two districts where we intervened, San Pedro and Caaguazú, only 44.9 and 45.9 percent of the registered population in 2012 were women, respectively (TSJE 2012). Women's voting patterns reflect a similar gender gap. In the 2008 general elections, women voted in lower numbers than men (42.7 percent and 45.1 percent of the voters were women in San Pedro and Caaguazú, respectively).

⁸ Eligible communities were rural, with a population between 250-2,500 individuals, and with at least one primary school. Population size was established with the Population Census of 2002, the latest available at the time of the fieldwork. The information on schools was obtained from the Ministry of Education, which had been updated up to 2011. The presence of a school was relevant to guarantee the existence of a building where people would usually meet. These eligibility criteria left us with a total of 724 localities in the two provinces, of which we randomly selected 300 to allocate across the control group and the two treatment groups (100 localities per group).

⁹ We run 100 repetitions of the randomization and chose the first one that showed no statistically significant t-statistic when comparing means of these variables across the three groups.

¹⁰ The campaigns were implemented in collaboration with the Centro de Información y Recursos para el Desarrollo (CIRD), an NGO based in Asunción, Paraguay. The printed material included two posters with photos encouraging women to register and vote, instructional posters and fliers with step-by-step instructions on how to register and how to vote, a one-page handout providing a rationalization for registering and voting, and a sticker with the campaign slogan and image. The slogan chosen for the campaign was "Register, Decide and Vote: Participate in Paraguay's Future" (Inscribite, Decidite y Votá: Participa en el futuro de Paraguay). An audio spot for public rallies was also created. Appendix 2 provides a sample of campaign materials. The messages were not translated into Guaraní as it is mostly a spoken, not written language. However, canvassing team members in both the door-to-door and public campaigns conducted their interventions in Guaraní.

The campaign was implemented in two rounds (see Figure 2 for details on the timing of the interventions.) The first round took place one month prior to the voter registration deadline of October 31, 2012, while the second one took place in March 2013, one month prior to the presidential election of April 21, 2013. While the focus of the first round of treatment was to encourage voter registration, providing information on how to register, and raising awareness about the importance of voting, in the second round, we reinforced the message on the importance of voting, and provided information on the procedures and requirements for voting. In the door-to-door campaign, two members of the canvassing team (one female, one male, wearing the Paraguayan national football team shirt) approached households and asked to speak to all women in the household 18 years or older and collected basic household data, including the respondent's identification number. 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.¹¹

In localities assigned to the public rallies treatment, the campaign was first 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 protocol established that the canvassing team, composed of a female and male canvasser, should first identify local leaders to explain the objective of the campaign and coordinate support for the visit the following weekend. Public rallies were usually scheduled after Sunday church services.^{12,13}

¹¹ To sample households, canvassers followed a random selection algorithm that ensured that treated households were evenly distributed throughout the locality. Supervisors first estimated the number of households in the locality and determined the number of households that should be treated, depending on the treatment arm to which the locality was assigned (30, 40, or 50 percent). With this information, they instructed enumerators to count doors, and contact 3, 4, or 5 out of every ten doors. Due to the small number of households available in the average village (not always an exact multiple of 0.3, 0.4, or 0.5), as well as imperfect compliance with the protocol, the proportion of treated households in each locality does not exactly correspond to the treatment assignment.

¹² In our preliminary fieldwork, we identified Sunday Mass as the natural gathering place for the majority of members of each locality. In both Caaguazú and San Pedro, over 95% of the population identify themselves as Catholic. When it was not possible to implement the campaign after Sunday church services, canvassing team members turned to schools, local women's committee meetings, the main plaza and community meetings. Although the geographical layout could affect the capacity to reach more people with the ads, in favor of "non-linear" localities, we assumed such losses were mostly unaffected by the choice of place/event to announce the massive campaigns.

¹³ During the day of the rally, an audio spot was played with a megaphone announcing the event as people were exiting the mass, asking them to gather to receive the information to be offered by the canvassing team. As interested women gathered, the team members orally presented the campaign material to the group and

During the second round of the campaign all public rally localities were revisited, while in door-to-door localities, we re-contacted the same households. Overall, we were unable to reach 14 localities initially sampled due to bad weather and poor transportation infrastructure. Additionally, in localities where the second round and surveying campaign took place, our survey team was able to reach 5,623 out of 5,987 women (see Figure 3.)

The roll out of the second round of the treatment was planned along with our household survey. We interviewed an average of 20 women in each locality, and the exact number of surveys collected was proportional to the population. In the public rally and control groups, households were chosen randomly using the same sampling algorithm as the one used to distribute the door-to-door treatment. Within door-to-door localities, the survey sampling was stratified by treatment and control households.¹⁴ Only one woman per household was interviewed, and she was chosen from the roster of all adult women in the household.¹⁵ The survey questionnaire included questions about the family roster, dwelling characteristics, socio-economic characteristics of the women interviewed, together with modules about political information, political preferences, nature and level of political participation, the interviewee's empowerment within the household, characteristics of their social networks, and information about the campaign events.

In order to obtain administrative records for our main dependent variables on registration and voting, we asked the women interviewed for their names and ID numbers. This information matched with data provided by the Paraguayan Supreme Electoral Council (TSJE) using both the ID numbers and names. Overall, we were able to match 4,922 observations out of 5,621 (88 percent).¹⁶

distributed fliers with information on the steps needed for registration/voting and highlighting the importance of voting. 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.

¹⁴ Half of the interviews were randomly chosen from the list of households visited during the first round of the treatment, and the other half was randomly chosen among untreated households. In treatment households, the survey was conducted just before the treatment was distributed.

¹⁵ The specific mechanism was based on a double-entry table that connected the last digit of the household number and the number of adult women in the family roster (see Rutstein and Staveteig 2014). The field protocol required the surveyor to search for the randomly chosen women at least three times before replacing her for another one. The replacement was made altering the number of adult women in the roster by the one that was missing.

¹⁶ 899 of these observations had missing data in the records of the TSJE. This is probably due to administrative failures, and therefore, these observations were excluded from our main analysis sample.

The classification of localities into linear and non-linear was done based on satellite images of the surveyed localities. Two reviewers, working independently did the coding. In the unusual cases when differences arose, the PIs weighed in to reach an agreement on how to classify the locality. These borderline cases are not critical to our results as their exclusion or switch do not change our findings. The precise guidelines used by the reviewers are described in Appendix B and illustrated with several examples.

4. EMPIRICAL STRATEGY

The empirical strategy follows directly from the experimental design. To estimate the causal effect receiving the different types of campaigns on registration and voting, we compare women in localities where we held public rallies (*Massive*), those who were treated in the door-to-door localities (*D2DT*), untreated women in door-to-door localities (*D2DC*), and those in the control group. More precisely, we estimate the following regression equation:

$$y_{ij} = \alpha + \beta_1 Massive_j + \beta_2 D2DT_{ij} + \beta_3 D2DC_{ij} + \gamma y_{ij}^{t-1} + \rho X_{ij} + \gamma Z_j + \varepsilon_{ij} \quad (1)$$

where y_{ij} represent an outcome variable – registered to vote after our first intervention or voting in the 2013 election for woman i in locality j ; $Massive_j$ is an indicator for whether locality j received the public rally campaign treatment; $D2DT_{ij}$ indicates whether woman i in D2D locality j received the door-to-door treatment; and $D2DC_{ij}$ denotes if woman i in D2D locality j was selected as a control (i.e. did not receive the door-to-door treatment.) X_{ij} is a vector of individual controls and Z_j are locality level characteristics, including a set of dummies indicating the district where the locality is.¹⁷ McKenzie et. al. (2012) shows

¹⁷ The individual level controls included in the regressions 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 level covariates included are: population, percent of female population, percent population 0-14, percent of population 15-64, percent of population 65+, percent of illiterate population, percent of population 6-14 attending school, percent of population with access to electricity, percent of population with access to running water, percent of population with access to sewage, percent of population that has a cellphone, percent of population that has a landline, rural, percent of women employed, percent of men employed, number of occupied houses in the village, distance to voting center and percent of population with access to trash collection. In cases in which we had missing values for the control variables, we impute a zero and control for a dummy indicating whether a specific variable was imputed.

that in randomized evaluations with a short t , efficiency gains can be 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. Given that the treatment assignment is done by locality, we cluster our standard errors at this level.

In regression (1), β_1 represents the causal effect of living in a village that received the public rally treatment. To avoid concerns about potential self-selection into treatment, e.g. those who are more interested in politics are also the ones who attend the rallies or who pay more attention to the information provided by the canvasser, we restrict ourselves to interpreting β_1 as intent-to-treat effects.¹⁸ β_2 is the average treatment effect of living in a locality assigned to the door-to-door treatment *and* being contacted by one of our canvassers. β_3 represents the effect of being in a locality assigned to the door-to-door treatment, but not having received the treatment directly, i.e. the indirect effect of the treatment on untreated women. Note that for the estimation of the direct and indirect effects of the door-to-door treatment (β_2 and β_3), clustering the standard errors at the locality level is conservative, since within D2D localities, assignment to treatment is determined at the individual level.

To investigate the spillover effects, we exploit the 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 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 not always coincide with the assignment 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:

$$y_{ij} = \alpha + \beta_1 \text{Massive}_j + \beta_2 \text{PercTreated}_{ij} + \gamma y_{ij}^{t-1} + \rho X_{ij} + \gamma Z_j + \varepsilon_{ij} \quad (2)$$

¹⁸ Arguably, from a policy perspective (cost-effectiveness), these are the relevant parameters to consider, rather than the ATT.

where $PercTreated_{ij}$ is the ratio of the number of treated households divided by the total number of households, instrumented by three dummies representing treatment assignment to different intensities of the treatment. β_2 represents the spillover effect: the effect of increasing the proportion of treated households by one percent for the average household in the locality, either directly treated or not. We hypothesize that the spillover effects will only be present in localities with a geographical distribution that facilitates social interactions, and therefore separately estimate equation (5) 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 this is not randomly assigned, but rather that within each type of locality, the effect of the intensity of the treatment can be interpreted as causal.

