Is Information Power?
Using Mobile Phones and Free Newspapers during an Election in Mozambique*

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Abstract:
African elections often reveal low levels of political accountability. We assess different forms of voter education during an election in Mozambique. Three interventions providing information to voters and calling for their electoral participation were randomized: an information campaign using SMS, an SMS hotline for electoral misconduct, and the distribution of a free newspaper. To measure impact, we look at official electoral results, reports by electoral observers, behavioral and survey data. We find positive effects of all treatments on voter turnout. We observe that the distribution of the newspaper led to more accountability-based participation and to a decrease in electoral problems.
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'FRELIMO did it, FRELIMO does it.'
- 2009 campaign slogan

I. Introduction

The idea of political accountability has been at the center of the development debate. The hope is that once democratic institutions are in place and reflect the interests of the majority, effective development policies focusing on the poor will be implemented. Economic theory is consistent with these claims: Becker (1983) shows that when political competition is secured, efficient policies will arise. Yet developing democratic institutions that favor the general population has been difficult to achieve in many countries. These problems have often been linked to information deficiencies (Grossman and Helpman, 1996). Empirically, the presence of the media and of local information provision have been established as conducive to the election of less corrupt politicians (Ferraz and Finan, 2008), pro-poor public policies (Besley and Burgess, 2002), and better public service delivery (Bjorkman and Svensson, 2009).

In Sub-Saharan Africa, the record of post-Soviet democratization in producing development has been mixed (e.g., Kudamatsu, 2012). One prominent concern is that elections do not discipline governments because of the many irregularities that have tainted their conduct (Chauvet and Collier, 2009; Collier and Vicente, 2012). However, some recent elections labeled as broadly unproblematic have resulted in landslide victories to incumbent parties. Elections like the Mozambican one we study in this paper suggest that dominant incumbents may have developed (licit or illicit) mechanisms to secure those victories well before the actual suffrage. Still, in the specific case of Mozambique, levels of political information are low (e.g., as pointed out by the Afrobarometer), and voter turnout has decreased dramatically over the years.

In this paper we test whether Mozambicans are responsive, both in terms of their electoral turnout and their demand for political accountability, to voter education providing neutral electoral information and calls for political participation. The voter education we study was delivered through innovative means, including text messages on mobile phones and a free newspaper that raised the attention of international media.[1] While political campaigns based on text messaging

[1] See the CNN report about the newspaper (CNN Market Place on the 16th October, 2010) at: http://www.youtube.com/watch?v=UyMozYTg3tc.

have become increasingly common in the US and Europe, to the best of our knowledge this is the first study of voter education based on mobile phones in a developing country.[2]

We designed and conducted a field experiment during the October 2009 elections in Mozambique. Polling locations in four provinces of the country were randomly selected to receive three randomized interventions. The first treatment (civic education) delivered electoral information via an official leaflet and, subsequently, a range of mobile phone messages. The second treatment (hotline) invited citizens to report electoral problems by sending text messages to pre-arranged numbers; after verification with local correspondents, the reports were disseminated primarily to treatment locations. The third treatment (newspaper) provided voter education information via free and independent newspaper @Verdade. This is the highest-circulation newspaper in Mozambique. By prior agreement with the editors of the newspaper, @Verdade included weekly information on civic education and access to a national hotline in both respects similar to our other treatments. All treatments aimed at disseminating electoral information, increasing electoral participation, and the demand for political accountability. However, the civic education treatment focused on information provision, by providing citizens with a range of details about the electoral process, whereas the hotline treatment focused on participation, by encouraging citizens to become actively engaged in the electoral process.

To measure the effects of these voter education interventions, we use the official electoral results and administrative records of electoral problems from electoral observation at the polling location level, and we employ survey and behavioral data at the individual level. Official electoral results allow an objective assessment of the impact of the treatments on voter turnout and voting patterns. Still, we were particularly careful in measuring turnout at the individual level, as survey biases are possible: in the post-election survey, we tested respondents' knowledge about the election-day voting process, as well as checked inked fingers. We also gathered a behavioral measure of demand for political accountability. Experimental subjects in all locations were invited to send mobile phone SMS proposing their priorities in terms of policy measures to the president-elect. Since sending a text message is a costly action, we interpret it as an incentive-compatible measure of demand for accountability.

[2] Mobile phone take-up in Africa increased by 550 percent in the five years up to 2009 (UNCTAD, 'Information Economy Report 2009: Trends and Outlook in Turbulent Times', 2009).

Our results provide evidence that all three treatments increased official voter turnout, by close to 5 percentage points, without statistically significant differences between the treatments. These effects are also identified in the individual data, where they are slightly larger. We observe that in particular the civic education treatment benefitted the incumbent in terms of official electoral score. We also report that the newspaper led to higher demand for political accountability: the probability that an individual sends a text message about his/her policy priorities increased by 10 percentage points. The newspaper was also the only treatment that affected the prevalence of electoral problems as reported by electoral observers; incidence of these occurrences was reduced by 0.58 problems. All treatments, with an emphasis on the newspaper, increased information about the elections. It is possible that the newspaper was particularly effective at delivering complex political information and at producing local oversight, which together could explain the effects on increasing the demand for political accountability and decreasing the prevalence of electoral problems.

Recent experimental studies have focused on voter education interventions aimed at counteracting specific illicit strategies during elections. Wantchekon (2003) and Fujiwara and Wantchekon (2013) target clientelism in Benin by studying clientelism-free political campaigning. Vicente (2014) looks at vote-buying (as cash for votes) in Sao Tome and Principe by analyzing an educational campaign against that practice. Collier and Vicente (2014) examine electoral violence in Nigeria by assessing the effects of grassroots mobilization against politically motivated violence. Ichino and Schundeln (2012) study voter registration irregularities in Ghana. Other recent experimental papers focused directly on participation and accountability. Gine and Mansuri (2011) assess the impact of a voter mobilization campaign that targeted women in Pakistan, and find a treatment effect on turnout comparable to ours. Banerjee et al. (2011) study the effects of the dissemination of information about candidate qualifications and legislator performance on electoral outcomes in India; they report an increase in voter participation. Chong et al. (2014) focus on the voter turnout effects of dissemination of information about corruption in Mexico; contrary to Banerjee et al., these authors find a decrease in voter participation. Humphreys and Weinstein (2012) analyze the effects of scorecards about legislator performance on both voter and politician behavior in Uganda.

This paper also relates to two other branches of the literature. First, it links to the vast array of experimental research on voter mobilization and electoral campaigning in American elections. This work ranges from the assessment of different voter mobilization activities (Gerber and

Green, 2000) and of partisan campaigning (Gerber, 2004), to the identification of the effects of newspapers in driving voting behavior (Gerber et al, 2009). We mention specifically the work of Dale and Strauss (2009), who look at the effect of text messages reminding citizens to vote in 2006 elections. Even though this literature for the US points to the importance of personal contact in mobilizing voters to vote (canvassing in Green and Gerber, 2000, increased turnout by 10 percentage points), Dale and Strauss still find that text messages increased voter turnout by 3 percentage points. The magnitudes of the effects on voter turnout we find in this paper are broadly comparable with this literature, as we used personal contact including the distribution of written materials, as well as follow-up text messages. Second, our paper links to the emerging literature on the effects of information and communication technology on various development outcomes. Jensen (2007) looks at the use of mobile phones to improve market efficiency in a local fish market in India. Aker (2010) studies the effects of mobile phone introduction on grain market outcomes in Niger. Other recent studies focus on information campaigns relating to health (e.g., Pop-Eleches et al., 2011, analyze a field experiment looking at text message reminders for AIDS treatment).

The paper is organized as follows. In section II we present the context of our field experiment, while providing a description of the recent political history of Mozambique. In section III we fully develop the experimental design, with treatments, sampling and assignment to treatment, and hypothesized mechanisms. We then describe our measurement in section IV, and our estimation strategy in section V. The following section (VI) provides the main econometric results. Interpretation of our findings appears in section VII. Section VIII presents robustness tests. We conclude in section IX.

II. Context

Mozambique, a country with 23.4 million inhabitants in 2009, is one of the poorest countries in the world with GDP per capita of 414 USD (current, 2009) - it ranks 221 in 228 countries in terms of GDP per capita.[3] Without prominent natural resources, and with 81 percent of the population directly dependent on agriculture,[4] it is an aid-dependent country with official aid assistance accounting for 21 percent of GNI in 2009.[5]

[3] World Development Indicators, 2014.
[4] CIA World Factbook, 2014.
[5] World Development Indicators, 2014.

