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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 rallies have no effect either on registration or on voter turnout in the 2013 presidential elections. However, households that received door-to-door (D2D) treatment are 4.6 percentage points more likely to vote. Experimental variation on the intensity of the treatment at the locality level allows us to estimate spillover effects, which are present in localities that are geographically more concentrated, and thus may favor social interactions. Reinforcement effects to the already treated population are twice as large as 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

While the role of social interactions as a vehicle to boost the impact of information campaigns is not a new one, the evidence on whether information spreads through social networks and is able to generate behavioral changes is rather mixed (Sinclair et al 2012, Fafchamps et al. 2015, Gine and Mansuri 2017.) Understanding how social interactions help spread information and generate behavioral change is important as it provide insights on the formation and relevance of social networks in the design of public policies.

In this paper we present findings of a field experiment on the effects of two get-out-the-vote (GOTV) campaigns targeted to women in rural Paraguay. Using individual level administrative voting data, survey information, and satellite images, we explicitly explore the role of social interactions and urbanization patterns in mediating the effects of these campaigns on registration and turnout. Prior to the 2013 presidential elections, we randomly assigned rural localities and provided information related to registration and the importance of voting using either public rallies or personalized door-to-door campaign (D2D). The experiment was designed to estimate spillover effects by randomly varying the intensity of the D2D treatment and tracking untreated women in treatment localities. We find that neither intervention leads to increases in voter registration, but while public rallies show no effect on voting, face-to-face interactions significantly increased turnout among treated women. Furthermore, we find evidence of spillover effects that leads to higher turnout in localities with urbanization patterns that appear to favor social interactions. These spillover effects are more important for treated women (reinforcement effect) than for untreated women (diffusion effects).

Public rallies are a cost-effective way to reach large audiences and while somewhat impersonal, they are an appealing option for politicians and are widely used in political campaigns, and more generally in information campaigns. Interestingly, despite their popularity, very few studies have studied their impact. On the other hand, door-to-door campaigns, while more capital and labor intensive, may be more effective in dense, urban areas in industrial countries, likely explained by the fact that it involves closer human contact and face-to-face interactions. The trade-off between a mobilization campaign that involves a more impersonal approach, which allows higher reach and is relatively economical, and one that is a more personal and interactive one (but is linked with less coverage and is more expensive), is at the core of our research and it is worth studying because it sheds light on the conditions under which mobilization works. We evaluate the impact of these two types of interventions side by side in the same context as means to evaluating which one provides higher returns on investment in terms of generating the desired behavioral change.

Our research focuses on poor rural areas of Paraguay, which as is typical in such areas, have very limited access to information, but are heavy reliant on social networks. We focus on women only, as the most basic way in which they may increase representation, namely, through electoral registration and voting is often overlooked despite the fact that in may countries there is a persistent participation gender gap. In the case of Paraguay women hold 18 percent of executive posts, only. Whereas in the 2013 presidential elections 52 percent of registered voters were men such gender gap was as large as ten percentage points (ONU Mujeres 2015).[1]

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

We explicitly take into account the role of geographical restrictions to social interactions in evaluating the information spillover effects of our intervention. Our

[1] 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.

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 town center, people may interact more frequently and naturally. The way in which messages are delivered are also expected to have different impacts in terms of their spillovers: a home visit 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. Likewise, neighbors or other members of the social network can reinforce a message, and this can enhance the effects of any information treatment. This applies to both, people not targeted by the intervention, as well as those who were targeted.

We provide four key findings. First, neither public rallies nor door-to-door treatments impact registration. Second, door-to-door campaigns that disseminate the information in a conversational manner increase turnout in 4.6 percentage points, whereas impersonal messages disseminated in massive public rallies are not effective at generating changes. Third, spillover effects from door-to-door campaigns are only relevant in localities where the geographical distribution of households facilitate social interactions, while in places without a natural gathering location, these effects are diluted. In particular, 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, this is the first time that this issue has been studied. Finally, we find that information dissemination not only 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. Information may flow within communities, and thus, it might also affect untreated women.

Section 2 discusses the current literature on field experiments and voting incentives to voting, and the one on the geographical layout of cities and villages. Section

3 describes our data and experimental design. Section 4 presents our empirical strategy, descriptive statistics and basic results, as well as robustness checks, Section 5 analyzes social interactions in the context of geographical restrictions, and Section 6 summarizes and concludes.

2. LITERATURE REVIEW

While there is a large and still growing literature that uses field experiments to understand voter behavior there are still limited studies on efforts to increase voter registration[2]. Impersonal messages are less likely to affect the likelihood of registration, for example, Bennion and Nickerson (2011) and Nickerson (2007) find no effects of e-mails on voter registration. However, face-to-face interactions seem to affect registration rates. Nickerson (2015) uses a door-to-door campaign and shows that voter registration increases by 4.4 percent in the US. Bracconiere et al (2017) randomize two different door-to-door registration campaigns in France and find an increase in registration of 2.4 and 4.7 percentage points, respectively.