Spillover effects can act in two diverse ways. On the one hand, untreated women can be affected by the diffusion of information from treated women (“diffusion effect”), i.e. the effect of being exposed to the campaign information only through your neighbors. On the other hand, the effect of being directly treated can also be reinforced by other members of the community who were treated (“reinforcement effect”). To estimate the diffusion effects, we compare untreated households in D2D localities with the pure control group, i.e., estimate equation (5) excluding the directly treated. On the other hand, to estimate the reinforcement effects, we compare treated households in treated localities with the control group (excluding control households in treatment localities from the estimation of equation (5)).

5. DESCRIPTIVE STATISTICS AND MAIN RESULTS

5.1. DESCRIPTIVE STATISTICS

Table 1 shows descriptive statistics at the individual level, comparing treatment groups, and women who live in linear and non-linear localities, using the information from our survey, and administrative data for our main outcome variables. We have information on all the relevant analysis variables for 4,033 women. The average age of the women interviewed is 42 years old, with about 6 years of schooling, and with low rates of economic activity (only 14% of the adult women were employed at the moment of the survey). The

registration rate before our intervention was about 83%, although less than 56% of adult women had voted in the previous election (2010). There are some small differences between women in the different groups. In particular, women in D2D communities are one year older, and have 0.23 more children. To account for these imbalances, we focus our attention on regressions that include individual level controls.

Partially, the imbalance in some of the individual level controls are due to three main sources of attrition: (i) we were not able to reach 14 localities for the second round of treatment and the survey due to bad weather (4.2% of our original sample), (ii) in the second round of the treatment (when the survey was implemented), we were able to reach 94 percent of households initially contacted (5,621 out of 5,987, this attrition only applies to D2DT households), and (iii) 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, however, some of them have missing values on our main outcome variables (registration and turnout) in the TSJE data, leaving us with 4,033 observations.

There is a large benefit of using administrative data on a variable that is prone to misreporting, and to our knowledge, ours and Leon (2017) are the only turnout studies in developing countries that rely on survey information merged with administrative data for our main outcome variable. However, the use of this data comes at the cost of losing observations along the way. Figure 3 summarizes the different sources of attrition and in the next section we show a battery of robustness checks to show that this attrition does not affect the magnitude or interpretation of our main results.

The randomization balance was done using the latest available census, and the descriptive statistics at the locality level in Table A.1 show that, despite the 14 localities that we couldn't reach, balance was 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.

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 distribution of linear and non-linear settlements in rural Paraguay appear to be balanced,

as demonstrated by our summary statistics comparing linear and non-linear towns (Table 1 and A.1) and appear to have been determined mostly by the fact that the political authority at the time of settlement, a dictator, loosely encouraged the design of rural towns with a linear shape, under the expectation that this shape would minimize social organization. During his time in power, President Stroessner provided incentives to people to colonize uninhabited rural areas taking advantage of the favorable economic situation in the country during the 1950s (Hetherington 2011), and was able to encourage people to settle in areas that he thought would help the country, for instance border areas with other countries, or the pampas. According to existing evidence, this was encouraged to potential settlers, who seemed happy to move to these new towns given the incentives provided, mostly agricultural plots (Hetherington, 2011).

There does not seem to be a lot of differences between linear and non-linear localities. Importantly, the main differences observed in Table A.1 shows that the geographical distribution of localities is not related to its wealth or size. For example, linear localities are slightly smaller both in terms of population and area, and furthermore, linear localities are *more* densely populated. In terms of public services available, non-linear localities seem to be poorer, with a lower coverage of electricity, running water, and phone lines. This pattern is mirrored by the individual level data, shown in Table 1, where the only statistically significant difference between people living in linear and non-linear localities is the household asset index, with those in non-linear localities being slightly poorer. Overall, the data in Table 1 and A1 shows 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. In our empirical analysis we control for all the variables that show significant differences between linear and non-linear localities.

5.2. MAIN RESULTS

Table 2 shows the effects of the different treatments on registration and turnout. We gradually include controls at the individual level, locality level, district fixed effects,

and the lag of the dependent variable. It is reassuring to see that including additional controls do not significantly affect the magnitude of the estimated coefficients, but rather, improves precision. Regardless of the method used to deliver the information campaign, providing information to women about the procedures to register and the importance of voting does not affect the probability that women registered to vote. Particularly, once we include the lagged value of the dependent variable, all of our point estimates in Panel A are very close to zero, with very tight standard errors. The lack of an effect can potentially be explained by the fact that registration is quite a costly action, since it requires voters to personally go to the registrar's office, show proof of having been born in Paraguay, their current residence, and age. Unlike in our case, studies in developed countries have found significant effects of similar campaigns. For example, Bracconiere et al (2017) randomize two different door-to-door registration campaigns in France. The first one informed citizens about how to register, while the second one provided the opportunity to register at home. The effects of these campaigns on registration were 2.4 and 4.7 percentage points, respectively.

Panel B of Table 3 shows the results of our turnout regressions. Raising awareness about the importance of voting and informing women about the steps that have to be followed to vote only affected the probability of voting if the message was delivered in a personalized way. The turnout rate in the control group was 74.8 percent, and women in localities that received the public rally treatment were not more likely to participate in the elections. Having directly received the door-to-door treatment significantly increased the probability of participating in the elections by 4.2 percentage points. Finally, the effect of living in a door-to-door village, but not having been directly contacted is small (1.5 percentage points) and not distinguishable from zero.

The size of the effect of the door-to-door canvassing campaign is comparable to the effects of similar interventions in very different settings. For example, Green and Gerber (1999) found that non-partisan personal canvassing increased turnout in about 6 percentage points in New Haven in the 1998 election. Gerber et al (2003) replicated the New Haven experiment in 6 other sites and found an average treatment effect on the treated of 7.1 percentage points. Further, Green and Gerber (2004) consistently find that personalized

methods and messages work better.¹⁹ Our findings are consistent with the evidence in that more personalized and directed treatments (door-to-door visits) were much more effective than impersonal messages delivered to a broad public (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. Between the three different sources of attrition, we end up considering in our final analysis sample about 65 percent of sampled observations (see Figure 3 for details). If the fact that we were (i) not able to reach a locality (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 full information on the outcomes (25.4 percent) were correlated with turnout, the main estimates shown in the tables would be biased.

To alleviate these concerns, in Table A.2 we analyze whether, after controlling for locality level covariates that we include in our main regression and district fixed effects, the fact that we have no observations in 14 localities in the final analysis is correlated with the treatment status of the locality. The results show that, after controlling for the relevant covariates and district fixed effects, the treatment status of the village is uncorrelated with attrition. Importantly, the linearity or non-linearity of the village is also insignificant in these regressions.

Similarly, in Table A.3, we use our individual level data to investigate if the treatment status is correlated with attrition. In this case, we consider all sampled observations in localities where we were able to reach for the second-round intervention, since only for those we have information on the relevant controls. The dependent variable equals one if she is considered in the main analysis and equals to zero if (i) we were not able to match the survey data with the TSJE records, or (ii) we were able to match the

¹⁹ 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.

survey, but the TSJE records had missing information on the outcomes. Again, we regress this variable on locality level covariates and district fixed effects. The results show that, after controlling for the appropriate covariates and fixed effects that are included in the main analysis, being in the massive group is uncorrelated with attrition, however, people in the D2DC group are less likely to be in our sample, while those in the D2DT are more likely to appear. This is not surprising, since tracking women in this group required being able to track them between the two rounds, unlike those in the other two groups.

To show that the selective attrition of women in D2D localities does not affect our estimates, in Table 3, we run a bounding exercise following Lee (2009), where we compute the upper and lower bounds of the point estimates under the best and worst case scenario assumptions for the behavior of women that we do not observe in the final sample. We run the bounding exercise in a conservative way, making unconditional comparisons between each of the three treatment groups and the control separately. Column (1) shows the unconditional treatment effects for each of the treatments, as compared to the control group. Columns 2 and 3 show the upper and lower bounds for these unconditional treatment effects. For the massive and D2DC, the upper and lower bounds are very close to zero and are statistically insignificant, while for the D2DT, the treatment effects go from 2.9 to 10.5 percentage points. The upper bound is statistically different from zero, while the lower bound has a p-value of 0.15.²⁰ This means that even assuming that all women missing in this treatment group did not vote, we would still observe a positive and significant treatment effect.

Overall, three pieces of evidence suggest that attrition does not seem to generate major biases in our estimation. First, Table 1 and Table A.1 show that the observations that we consider in our estimation sample are balanced across the treatment groups. Second, the regression analysis in Table A.2 and A.3 show that, after controlling for the relevant covariates, the treatment status at the village level, is uncorrelated with the probability of attrition, but this is not true at the individual level, where women in D2D villages are less

²⁰ Note that the mean of the dependent variable in this context is high, around 0.74, and therefore the assumption taken for the computation of the lower bound (that every one of the attriters abstained from the election) 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.

likely to be in the final sample. The Lee (2009) bounds in Table 3 demonstrate that this selective attrition does not affect our qualitative results.

6. URBANIZATION PATTERNS AND SPILLOVER EFFECTS

As described above, our experimental design generated exogenous variation on the intensity with which each D2D locality was treated, allowing us to test for the prevalence of spillover effects and their magnitude. For this estimation, we use the specification in equation (2) above. In localities assigned to have a door-to-door treatment, a third of them were assigned to receive the low intensity treatment, in the mid-intensity treatment arm, forty percent of household were assigned to the treatment, and in the high intensity treatment, half of households in the locality were assigned to treatment. However, due to the small (and not necessarily divisible) number of households, attrition between the two treatment rounds, and imperfect compliance with the assignment, the proportion of households treated did not necessarily correspond to the treatment assignment. Thus, to estimate the spillover effects, we instrument the percentage of households actually treated with three dummies for treatment assignment.

