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Marcel Fafchamps

University of Oxford

Ana Vaz

University of Oxford

Pedro C. Vicente

Nova School of Business and Economics

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Voting and Peer Effects: Experimental Evidence from Mozambique*

Marcel Fafchamps
University of Oxford†

Ana Vaz
University of Oxford‡

Pedro C. Vicente
Universidade Nova de Lisboa§

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Abstract

Voter education campaigns often aim to increase voter participation and political accountability. We follow randomized interventions implemented nationwide during the 2009 Mozambican elections using a free newspaper, leaflets, and text messaging. We investigate whether treatment effects were transmitted through social networks (kinship and chatting) and geographical proximity. For individuals personally targeted by the campaign, we estimate the reinforcement effect of proximity to other targeted individuals. For untargeted

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†Department of Economics, University of Oxford, Manor Road, Oxford OX1 3UQ, UK. Email: marcel.fafchamps@economics.ox.ac.uk. Fax: +44(0)1865-281447. Tel: +44(0)1865-281446.

‡Department of Economics, University of Oxford, Manor Road, Oxford OX1 3UQ, UK. Email: ana.vaz@economics.ox.ac.uk.

§Nova School of Business and Economics, Campus de Campolide, 1099-032 Lisboa, Portugal. Email: pedro.vicente@novasbe.pt. Fax: +35121-3871105. Tel: +35121-3801601.

individuals, we estimate the diffusion of the campaign depending on proximity to targeted individuals. We find evidence for both effects, similar across the different treatments and across the different connectedness measures. We observe that the treatments worked through networks by raising the levels of information and interest about the election, in line with the average treatment effects. However, differently from those average effects, we find negative network effects of voter education on voter participation. We interpret this result as a free riding effect, likely to occur for costly actions.

1. Introduction

The rationality of voter turnout in political elections is often questioned: unless a person casts the deciding vote, voting has no effect on the outcome (e.g., *Fedderson, 2004*). This is particularly true in elections where one contender has widespread support and the outcome is fairly certain. If no one votes, however, the electoral outcome is unlikely to reflect the preferences of the electorate. Not voting is therefore equivalent to free riding on other people's electoral participation. As a consequence, voting is often as a civic duty. Although some countries (e.g., Belgium, Brazil) make voting a legal obligation, most do not. The level of electoral participation therefore depends on the probability voters attribute to being pivotal and the social norms that are in place regarding voting. Peer influence may affect both.

The purpose of this paper is to study peer effects in political participation. A randomized control trial was organized in Mozambique to study the effect of voter education during the 2009 elections. The study of voter education in developing countries has seen recent attention, as electoral problems like vote-buying (*Vicente, 2007*), violence (*Collier and Vicente, 2009*), and low accountability (*Banerjee, Kumar, Pande, and Su, 2011*) have been identified to affect the likelihood that elections translate into public policies that produce broad-based development. Specifically Mozambique has seen a dramatic decrease on political participation since

the first democratic elections in 1994, which has accompanied the consolidation of power of the ruling party. The voter education intervention that we study in this paper therefore focused on increasing participation.

The voter education in Mozambique was implemented in collaboration with a free newspaper and a consortium of local NGOs. Three different treatments were administered nationwide across four provinces of the country. The first was the distribution of the free newspaper, which focused on neutral information about the elections. The second was a text messaging hotline to which citizens could report electoral problems. The third was civic education based on a leaflet and text messages focusing on information about the elections. All treatments embedded a clear appeal to voter participation in the elections.

The design of the experiment allowed the identification of targeted and untargeted individuals in treated locations for each of the three treatments, as we selected targeted and untargeted individuals randomly. We are interested in the effect that submitting the campaign to one individual, say i, has on another individual, say j, and whether this effect is stronger if i and j are close in a social or geographical sense. We distinguish between two types of effects, depending on whether j was himself/herself given the campaign or not. If both individuals i and j were submitted to the campaign, we test whether the effect of treatment on j is stronger when j is closer, in a social or geographical sense, to other individuals. We call this a reinforcement effect since it reinforces the effect of targeted treatment on j. To test for the presence of a reinforcement effect, we observe whether, relative to controls, the effect of the campaign on targeted individuals is reinforced by proximity to other individuals in the same location. If individual j was not given the campaign, j may nevertheless have experienced an indirect effect of the campaign compared to individuals in control locations. We test whether the effect of the campaign is stronger if j is socially or geographically close to other individuals. We call

this a diffusion effect since it diffuses the effect of the campaign to untargeted individuals. To investigate diffusion effects we test whether, compared to controls, untargeted individuals show stronger effects of the campaign when they have closer social ties to other individuals in their location.

In terms of outcomes variables, we exploit a rich individual dataset including a range of survey measures of individual turnout, a behavioral measure of political intervention as given by the sending of actual text messages, and measures of information and interest about politics. We also use actual voting records at the level of the ballot station to establish average treatment effects. Crucially we collected detailed measures of social and geographic connectedness between individuals, including measures of chatting, kinship and geographical distance between respondents' houses.

All treatments are documented to have produced increased voter turnout of targeted and untargeted individuals. The hotline was particularly effective in individual data. For ballot station records, both the newspaper and the civic education treatments achieved significant increases in voter turnout. We also document clear increase in information about the election. The peer effects on voter participation are however quite different as they are all negative, across reinforcement and diffusion, and using the different measures of network centrality. This is true for both voter turnout and our behavioral measure of political participation. These peer effects are particularly strong for the hotline. Information and interest about politics are in line with the average effects of the voter education, as they are positive.

We interpret these findings in line with a general framework of costly political participation. There, voter participation may be induced by the probability of affecting the electoral process and by non-instrumental motivations like civic-mindedness. We argue that the campaign gave information to reassure voters of the integrity of the process and that it raised civic-mindedness.

Both effects should be conducive to increased turnout. That is in line with the average effects that we find. However, peer effects could be prone to free-riding as more central voters realize that turnout increases and electoral competition diminishes (as the ruling party has an overwhelming advantage in the electorate).

Our estimation of network effects in the context of a randomized field experiment relates to a recent body of literature on the role of networks in aid interventions. *Miguel and Kremer (2004)* launched this literature by estimating externalities of a deworming school-based programme in Kenya. They estimated the impact of the treatment on control populations. Because their experimental design features programme randomization at the school level, it does not allow for an experimental estimation of externalities within treated schools. More recently, *Angelucci and De Giorgi (2009)* extend the study of externalities to a conditional cash transfer programme. By exploring a rich set of outcomes at the household level they are able to draw some light into specific mechanisms of influence of unexposed households. However, these authors do not use explicit network variables. Still in the context of a conditional cash transfer programme, *Macours and Vakis (2008)* introduce explicit interaction among households while focusing on reinforcement effects only, and *Angelucci, De Giorgi, Rangel, and Rasul (2010)* extend the analysis to diffusion but focus on kinship links. The studies by *Nickerson (2008)*, *Fafchamps and Vicente (2013)*, and *Gine and Mansuri (2011)* relate closely to our paper as they analyze peer effects of voter interventions. The first looks at a door-to-door randomized get-out-the-vote campaigning in the U.S. to identify peer-effects in two-member households. The second follows a campaign against political violence in Nigeria to identify reinforcement and diffusion network effects. The third assesses the impact of a voter awareness campaign on female turnout in Pakistan in which peer effects are estimated using geographical distance and friendship. Our analysis of kinship as a measure of social interaction is also related to *Bandiera and Rasul (2006)*

who study technology adoption in Mozambique in a non-experimental setting.

The paper is organized as follows. In Section 2 we provide a conceptual framework, as we present a general framework for the analysis of voter participation. In Section 3 we describe our testing strategy. We then offer the context of our experiment in Section 4. The treatments are introduced in detail in Section 5. Subsequently, in Section 6 we describe the data including outcome and network variables. In Section 7 we present our empirical results, including balance tests, average effects, peer effects on political participation, and peer effects on information and interest about politics. Section 8 concludes.

2. Conceptual framework

To structure our empirical analysis, we present a general framework for the analysis of voter political participation. The focus is primarily on turnout, even though we could think of other forms of political participation as well (e.g., open letter). The starting point of our effort is the idea that an educational campaign about elections raises the information level of voters. This affects their beliefs – e.g., in the fairness and transparency of the electoral process – and hence the interest they have about the voting process. People then adjust their behavior to reflect their new levels of information and interest. The campaign may also trigger various forms of peer effects which can impact the same outcomes.

We formalize this as follows, based on numerous sources as summarized by *Dhillon and Peralta (2002)* and *Fedderson (2004)*. Let us assume that an individual i takes an action vector x_i (e.g., casting a vote, voting for a specific candidate, sending text messages with political content) to maximize the following payoff function:

$$\max_{x_i} E_{\Omega_i} U(G(x_i, x_{-i}), x_i) - cx_i \tag{2.1}$$

where $G(x_i, x_{-i})$ is the outcome of the electoral process, x_{-i} is the combined action of individuals other than i, Ω denotes i's information set, and cx_i (with $c > 0$) is the total material cost of the action (e.g., transport cost, opportunity cost of time, cost of text messaging). To capture non-instrumental motivations – e.g., civic-mindedness – we allow x_i to enter the function U independently from the outcome of the voting process G. This could be related to either intrinsic or extrinsic motivations. The latter could be due to the subjective or social cost of deviating from a behavioral norm (e.g., *Akerlof, 1997*).

The first order condition is:

$$E_{\Omega_i}\left[\frac{\partial U}{\partial G}\frac{\partial G}{\partial x_i} + \frac{\partial U}{\partial x_i}\right] = c$$

This framework can be used to illustrate how we expect a voter education campaign to influence voter participation. First, the campaign can change voters' information set Ω_i. Distributing information about the electoral process may convince voters of the integrity of the process, thereby raising $E_{\Omega_i}\left[\frac{\partial U}{\partial G}\frac{\partial G}{\partial x_i}\right]$. Second, the campaign may increase non-instrumental motivation $\partial U/\partial x_i$ by raising civic-mindedness (and the emotional cost of not voting). Both channels are conducive to increasing voter participation: that is the direct impact we expect from the voter education campaign that we study.

If we take turnout as our explicit measure of voter participation, some difficulties may arise as we assume the absence of non-instrumental motivations (if $\frac{\partial U}{\partial x_i} = 0$), i.e., that optimal turnout requires $E_{\Omega_i}\left[\frac{\partial U}{\partial G}\frac{\partial G}{\partial x_i}\right] = c$. We know a single vote has little effect on the electoral outcome – $\partial G(x_i, x_{-i})/\partial x_i$ is small. It follows that voting is not individually rational unless the cost of voting c is minimal. That constitutes the paradox of not voting that dates back at least to *Downs (1957)*. However a lively debate has followed. *Palfrey and Rosenthal (1983)* proposed a game-theoretic voting game with two candidates and found a high turnout equilibrium: this

pattern is generated by a high probability of being pivotal, stemming from having nearly identical numbers of voters supporting each candidate. These results were short-lived as the same authors (*Palfrey and Rosenthal, 1985*) demonstrated that the introduction of uncertainty and large populations into their earlier model eliminates the possibility that high turnout arises in equilibrium. Recently, *Myatt (2012)* recovered the idea that the size of $\partial G(x_i, x_{-i})/\partial x_i$ depends on the perceived competitiveness of the election. Myatt considers a two-candidate election in which there is aggregate uncertainty about the popularity of each candidate. Despite an underdog effect through which higher turnout from the underdog compensates the advantage of the frontrunner, Myatt finds that turnout is high and that it peaks in elections that are expected to be close.

Apart from proposing a positive impact of voter education on political participation including voter turnout, we argue that it is very likely that the candidate that is expected to win (let us say without loss of generality it is the incumbent) will benefit most from this increased political participation. To explain this assertion, let us assume a simple world where there is no meaningful electoral competition - this may be a good approximation for the Mozambican context we study in this paper in which the incumbent secured 75 percent of the vote. The election could be thought simply as a turnout contest for the incumbent across locations, as ballot locations compete for clientelistic benefits after the election (the incumbent can look at turnout per location to attribute localized benefits). Or turnout could be driven by non-instrumental motivations. Either way, and mechanically, higher turnout will be favoring the incumbent.[1]

[1] Another more realistic explanation for favoring incumbents is the two-candidate voting contest of *Myatt (2012)*. Take the event that the underdog has complete turnout, i.e., that all his supporters actually vote (note that the underdog effect proposed by Myatt may constitute a likely explanation for complete turnout by the supporters of the underdog). Then, an increase in the value of the election induced by voter education can only increase turnout for the incumbent. The same result can be sustained without assuming complete turnout for the underdog to begin with: namely by postulating an increase in the value of the election only for the supporters of the incumbent (this is in line with the clientelistic story mentioned above).

We now turn to peer effects, that is, to the social reinforcement and diffusion effects triggered by the campaign. These effects may be similar to direct effects, as information and civic-mindedness produced by the campaign are cheaply transmitted to peers – see for instance *Montgomery and Casterline (1996)* on social learning. However, it is likely that peers will take as given the likely effects on political participation, namely the increase in turnout and the increase in voting for the incumbent. If that is the case, we may observe a free-riding peer effect, i.e., a negative effect on political participation as electoral competitiveness is perceived to diminish (given in our setting by the expectation of x_{-i}). This is consistent with *Myatt (2012)* as mentioned above.

3. Testing strategy

The combined (i.e., direct and indirect) average effects of the campaign can be estimated as follows. Let y_i be a measure of electoral information, interest, or behavior. Let $T_i = 1$ if individual i was visited by the campaign, and 0 otherwise. As we will see when the experiment is described in detail, the campaign took three distinct forms that we test separately. For the sake of the presentation, here we focus on a single treatment.

Assuming treatment is randomly assigned, the homogeneous (average) effect of the campaign can be estimated using a regression of the form:

$$y_i = \alpha + \beta T_i + \varepsilon_i$$

This regression can also be estimated with village and individual controls. A difference-in-differences version could also be used in which fixed effects are employed in order to net out possible time effects and individual unobservables. Coefficient β is the average treatment effect on electoral information, interest, or behavior.