Green and Gerber (2004) examine the impact and effectiveness of door-to-door canvassing, telephone calls, direct mail, and other campaign tactics and show that more direct interactions are usually more effective at increasing turnout, despite their larger implementation cost. There is evidence of a positive and significant effect of canvassing on turnout (Green et. al. 2013.) Pons (2017) carried out a large-scale experiment to mobilize voters and shows that personal interactions with the canvassers were effective at mobilizing voters.

It is still not clear that these types of campaigns are effective in mobilizing voters. Evidence from other countries show that the effect of GOTV campaigns on turnout show great variation in terms of magnitude (e.g. Guan and Green 2006, John and Brannan 2008, Gine and Mansuri 2017), and for European countries some show small or even zero effects (see studies surveyed in Bhatti et. al. 2016.)

The fact that conversational messages are more likely to change people's minds, and increase the likelihood of participating in elections suggests that closer interactions

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

are key in the success of GOTV campaigns, and spillover effects can be relevant. Recent studies show that social pressure has larger effects on turnout than those targeted at individuals (Della Vigna, et al, 2017; Gerber et al 2008).

Despite the popularity of public rallies there are few studies on its effects. Addonizio, et al (2007) estimate the effect of festivals held at polling sites, and find that they significantly increase turnout. Green and McCleland (2017) find that festivals may increase turnout in a cost-effective manner. Surprisingly, in developing countries, where these rallies are much more common, to our knowledge, no studies have evaluated their effects on political participation.[3] This is particularly interesting, because these strategies involve human interactions (as in other GOTV campaigns), but they are not face-to-face or customized.

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

Sinclair et al (2012), using a multilevel GOTV experiment designed to measure within household and within 9-digit zip code spillovers in Chicago, found evidence for the former, but regardless of the intensity of the treatment, no within zip code spillovers were found. Fafchamps et al. (2015) show that information about the candidates in the 2009 Mozambican election diffuses through kinship networks and chatting, but evidence on spillovers encouraging voting is mixed. In the closest study to ours, Gine and Mansuri (2017) assess the direct impact of a door-to-door voter awareness campaign on female participation, candidate choice, and vote shares in Pakistan. They show that treated women are 12 percentage points more likely to vote, and to choose a different candidate than their husbands. They also find that they are also more likely to vote and to make an independent choice, showing large spillover effects.

In terms of the literature studying the determinants of the development of linear and non-linear settlements, while barely studied in economics (Nijkamp and Reggiani

[3] In Fujiwara and Wantchekon (2013) public rallies are the status quo, and the authors are not interested in testing the effects of the rallies themselves.

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,[4] 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).[5] Researchers from different disciplines have long argued on the origins of linearities or non-linearities in formation of towns may not be necessarily linked to different observable characteristics. For instance, Nijkamp and Reggiani (1993) have considered that settlement evolution may be a random process, and Allen (1984) has argued that the evolution of settlements from a social science perspective may have become unpredictable and that nonlinear evolution and self-organizing systems can be chaotic, disorganized, events.

3. EXPERIMENTAL DESIGN AND DATA

Our intervention took place between August 2012 and March 2013, before the April 2013 presidential elections in Paraguay. We designed an experiment to estimate the causal effect of both public rallies and door-to-door campaigns on voter registration and turnout in the 2013 elections. With the experimental design, we 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 in rural Paraguay, Caaguazú and San Pedro.[6,7]

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[4] See: http://www.visionofbritain.org.uk/place/3159, last accessed on October 2nd, 2017.
[5] 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.
[6] In the two provinces where we intervened, San Pedro and Caaguazú, only 44.9 and 45.9 percent of the registered population in 2012 were women, respectively (TSJE, 2013).

Using locality-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.[8] Each of the 300 localities sampled was assigned with an equal probability to the public rally campaign, door-to-door campaign, or control. Additionally, within the door-to-door localities, we randomly allocated them to be treated with different intensities (30, 40, or 50 percent of households). The locality level randomization was done prior to any intervention.

The two rounds of our campaign were implemented in collaboration with CIRD, a local NGO. The first round took place one month prior to the voter registration deadline of October 31st, 2012. The aim was to provide information, explain steps, raise awareness and encourage registration and voting. In communities assigned to the door-to-door campaign, the supervisor decided the number of households to be contacted (based on the intensity of the treatment assigned), and canvassers followed a random selection algorithm to choose households to be treated. The treatment protocol established that two members of the canvassing team (one female, one male, wearing the Paraguayan national football team jersey to signal no party alliances) approached each of the randomly selected households and asked to speak to all women in the household 18 years or older. We scheduled up to three visits per household, and were able to reach almost the universe of sampled households. 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.