Columns (1) and (2) in Table 4 we show the results of our OLS and two stage least squares regression estimating the effect of the massive treatment and the spillover effects, or the effect of the increase in the proportion of treated household on turnout for the average women in the locality, regardless of their treatment status. As expected, the point estimates associated with the massive treatment remains small and insignificant in these regressions. In the OLS regression, we see that an increase of 10 percent in the proportion of treated women in the locality leads to an increase in turnout of 0.6 percentage points, but this effect is statistically insignificant. Once we account for potential biases induced by the imperfect compliance and potential differential attrition using the treatment status as an instrument for the proportion of treated households in the village, we see that a 10 percentage point increase in the proportion of treated households in the locality causes an increase in turnout of 10.3 percentage points.²¹

²¹ The first stage regression (Table A.4) shows that the three treatment assignment dummies significantly affect the proportion of treated households in a locality, and that in localities assigned to the 50 percent treatment, non-compliance was larger. The F-test of excluded instruments, shown at the bottom of the table, is 41.

We hypothesize that the spillover effects should be larger in localities where the geographic distribution of households is conducive to social interactions. Interestingly, the development of linear and non-linear settlements, while barely studied in economics (Nijkamp and Reggiani 1993) has been present in related literature 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 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,²² to Victoria in Hong Kong (Shelton, et al. 2011), and even Brooklyn in the United States, where at some point 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).²³ Researchers from different disciplines have long argued that the origins of linearities or non-linearities in formation of towns may not necessarily be linked to different observable characteristics. One notable exception in economics is the work by Libecap and Lueck (2011) that looks at the effects of land demarcation systems in the US on property right institutions. They first show evidence that centralized systems dominate localized ones by generating higher land values, fewer border and title disputes and more land transactions. More importantly for our study, they find both original demarcation patterns persist until today despite the superiority of centralized systems, suggesting high adjustment costs, even with the technological advances of today.

As mentioned above, in the Paraguayan case, the geographic layout of localities was influenced by the decision of a dictator several decades ago, but such demarcation patterns remained mostly unchanged today. Also, importantly, linear and non-linear localities do not look different in terms of our main outcome variables at baseline, and the

²² See: <http://www.visionofbritain.org.uk/place/3159>, last accessed on August 23, 2016.

²³ 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.

observable differences are not related to the wealth of the locality, level of urbanization, or population density.

We test whether the main effects estimated in the previous section are driven by people living in places more conducive to social interactions in columns (3) through (6) of Table 4, where we separately estimate equation (2) for linear and non-linear localities. In linear localities, our 2SLS estimates suggest that the spillover effects are small and statistically insignificant. In localities with a natural gathering point (non-linear), we see large and significant spillover effects. Women in D2D localities are 1.7 percentage points more likely to participate when the proportion of treated households increases by 10 percentage points, and the effect is statistically significantly different between linear and non-linear localities (difference of 0.207, t-stat of 5.04.).

Overall, we observe that the treatment effect of public rallies is independent of the urbanization patterns, which is consistent with the fact that these rallies took place in the usual gathering points. On the other hand, spillover effects are only present in localities where social interactions are less costly. It must be noted that, given that it is nearly impossible to randomize the geographical distribution of a locality, and absent of a good instrument, we don't claim that the effects from Table 4 are causal, but rather that *within* linear or non-linear communities, there are causal effects of the randomly assigned treatment.

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. 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 also talking about the treatment. 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 2015). 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. This is a plausible

assumption, given the randomization of treatment intensity and our instrumental variable approach.

Columns (1)-(3) in Table 5 estimate the reinforcement effects of our door-to-door campaign using our 2SLS strategy, i.e. the effect of the proportion of treated connections on each treated individual. We do this by comparing treated women in door-to-door localities treated with different intensities to women in the pure control group and on the massive rally treatment (we exclude control women in treated localities.) The average reinforcement effect is large (Column 1), but when we split the sample between linear and non-linear localities, we see that 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 causes 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 2.

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 massive 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 the effect diffuses among non-treated women. Increasing the proportion of treated households in the locality by 10 percentage points, leads un-contacted 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.

7. SUMMARY AND CONCLUSIONS

This paper set out to answer three questions: In a rural setting, is it more effective to mobilize women to register and vote using a door-to-door strategy or a public campaign?

Do social interactions among neighbors in the communities spread the campaign message beyond those that received the treatment? Do these interactions depend on urbanization patterns? To address these questions, we use individual level administrative voting data, survey information, and satellite images and evaluate the direct and indirect effects of two distinct GOTV campaigns in rural Paraguay. In our analysis, we explicitly take into account different urbanization patterns that facilitate or deter social interactions. In our experimental design, we randomly assigned localities (i.e. villages) to be exposed to massive rallies that disseminated information about registration and voting, or to receive the same information in personalized door-to-door campaigns (D2D). Further, the experiment was designed to estimate spillover effects, by randomly varying the intensity of the D2D treatment. Neither interventions led to increases in voter registration, but while massive campaigns had no effect on voting, face-to-face interactions significantly increased turnout among treated women. We find evidence of information spillovers that lead to higher turnout, and these effects are as important for treated women (reinforcement effect) as well as for untreated women (diffusion effects), but these are only present in localities that have particular urbanization patterns that favor social interactions.

Our findings on the direct impacts of personal canvassing and zero effects for impersonal, massive campaigning are in line with previous research showing that directly contacting people with personalized messages has a larger impact than impersonal messages. Further, we answer three additional questions: (i) is the total impact of these campaigns only limited to the people contacted, or does the message spill over to neighbors? (ii) is the effect on the contacted population due to the direct contact, or does it depend on the extent to which it is reinforced by the neighbors? and (iii) are there specific circumstances or village characteristics that facilitate the spread of the messages and their effectiveness? We contribute to the literature that analyzes the effects of GOTV campaigning by showing that the impacts of door-to-door contacts generate spillover effects, but they are only present in locations that facilitate social interactions. In the Paraguayan case, these localities are those that have a configuration with natural gathering spots (“non-linear” localities). Moreover, we find that the reinforcement effects of the treatment are as important as the direct impacts in these localities. Thus, on one hand, policies to increase women’s political participation as a way to enfranchise them and improve the provision of public goods that benefit them, need to include a better knowledge

of the way social interactions occur. On the other hand, more research is needed to understand the precise mechanisms through which these geographical layouts facilitate social interactions, and the specific channel through which it affects voting.

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Figure 1: Example of linear and non-linear localities



Note: Linear localities: Calle 6 Tacua Cora and Calle 8 Tacua Cora. Non-linear locality: Asentamiento 3 de mayo