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. 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 US. The civil war ended in 1992, with an agreement to hold multi-party elections. Presidential and 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 to just 36 percent in 2004. Figure 1 depicts the main election outcomes over the four elections.[6] The common factor across all national elections has been allegations of electoral irregularities (primarily ballot fraud) by FRELIMO. While these claims have been made primarily by RENAMO, international observers have corroborated them on several occasions.[7]

<Figure 1 near here>

Armando Guebuza became FRELIMO's leader and president in 2004, succeeding Joaquim Chissano. Guebuza had an important record within FRELIMO, from before independence 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. In the 2009 election he was running for re-election as president. His main opponent, Afonso Dhlakama, has been the leader of RENAMO since 1984. Dhlakama served as a guerilla leader during the civil war and RENAMO's presidential candidate at all national elections.

In this paper we study the presidential, parliamentary, and provincial assembly[8] elections of October 28, 2009. The run-up to the 2009 elections was relatively calm, with Guebuza and

[6] Since 2000, the Ibrahim Index has rated the quality of governance annually for each of the 53 countries of Africa. Over the period 2000-2009 whereas most African countries improved their governance according to this index, Mozambique experienced substantial deterioration, exceeded only by Madagascar and Eritrea.

[7] For instance, in the aftermath of the 2004 elections, the Carter Center released a statement outlining the numerous shortcomings encountered (Carter Center, 'Observing the 2004 Mozambican Elections' – Final Report). Hanlon and Fox (2006) provide convincing statistical evidence for ballot fraud during the 2004 elections.

[8] The provincial assembly elections happened for the first time in Mozambique during the 2009 round of elections. Information about these elections was very scarce. We therefore focus in the analysis of this paper on the presidential (primarily) and parliamentary elections.

FRELIMO expected to win. Prior to the elections, Dhlakama had been increasingly discredited and was widely seen as an outdated leader, as he often referred to the possibility of taking up arms. Interestingly, former RENAMO member and mayor of Beira (Mozambique's second largest city), Daviz Simango, split from RENAMO to launch MDM (Movimento Democrático Moçambicano) in early 2009. Simango was the third presidential candidate. As a member of the younger generation not directly linked with the references of the past, Simango was becoming popular among the urban youth. The main issues arising in the run-up to the election were allegations of bias in the voter registration process,[9] the exclusion of some parties (including MDM) by the National Electoral Commission of Mozambique (CNE) from contesting elections in several districts, campaign violence, and many instances of intimidation and use of state resources for campaigning.

The elections were conducted in a relatively unproblematic manner, as witnessed by national and international observers. These observers generally considered the elections to follow appropriate international standards, despite the existence of many small irregularities.[10] Results were unambiguous, giving 75 percent of the vote to both Guebuza (presidential elections) and FRELIMO (parliamentary elections). The opposition was split between RENAMO and MDM: Dhlakama/RENAMO had 16/19 percent and Simango/MDM had 9/4 percent (respectively for the presidential and parliamentary elections). This electoral outcome is indicative of the overwhelming degree of control FRELIMO has in Mozambique.

Numerous sources point to the low levels of information and political accountability in Mozambique. Freedom House currently considers Mozambique a 'partly-free' country. Afrobarometer data (see Pereira et al., 2002, 2003) find relatively low levels of support for democracy, and characterize Mozambique as a 'democracy with problems'. Citizens display clear resistance to proffer opinions about politics, and difficulty in grasping the role of democracy in improving economic outcomes. Mattes and Shenga (2008) hypothesize that the very low levels of

[9] See De Brito (2008) for a review of voter registration problems in Mozambique.

[10] The main international contingent of observers, deployed by the European Union, considered that: 'Voting was generally conducted in a calm manner and the process was well organised. [The counting] was conducted in a calm and orderly environment and was assessed as good or very good in 70 percent of the polling stations visited. […] As in 2004, the EU observed multiple cases of polling stations displaying turnouts of 100 percent and above. […] Among these with a very high turnout, results often showed 100 percent of votes cast for FRELIMO.' European Union, 'Electoral Observation Mission – Final Report, Mozambique 2009'. Observatório Eleitoral, which deployed over 1,600 national observers, wrote: '[We] give a vote of confidence to the electoral results, recognize the existence of irregularities, but consider that its correction does not challenge the probable winner.' Observatório Eleitoral, 'Declaration about the Presidential, Parliamentary, and Provincial Assembly Elections', 2009.

political accountability observed in Mozambique may be the result of deficient channels of information dissemination, exacerbated by poverty, low levels of education, and the state control of the media. De Brito (2007) underscores the marked decreasing trend of voter turnout, distinctive by regional standards in SADC countries (referred as the 'Mozambican exception'). He highlights the role of international donors in providing incentives to Mozambican politicians, perhaps at the expense of truly strengthening Mozambique's civil society. Consistently, the interventions in our field experiment aim to increase voter participation and to strengthen the demand for political accountability.

III. Experimental design

A. Treatments

We collaborated with newspaper @Verdade (http://www.verdade.co.mz/) and Observatório Eleitoral. @Verdade is a free newspaper created in 2008. It is a general-interest, privately owned newspaper, without a clear political leaning, but with a civic education and social responsibility mandate. Observatório Eleitoral is a consortium of eight Mozambican NGOs, including the main religious civil society organizations and governance NGOs.[11] Observatório Eleitoral blends the efforts of its member organizations in the area of good electoral conduct and electoral observation. The three interventions we study in this paper were designed and conducted with the institutional support and active collaboration of these organizations. Both organizations see the dissemination of information about the elections and the encouragement of voter participation as central to their missions. We now turn to the description of each specific intervention. Note that different interventions were allocated to different polling locations. Each intervention, including the full set of its activities, was directed at a specific set of experimental subjects within a location. We call these subjects the targeted individuals.

The civic education treatment provided citizens with specific information about the 2009 elections. The process was initiated with a door-to-door campaign approximately a month before the elections around selected polling locations. This campaign was implemented during the

[11] Observatório Eleitoral's members are: AMODE (Associação Moçambicana para o Desenvolvimento), CEDE (Centro de Estudos de Democracia e Desenvolvimento), CCM (Conselho Cristão de Moçambique), CISLAMO (Conselho Islâmico de Moçambique), Comissão Episcopal de Justiça e Paz da Igreja Católica, FECIV (Instituto de Educação Cívica), LDH (Liga Moçambicana dos Direitos Humanos), and OREC (Organização para Resolução de Conflitos).

baseline survey and was centered on the distribution of a leaflet designed and made available by the electoral commission (CNE/STAE). The leaflet summarized the voting process on the day of the election. 10,000 leaflets were distributed (i.e. 250 per location) to random houses in treated locations (all targeted individuals received the leaflet). The leaflet is displayed in Figure 2.

<Figure 2 near here>

Our civic education intervention modified the typical civic education approach by adding on a mobile phone dissemination component. Two weeks prior to the election, all targeted individuals in the civic education areas received a set of daily text messages on the mobile phone number they provided during the baseline survey. Specifically, they received five messages a day. On each day, messages were chosen from a group of 10 different messages. Messages focused on the importance of voter participation, as in a 'get-out-the-vote' campaign. Within their 160-character limit, these messages also provided specific information about the elections, such as the scheduled date, the types of elections taking place (presidential, parliamentary, and provincial assemblies), the presidential candidates and the parties running for the parliament, voter anonymity, and how to vote (i.e., mark only one X on each ballot paper).

The hotline treatment invited citizens to report electoral problems they observed prior to and during the election, through an SMS hotline. Two short-code phone numbers were contracted with the two mobile phone operators in Mozambique (Mcel and Vodacom) for that purpose.[12] During the baseline survey, a door-to-door campaign, providing information on how the hotline could be used, was conducted around selected polling locations. As part of this sensitization campaign, 10,000 leaflets (250 per location) were distributed to random houses in these locations (all targeted individuals received the leaflet). These leaflets provided basic information about the hotline system: short-codes, examples of problems, format of reports to be sent, and the sponsors of the initiative. The leaflet is depicted in Figure 3. Each leaflet was location-specific, so that it featured the name of the polling location corresponding to the location where the leaflet was distributed. The leaflets were also double-sided; one side provided an example of electoral problems prior to the election, whereas the other side provided an example of a problem during

[12] The two numbers were meant to cover the users of both operators. Note that the same price was agreed with both: 2 MZN (about 7 USD cents). This is the minimum price for an SMS in Mozambique – until the time of the 2009 election, there had never been free text messaging in the country.

the day of the election. The purpose of this specificity was to minimize any potential mistakes when reporting problems.