Estimating average treatment effects is not the focus of this paper, however. This parameter is covered in detail in *Aker, Collier, and Vicente (2011)*. Here we focus on peer effects. We first estimate the average peer effect of the campaign on individuals/households in treated villages but randomly selected not to be targeted by the campaign. Let these untargeted individuals be denoted by the superscript u. Following Fafchamps and Vicente (2009), we can then estimate a diffusion effect. Estimated regressions take the form:

$$y_i^u = \alpha^u + \beta^u T_v + \varepsilon_i$$

where $T_v = 1$ if the village was treated. The direct treatment variable T_i drops out since, by design, it is 0 for untargeted individuals in treated villages. Coefficient β^u is an estimate of the average diffusion effect of the campaign on the electoral information, interest, or behavior of untargeted individuals.

Evidence of possible diagonal effects can be investigated by comparing estimates of β^u with estimates of β. For instance, assume we find that $\beta = 0$ but that $\beta^u > 0$. This indicates that the campaign affected the outcome for the untargeted individuals($\beta^u > 0$), but not the outcome for those directly affected by the campaign ($\beta = 0$). Such evidence would suggest that treatment effects only operate through networks. This could happen in case social identity and pressure are more important than social learning.

A possible configuration of interest is when $\beta^u = 0$ for electoral information and interest but $\beta^u > 0$ for electoral behavior. This would suggest that peer effects operate primarily through social pressure: the information and interest of untargeted individuals do not change, but they change their behavior nonetheless. *Vaz (2011)* reports such effects on dependents of treated individuals and interpret these findings as suggesting that treated individuals – who typically are the head of household or his spouse – put pressure on dependents to go and vote,

without necessarily providing them with information or convincing them that voting is a civic duty. Whether similar pressure can be applied across households is unclear, and something we examine in this paper.

We also investigate whether peer effects are stronger for targeted individuals who are socially and geographically close to other targeted individuals. Let $g_{ij} = 1$ if individuals i and j are connected in a relevant social network sense, and 0 otherwise. Alternatively we can define g_{ij} to be the symmetric of the physical distance between i and j. We estimate an heterogeneous reinforcement effect model of the form:

$$y_i = \alpha + \beta T_i + \delta \frac{1}{N} \sum_{j \neq i} g_{ij} + \gamma \frac{1}{N} \sum_{j \neq i} g_{ij} T_i + \varepsilon_i \tag{3.1}$$

where N is the total number of sampled neighbors, and $\frac{1}{N} \sum_{j \neq i} g_{ij}$ is the proportion of neighbors of i to whom i is connected (i.e., i's degree of connectedness).[2] The above regressions are estimated using treated individuals only, in levels or first-difference. Network reinforcement effects are tested by examining whether the effect of treatment is larger among individuals with more direct links to other individuals, i.e., whether γ is positive.[3] *Fafchamps and Vicente (2013)* use a similar approach when analyzing the effect of a campaign against electoral violence in Nigeria.

In this setting the interesting possibility that $\gamma < 0$ while $\beta > 0$ for electoral behavior outcomes can be explained, within our conceptual framework, as a manifestation of free-riding: treatment raises the likelihood that others vote (or vote in a certain way); this in turn reduces the marginal usefulness of i's vote; if i is better connected and hence better able to observe

[2] When estimating the above regression, $\sum_{j \neq i} g_{ij}$ is expressed in difference relative to the mean in the expression $\sum_{j \neq i} g_{ij} T_i$ so as to keep the interpretation of the β's unaffected.

[3] In the event that $\gamma = 0$, we cannot rule out the possibility that social network effects are so strong as to spread evenly to all individuals in treated villages, in which case proximity to treated individuals does not matter.

the effect of the campaign on others' intention to vote, i's is also more aware of the reduced usefulness of his/her vote.

Using the same approach we can investigate the presence of heterogeneous diffusion effects on the untargeted:

$$y_i^u = \alpha^u + \beta^u T_v + \delta^u \frac{1}{N} \sum_{j \neq i} g_{ij} + \gamma^u \frac{1}{N} \sum_{j \neq i} g_{ij} T_v + \varepsilon_i \tag{3.2}$$

Interpretation here is similar to the one of heterogeneous reinforcement effects.

We use ordinary least squares in all our main regressions. Since the data we use is clustered by EA, we need to allow for within-group dependence: we report clustered standard errors at the enumeration area (EA) level.

4. Context

Mozambique, a country with 22.4 million inhabitants, is one of the poorest countries in the world with GDP per capita of 838 USD in 2008 - it ranks 161 in 189 countries (latest available years) in terms of GDP per capita. Without prominent natural resources, and with 81% of the population directly dependent on agriculture, it is an aid-dependent country with official aid assistance accounting for 22 percent of GNI in 2008.[4]

Politically, Mozambique became independent from Portugal in 1975, after which FRELIMO (Frente de Libertação de Moçambique), the independence movement, led a single-party, socialist regime. During that time, beginning in 1977, Mozambique suffered a devastating civil war, fought between FRELIMO and RENAMO (Resistência Nacional Moçambicana). RENAMO was supported by Apartheid South Africa and, in the context of the cold war, by the USA. The civil war finished in 1992 with an agreement to hold multi-party elections. Presidential and

[4] These figures were taken from World Development Indicators, 2009, and CIA World Factbook, 2010.

parliamentary elections were held in Mozambique in 1994, 1999, 2004, and 2009. FRELIMO and its sponsored presidential candidates won all national elections, with RENAMO as the main contender. More importantly, FRELIMO has been consistently increasing its vote share, while voter turnout has decreased massively from 88 percent in 1994 to just 36 percent in 2004.

Armando Guebuza became FRELIMO's leader and president in 2004, succeeding Joaquim Chissano. Guebuza had an important record within FRELIMO, from his time fighting against the Portuguese to the early years as minister of the interior under Samora Machel. He became a wealthy and powerful businessman after the privatization of public companies in the 90s. He ran for re-election in 2009. Afonso Dhlakama has been the leader of RENAMO since 1984, serving as a guerilla leader during the civil war and RENAMO's presidential candidate at all national elections. In this paper we focus on the presidential, parliamentary and provincial assembly elections of October 28, 2009. The 2009 elections were relatively calm, with FRELIMO and Guebuza expected to win. The elections were conducted in a relatively unproblematic manner, as witnessed by national and international observers. These observers generally considered it to be following appropriate international standards, despite the existence of many small irregularities. Results were unambiguous, giving 75 percent of the vote to both Guebuza and FRELIMO (at the presidential and parliamentary elections).

5. Treatments

The data used in this paper come from a randomized control trial implemented in Mozambique around the time of the 2009 presidential, parliamentary, and provincial assemblies election. Three treatments are investigated, all geared towards encouraging people to vote. The first treatment is a civic education campaign, which gave information about the election and focused on participation in the election; the second is the distribution of an independent newspaper

relaying the civic education message; and the third is a campaign to encourage voters to use an SMS-based hotline set up to report electoral problems. The three interventions were designed and conducted with the institutional support and active collaboration of newspaper @Verdade (http://www.verdade.co.mz/) and a consortium of eight Mozambican NGOs, named Observatorio Eleitoral. For more details on these organizations, see *Aker, Collier, and Vicente (2011).*

The civic education treatment was organized around a set of messages providing citizens in selected locations with specific information about the 2009 elections. The intervention started with a door-to-door campaign approximately a month before the elections. This was implemented during the baseline survey and was centered on the distribution of a leaflet designed and made available by the electoral commission (CNE/STAE). A copy of the leaflet is displayed in *Figure 1.* It explains in detail the voting steps on the election-day. 10,000 leaflets were distributed (i.e. 250 per location) primarily to survey respondents. Moreover, for two weeks prior to the election, subjects in the civic education treatment received five daily text messages on the cell phone number they provided during the baseline survey. The messages focused on the importance of voter participation, as in a 'get-out-the-vote' campaign. Within their 160-character limit, these messages also provide specific information about the electoral process, namely: the scheduled date; the type of elections taking place; the presidential candidates; the parties running for parliament; voter confidentiality; and how to vote.

The newspaper treatment was organized around the distribution of a free newspaper, *@Verdade*, to experimental subjects in selected locations. None of the treated locations had received the newspaper before.[5] The editors of the newspaper took a strictly independent approach to the electoral process, focusing their message on electoral education. The newspaper was distributed

[5]Despite being the highest circulation newspaper in Mozambique (with a minimum of 50,000 certified copies per week), the newspaper was only systematically distributed in the city of Maputo. As all newspaper locations lie outside the city of Maputo, they had never received the newspaper.

for the purpose of the research in the experimental locations from the baseline survey in September 2009 until the post-election survey in November 2009. Over this period the newspaper covered the contents of the civic education treatment by including a version of the CNE/STAE leaflet on the steps for voting (see middle panel of *Figure 2*). The newspaper also advertised a national hotline for reporting electoral problems, but it was branded with a different slogan and different short-codes (see right panel of *Figure 2*). For the distribution of the newspapers to treated villages, priority was given to survey respondents. 5,000 copies of the newspaper were distributed each week, with a total of 125 at each location.

The hotline treatment was organized around the setting-up of two short-code phone numbers contracted with the cell phone operators in Mozambique (Mcel and Vodacom). These short-codes constituted an SMS hotline as they were prepared to receive text messages reporting electoral problems. During the baseline survey, we conducted a door-to-door campaign providing information on the hotline: we distributed 10,000 leaflets (250 per location) primarily directed at survey respondents, providing basic information about the hotline: short-codes, examples, format of the messages to be sent,[6] and the name of the sponsors. The leaflet is depicted in *Figure 3*. We promised that the contents of these messages would be passed to the media for dissemination, and shared via SMS with all other respondents in hotline treatment locations. Before dissemination each message received on the hotline was verified with local correspondents that were hired in each of the hotline treatment locations. In addition to receiving hotline reports, respondents in hotline areas were sent SMS reminders about the existence of the hotline two weeks prior to the elections.

[6]Specifically, ballot location name first, and description of the problem second.

6. Data

The project took place in four provinces, Cabo Delgado, Zambezia, Gaza, and Maputo-Province. The sampling base was the 2004 electoral map of the country (the 2009 map became available only one month before the election), and the EA unit was the polling location. Because the use of cell phones was central to all our treatments, we eliminated from the sampling base all polling locations without cell phone coverage. For this purpose, we obtained detailed data from the two cell phone operators on the geographic location of each of their antennae. These were then plotted on a map using their geographical coordinates, with a five-km coverage radius drawn for each. All polling locations outside the covered area were dropped from the sampling base. Remarkably, 60 percent of all ballot locations in the country were found to be covered by at least one operator.

From this sampling base, 161 polling locations were selected using two-stage clustered representative sampling – first on provinces, then on EAs. The number of registered voters per polling location is used as sampling weight, based on information provided by the CNE/STAE in their electronic publication of (disaggregated) electoral data for the 2004 elections. Since all registered voters in the sampling frame have the same probability of being sampled, the 161 locations are nationally representative of the voting population of Mozambique that has access to mobile phone coverage. Of the 161 polling locations selected for our study, 40 were randomly assigned to each of the three treatments, and 41 locations serve as the control group, with no treatment administered. The allocation of locations to treatments and control follows a stratified randomization procedure (*Bruhn and McKenzie, 2009*). First, clusters of four similar locations were formed in each province, with similarity based on geography. Within each cluster, locations were then randomly assigned to one of the three treatments or to control. During the baseline survey, in the event that we found no cell phone coverage in a selected location, we replaced it

by the closest polling location with cell phone coverage. That happened in seven locations.

In each of the EAs we conducted two face-to-face household surveys, one before the election and treatment, and one after. Sampling in each EA followed standard procedures for household representativeness (e.g., n'th house call by enumerators starting from the center of the EA, typically a school). Interviews targeted the household head or his/her spouse. Interview and subsequent treatment are conditional on 'having access to a cell phone' for receiving and sending calls and messages. This criterion includes respondents that do not own a cell phone but have access to one via a neighbor or family member nearby. The baseline survey included 1,766 households/respondents, approximately 11 per EA. It took place from mid-September to mid-October 2009. The post-election survey started after the election results were announced in early November. It lasted for a similar period of time and sought the same respondents, reaching 1,154 of them. To verify the representativeness of our results, in the next section we check whether attrition varies systematically with treatment or with household characteristics at baseline.

Treatment was also randomized across respondents/households within each treated EA. Of the 11 households interviewed at baseline, two were, on average, randomly selected not to receive the treatment themselves. The other nine were directly targeted for treatment as described in the previous section. This was done specifically to study diffusion effects on individuals in treated locations not directly reached by the treatment.

6.1. Outcome and network variables

The outcomes of interest in this paper are based on both survey and behavioral data at the individual level, and on the official voting results at the level of the ballot station.

Table Appendix presents a summary of the survey outcome variables. These variables have been grouped into three sets: information, interest, and participation, in line with our conceptual

framework. Some of these variables were collected both before and after the election. Some questions are only asked in the follow-up survey, such as turnout at the election.

Our proxies of information and interest come, respectively, from questions asking respondents to list presidential candidates and parties running for the 2009 elections, and from questions asking about the interest respondents had on the presidential election, parliamentary election, provincial assemblies' election, and generally on public matters. The latter questions employed a subjective scale. To facilitate analysis and interpretation, we combine the questions described above into two indices, one for basic information about the elections and the other for political interest. The indices are constructed following the approach of *Kling, Liebman and Katz (2007).* We normalize the survey-indicators using z-scores and aggregate them using equally weighted averages of the normalized individual variables. The z-scores are calculated by subtracting the control group mean and dividing by the control group standard deviation. Thus, each component of the index has mean 0 and standard deviation 1 for the control group.[7]

We were particularly careful with our measurement of voter turnout. We propose six turnout measures. The first one is self-reported turnout. The second is self-reported turnout adjusted by considering as non-voters those who did not answer correctly questions regarding ballot papers and boxes.[8] The third one is an indicator of whether the respondent showed without hesitation to the enumerator a finger that had been dipped in indelible ink – the method used to prevent people from voting multiple times. Turnout Index 1 is a composite index measuring how well the respondent answered questions on the sequence of events during the election day. The answer to each question is coded according to how convincing the response was. Turnout Index 2 is based

[7] Like in *Kling, Liebman, and Katz (2007)*, if an individual has a valid response to at least one component measure of an index, then we impute any missing values for other component measures at the random assignment group mean for the corresponding time period.