Figure 2: Timeline of the Intervention

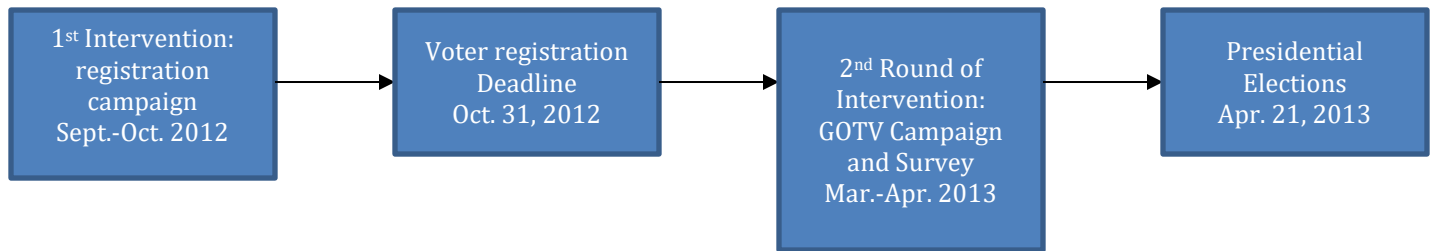


Figure 3: Sample and Sources of Attrition

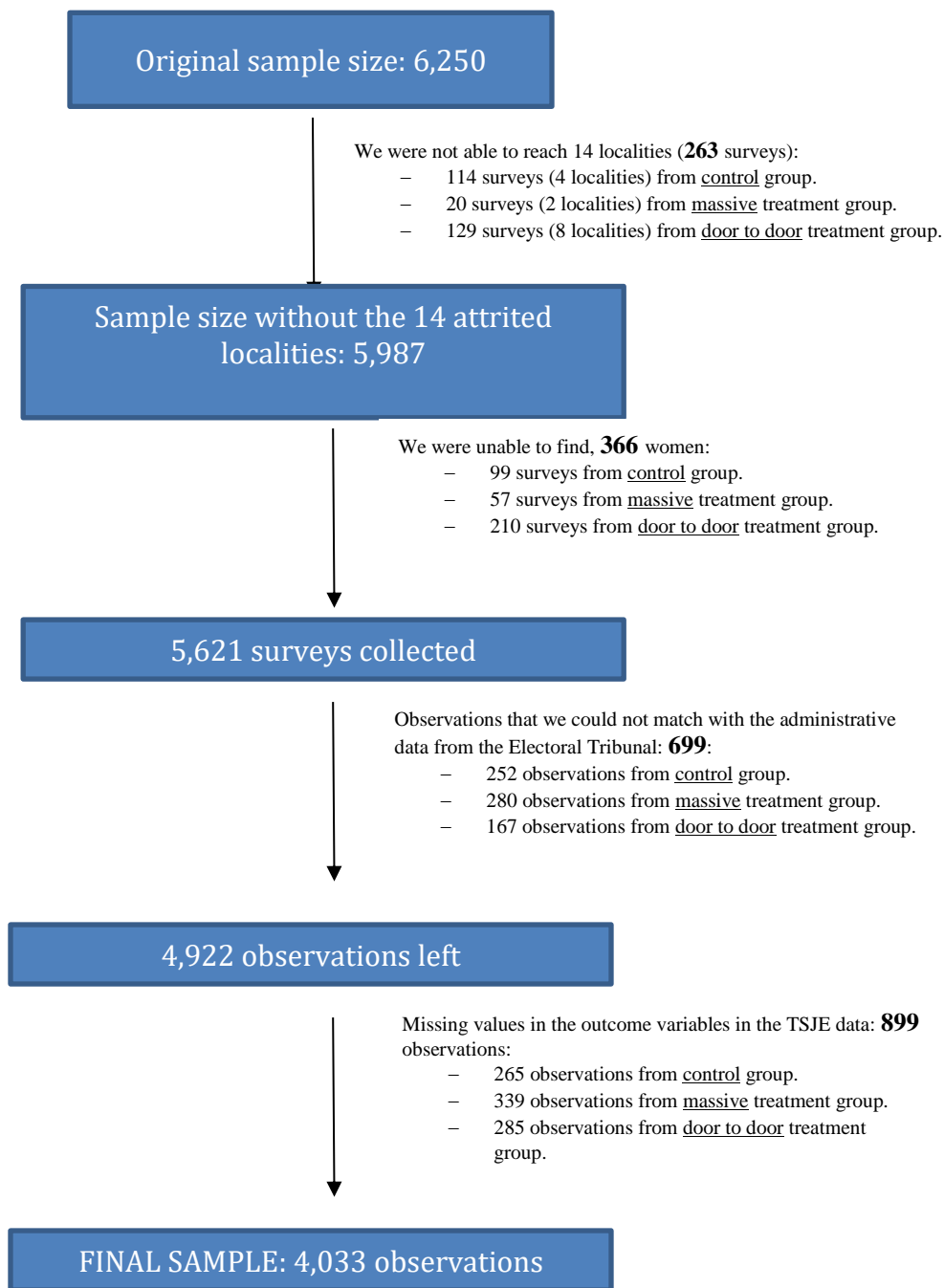


Table 1: Descriptive Statistics at the Individual Level

	N	Control	Massive	D2D	D2DT	D2DC	Non-Linear Localities	Linear Localities
Age	4,033	41.225 (0.491)	42.331 (0.450)	42.756 (0.592)	43.902 (0.683)	41.751 (0.799)	42.174 (0.373)	41.991 (0.505)
Married	4,033	0.512 (0.020)	0.514 (0.022)	0.470 (0.019)	0.486 (0.023)	0.457 (0.024)	0.489 (0.014)	0.519 (0.021)
At least 1 Child	4,033	0.924 (0.008)	0.922 (0.008)	0.914 (0.008)	0.920 (0.012)	0.909 (0.011)	0.923 (0.006)	0.915 (0.008)
Number of Children	4,033	3.966 (0.132)	4.297 (0.120)	4.284 (0.124)	4.334 (0.115)	4.240 (0.190)	4.199 (0.087)	4.163 (0.135)
Employed	4,033	0.138 (0.016)	0.146 (0.020)	0.141 (0.016)	0.120 (0.016)	0.160 (0.023)	0.142 (0.012)	0.141 (0.018)
Speaks only Guarani	4,033	0.528 (0.033)	0.599 (0.025)	0.619 (0.029)	0.644 (0.028)	0.596 (0.040)	0.602 (0.021)	0.544 (0.028)
Years of Education	4,033	6.223 (0.227)	5.967 (0.179)	6.010 (0.181)	5.796 (0.171)	6.198 (0.268)	5.895 (0.135)	6.381 (0.199)
Born in Locality	4,033	0.465 (0.028)	0.425 (0.031)	0.474 (0.033)	0.486 (0.038)	0.464 (0.036)	0.465 (0.023)	0.431 (0.029)
Household Asset Index	4,033	0.193 (0.187)	-0.046 (0.159)	-0.041 (0.153)	-0.169 (0.166)	0.071 (0.184)	-0.131 (0.107)	0.341 (0.186)
Treated Households (%)	4,033	0.000 (0.000)	0.000 (0.000)	0.301 (0.036)	0.319 (0.038)	0.284 (0.037)	0.084 (0.015)	0.117 (0.026)
Registered to Vote (2012)	4,033	0.836 (0.010)	0.841 (0.010)	0.870 (0.011)	0.878 (0.015)	0.862 (0.014)	0.849 (0.008)	0.848 (0.010)
Registered to Vote (2013)	4,033	0.878 (0.009)	0.889 (0.009)	0.899 (0.010)	0.913 (0.012)	0.887 (0.013)	0.888 (0.007)	0.890 (0.008)
Voted in municipal elections of 2010	4,033	0.559 (0.020)	0.577 (0.017)	0.573 (0.023)	0.614 (0.026)	0.536 (0.030)	0.576 (0.014)	0.560 (0.019)

* Note: Information collected in the survey conducted in March 2013. Reported means and standard deviations in parenthesis.

Table 2: Effect of GOTV Campaigns on Registration and Turnout

Panel A: Effect of the Treatment on Registration				
Dependent Variable: Registered to vote at the moment of the survey				
	(1)	(2)	(3)	(4)
Massive	0.011 (0.013)	0.007 (0.013)	0.008 (0.013)	0.006 (0.008)
D2DT	0.035** (0.015)	0.026* (0.014)	0.030** (0.015)	0.003 (0.008)
D2DC	0.009 (0.016)	0.004 (0.016)	0.009 (0.016)	-0.011 (0.008)
Mean of Dep. Var. for Control Group	0.889	0.889	0.889	0.889
# of obs.	4,033	4,033	4,033	4,033
R-squared	0.001	0.043	0.043	0.704
Panel B: Effect of the Treatment on Turnout				
Dependent Variable: Voted in presidential elections of 2013				
Massive	0.005 (0.021)	0.010 (0.020)	0.014 (0.020)	0.016 (0.016)
D2DT	0.046** (0.023)	0.036 (0.022)	0.043* (0.022)	0.042** (0.020)
D2DC	-0.009 (0.024)	-0.008 (0.022)	0.003 (0.022)	0.015 (0.019)
Mean of Dep. Var. for Control Group	0.748	0.748	0.748	0.748
# of obs.	4,033	4,033	4,033	4,033
R-squared	0.001	0.062	0.064	0.203
Individual covariates		Yes	Yes	Yes
Locality level covariates		Yes	Yes	Yes
Lagged dependent variable			Yes	Yes
District fixed effects				Yes
Note: The data comes from the survey conducted in March 2013, locality level census information and administrative information on turnout from the TSJE. OLS regressions reported. Individual covariates included in the regressions 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 level covariates included are: population, % of female population, percent population 0-14, percent of population 15-64, percent of population 65+, percent of illiterate population, percent of population 6-14 attending school, percent of population with access to electricity, percent of population with access to running water, percent of population with access to sewage, percent of population that has a cellphone, percent of population that has a landline, rural, percent of women employed, percent of men employed, number of occupied houses in the village, distance to voting center and percent of population with access to trash collection. Standard errors reported in parenthesis are clustered at the locality level. * significant at the 10% ** significant at the 5% *** significant at the 1%				

Table 3: Lee (2009) bounds for treatment-effects

	Baseline Specification	Lower Bound	Upper Bound
Massive	-0.003 (0.014)	-0.030 (0.019)	0.005 (0.015)
D2DT	0.045 (0.019)**	0.029 (0.020)	0.105 (0.027)***
D2DC	-0.019 (0.018)	-0.020 (0.019)	-0.017 (0.024)

Notes: Results in the baseline specification come from bivariate regressions of turnout on the specified treatment. The lower and upper bounds are computed using the procedure outlined in Lee (2009). Standard errors in parenthesis.

*** significant at the 1 percent, ** significant at the 5 percent, * significant at the 10 percent.

Table 4: Estimating Spillover Effects – OLS and 2SLS Estimates

	Dependent Variable: Voted in presidential elections of 2013					
	Full Sample		Linear		Non-Linear	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
Massive	0.012 (0.015)	0.019 (0.016)	-0.002 (0.023)	0.007 (0.025)	0.016 (0.021)	0.020 (0.022)
Treated Households (%)	0.060 (0.039)	0.103** (0.051)	-0.078 (0.050)	-0.033 (0.056)	0.144*** (0.035)	0.175** (0.069)
Individual covariates	Yes	Yes	Yes	Yes	Yes	Yes
Village level covariates	Yes	Yes	Yes	Yes	Yes	Yes
Lagged dependent variable	Yes	Yes	Yes	Yes	Yes	Yes
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Mean of Dep. Var. for Control Group	0.748	0.748	0.737	0.737	0.753	0.753
F-Test of excluded instruments		43.33		19.29		26.76
Observations	4,033	4,033	1,394	1,394	2,639	2,639
R-squared	0.203	0.203	0.188	0.188	0.226	0.226

Note: The data comes from the survey conducted in March 2013, locality level census information and administrative information on turnout from the TSJE. OLS and 2SLS regressions reported. As described in the text, in the 2SLS regressions, we instrument the percent of Treated households with the three treatment assignment dummies (30, 40 or 50 percent). The first stage regressions are reported in Table A.5. Individual covariates included in the regressions 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 level covariates included are: population, percent of female population, percent population 0-14, percent of population 15-64, percent of population 65+, percent of illiterate population, percent of population 6-14 attending school, percent of population with access to electricity, percent of population with access to running water, percent of population with access to sewage, percent of population that has a cellphone, percent of population that has a landline, rural, percent of women employed, percent of men employed, number of occupied houses in the village, distance to voting center and percent of population with access to trash collection. Standard errors reported in parenthesis are clustered at the locality level.