<Figure 3 near here>

A key feature of the hotline treatment is that the contents of reports were passed to the media for dissemination, and also sent via SMS to all targeted individuals in the hotline locations. This was pre-announced at the time of the leaflet distribution. Before any dissemination took place, each report received on the hotline was verified with local correspondents that were hired for this purpose in each of the hotline locations. This process was managed online through the Ushahidi system (www.ushahidi.com), which allowed viewing of received reports in real time. This software enables the received reports to be plotted automatically on a Google map after verification and classification of their contents. The archive for the messages received on our hotline is now publicly available at www.protegemosovoto.org. Note that, apart from receiving hotline reports, two weeks' prior to the elections, targeted respondents in hotline areas were sent daily SMS reminders about the existence of the hotline.[13]

The newspaper treatment was based on the distribution of free newspaper @Verdade around selected polling locations. Despite being the highest circulation newspaper in Mozambique (with a minimum of 50,000 certified copies per week), prior to this intervention, the newspaper was only systematically distributed in the city of Maputo. We agreed with the newspaper management that, specifically for this project, the newspaper would be distributed weekly around selected polling locations, which had never received the newspaper since they all lie outside the city of Maputo. This distribution was initiated with the baseline visit (September 2009) and continued until the post-election survey (November 2009). The newspapers were distributed to random houses (all targeted individuals received the newspaper). 5,000 copies of the newspaper were distributed each week, with a total of 125 in each location. Thus, this treatment was equivalent to an @Verdade subscription during the electoral period, offered to individuals who had previously not had systematic (if any) contact with that newspaper.

[13] In effect, the standard Ushahidi software was tailored in our case to enable the management of the messages to be sent by us to experimental subjects, not only for the hotline (reminder messages and dissemination of received reports), but also for the civic education messages.

The editors of the newspaper took a strictly independent approach to the electoral process, focusing its message on voter education. More specifically, the newspaper featured explicitly the contents of the civic education treatment by including a version of the CNE/STAE leaflet about the voting process on the day of the election (see middle panel of Figure 4) and by providing information on the types of elections taking place, specific candidates, political parties, and how to vote (similar to our civic education text messages). The newspaper also sponsored a national hotline for reporting electoral problems: its website, featuring an Ushahidi interface, was very popular during that period (http://www.verdade.co.mz/eleicoes2009). The newspaper's hotline was also a joint effort in that it was a replica of the hotline treatment, albeit branded with a different slogan and different short-codes to enable the identification of a control group for our hotline treatment (see right panel of Figure 4). The newspaper's hotline was disseminated through the newspaper itself, through the internet, and through networks of civil society organizations (including Observatório Eleitoral). It therefore had clear nationwide coverage.

<Figure 4 near here>

B. Sampling and assignment to treatment

Since treatments were clustered around polling locations, the sampling framework of our experiment was constructed from the electoral map of the country.[14] As the use of mobile phones was central to all our treatments, we eliminated all polling locations without mobile phone coverage. For that purpose, we obtained detailed data from the two mobile phone operators on the GIS coordinates of each of their towers. Eligible polling locations were those classified as having a mobile phone tower within a five-kilometer radius. Remarkably, 60 percent of all polling locations in Mozambique were covered by at least one operator. We selected 160 polling locations for our field experiment from our sampling framework. The registered voters corresponding to these polling locations are representative of Mozambican registered voters who have access to mobile phone coverage,[15] meaning that each registered voter in the considered universe had the same probability of having his/her polling location sampled. The selection of these locations is the product of two-stage clustered representative sampling, first on provinces (four provinces), then on polling locations (40 per province). The number of registered voters was used as sampling weight, based on information provided by the CNE/STAE in their publication

[14] We employ the 2004 electoral map, which was the one available at the time of our experiment.
[15] This was estimated at approximately 44 percent of the population in 2008 (GSM Association, 2009).

of disaggregated electoral data for the 2004 elections. During the baseline survey, in the event that we found no mobile phone coverage in any specific polling location, we replaced it by the closest polling location with mobile phone coverage. That happened in seven locations.[16]

The project took place in the provinces of Cabo Delgado, Gaza, Maputo-Province, and Zambezia. The allocation of the treatment and control groups to the full set of experimental polling locations followed a standard randomization procedure by which (i) clusters of four closest polling locations were formed within each province; and (ii) each of the three treatments was randomly allocated to one polling location in each cluster, with the remaining polling location serving as control. The final full sample of experimental locations, with each treatment represented, is depicted in the map of Figure 5.

<Figure 5 near here>

We conducted two face-to-face surveys in the catchment areas of the experimental polling locations, one before the elections, and one after.[17] Even though first-best random sampling for these surveys would be based on voter registration lists at the individual level, the Mozambican law does not allow the CNE/STAE to make these lists public. For this reason, sampling in the catchment area of each polling location - we refer to this area as an enumeration area (EA) - followed alternative random procedures during the baseline survey: namely, enumerators starting from the center of the EA (typically the polling location), sought houses with a pre-determined interval.[18] However, selection of the household was conditional on 'having access to a mobile phone' for receiving or sending messages. This criterion included households that did not own a

[16] We have one additional polling location in our control group (which is the reason we have 161 and not 160 polling locations in our experimental data): this is due to the fact that we surveyed in one substitute location that was to be unnecessary a posteriori. Results are robust to the exclusion of this polling location.

[17] The fieldwork was undertaken by four teams, contemporaneously in each province, including one supervisor per team and 31 enumerators in total. The surveys were administered mainly using electronic handhelds. The survey instruments in Portuguese are available upon request. At least one of the authors was in the field at all stages of the project and directly managed operations.

[18] To find this pre-determined interval, the procedure was as follows. We first divided the number of registered voters in the polling location by the average number of adult household members in Mozambique (three). This provided us with the approximate number of households in the corresponding polling location. We then divided the corresponding number by five, which was the number of directions in which the survey team had to walk from the center of the EA. We then divided the resulting number by two (an approximation since we aimed for interviewing 11 individuals/households per location), which gave us the pre-determined interval, i.e., the house number where enumerators stopped to recruit a respondent.

mobile phone, but had access to one via a neighbor or family member within the EA.[19] Moreover, enumerators selected household heads or their spouses. If it was not possible to secure both conditions, i.e., having access to mobile phones and being a household head or a spouse, enumerators substituted for the next house. Note that these conditions mean we do not have a representative sample of adults at the EA level. The baseline survey included 1,766 households/respondents, 11 per EA. It took place from mid-September to mid-October. The post-election survey started after the election results were announced in early November, lasting for a similar period of time. It sought the same respondents, reaching 1,154 of them.[20] We checked for selective attrition in the survey data employing several methods. We report on these results in the econometric results section of the paper.

Treatments were also randomized across individuals within each treated EA. Of the 11 individuals interviewed at baseline per treated EA, two were, on average, randomly selected not to receive the treatment. We call these experimental subjects the untargeted individuals. The remaining sampled individuals in treatment locations are the targeted individuals, who were the main targets of the treatment activities, as described in the previous sub-section.[21] Even though, our purpose was to identify indirect effects of the treatments within treatment locations,[22] in the main analysis that follows, when showing results employing individual outcomes, we classify both targeted and untargeted individuals as treated.

C. Hypothesized mechanisms

Despite the fact that all three treatments provide electoral information and attempt to mobilize voters to participate in the electoral process and to strengthen their demand for political accountability, they are likely to produce different mechanisms of impact.

[19] We verify that only 3 percent of our house calls in the baseline survey were unsuccessful because the corresponding households had no access to a mobile phone.
[20] The main specific reason for attrition in the post-survey period was reported to be the agricultural season. The rainy season in Mozambique, requiring work in the fields ('machambas'), occurs from November-January of each year. Agricultural workers often temporarily migrate for this reason.
[21] Note that leaflets (for the civic education and hotline treatments) and newspapers were distributed in much larger numbers than the ones corresponding to targeted individuals/houses. As mentioned in the previous sub-section, this distribution followed a random procedure. This was similar to the sampling rule that was used for the baseline survey. However, it deliberately left out untargeted individuals/houses.
[22] This exercise is related to the literature on the network effects of voter mobilization/education interventions (Nickerson, 2008; Fafchamps and Vicente, 2013; Gine and Mansuri, 2011).

The civic education treatment focused on the dissemination of detailed and neutral information about the electoral process. It assumes voters may not fully understand or trust the electoral process, or that they are forgetful. For those reasons, they may decide not to participate in the political process. We therefore expect that civic education increases information and trust in the electoral process, and that political participation increases as result. It is possible that, since all the information conveyed through the civic education treatment is positive (as it reassures voters about the reliability of the electoral process) and conveyed partially by CNE/STAE, which is closely linked to the incumbent party, voting for the incumbent candidate/party increases, and perceptions about electoral problems improve. It is unclear whether neutral information about the elections can affect the prevalence of electoral problems.

The hotline treatment is centered on the reporting of electoral problems during the electoral campaign and the day of the election, and therefore on citizen coordination during the electoral process. This may have a positive direct impact on political participation. Like the civic education treatment, the hotline disseminates information about the elections. This may nudge voters to seek the kind of neutral information in the civic education treatment. However, differently from civic education, the hotline channels information about electoral problems, has no branding from the CNE/STAE, and may disseminate negative information about any candidate/party. The levels of trust in the electoral process and of perceived electoral problems could improve, if the hotline produces a sense that problems are being addressed and of neutrality, or worsen, if the hotline produces a sense that the elections are problematic. If these perceptions worsen, there could even be a negative effect on participation. Effects on voting for the different candidates/party could go in any direction. It is possible that the prevalence of electoral problems decreases because of the monitoring effect that the hotline entails.