[8] According to the adjusted turnout those respondents who have reported to have voted but answered wrongly the questions regarding the number of ballot papers and ballot boxes were considered as not having voted and, thus, assigned a zero.

on the sub-group of these questions that focus on knowledge about the ballot station (e.g., the number of ballot papers, whether there were photos of the candidates, the number of ballot boxes, whether they were transparent, and whether they were colored). The last measure of turnout is an enumerator assessment on whether the respondent voted or not. The three last measures take values between 0 and 7 and are thus potentially most informative. To facilitate comparison with the other turnout measures, we normalize them by dividing by 7, so they measure the likelihood that the respondent voted. Finally, we have measures of the respondents' self reported voting for candidates and parties at the presidential and parliamentary elections of 2009.

We then have available a behavioral measure of demand for political accountability, which we refer to as the 'open letter'. During the post-election survey the enumeration team explained and distributed a leaflet to all survey respondents in all 161 experimental locations, which invited them to send SMS messages proposing policy priorities to the president-elect for his new mandate. We were clear in conveying the limited extent of the initiative (a small number of experimental localities in the whole of Mozambique), and promised that the contents of these messages would reach the President in person (through the newspaper @Verdade). As with the hotline, each message sent by experimental subjects had a small monetary cost. Sending the message therefore represents a clear costly action. It was observable to us, as all cell phone numbers that sent messages were recorded and matched with those of the experimental subjects. We interpret the sending of an open letter message as an incentive compatible measure of demand for political accountability. The leaflet is depicted in *Figure 4*.

Our official voting results at the level of the ballot station were made available by CNE/STAE, the electoral commission of Mozambique. We employ results for the presidential and parliamentary elections of 2009. Ballot stations were matched with the enumeration areas in our experiment, which were based on ballot stations themselves.

We collected three measures of social and geographical centrality. The first two variables are centrality measures based on chatting and kinship networks, respectively.[9] For the first one, a link from i to j exists if i can identify the name of j when prompted, and i stated that he/she talks to j on a regular basis.[10] For the second, a link from i to j exists if i can identify j by name and reports being related to j.[11] The third variable is a measure of geographical centrality calculated as the average distance to other sampled individuals in the same EA. Each enumerator was asked to locate each respondent on an approximate EA map, and to calculate the distance between interviews. See *Figure 5* for an example. To evaluate the position of each respondent on the map, we construct up-down and left-right coordinates for each of them. The distance between each ij pair is then calculated from these coordinates. Because maps differ in scale, distances are re-scaled to make them comparable across all locations.[12]

7. Empirical results

7.1. Balance

Table 1 presents descriptive statistics on demographic traits of the baseline and post-elections samples together with balance tests. Comparisons between treatment and control locations show that the samples are overall balanced. Regarding the sample of targeted respondents, only three demographic characteristics are significantly different at the 10% level. In the sample of untargeted individuals the number of significant differences is reduced to two. The comparison between control and treated EAs in the follow-up survey is also presented in *Table 1.* We see

[9] Because we only observe a fraction of the chatting and kinship networks, we refrain from using other measures of centrality (e.g., Bonacich centrality) that are more sensitive to sample truncation bias (*Chandrasekhar and Lewis, 2012*).

[10] The question asked was 'How frequently do you calmly chat about the day events with the following individuals or members of their households? Not at all-Frequently'.

[11] The exact question used was 'Are the following individuals relatives of yours, i.e. members of your family? Yes-No'.

[12] This is accomplished by using the subset of pairwise distances, i.e., distance between interviews, reported by enumerators.

a similar pattern: in both samples of targeted and untargeted respondents, most household demographics and EA characteristics are not significantly different. Panel attrition seems to have maintained comparability of the different experimental groups, as far as observables are concerned.

Social and geographical centrality variables are summarized in *Table 2*. The social centrality variable, chatting and kinship, were collected during the post-election survey and so we only display statistics for the post-election sample. We display average connectedness $\frac{1}{N}\sum_{j\neq i} g_{ij}$, as defined above, with the exception of geographical distance, which is the symmetric of connectedness. We do not observe any statistically significant differences across comparison groups.

Finally, we display averages for our baseline voting variables at the ballot station level. These are voting records from the presidential and parliamentary elections of 2004. Results are presented in *Table 3*. We do not observe any statistically significant differences across comparison groups. Note that voting variables from our baseline survey are explored in full detail in *Aker, Collier, and Vicente (2011)*: since treated respondents were asked questions on politics after receiving leaflets (for civic education and the hotline) and the newspaper, there may be differences between comparison groups for the targeted due to first reactions or conformity bias. However, no clear evidence in favor of those effects was found.

7.2. Average effects

We start by presenting regression results of the average effects of the campaign. These results are explored in full detail in *Aker, Collier, and Vicente (2011)*. We start with measures of political participation, which is the main intended effect of the campaign. *Table 4a* presents the average effects of the three treatments on the voter turnout and the sending of the open letter by targeted individuals. Since this information is only available in the follow-up survey,

all regressions are estimated post-elections data only. For each turnout measure we present two sets of estimates: the first with no controls, and the second including provincial dummies, EA controls and individual characteristics.[13]

Looking at the reported values of the outcomes for control individuals first (as given by the intercept term in every second column - regarding regressions with no controls), we note that self-reported turnout is 86 percent, which is larger than all other turnout estimates. This suggests that respondents tended to over-report having voted, a feature of the data that is consistent with respondents regarding voting as a civic duty: if respondents had seen voting as a perilous or controversial activity, we would expect the opposite finding, i.e., self-reported turnout lower than more objectively measured turnout. This being said, the lowest turnout measure still puts average turnout above 70 percent. The inked finger measure, which is arguably the most objective although not necessarily the most accurate, suggests a turnout of 81 percent on average among control individuals. Since our sample only contains household heads and their spouses, it is perhaps not surprising that average turnout in the sample is above the national average (44 percent). Note also that 17.5 percent of control individuals actually sent a text message in the context of the open letter.

The average turnout effect of the hotline on targeted individuals varies between 5 and 12 percentage points, depending on the turnout measure we use, and is significant for all six turnout measures. This is a large effect given the participation rate in the control population is already high. The civic education treatment is associated with a moderately large increase in turnout; the effect is significant or marginally significant in all cases when we use no controls. By the inked finger measure and when we use no controls, the civic education treatment caused a 5.6 percentage point increase in turnout, which compares well to the 6.3 percentage point associated

[13] We employ only unbalanced individual characteristics. Similar results are obtained if we include a longer list of controls but, following *Deaton's (2009)* advice, we report results with a more parsimonious list of controls.

with the hotline treatment. However, when we use controls the civic education treatment is significant only with the adjusted turnout and interviewer assessment measures. For the newspaper treatment we find a small positive effect when we use no controls (i.e., +3.1 percentage points on average across the six measures), but this effect is never statistically significant. When we use controls the effect diminishes (i.e., +1.4 percentage points on average). For the open letter we find positive effects of the civic education and of the newspaper treatments. However, these effects are never significant. The hotline has a negative effect on the sending of text messages - this is marginally significant when not employing controls.

The average diffusion effect of the campaign on untargeted individuals is reported in *Table 4b*. We find statistically significant diffusion effects for two treatments, newspaper and hotline.[14] If anything, point estimates are larger for untargeted than targeted respondents: averaged over the six measures (no controls), we find 10.5 and 9.2 percentage point increases in turnout among untargeted households for the newspaper and hotline treatments, respectively. The treatment effect is also large for the civic education treatment, but it is only statistically significant when using the adjusted turnout and no controls. Overall the result on the newspaper is particularly surprising given that the newspaper treatment has no statistically significant effect on turnout among the targeted. This suggests that the diffusion effect of the newspaper treatment is stronger than its direct effect, perhaps because of magnification of the importance of the newspaper among individuals without direct access to it. In contrast, the civic education treatment increases turnout among the targeted but not among the untargeted, suggesting that it has a smaller and possibly negligible diffusion effect. We do no find significant average diffusion effects for the open letter, even though the signs are the same as for the effects on targeted individuals.[15]

[14] The newspaper effect is not statistically significant when using adjusted turnout, and the hotline treatment is not significant when using the finger measure and including controls.

[15] We have run comparable regressions using individual fixed effects (available upon request). The comparison is between intended turnout at baseline and the three dichotomous turnout measures (self-reported, adjusted, and

We now look at the effects of the treatments on our measures of information and interest about politics. These are displayed in *Table 5* for comparisons with targeted and untargeted respondents. From our conceptual framework we hypothesized that these are the likely mechanisms of change produced by the voter education campaigns: first, distributing information about the electoral process may convince voters of the integrity of the process, thereby raising $E_{\Omega_i}\left[\frac{\partial U}{\partial G}\frac{\partial G}{\partial x_i}\right]$; second, the campaigns may increase non-instrumental motivation $\partial U/\partial x_i$ by raising civic-mindedness; both mechanisms explain higher political participation. We find significant effects of the hotline on increasing information about the elections. Specifically this effect ranges between 16.5 and 23.3 standard deviation units for the targeted; for the untargeted it is equal to 22.6 standard deviation units (regression with controls). The newspaper also has a positive impact on information, which is significant for the targeted (19.2 standard deviation units). However, neither the hotline nor the newspaper had a clear impact on interest about politics. The civic education treatment does not have any significant effects for any of these outcomes, even though the sign is consistently positive when analyzing impact on the information index.

Finally we provide in *Table 6* average effects of the treatments on the actual electoral outcomes (from official voting records) at the level of the ballot station. We find clear positive effects on voter turnout. These are particularly large for the newspaper and the civic education treatments: respectively 4 and 5.1 percentage points for the presidential elections, and 4.5 and 5.4 percentage points for the parliamentary elections. These effects are significant at the 5 (civic education) and 10 (newspaper) percent levels. These findings are reassuring as consistency to self-reported survey data is achieved. Note that estimates are comparable to the ones found for the targeted respondents. On voting patterns, we find a positive effect of all treatments

inked finger). We find similar results regarding the hotline treatment among targeted and untargeted respondents: the effect on turnout is positive and statistically significant in all regressions, both for targeted and untargeted respondents. For the average diffusion effect, the newspaper treatment remains significant in two of the three regressions, and civic education is significant in one (at the 10% level only). Virtually identical results are obtained if we omit individual fixed effects but include individual controls.

on voting for the incumbent president or party (FRELIMO) and a negative effect of all treatments on voting for the main challenger candidate or party (RENAMO). However, only civic education actually achieves statistical significance, and only for the parliamentary elections: it leads to a decrease in 4.9 percentage points on RENAMO's score. We therefore conclude that our treatments generally increased voter turnout and harmed the main challenger (benefited the incumbent).

7.3. Peer effects on political participation

Next we turn to peer effects. We focus here on political participation outcomes, i.e., voter turnout and text messages induced by the open letter proposal. In *Table 7* we show our regressions relating to voter turnout, inked finger measure. We employ the three centrality variables presented above: chatting, kinship, and geographic proximity. We start by measuring network reinforcement effects through estimating regression (3.1) - results displayed in columns (1)-(3). We then estimate network diffusion effects through estimating regression (3.2) - results displayed in columns (4)-(6). All regressions are estimated using follow-up variables only. We control for provincial dummies, EA characteristics and individual characteristics. The main focus is on the coefficient of $\frac{1}{N}\sum_{j\neq i} g_{ij}$ and of the interaction terms $\frac{1}{N}\sum_{j\neq i} g_{ij}T_j$ (reinforcement) and $\frac{1}{N}\sum_{j\neq i} g_{ij}T_v$ (diffusion).

We obtain strong positive coefficients for $\frac{1}{N}\sum_{j\neq i} g_{ij}$ particularly when using chatting and kinship as centrality measure, but also when using geographical proximity. This implies that, without treatment, individuals who are more central socially in their community are more likely to vote. This suggests that those individuals possibly feel more compelled than others to perform their voting duty. But from this data alone we cannot tell whether centrality causes people to be more civic-minded – e.g., because of social pressure or internalized norms – or whether more

civic-minded people become more central – e.g., because they are more sociable.

The coefficient of the interaction terms is negative for most treatments and centrality measures. We observe that reinforcement through geographical proximity is consistently negative and statistically significant for all three treatments: the average respondent in the control group (in terms of geographical distance) sees a decrease in the probability of voting by 12.8, 13, and 11.1 percentage points through network reinforcement for the newspaper, hotline, and civic education, respectively. Statistical significance also happens for diffusion through chatting when considering the hotline treatment. There, the average respondent in the control group (in terms of chatting) sees a decrease in the probability of voting by 9.4 percentage points through network reinforcement for the hotline.

We display full results for turnout measure Index 1 in *Table 8*. The same patterns of negative network effects emerge. These are particularly prominent for the hotline, as all proximity variables yield negative reinforcement and diffusion - the exception is reinforcement through geographical proximity, which is not significant. Reinforcement through kinship for the civic education treatment also yields a negative effect on voter turnout that is statistically significant. *Table 9* shows interaction coefficients for the remaining voter turnout measures. Significant effects are all negative. The hotline stands out as inducing most network reinforcement and diffusion effects. However, the newspaper also induces network reinforcement and diffusion through geographical proximity (self-reported and interviewer assessment turnout measures); and civic education also induces reinforcement effects through kinship and geographical proximity (self-reported and interviewer assessment turnout measures) [16]

According to the conceptual framework presented in Section 2, these results suggest free-

[16] We also estimated average treatment effects for the samples of targeted and untargeted individuals split into the 40 percent above the mean centrality and the 60 percent below the mean centrality. The findings described here are confirmed. Similar – if not stronger – effects are found if we combine baseline and follow-up survey data and apply individual fixed effects.

riding: more central individuals are in a better position to realize, due to their centrality in the local network, that others are more likely to vote because of the campaign, and that the lag between incumbent and other candidates is likely to increase. Hence their own electoral participation is less necessary, and they the likelihood that they turn out to vote decreases. This effect may be particularly relevant here, given that the dominant party enjoys such a large advantage over its two rivals so that the electoral outcome is predictable.