In localities assigned to the public rally 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 canvassing team was composed of a female and male canvasser using the Paraguayan football team jersey. Public rallies were usually scheduled after Sunday church services, since in our preliminary fieldwork, this was identified as a natural

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[7] Eligible communities were rural, with 250 to 2500 individuals and with at least one primary school. These criteria translated into 724 eligible localities in the two provinces, of which we randomly selected 300 to allocate across the control and the two treatment groups.

[8] 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.

gathering place for the majority of the population. At the beginning of the rally, a standardized audio spot was played with a megaphone announcing the event as people were exiting mass, asking people to gather to receive the information to be offered by the canvassing team. The team members orally presented the campaign material to the group and distributed fliers with information on the steps needed for registration/voting, and highlighting the importance of voting. Importantly, both the script with the orally disseminated message, as well as the fliers, were exactly the same as in the door-to-door campaign. After the speech, the team was available to answer specific questions. In this first round of treatment, nobody in the control group was contacted. In online Appendix C we provide examples of the campaign fliers, and pictures that illustrate the way the public rallies were conducted.

The second round of the treatment was intended to reinforce the message on the importance of participating in elections provided in the first campaign. Additionally, we provided useful information about the steps that need to be followed to issue a vote. This campaign took place in March 2013, one month prior to the presidential election of April 21st 2013. During this round of treatment, we followed the same format as in the first round of treatment; both in public rally and door-to-door localities (i.e. all households contacted in 2012 were attempted to be contacted again.)

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. 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. In door-to-door localities, the survey sampling was stratified between those households that had been contacted twice (treated households in D2D localities, henceforth DTDT), and untargeted (control households in D2D localities, henceforth D2DC), and within each strata, in a random sample of households one women per household was administered a survey. In the case of D2DT women, the survey was applied just before the treatment was provided. The survey included an informed consent, which asked women permission to access their voting records, and the questionnaire asked about the dwelling, socio-economic characteristics, political preferences and participation, and related information. We were unable to reach 14 localities initially sampled, and overall, we were able to reach 5,621 out of 5,987 women sampled for the survey. We also obtained names and ID numbers of interviewees and matched them with data from the Paraguayan Supreme Electoral Council (TSJE). We matched 4,922 observations, or 88 percent of the sample.[9]

The classification of localities into linear and non-linear was done based on satellite images. We used GoogleEarth, and downloaded the maps for all localities in our sample. Two reviewers, working independently did the coding. In the unusual cases when differences between reviewers arose, the PIs weighed in to reach an agreement on how to classify the locality. The few borderline cases are not critical to our results, as their exclusion or switch do not change our findings. Maps 1 and 2 provide examples of the maps and satellite images used, and more examples can be found in online Appendix C.

4. Empirical Strategy

The empirical strategy follows directly from the experimental design. To estimate the causal effect of receiving the different types of campaigns on registration and voting, we compare women in localities where we held public rallies (*Rally*), those in the door-to-door localities (*D2D*), and the control group. More precisely, we estimate the following regression equation:

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

where y_{ij} represent an outcome variable – registered to vote or voting in the 2013 election for woman i in locality j; $Rally_j$ is an indicator for whether locality j received the public rally campaign treatment; $D2D_{ij}$ indicates whether woman i lives in *D2D* locality j. X_{ij} is a vector of individual and locality level controls, including a set of dummies indicating the district where the locality is.[10] Efficiency gains can be achieved by including the lagged value of the dependent variable, thus we include y_{ij}^{t-1} in our preferred

[9] The attrition is balanced across the different groups. Unfortunately, TSJE did not provide information of people in the same localities not included in the survey.

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

specifications. Finally, ε_{ij} is the error term. The treatment assignment was done by locality, and thus, we cluster our standard errors at this level.

In both, public rally and D2D localities, the effects of registration should be interpreted as exposure to one round of treatment, while when we use turnout as the outcome, the treatment effects reflect a compounded treatment composed of two visits. In regression (1), β_1 represents the causal effect of living in a 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 to the rallies or who pay more attention to the information provided by the canvasser, we interpret β_1 as an intent-to-treat effect.[11] β_2 is the average treatment effect of living in a locality assigned to the door-to-door treatment. We estimate the direct effect of the door-to-door treatment by including in the estimation women in D2D localities who were contacted by one of our canvassers (D2DT, hence excluding women in D2DC). Likewise, when we compare untreated women in treated localities (D2DC) to the control group, we estimate the causal effect of being in a locality assigned to the door-to-door treatment, but not having received the treatment directly, i.e. we exclude from the sample women in D2DT, and estimate the spillover effect of the treatment.