The newspaper treatment is more difficult to define, as it is a multi-faceted object, including different types of information beyond politics, which may trigger many different mechanisms of impact with experimental subjects. Still, its political message focused on voter education that included contents of the civic education and hotline treatments, which could complement each other. Hence, we expect to find similar impacts on information and political participation. We are less sure about what to expect regarding voting for specific candidates/parties, trust in the electoral process, and perceptions of electoral problems, even though, like for the civic education, the CNE/STAE sponsors some of the information. Since the newspaper provides more complex and detailed political information than the other treatments, and its frequent distribution may be

understood as local presence, we may expect a stronger impact on the demand for political accountability. It is possible that the prevalence of electoral problems decreases because of the additional scrutiny the presence of the newspaper and its hotline brings to treatment locations.

IV. Measurement

To measure the impact of our interventions, we are ultimately interested in measuring the level of political participation, through voter turnout and the demand for political accountability. Voting for the different candidates/parties and the prevalence of electoral problems are also important political outcomes. Consistently with our hypothesized mechanisms, we also need to measure the levels of political information, trust in the electoral process, and perceptions about electoral problems. We divide our description of measurement between sources of data at the level of the polling location, i.e., official electoral outcomes and electoral problems reported by electoral observers, and those at the level of the individual, i.e., survey-related measures of political behavior, a behavioral measure of the demand for political accountability, and survey measures of political information, trust, and perceptions about the elections.

A. Measures at the level of the polling location

A.1. Official electoral outcomes

We measure effects of our treatments on voter behavior primarily at the polling location level. The official results for the presidential and parliamentary elections of 2009 at the level of the ballot station were made available by CNE/STAE. Apart from voter turnout, these data include voting for specific candidates/parties, blank and null votes. These data allow us to establish whether political participation at the polls was affected by the treatments and by how much.

A.2. Electoral problems reported by electoral observers

Another outcome of interest is electoral problems. We gathered a rich dataset of informal and formal electoral observation in the provinces of Cabo Delgado, Gaza, Maputo-Province and Zambezia. Four sources of data were used for the compilation of this dataset. First, we employ the data on electoral problems received at the national hotline of newspaper @Verdade, relating to both the campaign period and the day of the elections. Second, we obtained access to the

campaign observation sheets filled by the formal national electoral observers of Observatório Eleitoral. These observation sheets were structured as a questionnaire, which asked mainly about the use of public resources for campaigning, vandalism and intimidation. Third, we were given access to the election-day observation sheets filled by the formal national electoral observers of Observatório Eleitoral. These observation sheets were also structured as a questionnaire: it asked mainly about violence and intimidation, and about procedural deficiencies at the ballot stations. Fourth, we gained access to the election-day observation sheets filled by the formal international electoral observation mission organized by UNDP Mozambique. Diplomatic personnel from a number of local embassies formed this mission. These sheets were structured as a questionnaire, asking about violence/intimidation and procedural problems of the voting process.[23]

After matching the electoral problems with our experimental locations, we coded each of the problematic locations as having had election-day misconduct, campaign misconduct, and/or violence and intimidation. We also compiled a measure of the highest intensity of electoral problems for each problematic polling location. This measure has five categories: 1 corresponds to minor problems; 2 corresponds to non-violent occurrences including campaign misconduct and election-day problems; 3 corresponds to occurrences leading to physical intimidation, including vandalism; 4 corresponds to occurrences resulting in wounded people; and 5 corresponds to occurrences resulting in killed people.

B. Measures at the level of the individual

B.1. Survey-related measures of electoral behavior

Our measurements of individual electoral behavior were gathered in the context of our post-election survey. We were particularly careful with our measurement of voter turnout.[24] We dedicated a module of the questionnaire to asking questions about all details of the election-day experience of the respondent. We construct four alternative measures of individual turnout. The

[23] We report here the number of polling locations in the experimental provinces for which problems were reported (by source): 75 (national hotline of the newspaper @Verdade during the electoral campaign and day of the elections); 157 (national observers of Observatório Eleitoral during the electoral campaign); 92 (national observers of Observatório Eleitoral during the election-day); and 36 (international observers organized by UNDP during the election-day).

[24] This is in view of existing concerns with the standard direct question on voter turnout from Afrobarometer surveys in Mozambique, which consistently overestimates actual voter turnout. See for instance the report for Afrobarometer's 2008 (round 4) Mozambican survey.

details (including coding) of the questions used for the construction of these measures of turnout are given in Table 1a. All these measures are between 0 and 1. The first (self-reported) is self-reported turnout. The second (finger knowledge) is an indicator of whether the respondent showed the index finger without hesitation when asked which finger had been marked with indelible ink after voting – dipping one finger in indelible ink was part of the official voting procedure as a way to prevent people from voting multiple times. The third (inked finger) corresponds to the observation of whether the respondent's finger was inked at the time of the interview. The fourth (interviewer assessment) is a final enumerator assessment on the likelihood that the respondent voted. Enumerators asked a number of auxiliary questions before making this judgment (see auxiliary questions in Table 1a). Some of these questions related to ballot station facts, including the format of the ballot papers and of the ballot boxes (which were not part of the information conveyed through the treatments). Enumerators were also trained to watch body language.

<Tables 1 near here>

We also gathered measures of voting for the candidates/parties. We should note however that these measures are less protected against report biases than some of our individual measures of voter turnout, as described above. With respect to voting for the candidates/parties, we rely primarily on the official results at the polling location level.

B.2. Behavioral measure of demand for political accountability

We designed 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 experimental locations, which invited them to send text messages proposing policy priorities to the president-elect for his new mandate. The leaflet is depicted in Figure 6. Like the hotline leaflet, it had two sides with two different examples of possible messages. It also included short-codes, format of the message, and sponsors. We were clear in conveying the limited extent of the initiative (a small number of experimental locations in the whole of Mozambique), and promised that the contents of these messages would reach the President in person.[25] As with the hotline, each message sent had a small monetary cost. Sending

[25] The newspaper sends a copy of the newspaper to the President every week. The newspaper added the SMS list to the newspaper in January 2010.

the message therefore represents a costly action. It was observable to us, as all mobile 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.

<Figure 6 near here>

B.3. Other survey outcomes and survey design

Our survey data also includes information for several potential mediators for the main treatment effects on behavior. We constructed indices of individual questions for: information about the elections; perceptions about the electoral commission; perceived confusion between state and ruling party; perceived electoral problems in general; and perceived vote miscounting, vote-buying, and electoral violence and intimidation, in particular. Subjective questions were approached using verbal qualifiers, with most of them featuring stepwise scales in order to ensure that questions were asked in a balanced manner.[26]

We normalize all survey-question measures using z-scores. The indices are constructed following the approach of Kling et al. (2007). We aggregate survey-question measures using equally weighted averages of the normalized variables. Table 1b displays all individual variables with original scales, as well as the corresponding aggregation. Note that, for index components, the normalization also changed the sign of individual measures in order to make them consistent with the corresponding index. According to Kling et al. (2007), this aggregation improves statistical power to detect effects that go in the same direction within a domain. 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.[27]

We also adapted our survey design in order to offer evidence of possible first reactions to the treatments and conformity biases. Experimental subjects could change their views immediately after the treatments were initiated, and could in principle adapt their survey reports about politics

[26] For example, the question on fairness of the vote count was asked in the following way: 'To what extent do you think the counting process of the October 2009 elections was fair?' The scale featured seven points. The first possible answers were read as 'fair', 'neither fair nor unfair', and 'unfair'. Depending on the respondent's answer, the scale then developed to 'extremely', 'very', and 'slightly' fair/unfair.

[27] Like in Kling et al. (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 assignment group mean.

to whatever they perceived to be the expectations of the sponsors of the treatments.[28] We asked all questions about politics after, in the middle of the interview, the treated subjects were offered the leaflets (for the civic education and hotline interventions) and the newspaper, with corresponding discussion. This way, we are able to measure whether there were first reactions to the treatments, namely to the leaflets and newspaper, by contrasting treatment and control groups for baseline values. Note that differences in past behavior or perceptions about the past are evidence of conformity.

In Figure 7 we show the sequence of the experiment including treatments and measurement.