We now devote some attention to the open letter. Results for this outcome are displayed in *Table 10*. We find statistically significant effects (at the 1 and 5 percent levels) for network reinforcement and diffusion effects of the civic education treatment when considering kinship. These effects are negative just like for voter turnout: they imply that the average respondent in the control group (in terms of kinship) sees a decrease in the probability of sending a text message by between 4 and 10.3 percentage points through network reinforcement for the civic education. We believe this result to be in line with the results for voter turnout in terms of interpretation as sending text messages is a costly participation action that can suffer from free-riding.

We now summarize our results on political participation. We start with voter turnout. We find a strong effect of the hotline treatment on targeted and untargeted individuals. Furthermore the effect is largest among individuals who are less central in their community, either socially or geographically – with social centrality yielding a slightly more robust effect. This effect holds whether we consider targeted or untargeted individuals. We also find a clear effect of the newspaper treatment on untargeted individuals, suggesting a magnification effect through indirect treatment. A stronger effect of the newspaper on less central individuals (geographically) is also identified (for both reinforcement or diffusion). In contrast, the civic information treatment only affects targeted individuals. We do, however, find a stronger effect of this treatment on

targeted individuals who are less central socially or geographically. We now turn our attention to the open letter. We do not find robust effects of the treatments on either the targeted and untargeted individuals. We do however find a pattern of results that is similar to turnout in terms of network effects: less central individuals, in the social sense, are more likely to send text messages in response to the open letter proposal. Overall we find positive direct effects but negative network effects on political participation.

7.4. Information and interest about politics

We now seek to better identify the channel through which the treatments affect political participation. We know we have some evidence that information about the election increased both for targeted and untargeted individuals. We could not find statistically significant effects for interest about politics. The question now is what the pattern is of network diffusion and reinforcement effects on information and interest about politics. We want to know whether these information and interest are transmitted across networks, and, if yes, whether there is free-riding (as for political participation).

Like in the last section, we employ the three centrality variables presented: chatting, kinship, and geographic proximity. We measure network reinforcement effects through estimating regression (3.1) and network diffusion effects through estimating regression (3.2). All regressions are estimated using post-election data only. We control for provincial dummies, EA characteristics and individual characteristics. The main focus is on the coefficient of $\frac{1}{N}\sum_{j\neq i} g_{ij}$ and of the interaction terms $\frac{1}{N}\sum_{j\neq i} g_{ij}T_j$ (reinforcement) and $\frac{1}{N}\sum_{j\neq i} g_{ij}T_v$ (diffusion).

Table 11 shows results employing our index of information about the elections. We only find a statistically significant effect: network reinforcement through kinship when analyzing the impact of the newspaper treatment. This effect implies that the average respondent in the

control group (in terms of kinship) sees an increase in the index of information of 0.07 standard deviation units through network reinforcement for the newspaper treatment. This is a relatively small effect, but, differently from political participation, it is positive.

Our index of interest about politics is analyzed in *Table 12*. Here we find many more significant effects, all of which are positive. The newspaper has the most robust network effects: both reinforcement and diffusion are channeled through kinship and chatting; only diffusion is channeled through geographical proximity. All network effects of the newspaper are significant at the 1 percent level. They imply that: chatting increases interest by between 0.25 (reinforcement) and 0.29 (diffusion) standard deviation units; kinship increases interest by between 0.12 (reinforcement) and 0.21 (diffusion) standard deviation units; geographical proximity increases interest by 0.54 standard deviation units (diffusion). We also find network reinforcement effects for the hotline (through chatting and kinship) that are slightly lower than for the newspaper. And we find both reinforcement and diffusion network effects for the civic education treatment, but only through chatting (also lower than for the newspaper).

The positive effects we encounter for information and interest are in line with the average effects on the targeted and the untargeted on these outcomes. This is despite the fact that we do not actually find any statistically significant effects when looking at the average impacts of the treatments on interest about politics. It then looks like information and interest were passed across networks, and that there was no free-riding produced on these outcomes. This may relate to the idea that information and interest about politics are cheaply transmitted across peers as they do not necessarily entail behavioral costs.

7.5. Robustness check: multiple imputation

Although balancedness tests do not indicate that panel attrition significantly affected the comparability of treatment and control groups, we nevertheless to test how sensitive our results are to dropping of post-elections observations. We use the multiple imputation method to replace the missing values of outcome and control variables; and re-estimate the average and peer effects on political participation using the full sample of respondents. We use multivariate normal regressions and we impute missing data separately for each treatment group.[17] In the imputation model we include the variables that we use in our empirical analysis as well as indicator variables for each enumeration area. The inclusion of the indicators for the enumeration areas accounts for the clustered structure of the data at the EA level; but it does not allow us to impute the missing values of EA characteristics.[18]

Recalculated estimates of the average treatment effects on political participation are similar to the ones obtained earlier. In *Table 13a* we present the average effect of each of the three treatments average effects on political participation of targeted respondents. This table is to be compared with *Table 4a*. We find a higher number of significant effects in civic education areas, but the negative effect of the hotline on the behavioral measure open letter is no longer significant. When looking at the untargeted respondents the differences between the results are even smaller.

In terms of network effects, the coefficients of the interaction terms (treatment x network size)

[17] We follow *Schafer and Graham's (2002)* advice. In the case of experimental data, these authors suggest the estimation of a separate imputation model for each group of observations, in order "*to preserve interaction between group membership and other variables*" (p. 167). The disadvantage of this approach is that it reduces the number of variables we can include in the model (due to sample reduction).

[18] Given that most of our variables are categorical, we considered imputation using chained equations. However, the application of that method to our data had some shortcomings. First, it would not allow us to account for clustering. Second, it would not allow the imputation by group (the model did not converge). Third, it would not allow us to impute missing values of self-reported turnout (because this variable was perfectly determined by other turnout measures). Despite all this, the results obtained using this method are very similar to the ones reported here.

remain negative for most treatments and network measures. But they are significant in a smaller number of cases and never significant for geographical proximity. *Table 14* displays the estimates of peer effects on turnout measure Index 1 using imputed data. Comparing these results with the ones displayed in *Table 8* we see that most of the significant network effects remain so, although they have a lower magnitude. Exceptions are reinforcement through chatting and the diffusion through geographical proximity which become statistically insignificant for the hotline. Overall, the estimates using imputed data corroborate the network effects through chatting and kinship.

7.6. Robustness check: saturation effect

Our estimations of network effects on voting behavior show three results. First, individuals with a larger social network have a higher propensity to vote, *ceteris paribus*. Second, the treatments, especially the hotline, have a positive average effect on individual turnout. Third, the effect of the campaign is smaller among individuals with a larger social network. We interpret the latter result as evidence of free-riding.

A potential criticism of this interpretation is that the negative coefficient of the term $\frac{1}{N}\sum_{j\neq i} g_{ij}T_i$ may reflect a voter saturation effect instead of free-riding. As individuals with a larger social network vote with a high probability, it may be more difficult for them to further increase their likelihood of voting. This, and not free-riding, could explain why the effect of the treatment on these individuals is weaker than on individuals with a smaller social network.

To test the possibility of a saturation effect, we use a control function approach as follows. We first use control respondents to estimate a predicted propensity to vote $\widehat{y_i}$ for all respondents, in the absence of treatment. We then add this variable and its interaction with treatment T_i as additional regressors in regressions (3.1) and (3.2). Since interaction terms $\widehat{y_i}T_i$ (in the case of

targeted respondents) and $\widehat{y_i}T_v$ (in the case of untargeted respondents) capture the saturation effect directly, adding them to (3.1) and (3.2) purges network effect estimates of the saturation effect.

We estimate the following equation using control and targeted respondents:

$$y_i = \alpha + \beta T_i + \delta \frac{1}{N} \sum_{j \neq i} g_{ij} + \theta \widehat{y_i} + \gamma \frac{1}{N} \sum_{j \neq i} g_{ij} T_i + \pi \widehat{y_i} T_i + \varepsilon_i \tag{7.1}$$

The predicted propensity to vote ($\widehat{y_i}$) is obtained by regressing the turnout of control individuals on age, gender, and province location. Coefficient π captures the saturation effect of the individual's propensity to vote on his/her response to the campaign. The presence of a saturation effect implies that $\pi < 0$. Coefficient γ captures the network effect; as before $\gamma < 0$ suggests free-riding.

We display full results for turnout measure Index 1 in *Table 15*. The estimates of equation (7.1) are displayed in columns (1)-(3). The estimates of the equivalent equation for the untargeted respondents are displayed in columns (4)-(6). All regressions are estimated using follow-up observations only. We control for provincial dummies and individual characteristics. Comparing these results with the ones presented in *Table 8*, we see that all significant reinforcement and diffusion effects remain so, with the exception of the diffusion effect through geographical proximity for hotline. In addition, the coefficients of the interaction term between the propensity to vote and the treatments, although negative, are never significant. This evidence rejects the interpretation of our earlier results as a saturation effect.

8. Concluding remarks

Using a large scale field experiment, we have investigated how voter education treatments affect the extent of political participation in the 2009 elections in Mozambique. Three types of inter-

ventions were tested: distribution of an independent newspaper; access to a text message hotline; and a civic education campaign. The interventions are shown to increase voter turnout and to increase information of targeted and untargeted individuals in treated locations. The focus of this paper is on peer effects. Using several measures of network centrality based on social and geographical connectedness, we estimate reinforcement and diffusion network effects by comparing control individuals with, respectively, targeted and untargeted individuals in treated locations. We find that network effects on political participation are consistently negative, i.e., connectedness decreases participation in face of the voter education interventions. This is particularly clear for the hotline. Differently, however, information and interest about politics are positively transmitted. We interpret these findings in the context of a voter participation framework where information and interest can be affected by the voter education and subsequently change voter behavior. We argue that the sign of peer effects depends crucially on the relevance of free-riding on others: lower participation of central individuals may have been produced by the sense that the campaign was driving more people to vote and was decreasing electoral competitiveness.

These results have implications for the design of voter education campaigns: while social networks tend to magnify cheap information and interest effects, they tend to produce free-riding when costly behavior is elicited. However, we must emphasize that these findings may be specific to conutries similar to Mozambique. The 2009 elections pit against each other the two main protagonists of the civil war that followed independence. The voter education may have brought back memories of the war and, so doing, may have raised partisanship. This is in agreement with recent experimental evidence showing that civil war increases in-group egalitarianism but reduces it across groups (*Bauer, Cassar, Chytilova, and Henrich, 2012*). Since FRELIMO had an overwhelming dominance in these elections, this raised partisanship may have activated FRELIMO voters disproportionately. This may have helped the sense that

political competition had decreased, hence leading to the free-riding peer effects we document.

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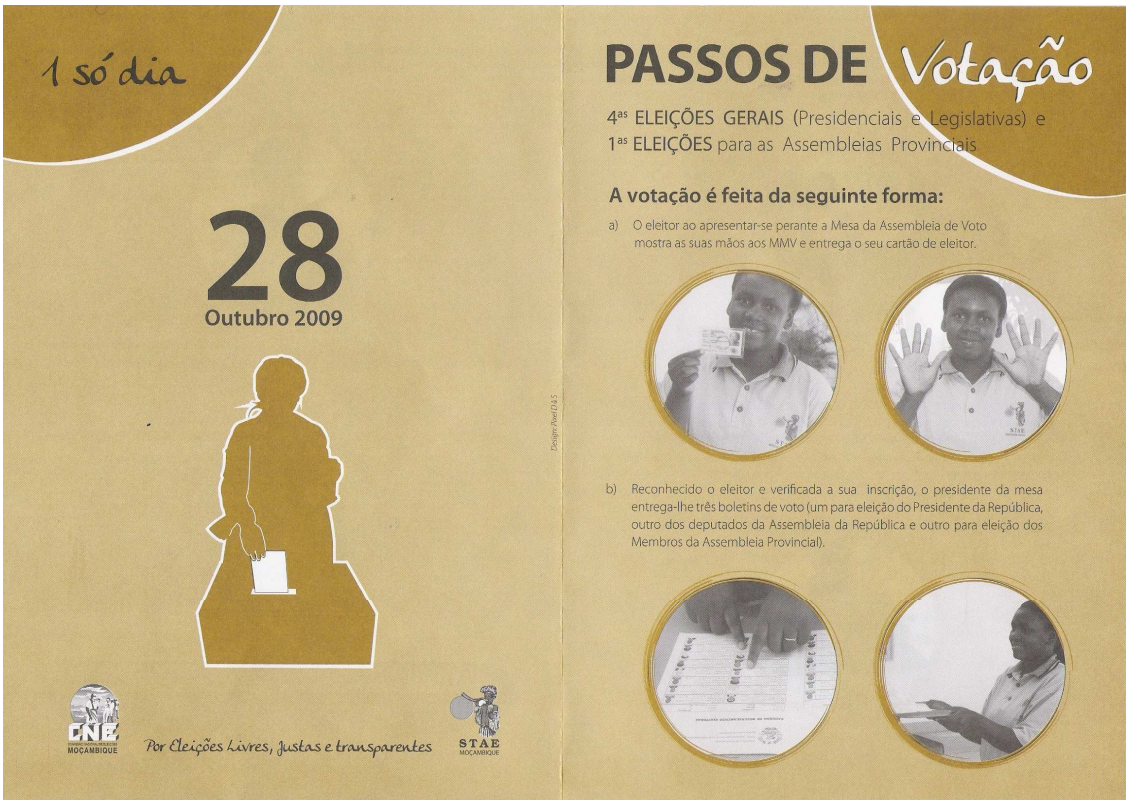
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Figure 1: Civic education leaflet by CNE/STAE