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 (1) for linear and non-linear localities. Note that we do not claim that there is a causal relationship between the geographic layout of a locality and turnout, since the geographical distribution of houses in a locality is not randomly assigned. Rather, *within* each type of locality, the effect of the treatment can be interpreted as causal.

Further, we investigate the intensive margin of the spillover effects by exploiting 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 non necessarily divisible) number of households, attrition between the two treatment rounds, and imperfect compliance with the assignment, the actual proportion of

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

households treated not always coincide with the assignment. One might be concerned that either imperfect compliance or attrition could be correlated with the outcome of interest, 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.[12] The main regression equation used to test whether information spillovers affect voting behavior is:

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

where $PercTreated_{ij}$ is the ratio of the number of treated households divided by the total number of households in the locality, instrumented by three dummies representing treatment assignment to different intensities of the treatment.[13] β_2 represents the spillover effect: the effect of increasing the proportion of treated households by one percent for the average treated or untreated household, depending on the sample used. Again, we estimate the effects separately for treated and untreated women in D2D localities, which allows us to identify two distinct types of spillover effects. On the one hand, untreated women can be affected by the diffusion of information from treated women ("diffusion effect") that is, 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".) We estimate these effects separately for women in linear and non-linear localities.

4.1. DESCRIPTIVE STATISTICS

Tables A.1-A.4 in the appendix show descriptive statistics using individual survey and administrative locality level data. Our sample is composed of 4,033 women who are, on average, 42 years old, with about 6 years of schooling, and with low rates of economic activity. The registration rate just before intervention was about 83%, although less than 56% of adult women had voted in the previous election (2010). There are some small

[12] A second reason for the use of the 2SLS method is to minimize the effects of measurement error.

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

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. Importantly, there is a statistically significant difference in the baseline registration levels between women in the D2D treatment and the control. This reinforces the importance of controlling for the lagged dependent variable in our registration regressions.

Partially, the imbalance between the treatment groups 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 voting) in the TSJE data, leaving us with 4,033 observations.[14]

The randomization balance was done using the latest available census, and the descriptive statistics at the locality level in Table A.3 show that, despite the 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 main difference between women living in linear and non linear localities is in the level of education and household wealth index, with women in former scoring significantly better on both variables (see Table A.2.) Table A.4. show the baseline differences at the locality level. Linear localities are slightly smaller in population and area, but are *more* densely

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

populated (mainly because they are slightly smaller.) On the other hand, non-linear localities seem to be poorer, with a lower coverage of electricity, water, and phone lines. Overall, the data in Table A.2 and A.4 show that the linearity of the locality reflects a pattern of special distribution of households, and it is not the case that these are more densely populated and richer (actually, it is the opposite). Registration and turnout rates are not statistically different between these types of localities. In our empirical analysis we control for all the variables that show significant differences between linear and non-linear localities.

4.2. RESULTS

Table 1 shows the effects of the different treatments on registration and turnout. The first set of columns (Targeted) show the results of the estimation of the effects of the public rally treatment, as well as the direct effects of the D2D treatment by excluding untreated women in D2D localities from the sample. On the other hand, the second set of columns (Untargeted), estimate the effect of the public rallies and the spillover effect of being untreated in a D2D locality. We do this by excluding from the estimation sample all the treated women in D2D localities. In each set of results we gradually include controls at the individual level, locality level, district fixed effects, and the lag of the dependent variable in the different columns.

We first evaluate the effects of being exposed to the first round of the campaign on registration, which are shown in Panel A of Table 1. The first thing to note in our regressions is that the point estimates do not change much when including controls and fixed effects (columns 1-3), which is reassuring. However, the inclusion of the lagged dependent variable reduces significantly the magnitude and statistical significance of the direct effect of the D2D treatment. This is because there is a significant imbalance in the baseline registration rates, as seen in Table A.1. Overall, after including the relevant controls, we see that public rallies had no effect on registration (columns 4 and 8.) Moreover, being directly or indirectly exposed to the door-to-door campaign does not affect registration either (first row, columns 4 and 8, respectively.) All the coefficients in these regressions are very close to zero and have very tight standard errors.

The lack of an effect can potentially be explained by the fact that registration was already high among Paraguayan women at the moment when we started our intervention, at around 80 percent, and registering to vote 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. This is unlike in developed countries, where registration is much less costly, and different studies have found significant effects of similar door-to-door campaigns (Nickerson 2015, Bracconiere et. al. 2017.)