<Figure 7 near here>

V. Estimation strategy

Our main objective is to estimate the impact of the voter education treatments on a variety of outcomes. As being assigned to a treatment does not necessarily imply that the individual received the full treatment information, we focus on intent-to-treat effects. The effect of interest (β) can be estimated through the following specification, exemplifying with data at the individual level:

$$Y_{i,l,p} = \alpha + T'_{l,p}\beta + W'_p\theta + Z'_{l,p}\delta + X'_{i,l,p}\gamma + \varepsilon_{i,l,p} \qquad (1),$$

where the variable $Y_{i,l,p}$ represents the outcome of interest of individual i in location l and province p in the post period,[29] $T_{l,p}$ is a vector of three binary variables representing the three treatments (civic education, hotline, and newspaper) with value 1 for treated units, W_p is a vector

[28] Note that conformity biases can be seen as a specific type of demand/reporting effect. The demand or Hawthorne effect is commonly defined as a form of reactivity whereby subjects modify an aspect of their behavior being measured simply in response to the fact that they know they are being studied. This is a source of bias that is potentially present in any study with human subjects, as long as subjects know they are being observed. Our individual measurements have this feature, as subjects know someone is seeing their responses. On top of these demand effects on behavior, we may have biases in reporting. These may have happened specifically in the surveys conducted. Report biases are a particular source of concern for reported voting behavior, given its sensitivity.

[29] Note that in the regressions shown in the paper we focus on simple-difference regressions not employing a possible time (before-after) dimension. Political behavior during the 2009 elections happened at one point in time, and so it was difficult to find comparable data before the treatments were initiated: previous elections had a different pool of candidates/parties; our baseline asks about intentions for the 2009 elections, which is a different object.

of province dummies, $Z_{l,p}$ is a vector of location characteristics, and $X_{i,l,p}$ is a vector of individual or household demographic covariates.[30] We cluster standard errors at the level of the EA in all regressions at the individual level. For ease of interpretation and transparency, we employ OLS estimations throughout the paper.

When employing data at the level of the individual, we consider as treated individuals both targeted and untargeted individual in treated locations. We do however run tests of the difference between direct treatment effects on the targeted individuals (in which case we contrast targeted individuals in treated locations to individuals in control locations), and indirect treatment effects on the untargeted individuals (in which case we contrast untargeted individuals in treated locations to individuals in control locations). These indirect treatment effects on the untargeted may likely be the product of social-network interactions with targeted individuals. However they may also include direct effects of the campaign due to the door-to-door distribution of leaflets and newspapers. Although leaflets and newspapers were directed to specific houses, general awareness about the contents of campaigning at the enumeration area level was difficult to restrict as fieldworkers distributing materials attracted attention.

VI. Main econometric results

In this section we present five sets of results. First, we report standard balance tests to assess the success of the randomization. Second, we analyze the effects of the interventions on electoral outcomes, using official results at the polling location level. Third, we look at individual electoral outcomes, with an emphasis on voter turnout. Fourth, we examine our behavioral measure of demand for accountability. In the end, we consider electoral problems as reported by electoral observers.

A. Balance

Tables 2 display baseline means for the control group and differences between control and treatment groups in our experiment. The statistical significance of the differences is tested to

[30] This is in line with Duflo et al. (2007), who argue that, although controls do not generally change the estimate for the average treatment effect, they can help explaining the dependent variable, and therefore typically lower the standard error of the coefficient of interest.

assess comparability across the different groups. Joint significance of the three treatments (relative to the control group) is also tested. We document these results for a wide range of observable characteristics. Table 2a shows location characteristics, mainly relating to the existence of local infrastructures. We also report in this table attrition rate differences across treatments and control. Tables 2b and 2c are devoted to the individual demographic profiles of our survey respondents. These include basic demographics (gender, age, household characteristics, marital status, schooling), ethnicity, religion, occupation, assets and expenditure. Note that these tables include not only the full (baseline) sample, but also the post-election survey sample, in order to assess the impact of panel attrition on balance of the data at the individual level. In Table 2d we display baseline electoral results for the 2004 (presidential and parliamentary) elections at the level of the polling location. In Table 2e we look at baseline individual survey outcomes. These include voting intentions for the 2009 elections, past voting in 2004, and survey outcomes relating to views about the electoral commission and about electoral problems. Like for individual demographics we present statistics for full and post-election samples.

<Tables 2 near here>

We observe few differences (at standard significance levels) between the treatment groups and the control group. In terms of location characteristics the only significant difference is that health centers are less likely to exist in newspaper areas. Importantly, attrition is not significantly different. For individual characteristics, we display a large number of estimates. However, for the baseline sample, only one occupational dummy for the civic education (public official) exhibits a significant difference. The picture is only slightly changed when the post-election sample is considered: only married or in a union for the newspaper, and two occupational dummies for the civic education show statistically significant differences. In terms of baseline electoral results for the 2004 elections, we see no statistically significant differences whatsoever across the different comparison groups. Overall, this is evidence that the randomization procedures were effective at isolating similar groups of locations and respondents, and that panel attrition did not significantly change the comparability of treatment and control groups.[31]

[31] We compute 279 differences across comparison groups and find three statistically significant differences at the 10 percent level, two at the 5 percent level, and none at the 1 percent level. We compute 93 joint significance tests and find no statistically significant ones. This pattern is consistent with what we would expect from randomized assignment.

Tables 2a-2d also provide a comprehensive description of our experimental sample. It is worth noting that the average control location has 98 percent probability of having a school but only 22 percent probability of having a sewage removal system. The average respondent in the control group was 38 years old. 80 percent of these individuals reported being literate. The main ethnicities represented were Changana (the dominant group in the South) and Macua (the dominant group in Cabo Delgado). The average expenditure per household was 127 MZN per day (just over 4 USD), and 71 percent of the households owned a mobile phone. In terms of voting, the average control location had 41 percent turnout in the 2004 elections, slightly higher than the national average in those elections.

Finally, we look at baseline outcome individual variables across treatment and control locations. We do not find any statistically significant differences, i.e., we do not observe changes with the distribution of the campaign materials and corresponding discussions at the time of the baseline survey. Hence, we do not to find evidence in favor of either first reactions (when considering voting intentions, current views, and expectations) or conformity biases (when considering reports about the past).

B. Official voting

We now turn to our main treatment effects. We start by analyzing the official voting results at the level of the polling location. Table 3 displays the effects of the interventions on voter turnout and the scores of the candidates at the presidential elections. The table also shows the treatment effects on the shares of blank and null votes. Results for the parliamentary elections are very similar, given the typical straight tickets for Guebuza/FRELIMO and for Dhlakama/RENAMO.[32] For each outcome variable we first control for provincial dummies only, and then add location controls.[33] For each regression, we test the null hypothesis of equality within each pair of treatments effects, and the null that the three treatments effects are equal to zero.

<Table 3 near here>

[32] Note that MDM did not run in the districts of our experiment. The results relating to the parliamentary elections are available upon request from the authors.

[33] These include the number of polling tables, whether the location has a school, a police station, electricity, piped water, sewage, a health center, a recreational facility, a temple, a meeting room, and paved road access.

We find clear effects of all three treatments on increasing voter turnout in the presidential election. These effects are all close to 5 percentage points, significant at the 5 percent level (p-values between 0.034 and 0.036) when including location controls (Table 3, column 2). This treatment effect magnitude represents an increase in 11 percent in the average turnout rate in control locations. These effects on voter turnout are not statistically different across the different treatments. We can also reject the hypothesis that the treatments are jointly equal to zero (when employing location controls).

Regarding the remaining electoral outcomes, we see positive effects of the treatments on the score of the incumbent (Guebuza) and negative effects on the scores of the challengers (Dhlakama and Simango). Specifically, the civic education treatment increased Guebuza's score by 5 percentage points (p-value 0.023) and decreased Dhlakama's score by 3 percentage points (p-value 0.051); the newspaper increased the score of the incumbent by 4 percentage points (p-value 0.047), and decreased Simango's score by 1 percentage point (only significant with location controls, with a p-value of 0.078). We do not observe significant effects of the treatments on the share of blank votes, but we identify negative effects of the newspaper on the share of null votes. These effects are close to 1 percentage point (significant at the 10 percent level): they are substantial as they represent 19 percent of the average share of null votes in control locations.

The increase in voter participation in the elections of October 2009, by close to 5 percentage points for all treatments, indicates that the interventions increased the marginal benefit of participating in the election. This change may have worked through extrinsic incentives related to the importance of the election and its outcome, or through intrinsic incentives related to civic-mindedness.[34]

The vote shift from the challengers to the incumbent, induced by civic education and (less clearly) by the newspaper, may be explained in different ways. First, given the limited electoral competition and the overwhelming dominance of the incumbent, in a clientelistic setting like the one in Mozambique, it is possible that the election became a turnout contest for Guebuza/FRELIMO across polling locations, with higher turnouts rewarded through post-election benefits at the local level. Then, broad voter education could mobilize voting for the incumbent.[35]

[34] See Dhillon and Peralta (2002) and Feddersen (2004) for reviews of the literature on the fundamentals of voter turnout.