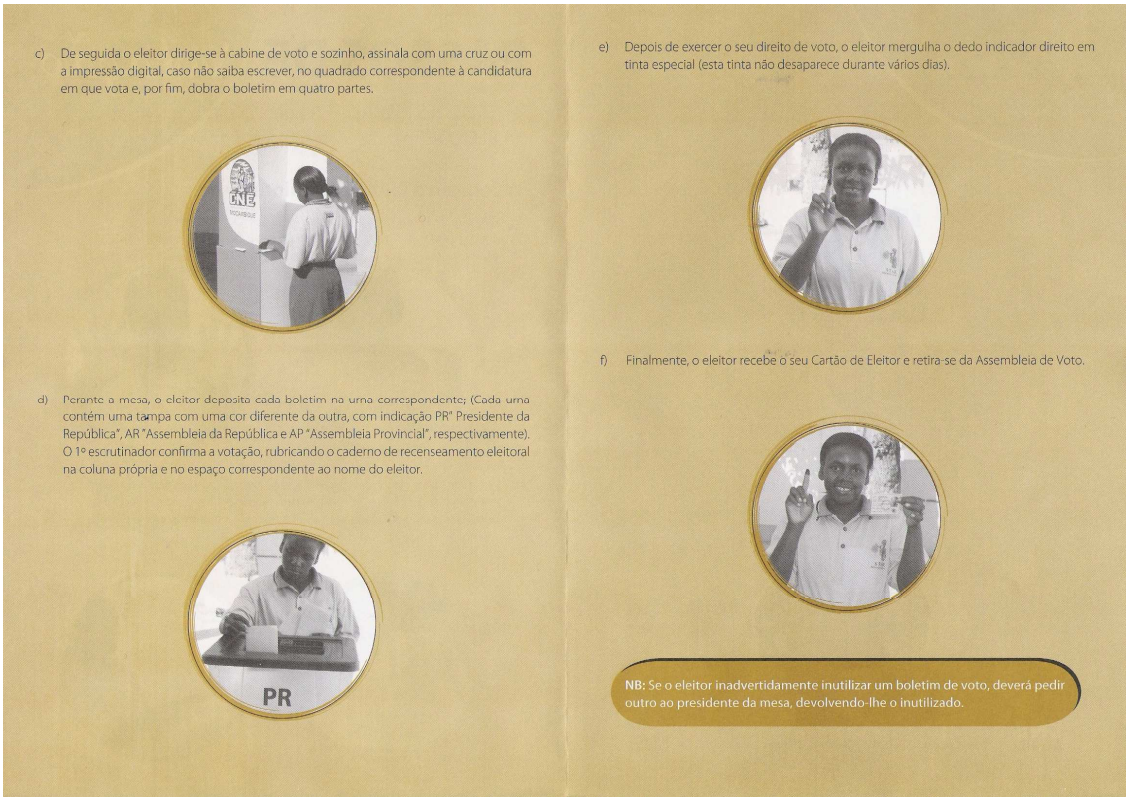


Figure 2: Newspaper @Verdade (front page – edition before election; civic education page; hotline page)



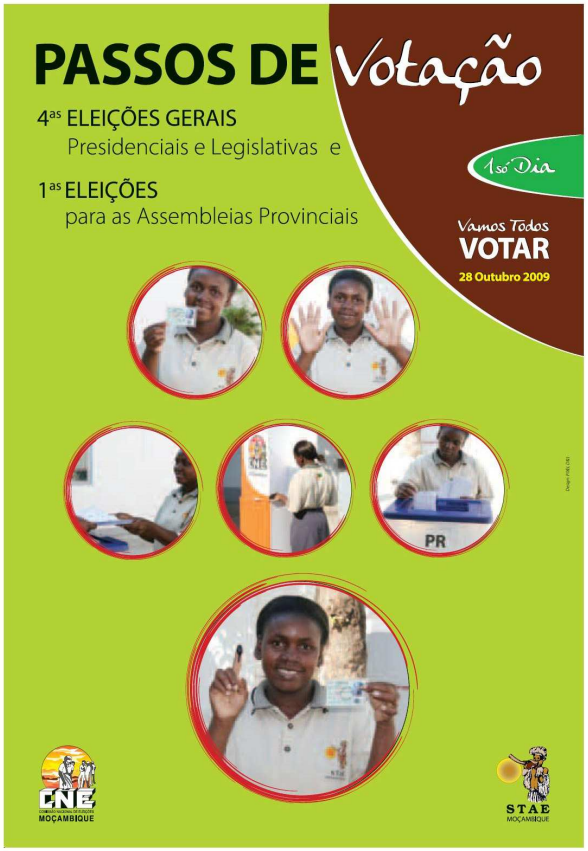




Figure 3: Hotline leaflet



Figure 4: Open Letter Leaflet



Figure 5: A map for an enumeration area

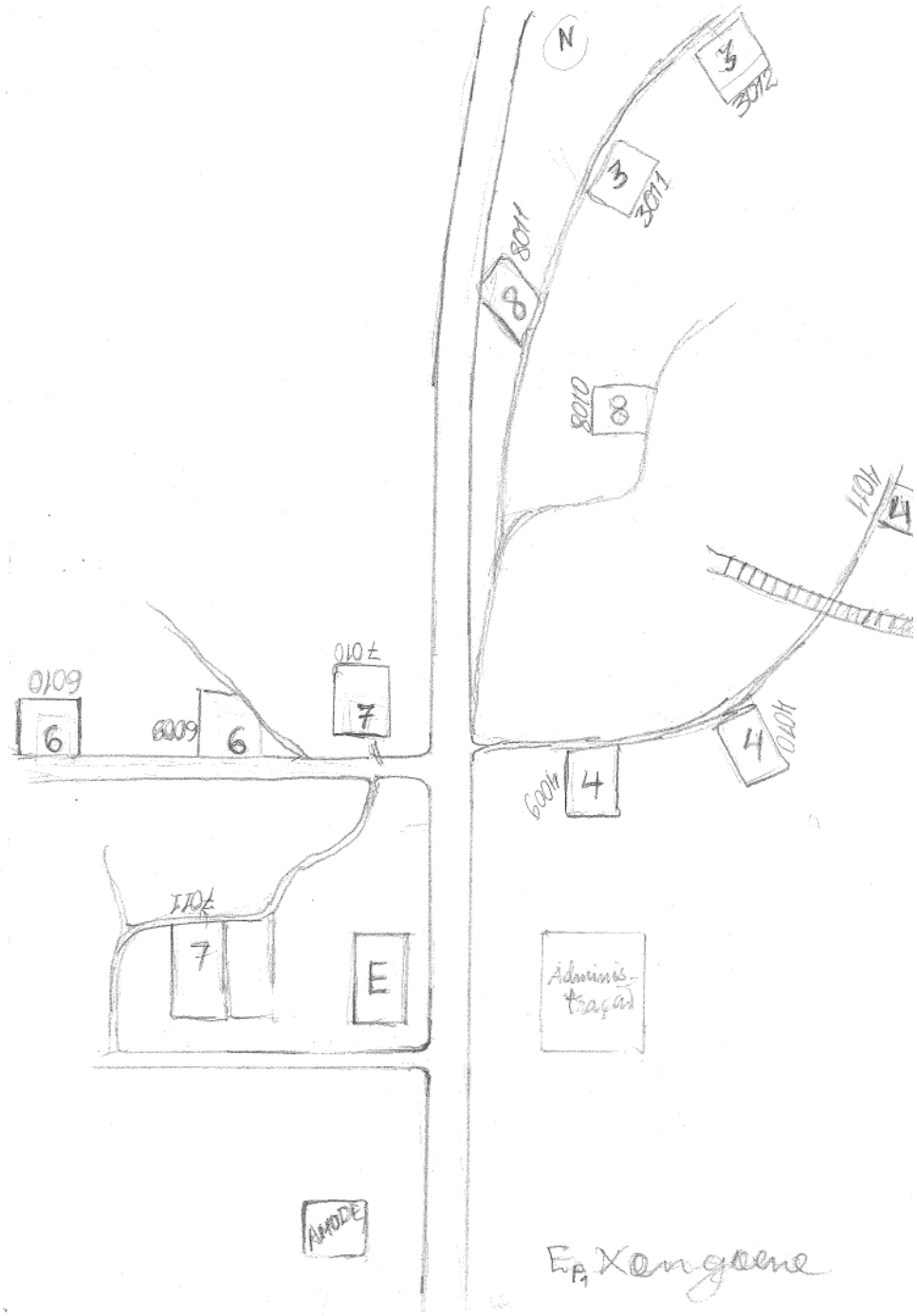


Table Appendix: Survey Outcomes

Measure	Indicators	Description / Phrasing of the question	Range / Scale	
Turnout	Self-reported turnout	Which of the following sentences best describes your situation during the 2009 Elections: (1) I was not a registered voter and I was not interested in voting; (2) I was not a registered voter but I would have liked to have voted; (3) I was a registered voter but I chose not to vote; (4) I was a registered voter but I was not able to vote; and (5) I was a registered voter and I voted.	0 - 1	Missing if (1) or (2); 0 if (3) or (4); 1 if (5)
	Adjusted turnout	Self-reported turnout adjusted by considering as non-voters those who did not answer correctly the basic questions about the elections day, namely abount the number of ballot papers and ballot boxes.	0 - 1	
	Based on ink finger	What finger was inked after voting?	0 - 1	1 if showed inked finger without hesitation
	Index 1	Composite index measuring how well the respondent aswered questions on circumstances and events during the election day	0 - 7	
	Index 2	Composite index based on the sub-group of these questions that focus on knowledge about the ballot station	0 - 7	
	Interviewer's assessment	Enumerator assessment of likelihood that respondent voted	0 - 7	
Index of Basic Electoral Information	Knows candidates' names	Do you know who were the candidates to President in the October 2009 elections?	0 - 3	
	Knows parties	Are you able to list five parties that run for the Parliament in the October 2009 elections?	0 - 3	
Index of Interest in Elections	Interest in presidential elections	With how much interest did you follow the 2009 presidential elections?	1 - 4	None-Very much
	Interest in parliamentary elections	With how much interest did you follow the 2009 parliamentary elections?	1 - 4	None-Very much
	Interest in provincial assemblies' elections	With how much interest did you follow the 2009 provincial assemblies' elections?	1 - 4	None-Very much
	Interest in public matters	With how much interest do you follow public matters?	1 - 4	None-Very much
Vote choice	Voted for Guebuza	For whom did you vote in the 2009 Presidential elections? Guebuza	0 -1	
	Voted for Dhlakama	For whom did you vote in the 2009 Presidential elections? Dhlakama	0 - 1	
	Voted for Simando	For whom did you vote in the 2009 Presidential elections? Simango	0 - 1	
	Voted for Frelimo	For which party did you vote in the 2009 Parliamentary elections? Frelimo	0 - 1	
	Voted for Renamo	For which party did you vote in the 2009 Parliamentary elections? Renamo	0 - 1	
	Voted for MDM	For which party did you vote in the 2009 Parliamentary elections? MDM	0 - 1	

Table 1a: Differences across Treatment and Control Areas - Demographics

		Baseline (Full) Sample							Post-Elections Sample						
			Targeted in treated locations			Untargeted in treated locations				Targeted in treated locations			Untargeted in treated locations		
		Control	Civic Education	Hotline	Newspaper	Civic Education	Hotline	Newspaper	Control	Civic Education	Hotline	Newspaper	Civic Education	Hotline	Newspaper
Basic demographics	female	0.546	0.021	-0.022	-0.014	0.058	0.047	-0.009	0.563	0.060	-0.024	-0.031	0.067	-0.016	-0.071
			(0.031)	(0.031)	(0.031)	(0.047)	(0.047)	(0.050)		(0.041)	(0.044)	(0.044)	(0.059)	(0.057)	(0.061)
	age	38.321	-0.993	-0.367	-2.176*	-2.055	0.993	0.578	36.957	0.280	1.713	-0.811	-1.145	2.411	1.587
			(1.297)	(1.321)	(1.209)	(1.687)	(1.759)	(1.858)		(1.329)	(1.461)	(1.394)	(1.945)	(2.027)	(2.077)
	single	0.164	0.007	0.025	0.011	0.059	0.001	0.049	0.186	-0.039	0.006	-0.042	0.036	0.001	0.024
			(0.029)	(0.029)	(0.030)	(0.063)	(0.045)	(0.051)		(0.036)	(0.041)	(0.037)	(0.080)	(0.055)	(0.065)
	married or in a union	0.717	0.011	0.014	0.034	-0.050	-0.014	-0.017	0.692	0.050	0.039	0.092**	-0.044	-0.004	0.010
			(0.035)	(0.035)	(0.032)	(0.068)	(0.054)	(0.056)		(0.044)	(0.047)	(0.040)	(0.085)	(0.061)	(0.073)
	literate	0.803	-0.020	0.017	0.036	-0.038	0.019	0.022	0.821	-0.038	-0.010	0.034	-0.062	0.007	-0.014
			(0.039)	(0.039)	(0.033)	(0.061)	(0.051)	(0.050)		(0.042)	(0.044)	(0.034)	(0.065)	(0.060)	(0.056)
	no school	0.197	0.020	-0.017	-0.036	0.038	-0.019	-0.022	0.179	0.038	0.010	-0.034	0.062	-0.007	0.014
			(0.039)	(0.039)	(0.033)	(0.061)	(0.051)	(0.050)		(0.042)	(0.044)	(0.034)	(0.065)	(0.060)	(0.056)
	informal schooling	0.071	-0.011	-0.004	-0.002	0.040	-0.026	0.017	0.082	-0.033	0.003	-0.015	0.010	-0.051*	0.023
			(0.020)	(0.022)	(0.022)	(0.039)	(0.025)	(0.033)		(0.024)	(0.027)	(0.027)	(0.043)	(0.029)	(0.044)
	primary school	0.283	0.032	-0.048	0.021	-0.073	0.039	0.029	0.276	0.031	-0.053	0.063	-0.091	0.115*	-0.013
			(0.035)	(0.031)	(0.040)	(0.055)	(0.056)	(0.050)		(0.043)	(0.041)	(0.047)	(0.061)	(0.067)	(0.061)
	incomplete secondary school	0.164	-0.009	0.014	-0.017	-0.016	0.003	-0.039	0.168	-0.013	0.003	-0.019	-0.002	-0.012	-0.046
			(0.032)	(0.033)	(0.033)	(0.045)	(0.047)	(0.045)		(0.037)	(0.037)	(0.038)	(0.056)	(0.051)	(0.054)
	household head	0.748	-0.006	-0.014	0.006	-0.019	0.032	0.065	0.742	-0.017	-0.024	0.037	-0.020	0.055	0.065
			(0.033)	(0.035)	(0.036)	(0.060)	(0.053)	(0.050)		(0.044)	(0.044)	(0.049)	(0.077)	(0.059)	(0.063)
	household size	5.657	0.321	0.414*	0.175	0.374	0.118	0.187	5.789	0.068	0.483*	0.227	0.665	0.094	-0.060
			(0.274)	(0.228)	(0.238)	(0.372)	(0.377)	(0.355)		(0.286)	(0.261)	(0.282)	(0.467)	(0.439)	(0.367)
Ethnicity	changana	0.342	0.038	0.003	0.017	-0.017	0.031	0.083	0.355	0.018	0.013	0.012	-0.034	0.005	0.031
			(0.089)	(0.087)	(0.091)	(0.094)	(0.099)	(0.100)		(0.092)	(0.092)	(0.095)	(0.099)	(0.106)	(0.107)
	macua	0.231	-0.025	-0.008	-0.036	-0.056	-0.000	-0.006	0.244	-0.035	-0.013	-0.054	-0.055	0.006	0.019
			(0.081)	(0.081)	(0.078)	(0.083)	(0.087)	(0.085)		(0.085)	(0.085)	(0.081)	(0.093)	(0.098)	(0.096)
	lomue	0.104	-0.015	-0.026	0.008	0.021	-0.049	-0.017	0.118	-0.016	-0.046	0.008	0.033	-0.056	-0.031
			(0.056)	(0.051)	(0.061)	(0.061)	(0.051)	(0.058)		(0.066)	(0.058)	(0.072)	(0.075)	(0.061)	(0.068)
	chuabo	0.093	0.015	-0.001	-0.001	-0.006	-0.005	0.007	0.100	0.010	-0.023	-0.014	-0.025	-0.007	0.005
			(0.051)	(0.051)	(0.054)	(0.051)	(0.053)	(0.058)		(0.060)	(0.051)	(0.058)	(0.053)	(0.062)	(0.062)
	chironga	0.064	-0.035	-0.033	-0.013	0.036	0.023	-0.027	0.061	-0.028	-0.027	-0.011	0.015	0.002	-0.043*
			(0.026)	(0.025)	(0.031)	(0.046)	(0.045)	(0.031)		(0.024)	(0.022)	(0.031)	(0.042)	(0.042)	(0.026)
	maconde	0.040	0.006	0.001	0.000	0.023	-0.018	-0.015	0.018	0.023	0.021	0.023	0.058	0.013	-0.000
			(0.032)	(0.034)	(0.028)	(0.042)	(0.030)	(0.032)		(0.026)	(0.032)	(0.020)	(0.052)	(0.033)	(0.021)