* significant at 10% ** significant at 5% *** significant at 1%

Table 5: Reinforcement and Diffusion effects – 2SLS Estimates

	Dependent Variable: Voted in presidential elections in 2013					
	Reinforcement Effects: Excluding D2DC			Diffusion Effects: Excluding D2DT		
	D2DC = 0	Linear Localities	Non-Linear Localities	D2DT = 0	Linear Localities	Non-Linear Localities
Massive	0.016 (0.016)	0.000 (0.025)	0.020 (0.022)	0.018 (0.016)	0.006 (0.025)	0.020 (0.022)
Treated Households (%)	0.124** (0.056)	-0.040 (0.069)	0.231*** (0.074)	0.079 (0.060)	-0.056 (0.062)	0.124 (0.085)
Individual covariates	Yes	Yes	Yes	Yes	Yes	Yes
Village level covariates	Yes	Yes	Yes	Yes	Yes	Yes
Lagged dependent variable	Yes	Yes	Yes	Yes	Yes	Yes
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Mean of Dep. Var. for Control Group	0.751	0.745	0.754	0.741	0.738	0.743
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
Massive	0.016	0.000	0.020	0.018	0.006	0.020

Note: The data comes from the survey conducted in March 2013, locality level census information and administrative information on turnout from the TSJE. 2SLS regressions reported. 2SLS regressions reported. As described in the text, 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.5. Individual covariates included in the regressions 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 level covariates included are: population, percent of female population, percent population 0-14, percent of population 15-64, percent of population 65+, percent of illiterate population, percent of population 6-14 attending school, percent of population with access to electricity, percent of population with access to running water, percent of population with access to sewage, percent of population that has a cellphone, percent of population that has a landline, rural, percent of women employed, percent of men employed, number of occupied houses in the village, distance to voting center and percent of population with access to trash collection. Standard errors reported in parenthesis are clustered at the locality level.

* significant at the 10% ** significant at the 5% *** significant at the 1%

Appendix A

Table A.1: Descriptive Statistics at the Locality Level

	N	Control	Massive	D2D	Non-linear Localities	Linear Localities
Censal population (logs)	286	6.381 (0.055)	6.510 (0.055)	6.397 (0.056)	6.495 (0.040)	6.327 (0.051)
Share of women (%)	286	47.197 (0.252)	47.270 (0.250)	47.219 (0.258)	47.218 (0.186)	47.247 (0.235)
Female labor force participation (%)	286	9.128 (0.864)	10.157 (0.856)	10.439 (0.883)	9.392 (0.637)	10.719 (0.806)
Population age 0-14 (%)	286	43.403 (0.618)	43.820 (0.612)	42.507 (0.631)	43.184 (0.457)	43.376 (0.579)
Population age 15-64 (%)	286	52.035 (0.480)	51.953 (0.475)	52.743 (0.490)	52.015 (0.354)	52.586 (0.448)
Population age 65+ (%)	286	4.557 (0.231)	4.223 (0.229)	4.751 (0.236)	4.800 (0.169)	4.034 (0.214)
Share of literate (%)	286	90.005 (0.788)	90.520 (0.780)	90.401 (0.805)	89.731 (0.578)	91.234 (0.731)
School attendance (ages 6-14) (%)	286	89.200 (0.812)	90.158 (0.803)	89.639 (0.829)	88.891 (0.594)	90.915 (0.752)
Share of women employed (%)	286	13.765 (1.264)	15.694 (1.251)	15.976 (1.291)	14.478 (0.933)	16.191 (1.180)
Share of men employed (%)	286	74.659 (1.165)	73.796 (1.153)	74.226 (1.190)	74.303 (0.859)	74.098 (1.087)
Have eletricity (%)	286	78.723 (1.888)	79.570 (1.869)	76.068 (1.929)	75.614 (1.375)	82.232 (1.739)
Have running water (%)	286	22.729 (3.295)	27.169 (3.261)	27.239 (3.366)	21.003 (2.393)	33.219 (3.027)
Trash Collection (%)	286	1.836 (0.650)	1.853 (0.644)	2.362 (0.664)	1.374 (0.476)	3.031 (0.602)
Have landline (%)	286	2.602 (0.540)	1.736 (0.534)	1.962 (0.551)	1.579 (0.396)	2.932 (0.500)
Have cellphone (%)	286	11.267 (1.126)	10.818 (1.114)	11.161 (1.150)	10.773 (0.830)	11.568 (1.050)
Number of occupants per HH	286	5.247 (0.063)	5.261 (0.062)	5.135 (0.064)	5.159 (0.046)	5.307 (0.058)
Rural (%)	286	0.885 (0.034)	0.867 (0.034)	0.859 (0.035)	0.898 (0.025)	0.827 (0.032)
Distance to voting Center (log of km)	286	1.277 (0.208)	1.381 (0.206)	0.892 (0.213)	1.205 (0.154)	1.162 (0.195)
Identity document (%)	286	55.139 (1.311)	53.906 (1.297)	53.376 (1.339)	53.670 (0.967)	54.916 (1.223)
Birth is registered (%)	286	93.472 (0.989)	92.683 (0.979)	91.641 (1.011)	92.370 (0.731)	93.000 (0.925)
Foreign (%)	286	12.031 (3.770)	7.337 (3.732)	7.554 (3.852)	11.216 (2.776)	5.409 (3.512)
Number of occupied households (log)	286	4.728 (0.071)	4.807 (0.070)	4.723 (0.072)	4.834 (0.052)	4.624 (0.065)
Have drainage (%)	286	0.000 (0.033)	0.061 (0.033)	0.003 (0.034)	0.031 (0.025)	0.007 (0.031)
Population density	286	2.317 (0.606)	2.476 (0.600)	2.325 (0.619)	1.541 (0.440)	3.707 (0.556)
Population density (Census)	286	2.317 (0.692)	2.476 (0.685)	3.409 (0.707)	1.686 (0.502)	4.382 (0.635)
Village extension (ha)	286	7.127 (0.152)	7.045 (0.151)	6.908 (0.155)	7.477 (0.104)	6.310 (0.131)
Treated households (%)	286	0.000 (0.017)	0.000 (0.017)	0.333 (0.017)	0.099 (0.017)	0.121 (0.022)
Attendance at mass rallies (%)	286	0.000 (0.006)	0.072 (0.005)	0.000 (0.006)	0.019 (0.005)	0.033 (0.006)

*Locality level data comes from the latest available census (2002). Data on percent of treated households and percent attendance at mass rallies was collected during the fieldwork in March 2013. The universe of localities are those that we were able to track in the second round of the survey and for which we have at least one respondent with full information for the analysis.

Table A.2: Correlates of Attrition at the Locality Level

Dependent variable: Dummy whether the observation is in the sample				
	(1)	(2)	(3)	(4)
Massive	0.020 (0.030)	0.002 (0.030)	0.002 (0.030)	0.002 (0.030)
D2D	-0.040 (0.030)	-0.058 (0.032)	-0.060 (0.036)	-0.060 (0.036)
Censal Population (logs)		0.024 (0.025)	0.024 (0.025)	0.022 (0.026)
Share of Women (%)		0.298 (0.621)	0.300 (0.622)	0.298 (0.623)
Population age 0-14 (%)		0.182 (0.240)	0.185 (0.240)	0.185 (0.241)
Population age 15-64 (%)		0.184 (0.240)	0.187 (0.240)	0.188 (0.241)
Population age 65+ (%)		0.193 (0.240)	0.197 (0.240)	0.197 (0.241)
Share of literate (%)		0.003 (0.002)	0.003 (0.002)	0.003 (0.002)
School attendance (ages 6-14) (%)		-0.002 (0.002)	-0.002 (0.002)	-0.002 (0.002)
Share of women employed (%)		0.002 (0.001)	0.002 (0.001)	0.002 (0.001)
Share of men employed (%)		-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)
Have electricity (%)		-0.002* (0.001)	-0.002* (0.001)	-0.002* (0.001)
Have running water (%)		0.000 (0.000)	0.000 (0.001)	0.000 (0.001)
Have drainage (%)		-0.013 (0.039)	-0.012 (0.039)	-0.012 (0.039)
Trash Collection (%)		0.005 (0.003)	0.005 (0.003)	0.005 (0.003)
Have landline (%)		-0.011*** (0.004)	-0.011*** (0.004)	-0.011*** (0.004)
Have cellphone (%)		-0.001 (0.001)	-0.000 (0.002)	-0.000 (0.002)
Number of occupants per HH		0.071** (0.030)	0.074** (0.030)	0.075** (0.030)
Rural (%)		-0.088 (0.057)	-0.090 (0.057)	-0.089 (0.058)
Non-Linear				0.005 (0.028)
Constant	0.960*** (0.021)	-17.831 (23.968)	-18.168 (24.006)	-18.226 (24.050)
District fixed effects	No	No	Yes	Yes
Observations	300	300	300	300
R-squared	0.014	0.093	0.094	0.094

Note: The data at the locality level comes from the 2002 national census, while the variable “Non-Linear” corresponds to our own coding as described in the text. The dependent variable equals one if we managed to collect surveys in the second round of intervention (286 localities, as shown in Table A.1). All regressions shown are OLS.