[35] Recent theoretical foundations for voter turnout, as in Myatt (2012), predict an 'underdog effect', i.e., less turnout by the supporters of the incumbent and likely winner of the election than for the supporters of

Second, we note that the shifts were caused by the civic education treatment and by the newspaper, while there is no significant effect of the hotline. Civic education, and the newspaper to less extent, focused on positive messages about the election, which could be associated with the incumbent via the CNE/STAE-sponsored information. On the contrary, the hotline focused on messages reporting electoral problems. These differences across treatments may then have produced the changes in voting that we observe. This is supported by survey evidence on electoral perceptions, presented below. Third, it is possible that the treatments were not implemented in a fully neutral way, since fieldworkers may have their own views about the election and may have shown a bias in favor of the incumbent (without our knowledge). As a check of this, we estimate below the regressions for survey-based voting reports using enumerator dummies as controls.

Finally, the fact that the newspaper decreased the share of null votes can be easily explained as a direct effect of voter education. Information about the voting process is likely to have decreased the number of errors by voters when voting.

C. Individual voting

While the main voting outcomes in this paper are the official ones at the level of the polling location, we also have available voting outcome measures at the individual level. Table 4 reports on individual (survey-related) voter turnout. We employ our four different proxies of individual turnout: the simple self-reported turnout measure (self-reported), the measure based on whether the respondent showed the index finger without hesitation when asked which finger had been marked with indelible ink after voting (finger knowledge), the measure of whether the respondent's finger was inked at the time of the interview (inked finger), and the measure based on the interviewer's assessment of the likelihood that the respondent voted (interviewer assessment). For each outcome we show one regression with province dummies and one

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the challenger and underdog. A campaign analogous to voter education that increases the marginal benefit of voting can then lead to higher turnout by incumbent supporters. The same result would arise if the increase in the marginal benefit of voting only happens for incumbent supporters. This is in line with the simple clientelistic setting we allude to. Clientelism is pervasive in Mozambique, particularly in rural areas. Our survey respondents reported that local chiefs (typicaly appointed by the incumbent) were responsible for obtaining residence documents (85 percent), essential for school attendance among other benefits, for undertaking dispute resolution (88 percent), for allocating wells (70 percent), land (55 percent), public funds (43 percent), for distributing food/seeds (29 percent), and construction materials (19 percent).

regression with location and individual demographic controls,[36] in addition to the province dummies. We display tests for the equality between the different treatment effects, as a well as a joint significance test. We also test whether each treatment effect is the same when employing targeted vs. control individuals and when employing untargeted vs. control individuals.

<Tables 4 near here>

Average individual turnout in the control group using our four individual turnout measures is 88 percent for self-reported, 81 percent for finger knowledge, 24 percent for inked finger, and 75 percent for interviewer assessment. These figures are clearly different from the average turnout rate in the control polling locations, which is 44 percent (Table 3). Still, we have several plausible explanations for these discrepancies. We begin by noting the difference of 13 percentage points between self-reported turnout and the final assessment of the interviewers, which is indicative of clear over-reporting of electoral participation by the survey respondents, noticed by enumerators during the post-election survey interview. At the same time, our design assumed that measures related to inked fingers could be more objective. However, 44 percent average turnout in the control polling locations is clearly lower than 81 percent for the finger knowledge measure, and clearly higher than 24 percent for the inked finger measure. Three remarks are due here. First our survey sample is composed of heads of households and spouses who had access to mobile phones. These individuals are more active politically than the full pool of voters, as verified in Afrobarometer data for Mozambique. Still, being a household head using cell phones increases turnout by only 13 percentage points in Afrobarometer data for our experimental provinces.[37] Second, our finger knowledge measure guarantees that the respondent confidently knew which finger should have been inked during the voting. However, it is possible that survey respondents were aware of the details of the inking procedure without having voted. It is then likely that the finger knowledge measure overestimates turnout. Third, even though the inked finger measure is more reliable in principle, the post-election survey lasted for several weeks after the elections: it is likely that ink marks disappeared for some voters before they were interviewed. It is then expected that the inked finger measure underestimates turnout. Despite the fact that our individual measures of turnout have imperfections, they are likely to be complementary.

[36] Individual controls include gender, age, household characteristics, marital status, schooling, ethnicity, religion, occupation, assets and expenditure.

[37] We employed Afrobarometer data for Mozambique (rounds 3-5) to test the effect of being a head of household and/or using cell phones on reported voting in the 2004 and 2009 elections. These regressions are available upon request.

Overall, we observe clear effects of all treatments on our individual turnout measures. The size of the effects is 6-7 percentage points for the civic education treatment, 5-9 percentage points for the hotline treatment, and 7-10 for the newspaper treatment. All treatments are statistically significant for the inked finger (at the 5 or 10 percent levels) and for the interviewer assessment (at the 1 or 5 percent levels). Note however that the civic education and the newspaper treatment affects are not statistically significant for the self-reported measure (p-values 0.158 and 0.237 with controls), and that the newspaper treatment effect is not statistically significant for the finger knowledge measure (p-value is 0.178 with controls). For most regressions, there are no statistically significant differences across the treatments, and the three treatments are jointly different from zero. When comparing treatment effects for the targeted and for the untargeted (vs. control), we find that the only systematic difference is for the newspaper.[38] We conclude that consistently with the polling location results, all interventions seem to have had a considerable impact on voter turnout, generally larger than the one found with the official results. This is likely due to the fact that targeted individuals, who were more exposed to the interventions than average voters in the same locations, form most of our survey sample.

We also analyze the effects of the interventions on self-reported voting for the presidential candidates. These are presented in Table 5. We display regressions with province dummies, regressions adding location and individual controls on top of province dummies, and regressions adding enumerator dummies on top of all other controls.

<Table 5 near here>

Consistently with the official results at the polling location level, we find, in our regressions including location and individual controls, for the civic education treatment, a positive effect on voting for the incumbent (6 percentage points, significant at the 10 percent level) and a negative effect on voting for Dhlakama (2 percentage points, significant at the 10 percent level). Differently from the official results we do not find statistically significant effects for the newspaper treatment, and we find a statistically significant positive effect of the hotline treatment on voting for the incumbent (6 percentage points, significant at the 10 percent level).[39] We also

[38] Indeed, the newspaper has a larger effect for the untargeted. A possibility is that not receiving the newspaper on one's hands raised additional interest about the newspaper received by neighbors.

[39] The positive effect of the hotline on voting for the incumbent (which we do not find at the level of the polling location with official results) may be related to the violence perpetrated close to some of our

note that adding enumerator dummies to our specification does not change treatment effects significantly. This is reassuring as conformity biases could be particularly prominent in survey reports of voting for the candidates. In addition, one possibility was that the treatment effects on voting for the candidates at the polling location level (Table 3) were produced by biased fieldworkers. We do not find evidence consistent with this concern.

D. Demand for political accountability

We now turn to the effects of the interventions on respondents' demand for political accountability, as measured by our open letter behavioral measure. The open letter invitation reached all respondents in our post-election survey. Respondents were instructed to send their policy priorities via SMS to the new president-elect. Any message sent can reasonably be interpreted as representing demand for political accountability. We matched the mobile numbers that sent SMS with those recorded for the survey respondents, and therefore are able to construct a dummy variable with value equal to one for those experimental subjects who sent a message through our open letter arrangement. We run regressions with and without location and individual controls. The results are displayed in Table 6.

<Table 6 near here>

We first note that 15 percent of the experimental subjects in the control group sent at least one message through the open letter.[40] This represents a clear degree of adherence to the initiative. When looking at the difference between the treatment and control groups, we find that individuals in both the civic education and newspaper treatment locations were more likely to send a text message to the open letter system. These effects are not statistically different from each other. However, we only find a statistically significant impact for the newspaper treatment, which is 9 percentage points (p-value 0.063). We can also observe that treatment effects are not statistically different for the targeted and for the untargeted. We may then conclude that only the newspaper clearly increased the demand for political accountability as measured by our open letter. It is likely that the treatment substance relating to political accountability was relatively complex and

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experimental locations in Cabo Delgado by the main RENAMO convoy during the electoral campaign, as this occurrence was specifically reported through the hotline treatment. The information was disseminated through targeted individuals in our survey.

[40] 48 percent of the messages received on our open letter system were matched to respondents' mobile phone numbers, as reported in the surveys.

required detailed information, as provided by the newspaper. It is also possible that the frequent newspaper distribution created local presence, which facilitated the demand for political accountability.

E. Electoral problems

We now turn to electoral problems as reported by electoral observers during the electoral period of October 2009. Table 7 presents treatment effects on electoral problems. Using data on electoral problems described above, we classified each problematic polling location in our experiment as having had election-day misconduct, campaign misconduct, and/or violence and intimidation. We are thus able to count reports for each type of problem at the level of the polling location. This is the way we compose incidence measures for each type of problem. We also employ a measure of intensity of problems by classifying each problematic polling location in terms of the most serious problem that it had: we apply the 1-5 scale we described above, from minor problems to occurrences resulting in dead people. Polling locations that had no electoral problems are given the score of 0. We display a specification using province dummies and one specification that adds location controls to the province dummies.