Note: Standard errors reported; these are corrected by clustering at the location (enumeration area) level.

* significant at 10%; ** significant at 5%; *** significant at 1%. These results come from OLS regressions.

Table 1b: Differences across Treatment and Control Areas - Demographics

		Baseline (Full) Sample							Post-Elections Sample						
			Targeted in treated locations			Untargeted in treated locations				Targeted in treated locations			Untargeted in treated locations		
		Control	Civic Education	Hotline	Newspaper	Civic Education	Hotline	Newspaper	Control	Civic Education	Hotline	Newspaper	Civic Education	Hotline	Newspaper
Religion	catholic	0.398	-0.056	-0.045	-0.052	-0.028	-0.091	-0.023	0.401	-0.024	-0.060	-0.077	0.043	-0.136*	-0.051
			(0.049)	(0.055)	(0.052)	(0.075)	(0.067)	(0.072)		(0.056)	(0.062)	(0.060)	(0.093)	(0.077)	(0.081)
	protestant	0.341	0.026	-0.004	0.015	0.067	0.044	-0.028	0.319	0.029	0.036	0.050	0.014	0.072	-0.021
			(0.060)	(0.065)	(0.064)	(0.082)	(0.077)	(0.071)		(0.063)	(0.071)	(0.067)	(0.088)	(0.086)	(0.072)
	muslim	0.206	0.003	0.035	0.038	-0.021	0.036	0.007	0.215	-0.018	0.020	0.037	-0.011	0.066	0.031
			(0.061)	(0.067)	(0.069)	(0.068)	(0.077)	(0.072)		(0.064)	(0.071)	(0.073)	(0.079)	(0.088)	(0.083)
Occupation	has a job	0.259	-0.047	0.028	-0.007	0.013	-0.039	-0.009	0.240	-0.052	0.029	-0.033	0.038	-0.053	0.058
			(0.037)	(0.042)	(0.036)	(0.058)	(0.052)	(0.056)		(0.042)	(0.049)	(0.041)	(0.068)	(0.059)	(0.067)
	agriculture	0.344	0.001	-0.033	-0.062	0.113	0.008	0.087	0.353	-0.000	-0.028	-0.064	0.055	0.022	0.058
			(0.057)	(0.060)	(0.059)	(0.078)	(0.079)	(0.080)		(0.065)	(0.070)	(0.066)	(0.089)	(0.091)	(0.090)
	retail informal sector	0.033	0.018	0.013	0.007	-0.021	0.022	-0.021	0.043	0.006	0.008	0.002	-0.025	0.004	-0.025
			(0.016)	(0.014)	(0.015)	(0.015)	(0.025)	(0.016)		(0.019)	(0.019)	(0.022)	(0.023)	(0.030)	(0.022)
	artisan	0.044	-0.017	0.002	0.010	-0.020	0.011	-0.019	0.050	-0.030*	0.001	0.017	-0.050***	-0.019	-0.015
			(0.013)	(0.014)	(0.015)	(0.019)	(0.025)	(0.020)		(0.016)	(0.019)	(0.022)	(0.013)	(0.025)	(0.028)
	unskilled worker	0.056	-0.015	0.014	0.022	0.006	0.021	-0.030	0.054	-0.013	0.006	0.018	-0.017	-0.007	-0.018
			(0.017)	(0.018)	(0.020)	(0.029)	(0.033)	(0.021)		(0.020)	(0.021)	(0.023)	(0.030)	(0.029)	(0.029)
	wage employee	0.029	-0.007	0.006	-0.006	-0.029***	-0.007	0.022	0.032	-0.004	-0.007	-0.023*	-0.032***	-0.001	0.003
			(0.012)	(0.013)	(0.013)	(0.009)	(0.018)	(0.026)		(0.015)	(0.015)	(0.013)	(0.012)	(0.025)	(0.027)
	teacher	0.044	-0.009	0.025	0.007	0.042	0.011	0.019	0.047	-0.022	0.013	-0.002	0.064	0.000	0.025
			(0.015)	(0.027)	(0.016)	(0.032)	(0.026)	(0.029)		(0.015)	(0.027)	(0.019)	(0.042)	(0.029)	(0.037)
	public servant	0.020	0.023*	0.015	0.003	0.042	0.002	0.018	0.025	0.032	0.013	-0.003	0.030	-0.010	0.011
			(0.014)	(0.013)	(0.013)	(0.032)	(0.017)	(0.023)		(0.020)	(0.015)	(0.018)	(0.032)	(0.018)	(0.027)
	student	0.031	0.021	-0.002	0.015	-0.031***	0.013	-0.006	0.040	0.018	-0.014	0.001	-0.040***	0.007	-0.022
			(0.017)	(0.014)	(0.014)	(0.009)	(0.023)	(0.020)		(0.022)	(0.019)	(0.018)	(0.013)	(0.030)	(0.022)
	domestic	0.138	-0.005	-0.027	-0.011	-0.014	-0.006	-0.036	0.147	-0.008	-0.032	-0.017	0.019	-0.038	-0.022
			(0.025)	(0.026)	(0.026)	(0.042)	(0.045)	(0.041)		(0.031)	(0.032)	(0.032)	(0.059)	(0.048)	(0.047)
Assets and expenditure	house	0.847	-0.005	0.011	-0.028	0.030	-0.023	0.003	0.853	0.008	0.027	-0.015	0.036	-0.025	-0.028
			(0.030)	(0.028)	(0.030)	(0.047)	(0.058)	(0.055)		(0.033)	(0.031)	(0.034)	(0.054)	(0.056)	(0.066)
	land	0.608	-0.018	0.044	-0.023	0.034	-0.080	0.005	0.652	-0.062	-0.028	-0.044	-0.023	-0.105	-0.056
			(0.053)	(0.048)	(0.051)	(0.073)	(0.080)	(0.082)		(0.059)	(0.060)	(0.059)	(0.088)	(0.084)	(0.090)
	cattle	0.255	0.003	-0.017	0.011	-0.020	0.020	0.020	0.254	-0.000	-0.011	0.016	-0.032	0.074	-0.044
			(0.041)	(0.044)	(0.046)	(0.062)	(0.061)	(0.073)		(0.047)	(0.052)	(0.052)	(0.072)	(0.077)	(0.073)
	cell phone	0.710	-0.008	0.046	0.018	0.031	0.071	0.103	0.706	0.007	0.046	0.006	0.090	0.091	0.083
			(0.060)	(0.059)	(0.055)	(0.074)	(0.069)	(0.067)		(0.068)	(0.068)	(0.065)	(0.081)	(0.083)	(0.080)
	expenditure	127.203	-6.942	-5.576	4.611	51.242	17.605	-2.103	122.452	-15.275	3.641	4.816	77.759	28.799	1.572
			(16.118)	(15.166)	(16.601)	(42.386)	(28.712)	(20.031)		(14.056)	(15.622)	(17.013)	(60.203)	(35.099)	(22.193)

Note: Standard errors reported; these are corrected by clustering at the location (enumeration area) level.

* significant at 10%; ** significant at 5%; *** significant at 1%. These results come from OLS regressions.

Table 2: Network variables

		Targeted in treated locations			Untargeted in treated locations		
	Control	Civic	Hotline	Newspaper	Civic	Hotline	Newspaper
chatting (0-1)	0.215	0.020	0.013	0.002	-0.025	0.015	-0.021
		(0.042)	(0.041)	(0.041)	(0.050)	(0.048)	(0.047)
kinship (0-1)	0.109	0.011	0.018	0.011	-0.034	0.043	-0.033
		(0.027)	(0.026)	(0.026)	(0.025)	(0.035)	(0.028)
distance (km)	1.218	0.071	-0.046	-0.075	0.209	-0.067	-0.040
		(0.165)	(0.154)	(0.148)	(0.177)	(0.147)	(0.153)

Note: Standard errors reported; these are corrected by clustering at the location (enumeration area) level.

* significant at 10%; ** significant at 5%; *** significant at 1%. These results come from OLS regressions.

Table 3: Differences across Treatment and Control Areas - Ballot Stations' 2004 Electoral Results

	Control	Civic Education	Hotline	Newspaper
presidential turnout	0.405	-0.018	-0.007	-0.034
		(0.027)	(0.035)	(0.029)
parliamentary turnout	0.410	-0.023	-0.002	-0.037
		(0.028)	(0.036)	(0.029)
turnout (average)	0.407	-0.021	-0.004	-0.035
		(0.027)	(0.035)	(0.029)
% blank votes	0.043	-0.005	-0.002	-0.002
		(0.006)	(0.007)	(0.006)
% null votes	0.036	-0.005	0.003	-0.001
		(0.004)	(0.006)	(0.004)
% votes for Guebuza (incumbent)	0.763	0.032	0.007	-0.016
		(0.047)	(0.048)	(0.049)
% votes for Frelimo (incumbent)	0.736	0.034	0.015	-0.016
		(0.046)	(0.047)	(0.048)
% votes for Dhlakama (oposition)	0.203	-0.030	-0.006	0.018
		(0.044)	(0.046)	(0.047)
% votes for Renamo (oposition)	0.201	-0.034	-0.016	0.012
		(0.042)	(0.043)	(0.045)

Note: Standard errors reported; these are corrected by clustering at the location (enumeration area) level.

* significant at 10%; ** significant at 5%; *** significant at 1%. These results come from OLS regressions.

Table 4a: Average effect of the three treatments on targeted respondents' political participation

	Turnout												Open letter	
	Self-reported		Adjusted		Finger measure		Index 1		Index 2		Interviewer's assessment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Newspaper	0.024	0.012	0.051	0.015	0.011	0.005	0.028	0.013	0.034	0.016	0.035	0.024	0.047	0.065
	(0.037)	(0.038)	(0.048)	(0.042)	(0.042)	(0.042)	(0.035)	(0.035)	(0.035)	(0.034)	(0.039)	(0.038)	(0.051)	(0.056)
Hotline	0.080***	0.070**	0.120***	0.094**	0.063**	0.053*	0.075***	0.063**	0.080***	0.065**	0.094***	0.085***	-0.063*	-0.039
	(0.026)	(0.029)	(0.044)	(0.041)	(0.028)	(0.030)	(0.025)	(0.027)	(0.027)	(0.027)	(0.026)	(0.028)	(0.037)	(0.041)
Civic education	0.045	0.032	0.085*	0.073*	0.056*	0.043	0.043	0.031	0.052*	0.039	0.073***	0.069**	0.018	0.032
	(0.028)	(0.030)	(0.044)	(0.042)	(0.029)	(0.033)	(0.026)	(0.027)	(0.028)	(0.028)	(0.027)	(0.029)	(0.049)	(0.057)
constant	0.859***	0.887***	0.706***	0.824***	0.807***	0.783***	0.788***	0.841***	0.757***	0.835***	0.753***	0.728***	0.175***	0.138*
	(0.021)	(0.060)	(0.031)	(0.074)	(0.020)	(0.070)	(0.019)	(0.056)	(0.020)	(0.056)	(0.020)	(0.063)	(0.029)	(0.080)
Adjusted R-squared	0.006	0.023	0.008	0.061	0.002	0.016	0.006	0.026	0.007	0.041	0.010	0.024	0.008	0.003
No. of observations	953	845	953	845	953	845	953	845	953	845	953	845	973	817
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Note: Regressions include observations for targeted (in treated locations) and control respondents. All regressions are OLS and use only second-round data. In the second column for each outcome we control for demographic characteristics, enumeration area characteristics and province dummies. Standard errors reported; these are corrected by clustering at the enumeration area level.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4b: Average effect of the three treatments on untargeted respondents' behavior outcomes