Panel B of Table 1 shows the results of our turnout regressions. The coefficients estimated for D2D and Public Rallies are very stable across specifications. We find that raising awareness about the importance of voting and informing women about the steps that have to be followed to vote affected the probability of voting if the message was delivered in a personalized way (column 4). These women were 4.6 percentage points more likely to vote. It is worth noting that the turnout rate in the control group was 74.8 percent, a relatively high proportion compared to the one observed in developed democracies, like the US or France. Therefore, the scope for increasing turnout was lower than other places where GOTV campaigns have been conducted, and still, we find a comparable effect of our treatment (as in e.g. Green and Gerber 2000, Gerber et al 2003, Pons 2017.)[15] On the other hand, for women in D2D localities who were not targeted by the campaign, we see a small (1.4 p.p.) and statistically insignificant effect.[16]

Finally, living in a locality where public rallies were held did not increase the probability of turning out to vote in Election Day. Very few studies test interventions similar to ours. Addonizio, et al (2007) and Green and McCleland (2017) are the closest ones, and they find that in the US, the effect of festivals held at polling sites increase turnout significantly, and are a cost effective way of doing so.

All in all, our findings are consistent with previous evidence in that more personalized and directed treatments (door-to-door visits) are much more effective than

[15] Gerber and Green (2002) conducted an experiments 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.

[16] Following Nickerson (2008), this implies that 35 percent of the direct effect of the direct treatment was passed on to untreated women. However, the coefficient on the latter is not statistically significant, so we should not over interpret this result. Moreover, we use a single regression with the pooled sample, and find that the ratio of the coefficients is indistinguishable from zero.

impersonal messages delivered to a broad public (through e.g. rallies) or messages distributed indirectly.

4.3. 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 1 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 a full information on the outcomes (25.4 percent) were correlated with turnout, the main estimates shown in the tables above would be biased.

To alleviate these concerns, in Table A.5 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.6, 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 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 public rally group is uncorrelated with attrition, however, women in the D2DC group are less likely to be in our sample, while those in the D2DT are more likely to appear. In this case though, clearly the F-test rejects the null that all coefficients are equal to zero.

To show that the selective attrition of women in D2D localities does not affect our estimates, in Table 2, we run a bounding exercise following Lee (2009) and compute upper and lower bounds of the point estimates under the best and worst case scenario assumptions for the behavior of women differentially attritied in the different treatment groups versus the control. The bounding exercise does not rely on any assumptions about the selection mechanism, but does assume monotonicity, which effectively means that assignment to treatment can only affect attrition in one direction. In our case, there is no reason to believe that being able to match the survey and administrative data is correlated with treatment assignment. On the other hand, given that women in D2D localities were contacted directly twice, it is less likely that we were able to find them, hence, the monotonicity assumption is a plausible one.[17]

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 public rally and D2D-Untargeted, the upper and lower bounds are very close to zero and are statistically insignificant, while for the D2D-Targeted, the treatment effects go from 3.1 to 10 percentage points. The upper bound is statistically different from zero, while the lower bound has a p-value of 0.16. Note that the mean of the dependent variable in this context is high, around 0.74, and therefore the assumption taken for the computation of the lower bound (that the differential attrition 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.

Overall, three pieces of evidence suggest that attrition does not seem to generate major biases in our estimation. First, Tables A.1 and A.4 show that the observations that we consider in our estimation sample are mostly balanced across the treatment groups.

[17] Lee (2009) bounds compute treatment effect bounds for samples with non-random sample attrition. These bounds are computed under extreme assumptions on the missing information in the observed data. The trimming is done on the group that suffers less of attrition, and therefore all the sources of the attrition (missing localities, missing survey or mismatch with administrative data) are considered here. The reason why we chose to use this bounding method is that it relies only on the assumption of monotonicity, which as explained on the new text is likely to hold in this case. Typically, Lee bounds are much tighter that Manski bounds (which in many practical applications ends up being uninformative,) and thus we chose the later.

Second, the regression analysis in Table A.5 and A.6 show that, after controlling for the relevant covariates, the treatment status at the locality level is uncorrelated with the probability of attrition, but this is not true at the individual level, where women in D2DC villages are less likely to be in the final sample. The Lee (2009) bounds in Table 2 demonstrate that this selective attrition does not affect our qualitative results.

5. URBANIZATION PATTERNS, TURNOUT, AND SPILLOVER EFFECTS

We hypothesize that the effects of our treatments should be stronger in localities that have an urbanization pattern that in conducive to social interactions. As mentioned above, in the Paraguayan case, there are interesting heterogeneities in the geographic layout of localities, in particular, some of them are set up as a long street, while others have a more concentric shape. In the latter, social interactions are more likely to take place.[18] In this section, we test whether the main effect estimated in the previous section is larger in these localities. Table 3 present these results, where we use our specification from columns (4) and (8) from Table 1, and split the sample between linear and non-linear localities.