* significant at the 10% ** significant at the 5% *** significant at the 1%

Table A.3: Correlates of Attrition at the Individual Level

Dependent variable: Observation is in the sample					
Massive	-0.013 (0.012)	-0.016 (0.012)	-0.016 (0.012)	-0.015 (0.011)	-0.013 (0.012)
D2DT	0.032** (0.014)	0.045*** (0.016)	0.043*** (0.016)	0.045*** (0.016)	0.049*** (0.016)
D2DC	-0.045*** (0.015)	-0.044*** (0.014)	-0.044*** (0.014)	-0.042*** (0.015)	-0.038*** (0.014)
Age			0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Married			0.013 (0.011)	0.013 (0.011)	0.013 (0.011)
At least 1 Child			0.057*** (0.018)	0.057*** (0.018)	0.056*** (0.018)
Number of Children			0.003* (0.002)	0.003* (0.002)	0.004* (0.002)
Employed			0.004 (0.015)	0.004 (0.015)	0.003 (0.015)
Speaks only Guarani			0.006 (0.012)	0.006 (0.012)	0.006 (0.012)
Years of Education			0.002* (0.001)	0.002* (0.001)	0.002* (0.001)
Born in Locality			-0.010 (0.010)	-0.009 (0.010)	-0.010 (0.010)
Household Asset Index			-0.000 (0.003)	-0.000 (0.003)	-0.000 (0.003)
Non-Linear					0.028** (0.011)
Constant	0.854*** (0.008)	-8.467 (9.164)	-8.701 (9.097)	-7.823 (9.102)	-5.783 (9.087)
Locality controls	No	Yes	Yes	Yes	Yes
Department fixed effects	No	No	No	Yes	Yes
Observations	5,598	5,598	5,598	5,598	5,598
R-squared	0.004	0.035	0.041	0.041	0.042

Note: The data at the locality level comes from the 2002 national census, while the variable “Non-Linear” corresponds to our own coding as described in the text. The universe on individuals considered in the regressions corresponds to all sampled individuals in villages that we were able to reach (286). The dependent variable equals one if (i) we were not able to find the sampled person for the survey in the second round of the intervention, (ii) we were able to find her, but the ID number did not match the records of the TSJE, or (iii) the covariates considered in the analysis are missing. All regressions shown are OLS.

* significant at the 10% ** significant at the 5% *** significant at the 1%

Table A.4: Estimating Spillover Effects - First Stage

	% of Treated Households		
	Full Sample	Linear	Non-Linear
30% treated	0.308*** (0.040)	0.346*** (0.065)	0.318*** (0.049)
40% treated	0.427*** (0.067)	0.511*** (0.096)	0.376*** (0.082)
50% treated	0.315*** (0.045)	0.325*** (0.054)	0.331*** (0.059)
Individual covariates	Yes	Yes	Yes
Village level covariates	Yes	Yes	Yes
Lagged dependent variable	Yes	Yes	Yes
District fixed effects	Yes	Yes	Yes
# of obs.	4,033	1,394	2,639
R-squared	0.575	0.642	0.582

Note: The data comes from the survey conducted in March 2013, locality level census information and administrative information on turnout from the TSJE. All regressions correspond to the first stage estimation of the 2SLS regressions reported in Table 4. Individual covariates included in the regressions 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 level covariates included are: population, percent of female population, percent population 0-14, percent of population 15-64, percent of population 65+, percent of illiterate population, percent of population 6-14 attending school, percent of population with access to electricity, percent of population with access to running water, percent of population with access to sewage, percent of population that has a cellphone, percent of population that has a landline, rural, percent of women employed, percent of men employed, number of occupied houses in the village, distance to voting center and percent of population with access to trash collection. Standard errors reported in parenthesis are clustered at the locality level.

* significant at the 10% ** significant at the 5% *** significant at the 1%

Table A.5: Reinforcement and Diffusion effects – First Stage

	Reinforcement Effects (D2DC = 0)			Diffusion Effects (D2DT = 0)		
	Full Sample	Lineal	Not Lineal	Full Sample	Lineal	Not Lineal
30% Treat	0.329*** (0.041)	0.384*** (0.080)	0.304*** (0.033)	0.293*** (0.043)	0.304*** (0.059)	0.316*** (0.063)
40% Treat	0.444*** (0.067)	0.508*** (0.089)	0.402*** (0.089)	0.410*** (0.073)	0.529*** (0.129)	0.351*** (0.075)
50% Treat	0.318*** (0.046)	0.344*** (0.047)	0.316*** (0.065)	0.306*** (0.049)	0.296*** (0.056)	0.336*** (0.062)
Individual covariates	Yes	Yes	Yes	Yes	Yes	Yes
Village level covariates	Yes	Yes	Yes	Yes	Yes	Yes
Lagged dependent variable	Yes	Yes	Yes	Yes	Yes	Yes
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
# of obs.	3,350	1,122	2,228	3,434	1,154	2,280
R-squared	0.619	0.686	0.603	0.570	0.634	0.588

Note: The data comes from the survey conducted in March 2013, locality level census information and administrative information on turnout from the TSJE. 2SLS regressions reported. All regressions correspond to the first stage estimation of the 2SLS regressions reported in Table 5. Individual covariates included in the regressions 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 level covariates included are: population, percent of female population, percent population 0-14, % of population 15-64, percent of population 65+, percent of illiterate population, percent of population 6-14 attending school, percent of population with access to electricity, percent of population with access to running water, percent of population with access to sewage, percent of population that has a cellphone, percent of population that has a landline, rural, percent of women employed, percent of men employed, number of occupied houses in the village, distance to voting center and percent of population with access to trash collection. Standard errors reported in parenthesis are clustered at the locality level.

* significant at the 10% ** significant at the 5% *** significant at the 1%

APPENDIX B – CAMPAIGN MATERIALS

Figure B.1: Posters – Registration Campaign



Campana por la participaci3n electoral de las mujeres paraguayas

**Inscrib3te,
decid3te y vot3**

Particip3 en el futuro de Paraguay

Los 3 pasos para la inscripci3n

REGISTRO ELECTORAL

1

Para poder votar inscrib3te en el Registro Electoral. Acude con tu C3dula de Identidad a la Oficina del Registro Electoral m3s cercana a tu hogar.

REGISTRO ELECTORAL

2

Llenas el formulario con tus datos personales.

REGISTRO ELECTORAL

3

Firma el formulario de inscripci3n, no olvides revisar tu boleto de registro. An3da en este momento para votar en las elecciones!

Los 3 pasos para la votaci3n

1

Cuando asistas a votar, presenta en la mesa tu C3dula de Identidad.

2

En el casero encara marca al candidato de tu preferencia. Tu voto es secreto y la elecci3n es personal. Luego, an3da tus boletines con los timbres para afuera y regresa a la mesa.

3

Entrega tus boletines al presidente de la mesa y cuando termines de firmar los depositalos en la urna. Marca con tinta indeleble el d3ito 3ndice de tu mano derecha. ¡Tu voto es importante y decisivo!

INSCRIPCIONES HASTA EL 31 DE OCTUBRE DE 2012

**Inscrib3te,
decid3te y vot3**

Campana por la participaci3n electoral de las mujeres paraguayas

**Inscrib3te,
decid3te y vot3**

Particip3 en el futuro de Paraguay

Los 3 pasos para la inscripci3n

REGISTRO ELECTORAL

1

Para poder votar inscrib3te en el Registro Electoral. Acude con tu C3dula de Identidad a la Oficina del Registro Electoral m3s cercana a tu hogar.

REGISTRO ELECTORAL

2

Llenas el formulario con tus datos personales.

REGISTRO ELECTORAL

3

Firma el formulario de inscripci3n, no olvides revisar tu boleto de registro. An3da en este momento para votar en las elecciones!

Los 3 pasos para la votaci3n

1

Cuando asistas a votar, presenta en la mesa tu C3dula de Identidad.

2

En el casero encara marca al candidato de tu preferencia. Tu voto es secreto y la elecci3n es personal. Luego, an3da tus boletines con los timbres para afuera y regresa a la mesa.

3

Entrega tus boletines al presidente de la mesa y cuando termines de firmar los depositalos en la urna. Marca con tinta indeleble el d3ito 3ndice de tu mano derecha. ¡Tu voto es importante y decisivo!

INSCRIPCIONES HASTA EL 31 DE OCTUBRE DE 2012

CIRD
CENTRO INTERAMERICANO DE INVESTIGACIONES Y DESARROLLO

Figure B.2: Fliers – Registration Campaign

TAPA

MI VOTO, MI ELECCIÓN



"Campana por la participación electoral de las mujeres paraguayas"

**INSCRIPCIONES
HASTA EL
31
DE OCTUBRE
DE 2012**

Inscribíte, decidíte y votá

Participá en el futuro
de Paraguay

INTE

Los 3 pasos para la inscripción

1

Estar afiliada a un partido político no es suficiente, para poder votar y elegir al candidato de tu preferencia, tenes que estar inscrita en el Registro Electoral. Acudí a la Oficina Distrital del Registro Electoral más cercana a tu hogar y presentá tu Cédula de Identidad.

2

Para poder inscribirte deberán brindar al funcionario electoral tus datos personales: Número de cédula, Sexo, Estado Civil, Fecha de Nacimiento, Nombre, Apellido, Profesión, Nacionalidad, Domicilio.

3

Cuando firmes el formulario de la inscripción, el funcionario electoral te entregará un comprobante de registro, necesario para futuros reclamos o modificaciones. ¡Ahora ya estás habilitada para votar en las elecciones!

CONTRATAPA

MI VOTO, MI ELECCIÓN



Inscribíte, decidíte y votá

Inscribite en:

CIIRD
 Centro de Información
y Recursos para el Océano

ROR

Los 3 pasos para la votación

1

Cuando asistas a votar, entregá al presidente de la mesa tu Cédula de Identidad, la misma te será devuelta al terminar el procedimiento. En la mesa se comprobará que no tengas los dedos de la mano entintados o cubiertos de alguna sustancia que impida que la tinta indeleble se fije. No se admiten fotocopias contrasentidas, ni pasaportes. (Art. 206, C.E.). Si cuentan con una cédula vencida, igualmente quedarán acreditada para votar. (Ley 1570/2009). Los dos vocales firmarán al dorso de los boletines de voto.