<Table 7 near here>

First of all, when looking at the control polling locations, we can report on average almost one problem per location (0.95). However, the overall average intensity is fairly low (0.82 on the scale of 0-5). The incidence of electoral problems was higher for campaign misconduct than for election-day misconduct, or violence and intimidation. When considering incidence of any type of electoral problems, we find that all treatments had negative effects, i.e., they decreased the number of problems. However, only the newspaper treatment effect is statistically significant: it leads to 0.58 less problems (p-value is 0.071). We find a similar pattern for the intensity score, where, again, only the newspaper treatment is significant: it decreases the intensity of problems by 0.47 points, a 57 percent decrease relative to the average score in the control group (p-value is 0.054). Note that the effect of the newspaper on intensity is statistically different from those of the other two treatments. Looking at the incidence of specific problems, the only significant effect is that of the newspaper treatment, and only for the incidence of campaign misconduct. The magnitude of this effect is 0.51 problems (significant at the 10 percent level). Overall, we find that the newspaper decreased the incidence and intensity of electoral problems. This is

particularly the case for campaign misconduct. These findings suggest that the newspaper was particularly effective at improving politician behavior locally, possibly because of increased perceived monitoring, in line with the idea of political accountability it was able to convey.

VII. Mechanisms

Our main results suggest that the voter education interventions were equally effective at increasing voter turnout, and that the civic education and (to a lesser extent) the newspaper shifted the vote towards the incumbent and against the challengers. In addition, the newspaper increased the demand for political accountability, and reduced the number and intensity of electoral problems (particularly during the electoral campaign). We now revisit our proposed mechanisms in view of this evidence, while offering additional survey evidence.

In Tables 8, we look at measures of information about the elections, perceptions about the electoral commission, perceived confusion between state and ruling party, perceived electoral problems in general, and perceptions about vote miscounting, about vote-buying, and about electoral violence and intimidation. All measures are normalized as z-scores. Most measures we analyze are aggregations of different survey questions as described in Table 1b. As before, for each dependent variable, we show a specification with province dummies only, and one specification adding location and individual controls.

<Tables 8 near here>

We hypothesized that the civic education treatment would increase information and trust in the electoral process. As discussed, both are conducive to increased voter turnout (as we found), either through extrinsic motives related to the electoral outcomes, or through civic mindedness. We verify that, indeed, civic education increased our index of information about the elections by 0.16 standard deviation units (significant at the 1 percent level). We also observe that trust in the electoral commission increased by 0.2 standard deviation units (significant at the 5 percent level). Since the CNE/STAE is closely associated to the incumbent, the civic education may have then increased trust in the incumbent and its vote share. This is one of the possible explanations that we proposed above for the changes in voting for the candidates produced by civic education (the main other being that incumbent-related clientelism could be the main reason to vote). Since this treatment reassured voters about the reliability of the electoral process, we also anticipated that

perceptions about electoral problems could improve as a result. Consistently, we see a reduction in the perception of vote miscounting and the perception of electoral violence and intimidation, both by 0.14 standard deviation units (respectively at the 10 and 1 percent levels of statistical significance).

The hotline treatment was expected to have a positive direct impact on political participation, since it coordinated citizens' participation in the electoral process through the hotline system. We indeed found a robust effect of the hotline on voter turnout. The hotline was also anticipated to have a positive effect on the level of electoral information, through nudging the importance of the election. Consistently, we find that the hotline increased individuals' information about the elections by 0.18 standard deviation units (significant at the 1 percent level). Effects on trusting the electoral system and on the level of perceived electoral problems could go either way, as the hotline conveyed information about electoral problems, and voters could either feel reassured that problems were being resolved through neutral media dissemination, or feel the election was particularly problematic. We find that the hotline treatment increased trust in the electoral commission by 0.14 standard deviation units (significant at the 10 percent level with controls). Moreover, it increased the perceived neutrality of the electoral commission by 0.18 standard deviation units (significant at the 5 percent level), and reduced the confusion between the state and the ruling party by 0.22 standard deviation units (significant at the 1 percent level). The fact that the hotline reported misbehavior by the supporters of all candidates may have contributed to these effects. The CNE/STAE may have been associated with the hotline despite not being a sponsor. The positive effects on trust and neutrality are likely to have reinforced effects on voter turnout. Still, we find an increase in the perception that elections were problematic, by 0.19 standard deviation units (significant at the 5 percent level). We did not find effects of the hotline on voting for the candidates in the official results at the level of the polling location. This is likely due to the fact that the hotline brought neutrality to the electoral process, differently from the civic education.

While citizens can perceive the newspaper treatment in a specific way, it included both the information conveyed by the civic education and a national hotline similar to the experimental one. We therefore expected the hotline treatment to have impacts similar to the ones encountered for the other treatments (when they were similar among them). We found a clear effect on voter turnout. The newspaper was also the only treatment to decrease the share of null votes, pointing to strong information effects. Consistently, we find an increase in our index of information about

the elections by 0.16 standard deviation units (significant at the 5 percent level). We also find that the newspaper increased trust in the electoral commission by 0.17 standard deviation units (significant at the 5 percent level), improved the perception of neutrality of the electoral commission by 0.15 standard deviation units (significant at the 10 percent level with province dummies only), and decreased confusion between state and ruling party by 0.21 standard deviation units (significant at the 1 percent level). Effects on perceptions of electoral problems are mixed, like the ones we encountered for the two other treatments: while the perception of electoral violence and intimidation decreased (by 0.07 standard deviation units, similarly to what we found for the civic education treatment - the difference between the two effects is not statistically significant), the perception of campaign money misbehavior increased (by 0.14 standard deviation units).[41] The fact that the newspaper had a positive effect on voting for the incumbent (even though less robust than the one for the civic education) may be explained by the sponsoring of some information by CNE/STAE, analogously to the civic education treatment effects. Finally, improved trust and neutrality in the electoral process, together with the ability of the newspaper to convey complex messages on a frequent basis, can justify the positive impact of the newspaper on the demand for political accountability. This effect, by increasing local scrutiny, may be driving the decrease in the prevalence of electoral problems that we find.

VIII. Robustness

We now turn to robustness exercises. We begin by reporting robustness tests for panel attrition. We then discuss some additional evidence we have testing for conformity biases.

We begin by noting that panel attrition is necessarily of limited importance for our results, as it only relates to our individual-level data. Despite this fact, we undertook a number of exercises checking the robustness to panel attrition. First, as mentioned above, we verified that attrition rates are not statistically different across treatment and control groups (Table 2a). We also checked balance of baseline individual characteristics (Tables 2b-2c) and baseline outcomes (Table 2e) for the post-election survey sample: we found few statistically significant differences

[41] While the newspaper produced the only significant effect on electoral problems reported by electoral observers, the diferences in survey perceptions, i.e., improvements for violence/intimidation and deterioration for vote-buying, still do not correspond to the ones encountered by electoral observers, i.e., improvements in campaign misconduct. Note however that electoral observation sheets did not include questions on vote-buying. Hence, the perceptions about vote-buying do not have an obvious comparison term in the electoral observation data that we employ.

across treatment and control groups.[42] In Table 9, we employ a method of sensitivity analysis proposed by Lee (2005) whereby best-case and worst-case scenarios for differential attrition are constructed by trimming the top or the bottom of the distribution of the outcome in the group with less attrition (which in our case is always the treatment group). We display treatment-effect bounds for our main individual outcomes of interest, including turnout measures, open letter, and voting for the presidential candidates. We show mean differences between treatments and control as reference points. We find that turnout effects are positive and typically significant for the upper bounds. They are all positive even though sometimes not significant for the lower bounds. Overall, these results reassure us about the positive turnout effects we found for all three treatments, when employing individual data. The positive effect of the newspaper on sending of messages for the open letter also appears here, even though the lower bound is not significant. The positive effects of the civic education and of the hotline on voting for the incumbent and the negative effect of the civic education on voting for Dhlakama are also confirmed (even though we find one of the bounds for each effect not to be statistically significant).[43] We conclude that attrition does not seem to substantially affect our results.

<Table 9 near here>

We also checked the possibility of treatment contamination to nearby untreated polling locations. This is a test of conformity bias, as citizens living in untreated polling locations were not given voter education materials or sent voter education text messages. They did not have direct contact with campaigners. No contamination would be consistent with conformity. We regressed our main outcomes (turnout and voting for the candidates at the level of the polling location; turnout and open letter at the level of the individual) on distance to closest treatment polling location or on a dummy variable for treatment in the same district (distinguishing by treatment), while employing observations from untreated locations only.[44] We find clear evidence of positive

[42] We also verify the characteristics of the panel drops (results available upon request). The only significant demographic characteristics are household size (lower for panel drops), having a job (higher for panel drops), being an unskilled worker (higher for panel drops) and owning a house (lower for panel drops). We also verify that these characteristics do not correspond to the few new unbalanced characteristics across treatments and control in the post-election sample.