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

Second, Panel B shows the effects of both treatments on turnout. We find that the effect of public rallies is small and statistically insignificant in both linear and non-linear localities. This is consistent with the fact that these rallies took place in the usual gathering points, and should not depend further social interactions.

We find that all of the effect of the door-to-door treatment on directly contacted women is explained by women living in non-linear localities. These women are 7.5 percentage points more likely to vote, while for those in linear localities, we see a very precise zero effect of the treatment (see Columns 1-3). The difference between the effect

[18] 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.

for linear and non-linear localities is significant at the 5 percent level.[19] For untreated women in D2D localities, we see that the treatment effects are slightly larger in non-linear localities, but they are generally indistinguishable from zero (and not different from each other.)[20] 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 3 are causal, but rather that *within* linear or non-linear communities, there are causal effects of the randomly assigned treatment.

If the treatment is similar in both linear and non-linear localities, how could it be that the effect is so much larger in the latter? Just listening to a message delivered directly might not be enough to change voting behavior, but rather, this message may have to be reinforced by the members of your social network. Likewise, it might be that there is a need for a critical mass of social connections to convey a massage for it to be effective in changing behavior. In other words, the effect of the treatment might be reinforced if more people are also talking about the treatment, and therefore increasing its effectiveness. Identifying these reinforcement and diffusion effects is not straightforward, since usually it requires very detailed data on network connections (as in Fafchamps and Vicente 2013 and Fafchamps et al 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.

In Table 4, we estimate the effect of the proportion of treated households on turnout using the 2SLS strategy described above, i.e. we instrument the proportion of households actually treated with the assigned one (30, 40, or 50%.) Note that given the zero effect found for registration so far, here we only focus on turnout. 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 public rally treatment. This allows us to

[19] We compute the p-value for the difference in the coefficients by pooling the data, and interacting our variable of interest with the linear dummy. We report the p-value of the difference between the coefficients.
[20] Again, the point estimates imply a transmission rate of about 0.37, but this rate is not different from zero.

estimate the reinforcement and diffusion effects, i.e. the effect of the intensity of the treatment on treated and women in D2D localities, respectively.

The average reinforcement effect is large (Column 1). An increase in the proportion of treated households of 10 percentage points leads to 1.2 percentage points higher turnout rates.[21] When we split the sample between linear and non-linear localities, we see that again the effect is entirely driven by those in non-linear localities. An increase in 10 percentage points in the proportion of treated households in a locality 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 1.

The diffusion effects of the treatment are estimated in Columns (3)-(6). This is, the effect of being indirectly exposed to the door-to-door treatment through your network connections. We do this by comparing untreated women in door-to-door localities with those in pure control localities, as well as the ones in 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 does 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.

6. CONCLUSIONS

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[21] The first stage regressions are shown Table A.7. The predictive power of our instruments is large, and the F-stat associated with the excluded instruments is between 17 and 45, well above the conventional standards. We report the OLS version of this table in appendix A.8. As expected, these results are qualitatively similar, but measurement error and imperfect compliance with the experimental protocol lead to attenuation bias.

We randomly assign localities to either be exposed to massive rallies that disseminate information about registration and voting, or to receive the same information in personalized door-to-door campaigns (D2D). We also estimate spillover effects, by randomly varying the intensity of the D2D treatment and take into account urbanization patterns as they may enhance social interactions and result in better dissemination of the information. We find that neither intervention lead to increases in registration. However, while massive campaigns have no effect on voting, face-to-face interactions significantly increase turnout.

We find evidence of information spillovers that lead to higher turnout, which is as important for the treated (reinforcement effect) as for the untreated (diffusion effects). In particular, the former are as important as the direct impacts. Remarkably, spillovers only occur in localities that have non-linear urbanization patterns, which may favor social interactions.

Overall, our results suggest that the design of GOTV should take into account the geographical constraints that affect the frequency of social interactions, and therefore could limit the extent of spillovers effects. While ours is one of the first studies to find evidence on this issue, it is clear that more research is needed to understand the mechanisms through which geographical layouts may play a role in the design of informational campaigns.