2

Pasa a la casilla de votación (o cuarto oscuro) y marca cada boletín en el recuadro del candidato de tu preferencia. Tienes derecho a mantener en secreto tu voto, tu elección es personal y, por tanto, puedes elegir al candidato de otra preferencia. El voto también es directo, libre e intransferible. Luego, cubrí los boletines uno a uno, de tal forma que las firmas queden para afuera y regresá a la mesa.

3

Entrega los boletines de votos debidos al Presidente de Mesa para que ellos los firme al dorso por orden (por cargos a ser elegidos). Cuando lo los entregues, depositalos de a uno en la urna correspondiente. Al terminar de depositar todos sus boletines, marca con tinta indeleble el dedo índice de tu mano derecha. Como último paso, los Miembros de Mesa anotarán en el padrón la palabra "VOTO" en la casilla correspondiente a tu nombre. (arts.21y 21d, C.E.) Significa que tu voto es importante y decidido. Ahora sólo tenes que esperar que se cuenten todos los votos y se anuncie oficialmente a quién eligió la mayoría.

Figure B.3: Posters – Voting Campaign

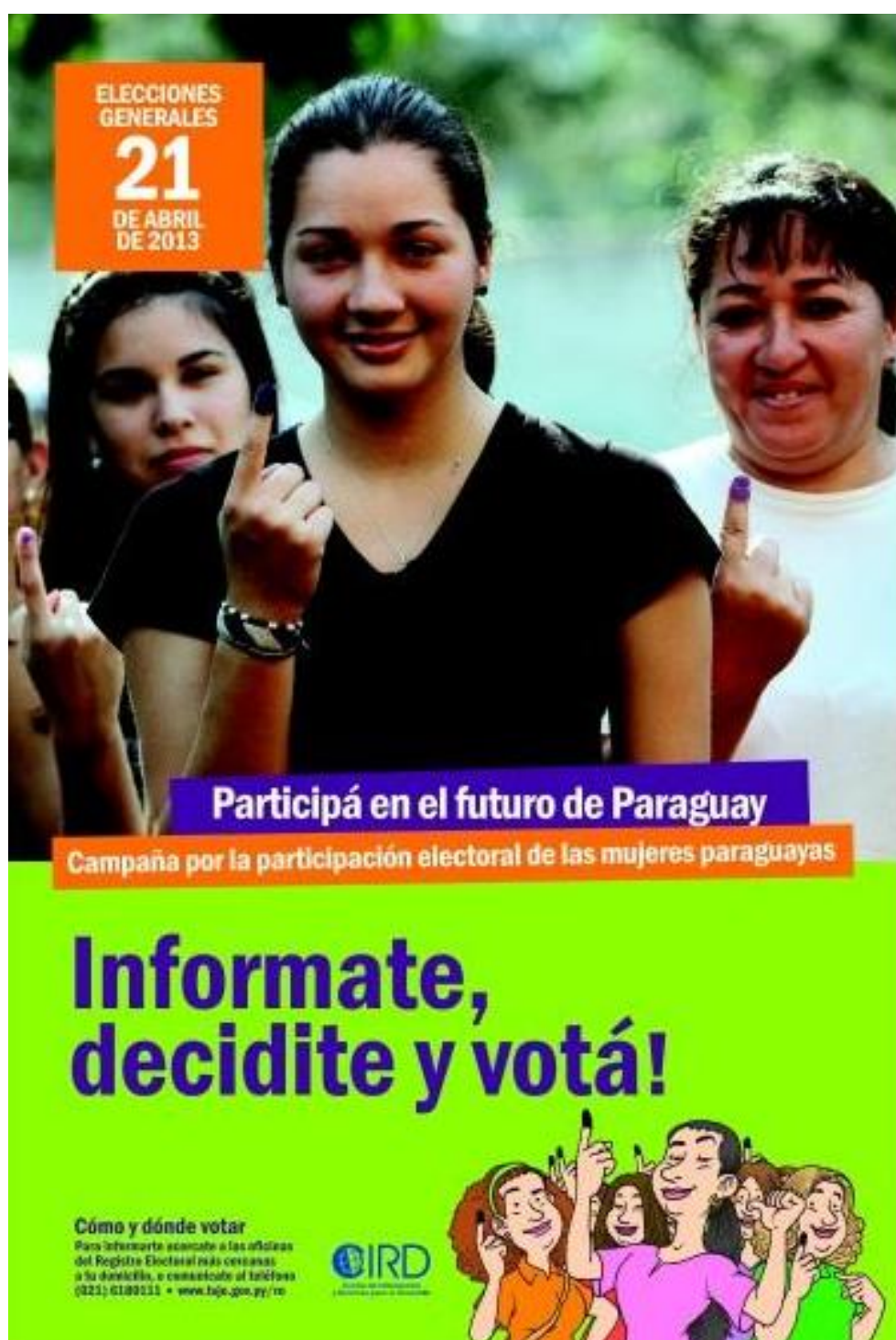


Figure B.4: Fliers – Voting Campaign

Campana por la participación electoral de las mujeres paraguayas

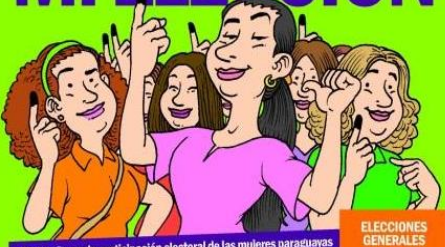
Informate, decidite y votá!

Participá en el futuro de Paraguay

Para más información sobre cómo y dónde votar, acercate a las oficinas del Registro Electoral más cercanas a tu domicilio, o comunicate al teléfono (021) 6180111 • www.tsje.gov.py/re

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Campana por la participación electoral de las mujeres paraguayas

Informate, decidite y votá!

Participá en el futuro de Paraguay

ELECCIONES GENERALES 21 DE ABRIL DE 2013

MI VOTO, MI ELECCIÓN

El voto produce cambios en nuestras vidas

Tu voto es de gran importancia. A través del voto, elegís a los representantes quienes toman decisiones respecto a los bienes y servicios que recibe tu comunidad en las áreas de educación, salud, caminos, seguridad, entre otros.

Tu voto marcará la diferencia

Tu voto **SÍ** marca la diferencia. El voto de cada paraguayo es de igual importancia y tiene el mismo peso, independientemente de si eres rico, pobre, hombre o mujer.

Tu voto es muy poderoso. Las mujeres somos la mitad de la población en Paraguay. Si participamos más, podemos transformar el ámbito político de manera importante, eligiendo a personas que representen nuestros intereses como mujeres, por ejemplo, en los temas relacionados con la discriminación, la salud materna y la igualdad entre hombres y mujeres.


Es importante que todos exijamos el funcionamiento correcto del gobierno y esto empieza con el voto. Dejar de votar significa renunciar a ese derecho y depositarlo en manos de otros.

Debemos alzar nuestra voz y realizar un voto responsable, pensando en la construcción de un mejor futuro para nosotras y nuestros hijos e hijas.


Pasos para la votación

Acordate que tu voto es secreto, es tu derecho. Tu decisión es personal y por tanto podés elegir al candidato o candidata de tu preferencia.

ELECCIONES GENERALES 21 DE ABRIL DE 2013

- 

Entregá al Presidente de Mesa tu **Cédula de Identidad** que te devolverán cuando termines de votar. Podés votar con tu cédula vencida, pero no se admiten fotocopias, contraseñas ni otros documentos. Te entregarán boletines de voto, firmados al dorso por los dos vocales de la mesa.
- 

Pasá al **cuarto oscuro** y marcá en cada boletín una cruz en el recuadro del candidato o candidata de tu preferencia. Luego, doblá los boletines uno a uno, de tal forma que las firmas queden para afuera y regresá a la mesa de votación.
- 

Entregá los boletines de votos doblados al Presidente de Mesa para que los firme. Cuando te los devuelva, depositalos uno a uno en la urna. Luego marcá con tinta indeleble el dedo índice de tu mano derecha. Los Miembros de Mesa anotarán en el padrón electoral la palabra "VOTO" en la casilla correspondiente a tu nombre. Tu voto es importante y decisivo!