	Turnout												Open letter	
	Self-reported		Adjusted		Finger measure		Index 1		Index 2		Interviewer's assessment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Newspaper	0.088**	0.089**	0.079	0.047	0.140***	0.149***	0.088**	0.087**	0.094***	0.091**	0.142***	0.138***	0.018	0.030
	(0.037)	(0.040)	(0.064)	(0.058)	(0.037)	(0.040)	(0.034)	(0.036)	(0.036)	(0.036)	(0.038)	(0.041)	(0.066)	(0.066)
Hotline	0.090**	0.080*	0.158***	0.092*	0.092**	0.069	0.087***	0.074*	0.095***	0.068*	0.099**	0.081*	-0.048	-0.032
	(0.035)	(0.043)	(0.054)	(0.056)	(0.043)	(0.050)	(0.034)	(0.041)	(0.035)	(0.040)	(0.040)	(0.042)	(0.048)	(0.053)
Civic education	0.047	0.022	0.105*	0.071	0.061	0.052	0.053	0.033	0.065	0.045	0.042	0.023	0.048	0.043
	(0.045)	(0.050)	(0.061)	(0.058)	(0.049)	(0.055)	(0.040)	(0.045)	(0.040)	(0.044)	(0.046)	(0.048)	(0.067)	(0.071)
constant	0.859***	0.838***	0.706***	0.773***	0.807***	0.668***	0.781***	0.812***	0.757***	0.765***	0.753***	0.628***	0.175***	0.137
	(0.021)	(0.095)	(0.031)	(0.114)	(0.020)	(0.095)	(0.085)	(0.070)	(0.020)	(0.085)	(0.020)	(0.084)	(0.029)	(0.106)
Adjusted R-squared	0.008	-0.008	0.012	0.056	0.014	0.004	-0.002	0.003	0.014	0.017	0.019	0.023	-0.002	0.002
No. of observations	437	379	437	379	437	379	379	382	437	379	437	379	449	386
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Note: Regressions include observations for untargeted (in treated locations) and control respondents. All regressions are OLS and use only second-round data. In the second column for each outcome we control for demographic characteristics, enumeration area characteristics and province dummies. Standard errors reported; these are corrected by clustering at the enumeration area level.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5: Average effect of the three treatments on respondents' information and interest

	Homogeneous effects on targeted respondents				Homogeneous effects on untargeted respondents			
	Basic electoral information		Interest in elections		Basic electoral information		Interest in elections	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Newspaper	0.122	0.192**	0.008	-0.059	0.124	0.182	0.015	-0.038
	(0.095)	(0.085)	(0.104)	(0.084)	(0.135)	(0.115)	(0.181)	(0.147)
Hotline	0.165*	0.233***	0.103	0.090	0.181	0.226*	0.102	0.063
	(0.089)	(0.083)	(0.106)	(0.078)	(0.132)	(0.122)	(0.131)	(0.102)
Civic education	0.037	0.046	0.078	-0.001	0.152	0.179	-0.003	-0.080
	(0.097)	(0.101)	(0.094)	(0.063)	(0.119)	(0.128)	(0.154)	(0.133)
constant	0.000	-0.226	0.000	0.026	0.000	0.164	0.000	0.202
	(0.066)	(0.145)	(0.075)	(0.118)	(0.066)	(0.188)	(0.075)	(0.183)
Adjusted R-squared	0.003	0.062	-0.000	0.110	0.001	0.179	-0.005	0.220
No. of observations	976	865	976	865	453	395	454	396
Controls	Yes	Yes	Yes	Yes	No	Yes	No	Yes

Note: Regressions include observations for targeted (in treated locations) and control respondents. All regressions are OLS and use only second-round data. The dependent variables are indices. In the second column for each outcome we control for demographic characteristics, enumeration area characteristics and province dummies. Standard errors reported; these are corrected by clustering at the enumeration area level.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6: Average effect of the three treatments on the official electoral results (ballot-station level)

	Presidential elections				Parliamentary elections			
	Turnout	% null votes	% votes in Guebuza	% votes in Dlakhama	Turnout	% null votes	% votes FRELIMO	% votes RENAMO
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Newspaper	0.040*	-0.006	0.024	-0.014	0.045*	-0.001	0.011	-0.007
	(0.024)	(0.004)	(0.025)	(0.022)	(0.025)	(0.003)	(0.025)	(0.021)
Hotline	0.024	-0.003	0.013	-0.016	0.028	0.001	0.017	-0.017
	(0.023)	(0.003)	(0.024)	(0.021)	(0.024)	(0.003)	(0.024)	(0.020)
Civic education	0.051**	-0.004	0.026	-0.030	0.054**	-0.002	0.027	-0.049**
	(0.023)	(0.003)	(0.025)	(0.021)	(0.024)	(0.003)	(0.024)	(0.020)
constant	0.438***	0.044***	0.678***	0.225***	0.451***	0.034***	0.671***	0.181***
	(0.046)	(0.007)	(0.048)	(0.042)	(0.048)	(0.006)	(0.047)	(0.040)
Adjusted R-squared	0.432	0.217	0.580	0.568	0.417	0.160	0.599	0.648
No. of observations	161	161	161	161	161	161	161	161
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Regressions include ballot stations in control and treated locations. All regressions are OLS. We control for enumeration area characteristics and province dummies. Standard errors reported.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 7: Network effects on turnout measure based on inked finger

	reinforcement effect (targeted vs. control)			diffusion effect (untargeted vs. control)		
	chatting	**kinship**	**proximity**	**chatting**	**kinship**	**proximity**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Newspaper	0.016	0.004	0.005	0.167***	0.163***	0.161***
	(0.042)	(0.043)	(0.050)	(0.040)	(0.040)	(0.044)
Hotline	0.059*	0.052*	0.074**	0.076	0.074	0.070
	(0.033)	(0.031)	(0.035)	(0.052)	(0.052)	(0.056)
Civic education	0.048	0.041	0.055	0.081	0.074	0.022
	(0.035)	(0.034)	(0.041)	(0.052)	(0.051)	(0.064)
Network	0.268**	0.233**	0.082*	0.341***	0.302***	0.093*
	(0.128)	(0.112)	(0.043)	(0.131)	(0.112)	(0.049)
Network x Newspaper	**0.033**	**-0.207**	**-0.105***	**-0.106**	**-0.088**	**-0.033**
	(0.152)	(0.186)	(0.058)	(0.166)	(0.189)	(0.046)
Network x Hotline	**-0.125**	**-0.215**	**-0.107****	**-0.438****	**-0.231**	**-0.103**
	(0.155)	(0.162)	(0.053)	(0.216)	(0.170)	(0.082)
Network x Civic education	**-0.116**	**-0.171**	**-0.091****	**-0.075**	**0.145**	**-0.107**
	(0.173)	(0.154)	(0.043)	(0.162)	(0.231)	(0.071)
constant	0.772***	0.782***	0.783***	0.640***	0.654***	0.610***
	(0.070)	(0.070)	(0.076)	(0.098)	(0.099)	(0.100)
Adjusted R-squared	0.032	0.015	0.017	0.027	0.008	0.015
Number of observations	845	845	721	379	379	324
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: Regressions on targeted vs. control include observations for targeted (in treated locations) and control respondents; regressions on untargeted vs. control include observations for untargeted (in treated locations) and control respondents. All regressions are OLS and use only second-round data. We control for demographic characteristics, enumeration area characteristics and province dummies. Standard errors reported; these are corrected by clustering at the enumeration area level.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 8: Network effects on turnout measure Index 1

	reinforcement effect (targeted vs. control)			diffusion effect (untargeted vs. control)		
	chatting	**kinship**	**proximity**	**chatting**	**kinship**	**proximity**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Newspaper	0.020	0.013	-0.004	0.100***	0.098***	0.096**
	(0.034)	(0.035)	(0.040)	(0.036)	(0.036)	(0.039)
Hotline	0.066**	0.062**	0.068**	0.080*	0.081**	0.087**
	(0.028)	(0.027)	(0.029)	(0.043)	(0.041)	(0.041)
Civic education	0.035	0.030	0.037	0.053	0.051	0.016
	(0.028)	(0.028)	(0.033)	(0.045)	(0.043)	(0.050)
Network	0.247**	0.219**	0.064	0.270***	0.253**	0.073*
	(0.098)	(0.099)	(0.041)	(0.101)	(0.102)	(0.044)
Network x Newspaper	**-0.053**	**-0.119**	**-0.060**	**-0.116**	**-0.071**	**-0.051**
	(0.118)	(0.129)	(0.051)	(0.135)	(0.175)	(0.040)
Network x Hotline	**-0.200***	**-0.255****	**-0.069**	**-0.307****	**-0.305***	**-0.110***
	(0.113)	(0.110)	(0.046)	(0.137)	(0.163)	(0.063)
Network x Civic education	**-0.181**	**-0.254***	**-0.061**	**-0.112**	**0.116**	**-0.040**
	(0.136)	(0.131)	(0.041)	(0.118)	(0.198)	(0.060)
constant	0.834***	0.838***	0.839***	0.761***	0.769***	0.752***
	(0.056)	(0.056)	(0.062)	(0.088)	(0.090)	(0.088)
Adjusted R-squared	0.039	0.028	0.031	0.017	0.004	0.015
Number of observations	845	845	721	379	379	324
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: Regressions on targeted vs. control include observations for targeted (in treated locations) and control respondents; regressions on untargeted vs. control include observations for untargeted (in treated locations) and control respondents. All regressions are OLS and use only second-round data. We control for demographic characteristics, enumeration area characteristics and province dummies. Standard errors reported; these are corrected by clustering at the enumeration area level.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 9: Estimates of the interaction coefficients in turnout regressions using the remaining survey proxies

		reinforcement effect (targeted vs. control)			diffusion effect (untargeted vs. control)		
		chatting	kinship	proximity	chatting	kinship	proximity
		(1)	(2)	(3)	(4)	(5)	(6)
Self-reported	Network x Newspaper	-0.057	-0.135	-0.081	-0.124	-0.066	-0.073*
		(0.128)	(0.145)	(0.055)	(0.153)	(0.186)	(0.043)
	Network x Hotline	-0.220*	-0.286**	-0.093*	-0.301**	-0.363**	-0.163***
		(0.123)	(0.124)	(0.051)	(0.141)	(0.170)	(0.063)
	Network x Civic education	-0.197	-0.274*	-0.079*	-0.158	0.019	-0.079
		(0.144)	(0.145)	(0.044)	(0.132)	(0.216)	(0.065)
Adjusted	Network x Newspaper	-0.237	-0.024	-0.011	-0.056	0.197	-0.099
		(0.152)	(0.162)	(0.080)	(0.203)	(0.232)	(0.089)
	Network x Hotline	-0.275*	-0.219	-0.020	-0.368**	-0.445*	0.038
		(0.153)	(0.160)	(0.079)	(0.178)	(0.229)	(0.128)
	Network x Civic education	-0.167	-0.132	-0.006	-0.032	0.396	0.125
		(0.167)	(0.159)	(0.064)	(0.166)	(0.292)	(0.077)
Index 2	Network x Newspaper	-0.078	-0.107	-0.058	-0.126	-0.044	-0.053
		(0.116)	(0.123)	(0.053)	(0.136)	(0.164)	(0.044)
	Network x Hotline	-0.193*	-0.239**	-0.072	-0.271**	-0.281*	-0.079
		(0.110)	(0.111)	(0.049)	(0.138)	(0.160)	(0.068)
	Network x Civic education	-0.131	-0.207	-0.048	-0.092	0.189	-0.029
		(0.137)	(0.130)	(0.043)	(0.114)	(0.201)	(0.059)
Interviewer assessment	Network x Newspaper	-0.017	-0.202	-0.104**	-0.061	-0.010	-0.048
		(0.135)	(0.159)	(0.052)	(0.155)	(0.220)	(0.049)
	Network x Hotline	-0.155	-0.256*	-0.107**	-0.201	-0.247	-0.102
		(0.131)	(0.137)	(0.049)	(0.178)	(0.180)	(0.067)
	Network x Civic education	-0.223	-0.316*	-0.092**	-0.132	-0.131	-0.060
		(0.160)	(0.173)	(0.039)	(0.173)	(0.205)	(0.067)