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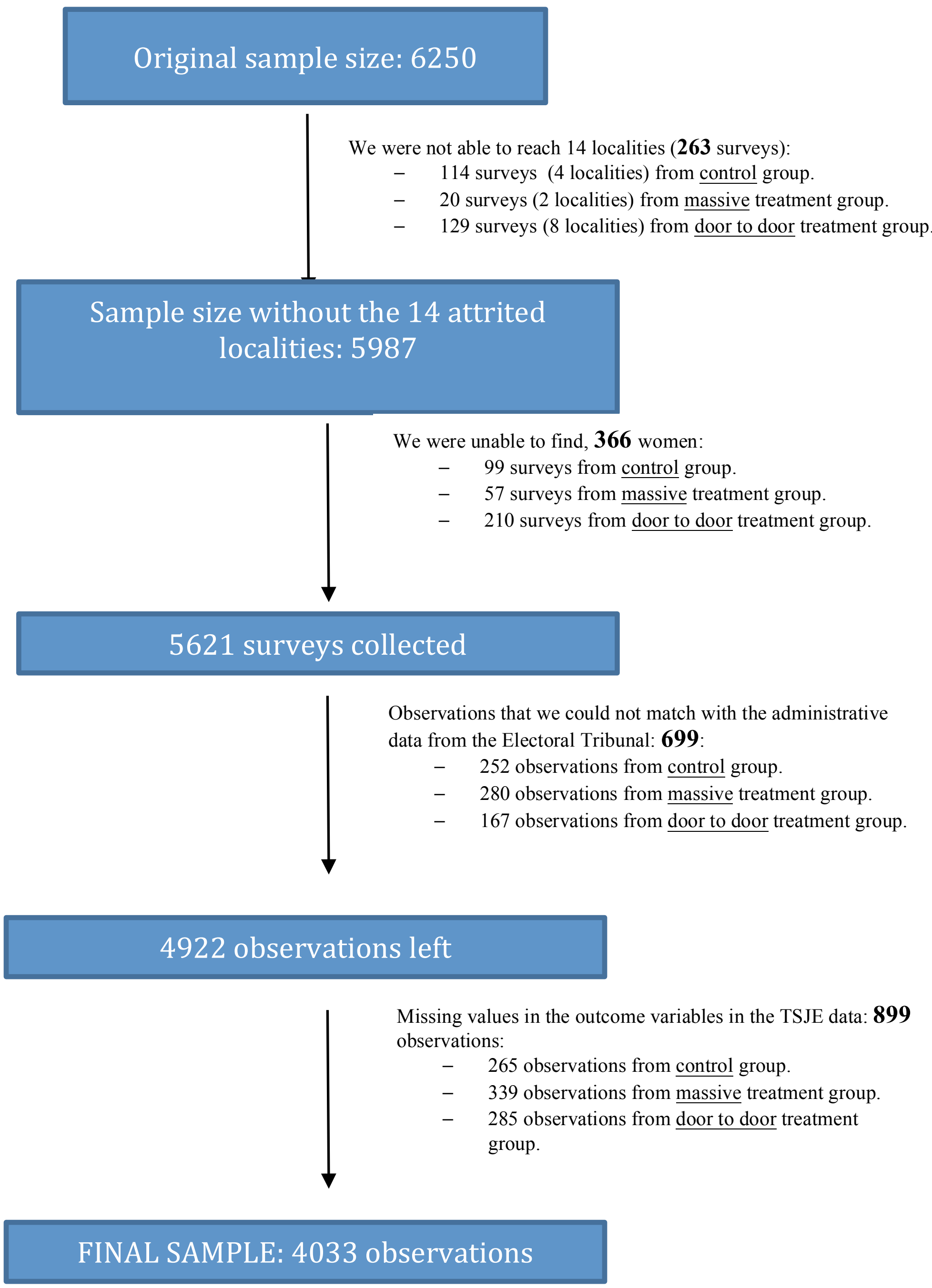
Figure 1: Sample and Sources of attrition