Figure B.5: Stickers – Voting Campaign



Figure B.6: Pictures: Massive Voting Campaigns



Yhaí – Raúl Arsenio Oviedo – Caaguazú



Primera Línea Capiati – Raúl Arsenio Oviedo – Caaguazú



Santa Librada– Coronel Oviedo - Caaguazú



Santa Librada– Coronel Oviedo - Caaguazú



Yukyty– Coronel Oviedo - Caaguazú



Aguapety– Coronel Oviedo - Caaguazú



Aguapety– Coronel Oviedo - Caaguazú



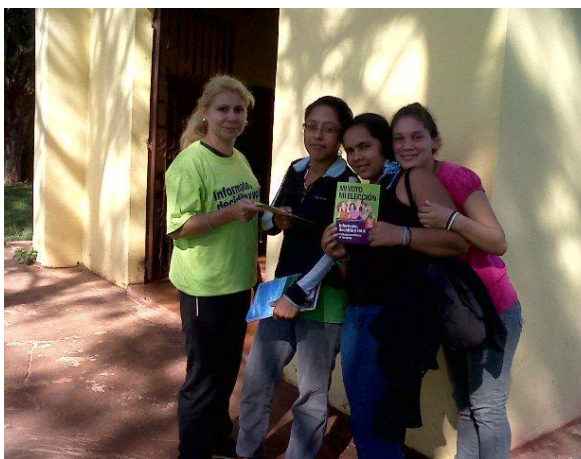
Costa San Antonio – Coronel Oviedo - Caaguazú



Costa San Antonio – Coronel Oviedo - Caaguazú



Costa San Antonio – Coronel Oviedo - Caaguazú



Costa San Antonio – Coronel Oviedo - Caaguazú



Mbocayaí – Vaquería - Caaguazú

Appendix C: Coding linear and Non-Linear Localities

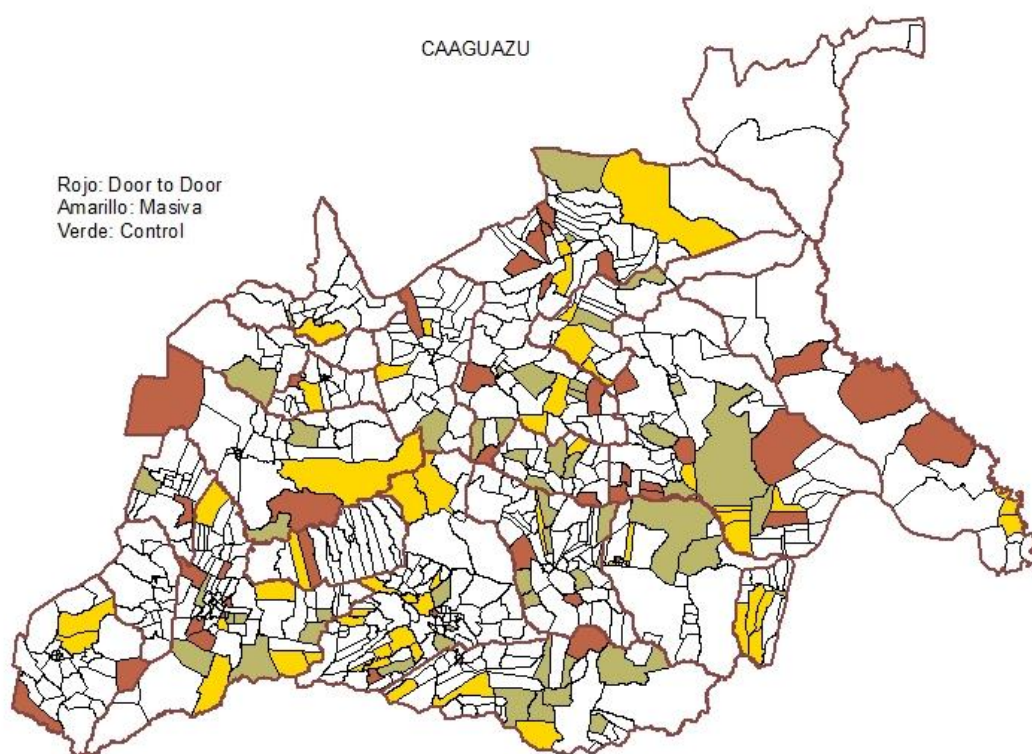
Map 1 shows the department of Caaguazu, highlighting the localities sampled for the study, and their treatment status. **Map 2** shows examples of localities coded as linear and non-linear. All images were taken from GoogleEarth.

To get a closer look at the distribution of households in the locality, we proceeded to mark the reference area in GoogleEarth, and look at the satellite pictures. In the maps shown, we we zoom in and analyze these localities.

- **Map 2** shows the difference in the shape of linear and non linear localities. Clearly, in linear localities, all households are placed along the road, and there are no blocks or natural gathering spaces.

Map 3 and 4 zooms into one of these linear localities, while maps 5-7 show examples of non-linear localities.

Map 1: CAAGUAZU



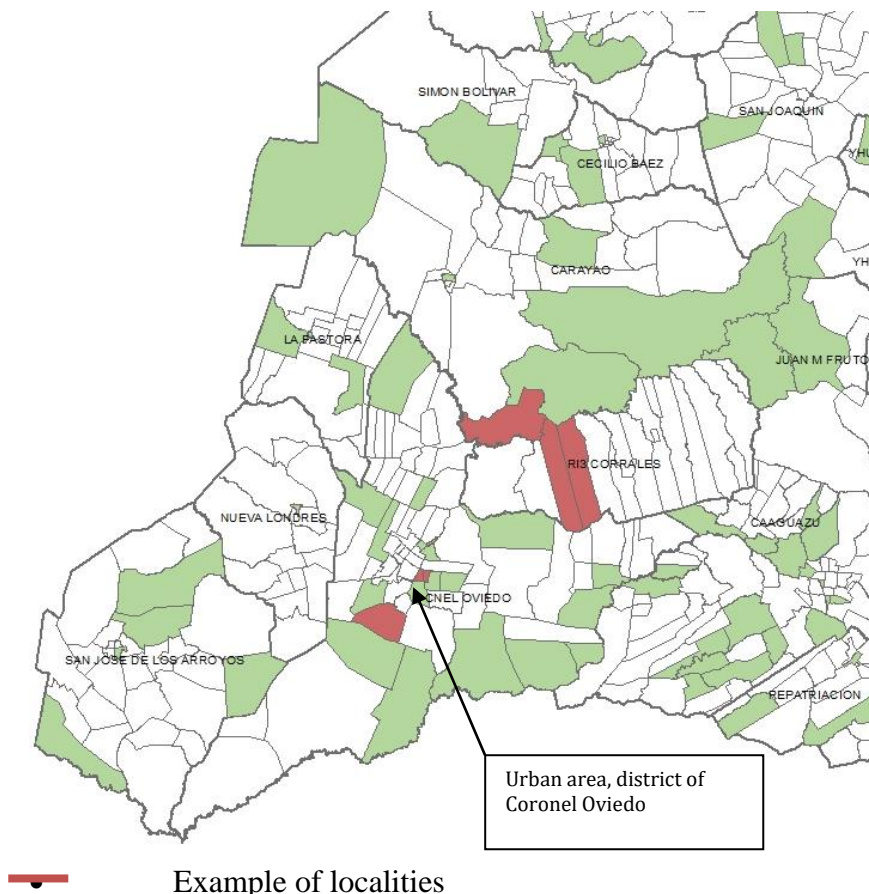
Linear localities:

- Calle 6 Tacua Cora – distrito RI3 Corrales
- Calle 8 Tacua Cora – distrito RI3 Corrales

Non linear localities:

- Asentamiento 3 de mayo – distrito Carayao
- Tuyu Pucu – Distrito Coronel Oviedo
- Calle Marista, Villa del maestro – Distrito Coronel Oviedo

Map 2: Example of Linear and Non-Linear Localities



Map 3: Examples of Linear and Non-Linear Localities



Note: Linear localities: Calle 6 Tacua Cora y Calle 8 Tacua Cora. Non linear locality: Asentamiento 3 de mayo

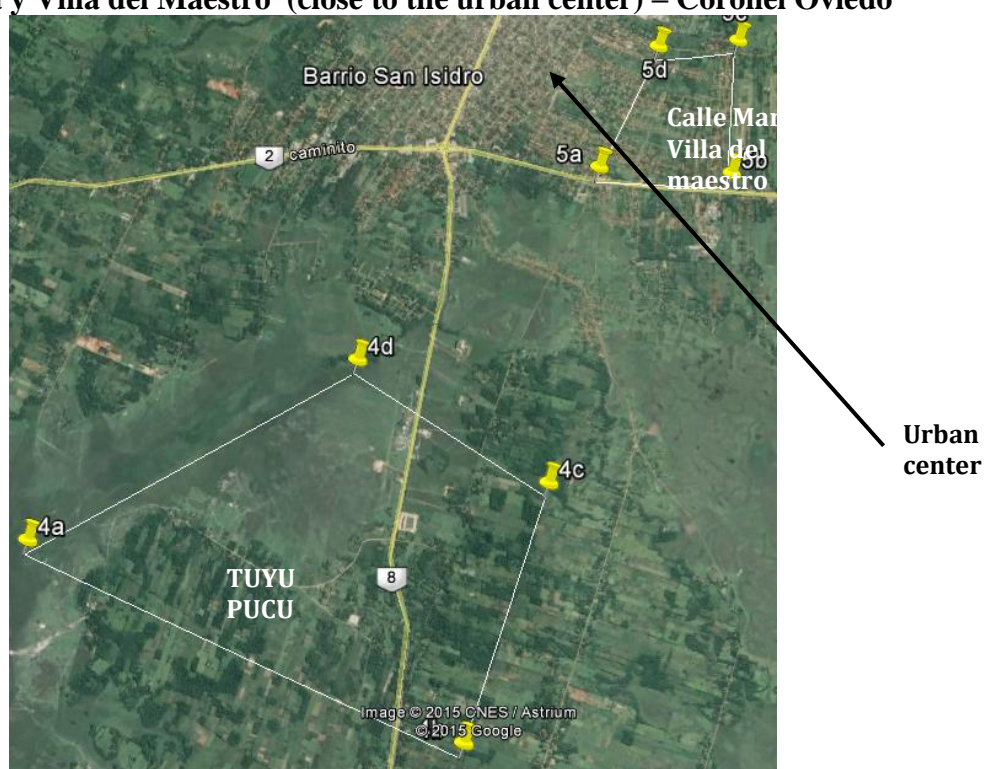
Map 4: Distribution of households in the Linear localities of Calle 6 Tacua Cora y Calle 8 Tacua Cora



MAP 5. Distribution of households in the non-linear locality of Asentamiento 3 de mayo- Carayao



Map 6: Non linear locality of Tuyu Pucu (far from the urban center), Calle Marista y Villa del Maestro (close to the urban center) – Coronel Oviedo



Note: Non linear localities in Coronel Oviedo. Close and far away from the urban center.

Map 7: Distribution of households in the non linear localities of Calle Marista and Villa del Maestro – Coronel Oviedo



Notes: area of greater urban agglomeration in the sample.