Note: Regressions on targeted vs. control include observations for targeted (in treated locations) and control respondents; regressions on untargeted vs. control include observations for untargeted (in treated locations) and control respondents. All regressions are OLS and use only second-round data. We control for demographic characteristics, enumeration area characteristics and province dummies. Standard errors reported; these are corrected by clustering at the enumeration area level.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 10: Network effects on behavior measure open letter

	reinforcement effect (targeted vs. control)			diffusion effect (untargeted vs. control)		
	chatting	**kinship**	**proximity**	**chatting**	**kinship**	**proximity**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Newspaper	0.068	0.064	0.060	0.035	0.057	0.030
	(0.055)	(0.056)	(0.058)	(0.067)	(0.075)	(0.071)
Hotline	-0.038	-0.040	-0.043	-0.031	-0.044	-0.045
	(0.041)	(0.042)	(0.045)	(0.054)	(0.054)	(0.060)
Civic education	0.036	0.031	0.027	0.038	0.014	0.059
	(0.057)	(0.057)	(0.064)	(0.069)	(0.064)	(0.084)
Network	0.025	0.109	0.007	0.009	0.133	0.057
	(0.108)	(0.164)	(0.049)	(0.116)	(0.177)	(0.045)
Network x Newspaper	**0.049**	**-0.125**	**0.030**	**0.206**	**0.451**	**0.103**
	(0.155)	(0.194)	(0.065)	(0.265)	(0.576)	(0.083)
Network x Hotline	**0.085**	**-0.178**	**-0.003**	**0.099**	**0.223**	**0.030**
	(0.145)	(0.199)	(0.053)	(0.278)	(0.324)	(0.084)
Network x Civic education	**-0.193**	**-0.366****	**0.087**	**-0.147**	**-0.946*****	**-0.047**
	(0.152)	(0.184)	(0.066)	(0.200)	(0.358)	(0.091)
constant	0.138*	0.136*	0.075	0.127	0.111	0.016
	(0.079)	(0.079)	(0.092)	(0.106)	(0.104)	(0.114)
Adjusted R-squared	0.003	0.003	0.015	-0.005	0.015	-0.002
Number of observations	817	817	699	386	386	332
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: Regressions on targeted vs. control include observations for targeted (in treated locations) and control respondents; regressions on untargeted vs. control include observations for untargeted (in treated locations) and control respondents. All regressions are OLS and use only second-round data. We control for demographic characteristics, enumeration area characteristics and province dummies. Standard errors reported; these are corrected by clustering at the enumeration area level.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 11: Network effects on the index of basic electoral information

	reinforcement effect (targeted vs. control)			diffusion effect (untargeted vs. control)		
	chatting	**kinship**	**proximity**	**chatting**	**kinship**	**proximity**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Newspaper	0.194**	0.194**	0.192**	0.185	0.159	0.196
	(0.088)	(0.084)	(0.095)	(0.118)	(0.136)	(0.122)
Hotline	0.233***	0.236***	0.231**	0.232**	0.269**	0.232*
	(0.082)	(0.082)	(0.096)	(0.118)	(0.109)	(0.135)
Civic education	0.047	0.046	0.044	0.187	0.181	0.299**
	(0.099)	(0.099)	(0.118)	(0.128)	(0.128)	(0.123)
Network	-0.074	-0.394	0.125	-0.051	-0.357*	0.108
	(0.246)	(0.275)	(0.129)	(0.244)	(0.215)	(0.112)
Network x Newspaper	**0.274**	**0.641***	**-0.206**	**0.091**	**-0.122**	**-0.270**
	(0.368)	(0.376)	(0.145)	(0.566)	(1.171)	(0.181)
Network x Hotline	**-0.126**	**0.073**	**-0.110**	**-0.645**	**-0.698**	**-0.119**
	(0.314)	(0.382)	(0.127)	(0.590)	(0.727)	(0.182)
Network x Civic education	**-0.270**	**-0.163**	**-0.177**	**0.273**	**0.612**	**-0.207**
	(0.360)	(0.353)	(0.141)	(0.481)	(0.658)	(0.132)
constant	-0.227	-0.232	-0.211	0.153	0.183	0.126
	(0.144)	(0.145)	(0.174)	(0.181)	(0.180)	(0.213)
Adjusted R-squared	0.062	0.066	0.074	0.176	0.183	0.182
Number of observations	865	865	741	395	395	340
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: Regressions on targeted vs. control include observations for targeted (in treated locations) and control respondents; regressions on untargeted vs. control include observations for untargeted (in treated locations) and control respondents. All regressions are OLS and use only second-round data. We control for demographic characteristics, enumeration area characteristics and province dummies. Standard errors reported; these are corrected by clustering at the enumeration area level.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 12: Network effects on the index of interest in elections

	reinforcement effect (targeted vs. control)			diffusion effect (untargeted vs. control)		
	chatting	**kinship**	**proximity**	**chatting**	**kinship**	**proximity**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Newspaper	-0.034	-0.051	-0.076	-0.012	0.039	-0.056
	(0.079)	(0.079)	(0.091)	(0.141)	(0.142)	(0.148)
Hotline	0.102	0.094	0.118	0.064	0.085	0.076
	(0.071)	(0.073)	(0.087)	(0.098)	(0.095)	(0.116)
Civic education	0.006	0.006	0.005	-0.040	-0.084	0.021
	(0.060)	(0.061)	(0.065)	(0.129)	(0.152)	(0.142)
Network	-0.262	-0.344	0.010	-0.317*	-0.209	-0.054
	(0.178)	(0.270)	(0.064)	(0.167)	(0.315)	(0.087)
Network x Newspaper	**1.158*** **	**1.110*** **	**0.021**	**1.372*** **	**1.925*** **	**0.444** **
	(0.266)	(0.399)	(0.091)	(0.484)	(0.675)	(0.181)
Network x Hotline	**0.839*** **	**0.809** **	**-0.118**	**-0.077**	**-0.063**	**0.077**
	(0.234)	(0.380)	(0.110)	(0.428)	(0.634)	(0.225)
Network x Civic education	**0.445***	**0.135**	**-0.059**	**1.226*** **	**-0.043**	**0.041**
	(0.255)	(0.432)	(0.072)	(0.453)	(1.029)	(0.141)
constant	-0.010	0.004	0.009	0.176	0.147	0.247
	(0.111)	(0.115)	(0.130)	(0.176)	(0.177)	(0.204)
Adjusted R-squared	0.130	0.120	0.091	0.237	0.225	0.229
Number of observations	865	865	741	396	396	341
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: Regressions on targeted vs. control include observations for targeted (in treated locations) and control respondents; regressions on untargeted vs. control include observations for untargeted (in treated locations) and control respondents. All regressions are OLS and use only second-round data. We control for demographic characteristics, enumeration area characteristics and province dummies. Standard errors reported; these are corrected by clustering at the enumeration area level.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 13a: Average effect of the three treatments on targeted respondents' political participation when imputing missing information

	Turnout												Open letter	
	Self-reported		Adjusted		Finger measure		Index 1		Index 2		Interviewer's assessment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Newspaper	0.026	0.026	0.052	0.050	0.015	0.014	0.033	0.032	0.038	0.038	0.042	0.042	0.055	0.055
	(0.034)	(0.033)	(0.044)	(0.040)	(0.038)	(0.037)	(0.030)	(0.029)	(0.030)	(0.029)	(0.032)	(0.031)	(0.062)	(0.063)
Hotline	0.083***	0.084***	0.124***	0.124***	0.064**	0.064**	0.079***	0.080***	0.084***	0.084***	0.097***	0.095***	-0.057	-0.056
	(0.028)	(0.028)	(0.041)	(0.039)	(0.030)	(0.029)	(0.024)	(0.024)	(0.026)	(0.025)	(0.026)	(0.025)	(0.050)	(0.050)
Civic education	0.052*	0.052*	0.094**	0.093**	0.060**	0.058**	0.050**	0.050**	0.058**	0.058**	0.078***	0.076***	0.021	0.021
	(0.028)	(0.028)	(0.040)	(0.037)	(0.029)	(0.029)	(0.025)	(0.024)	(0.026)	(0.025)	(0.027)	(0.027)	(0.058)	(0.059)
constant	0.858***	0.847***	0.706***	0.705***	0.808***	0.756***	0.787***	0.789***	0.756***	0.763***	0.752***	0.688***	0.171***	0.201***
	(0.023)	(0.033)	(0.030)	(0.044)	(0.023)	(0.038)	(0.019)	(0.029)	(0.020)	(0.030)	(0.021)	(0.033)	(0.039)	(0.076)
No. of observations	1,514	1,514	1,514	1,514	1,514	1,514	1,514	1,514	1,514	1,514	1,514	1,514	1,514	1,514
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Note: Regressions include observations for targeted (in treated locations) and control respondents. All regressions are OLS and use only second-round data. In the second column for each outcome we control for demographic characteristics and province dummies. Standard errors reported; these are corrected by clustering at the enumeration area level.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 13b: Average effect of the three treatments on untargeted respondents' political participation when imputing missing information

	Turnout												Open letter	
	Self-reported		Adjusted		Finger measure		Index 1		Index 2		Interviewer's assessment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Newspaper	0.087**	0.089**	0.083	0.087	0.137***	0.135***	0.083**	0.085**	0.089**	0.092**	0.134***	0.132***	-0.008	-0.007
	(0.043)	(0.043)	(0.058)	(0.054)	(0.043)	(0.043)	(0.037)	(0.036)	(0.037)	(0.036)	(0.040)	(0.039)	(0.071)	(0.071)
Hotline	0.095***	0.096***	0.147***	0.149***	0.090**	0.087*	0.090***	0.093***	0.095***	0.098***	0.106***	0.106***	-0.054	-0.058
	(0.034)	(0.034)	(0.048)	(0.046)	(0.044)	(0.044)	(0.030)	(0.030)	(0.031)	(0.030)	(0.035)	(0.034)	(0.064)	(0.064)
Civic education	0.048	0.045	0.093*	0.094*	0.063	0.061	0.056	0.055	0.067*	0.066*	0.055	0.050	0.076	0.063
	(0.041)	(0.041)	(0.056)	(0.053)	(0.047)	(0.047)	(0.036)	(0.036)	(0.036)	(0.036)	(0.039)	(0.039)	(0.075)	(0.073)
constant	0.858***	0.895***	0.706***	0.735***	0.808***	0.763***	0.787***	0.834***	0.756***	0.799***	0.752***	0.695***	0.171***	0.192**
	(0.023)	(0.045)	(0.030)	(0.060)	(0.023)	(0.050)	(0.019)	(0.038)	(0.020)	(0.038)	(0.021)	(0.044)	(0.039)	(0.089)
No. of observations	704	704	704	704	704	704	704	704	704	704	704	704	704	704
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Note: Regressions include observations for targeted (in treated locations) and control respondents. All regressions are OLS and use only second-round data. In the second column for each outcome we control for demographic characteristics and province dummies. Standard errors reported; these are corrected by clustering at the enumeration area level.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 14: Network effects on turnout measure Index 1 using imputed data

	reinforcement effect (targeted vs. control)			diffusion effect (untargeted vs. control)		
	chatting	**kinship**	**proximity**	**chatting**	**kinship**	**proximity**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Newspaper	0.031	0.030	0.027	0.085**	0.087**	0.083**
	(0.028)	(0.028)	(0.029)	(0.035)	(0.035)	(0.037)
Hotline	0.078***	0.078***	0.078***	0.091***	0.092***	0.092***
	(0.024)	(0.024)	(0.024)	(0.029)	(0.030)	(0.029)
Civic education	0.048**	0.049**	0.053**	0.056	0.061*	0.052
	(0.023)	(0.024)	(0.024)	(0.035)	(0.037)	(0.036)
Network	0.133***	0.187**	0.012	0.210***	0.209***	0.022
	(0.037)	(0.074)	(0.009)	(0.065)	(0.074)	(0.024)
Network x Newspaper	**-0.026**	**-0.121**	**0.060**	**-0.073**	**-0.082**	**-0.019**
	(0.070)	(0.098)	(0.068)	(0.119)	(0.160)	(0.027)
Network x Hotline	**-0.089**	**-0.187****	**-0.003**	**-0.202****	**-0.228***	**-0.041**
	(0.058)	(0.091)	(0.053)	(0.103)	(0.122)	(0.041)
Network x Civic education	**-0.104**	**-0.190***	**-0.008**	**-0.119**	**-0.082**	**-0.027**
	(0.070)	(0.097)	(0.069)	(0.098)	(0.182)	(0.028)
constant	0.770***	0.774***	0.799***	0.803***	0.817***	0.857***
	(0.030)	(0.031)	(0.030)	(0.039)	(0.039)	(0.044)
Number of observations	1,514	1,514	1,514	704	704	704
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: Regressions on targeted vs. control include observations for targeted (in treated locations) and control respondents; regressions on untargeted vs. control include observations for untargeted (in treated locations) and control respondents. All regressions are OLS and use only second-round data. We control for demographic characteristics and province dummies. Standard errors reported; these are corrected by clustering at the enumeration area level.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 15: Network effects on turnout measure Index 1 controlling for potential saturation effect

	reinforcement effect (targeted vs. control)			diffusion effect (untargeted vs. control)		
	chatting	**kinship**	**proximity**	**chatting**	**kinship**	**proximity**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Newspaper	0.024	0.022	0.002	0.088***	0.089***	0.091***
	(0.028)	(0.028)	(0.031)	(0.034)	(0.033)	(0.035)
Hotline	0.068***	0.067***	0.070***	0.089**	0.091**	0.102***
	(0.025)	(0.025)	(0.027)	(0.037)	(0.036)	(0.035)
Civic education	0.040	0.040	0.037	0.058	0.064	0.039
	(0.026)	(0.026)	(0.028)	(0.043)	(0.041)	(0.051)
Network	0.235***	0.218**	0.054	0.242***	0.246***	0.048
	(0.075)	(0.087)	(0.044)	(0.082)	(0.092)	(0.045)
Predicted propensity to vote	1.078**	1.062**	0.797	0.845	0.859	0.234
	(0.502)	(0.511)	(0.637)	(0.806)	(0.813)	(0.851)
Network x Newspaper	**-0.073**	**-0.119**	**-0.037**	**-0.125**	**-0.121**	**-0.063**
	(0.097)	(0.117)	(0.054)	(0.134)	(0.203)	(0.050)
Network x Hotline	**-0.166***	**-0.230****	**-0.032**	**-0.245****	**-0.276***	**-0.070**
	(0.090)	(0.106)	(0.053)	(0.114)	(0.158)	(0.059)
Network x Civic education	**-0.156**	**-0.241****	**-0.061**	**-0.108**	**0.113**	**-0.014**
	(0.101)	(0.112)	(0.049)	(0.114)	(0.193)	(0.062)
Propensity to vote x Newspaper	**-0.485**	**-0.495**	**-0.365**	**-0.576**	**-0.528**	**0.057**
	(0.419)	(0.420)	(0.615)	(0.734)	(0.728)	(0.757)
Propensity to vote x Hotline	**-0.150**	**-0.085**	**0.030**	**-0.533**	**-0.475**	**-0.289**
	(0.433)	(0.437)	(0.556)	(0.420)	(0.405)	(0.558)
Propensity to vote x Civic education	**0.023**	**0.036**	**0.141**	**-0.894**	**-0.870**	**-1.245**
	(0.461)	(0.467)	(0.580)	(0.690)	(0.659)	(0.875)
constant	-0.151	-0.123	0.181	0.095	0.099	0.715
	(0.430)	(0.436)	(0.553)	(0.700)	(0.704)	(0.741)
Adjusted R-squared	0.047	0.037	0.038	0.030	0.020	0.029
Number of observations	946	946	803	434	434	365
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: Regressions on targeted vs. control include observations for targeted (in treated locations) and control respondents; regressions on untargeted vs. control include observations for untargeted (in treated locations) and control respondents. All regressions are OLS and use only second-round data. We control for demographic characteristics and province dummies. Standard errors reported; these were computed using bootstrap.

* significant at 10%; ** significant at 5%; *** significant at 1%.