Map 1: Example of Linear and Non-Linear Localities

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

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

Panel A: Linear

Panel B: Non-Linear

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

Table 1: Effect of GOTV Campaigns on Registration and Turnout

	Targeted Households				Untargeted Households			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Effect of the treatment on Registration								
Dependent Variable: Registered to vote								
D2D	0.035*	0.027	0.031*	0.004	0.009	0.002	0.009	-0.011
	(0.015)	(0.014)	(0.015)	(0.008)	(0.016)	(0.016)	(0.016)	(0.009)
Public Rallies	0.011	0.005	0.008	0.005	0.011	0.007	0.009	0.006
	(0.013)	(0.012)	(0.012)	(0.008)	(0.013)	(0.013)	(0.013)	(0.008)
Mean of Dep. Var. for Control Group	0.751	0.751	0.751	0.751	0.741	0.741	0.741	0.741
# of obs.	3,350	3,350	3,350	3,350	3,434	3,434	3,434	3,434
R-squared	0.002	0.048	0.049	0.688	0.000	0.045	0.046	0.707
Panel B: Effect of the treatment on Turnout								
Dependent Variable: Voted in presidential elections of 2013								
	Targeted Households				Untargeted Households			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
D2D	0.046*	0.036	0.044	0.046*	-0.009	-0.009	0.001	0.014
	(0.023)	(0.022)	(0.023)	(0.020)	(0.024)	(0.022)	(0.022)	(0.019)
Public Rallies	0.005	0.008	0.013	0.015	0.005	0.011	0.014	0.016
	(0.021)	(0.020)	(0.020)	(0.016)	(0.021)	(0.020)	(0.020)	(0.016)
Mean of Dep. Var. for Control Group	0.751	0.751	0.751	0.751	0.741	0.741	0.741	0.741
# of obs.	3,350	3,350	3,350	3,350	3,434	3,434	3,434	3,434
R-squared	0.001	0.064	0.066	0.208	0.000	0.071	0.072	0.211
Individual Covariates		Yes	Yes	Yes		Yes	Yes	Yes
Locality Level Covariates		Yes	Yes	Yes		Yes	Yes	Yes
Department Fixed effects			Yes	Yes			Yes	Yes
Lagged dependent variable				Yes				Yes

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

Table 2: Lee (2009) bounds for treatment-effects

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

Note: Results in the baseline specification come from bivariate OLS regressions of turnout on the specified treatment, comparing each treatment group only with the control. The lower and upper bounds are computed using the procedure outlined in Lee (2009). Standard errors in parenthesis. * significant at the 0.05 level ** significant at the 0.01 level.

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

	Panel A: Effect of the treatment on Registration							
	Dependent Variable: Registered to vote							
	Targeted Households			Untargeted Households				
	Full Sample	Linear	Non Linear	Full Sample	Linear	Non Linear	P-value	
	(1)	(2)	(3)	(4)	(5)	(6)	(3)-(2)	(6)-(5)
D2D	0.004	0.000	0.005	-0.011	-0.005	-0.016	0.787	0.537
	(0.008)	(0.014)	(0.010)	(0.009)	(0.014)	(0.010)		
Public Rallies	0.005	-0.001	0.008	0.006	-0.002	0.010		
	(0.008)	(0.013)	(0.010)	(0.008)	(0.013)	(0.010)		
Mean of Dep. Var. for Control Group	0.751	0.751	0.751	0.741	0.741	0.741		
# of obs.	3,350	3,350	3,350	3,434	3,434	3,434		
R-squared	0.688	0.689	0.689	0.707	0.707	0.707		

	Panel B: Effect of the treatment on Turnout							
	Dependent Variable: Voted in presidential elections of 2013							
	Targeted Households			Untargeted Households				
	Full Sample	Linear	Non Linear	Full Sample	Linear	Non Linear	P-value	
	(1)	(2)	(3)	(4)	(5)	(6)	(3)-(2)	(6)-(5)
D2D	0.046*	0.000	0.075**	0.014	-0.012	0.028	0.051	0.290
	(0.020)	(0.030)	(0.025)	(0.019)	(0.029)	(0.025)		
Public Rallies	0.015	0.005	0.019	0.019	0.004	0.022		
	(0.016)	(0.026)	(0.021)	(0.022)	(0.026)	(0.021)		
Mean of Dep. Var. for Control Group	0.751	0.751	0.751	0.741	0.741	0.741		
# of obs.	3,350	3,350	3,350	3,434	3,434	3,434		
R-squared	0.208	0.208	0.208	0.211	0.211	0.211		

Note: All regressions follow the same specification as the one in column (4) in Table 1. Standard errors in parenthesis clustered at the locality level.
* significant at the 0.05 level ** significant at the 0.01 level.

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

	Dependent Variable: Voted in presidential elections of 2013					
	Reinforcement Effect			Diffusion Effect		
	Full Sample	Linear	Non Linear	Full Sample	Linear	Non Linear
Public Rallies	0.016	0.000	0.020	0.018	0.006	0.020
	(0.016)	(0.025)	(0.022)	(0.016)	(0.025)	(0.022)
% Treated Households	0.124*	-0.040	0.231**	0.079	-0.056	0.124
	(0.056)	(0.069)	(0.074)	(0.060)	(0.062)	(0.085)
Mean of Dep. Var. for Control Group	0.751	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
# of obs.	3,350	1,122	2,228	3,434	1,154	2,280
R-squared	0.208	0.196	0.228	0.212	0.206	0.230

Note: All regressions are 2SLS, and include the same set of controls and fixed effects as in column (4) in Table 1. We instrument the % of Treated households with the three treatment assignment dummies (30, 40 or 50 percent). The first stage regressions are reported in Table A.6. Standard errors reported in parenthesis are clustered at the locality level. * significant at the 0.05 level ** significant at the 0.01 level.