[43] We also employed multiple imputation by chained equations, as an alternative robustness exercise for panel attrition. This method assumes data are missing at random. The corresponding results are available upon request. When considering treatment effects on turnout, the open letter, and voting for the candidates, most results are maintained (namely on turnout and the open letter), even though we generally find lower point estimates, with lower levels of statistical significance.

[44] These results are available upon request to the authors.

contamination for all treatments. It is then possible that some of these treatment effects are underestimated.

IX. Concluding remarks

We have analyzed the impact of three types of voter education interventions in the context of the 2009 Mozambican elections. Mozambique has been marked by low voter turnout and weak political accountability. The three voter education interventions were: a civic education campaign based on the distribution of an official leaflet and the sending of text messages conveying neutral information about the elections; an SMS hotline that received and disseminated information about electoral problems; and the distribution of a free newspaper focusing on civic education and embedding a national hotline for electoral problems. We find that all three treatments increased the level of information about the election and the level of trust in the electoral process. As a likely implication, all treatments increased voter turnout. The free newspaper was particularly effective in increasing the demand for political accountability and in decreasing the prevalence of electoral problems. Probably because of its official sponsorship, the civic education increased voting for the incumbent. The civic education improved and the hotline deteriorated perceptions about electoral problems.

In a moment where many African elections have become less violent, less dependent on obvious vote-buying, and less fraudulent (if we understand fraud strictly as a voting-day possibility), it is important to understand why incumbents have been reinforcing their positions. While there is value in making elections more transparent and in tackling specific electoral problems, those efforts may not suffice to realize genuine electoral competition. Incumbents may have learnt ways to bend the electoral system in their favor, well before election-day by taking advantage of weak accountability. While education levels may take generations to change, voter education, specifically oriented to increase political participation and the demand for policy-accountability, may be an effective tool to improve the political incentives for development. In designing voter education, this paper has shown that the use of information and communication technologies, recently available and expanding in the African context, as well as of social enterprise innovations like free newspapers, may open new and effective avenues for long-term building of a more relevant citizenry.

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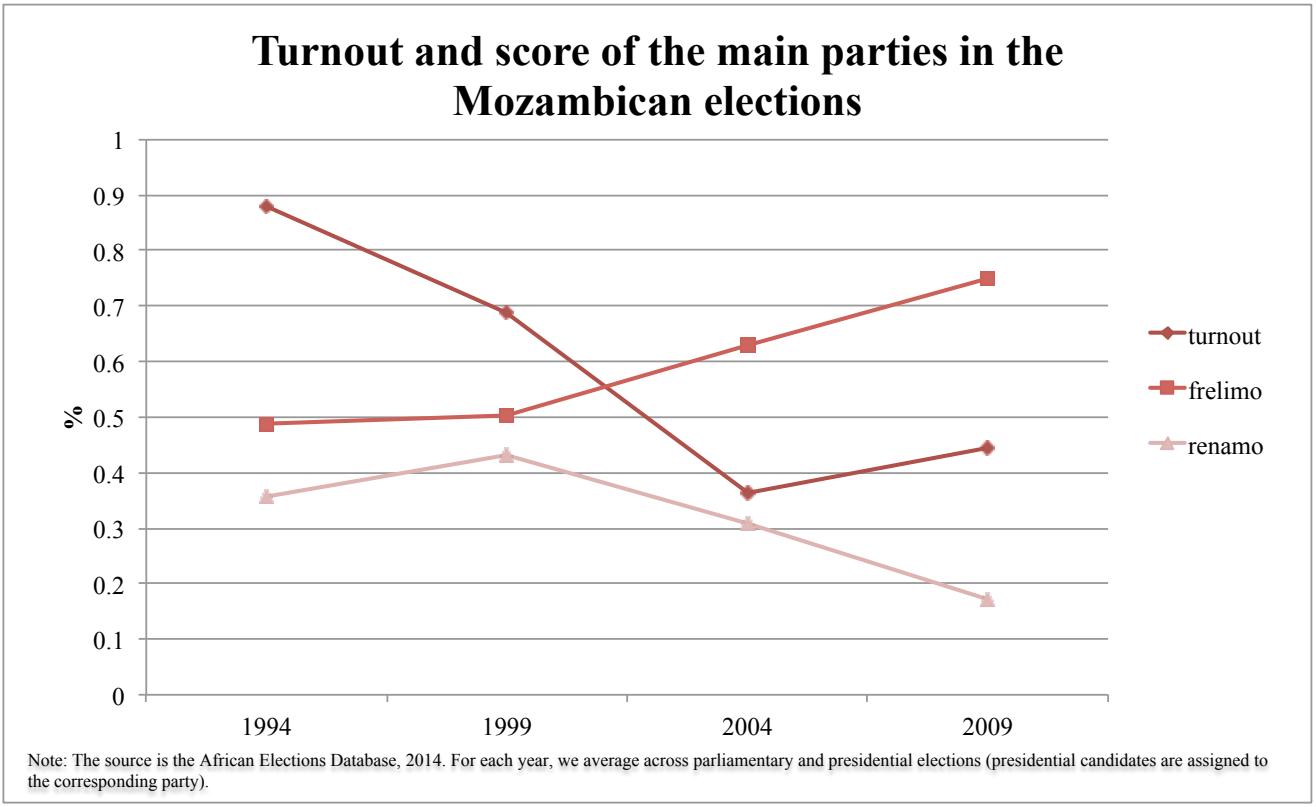
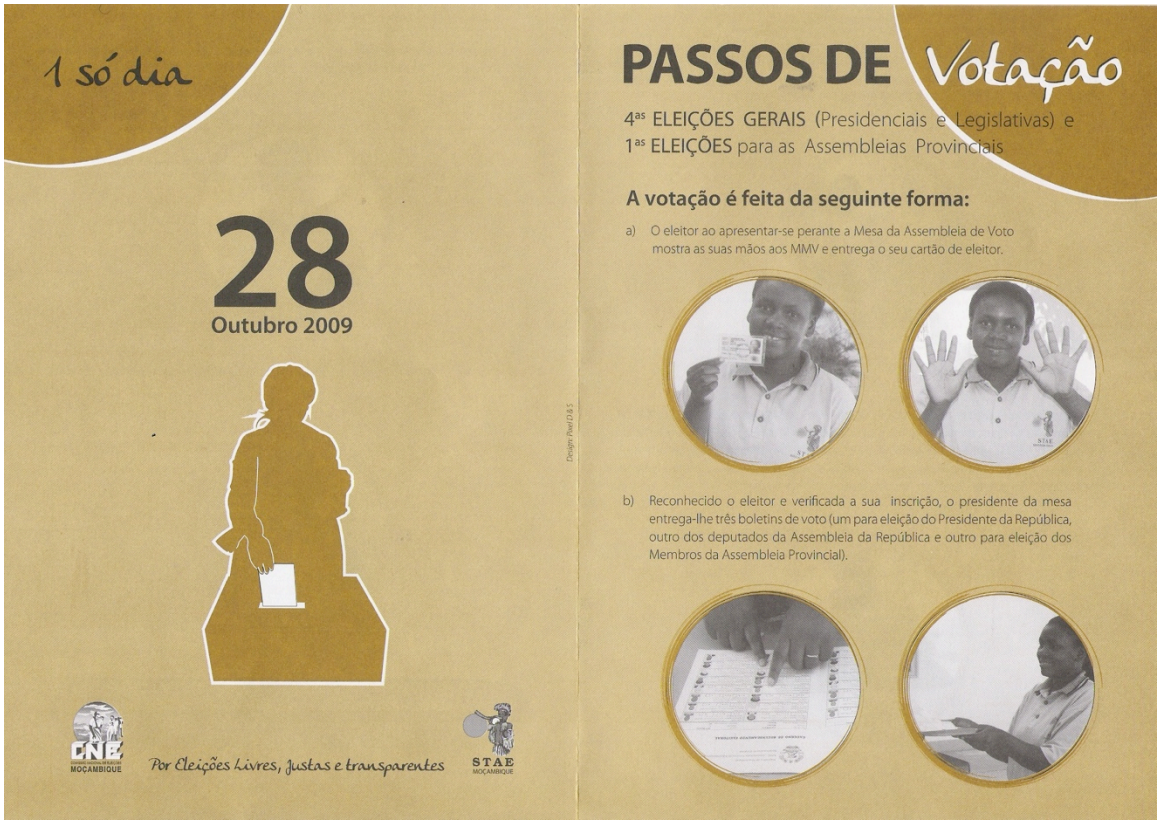
Appendix

Figure 1: Turnout and score of the main parties in the Mozambican elections



Note: The source is the African Elections Database, 2014. For each year, we average across parliamentary and presidential elections (presidential candidates are assigned to the corresponding party).

Figure 2: Civic education leaflet by CNE/STAE



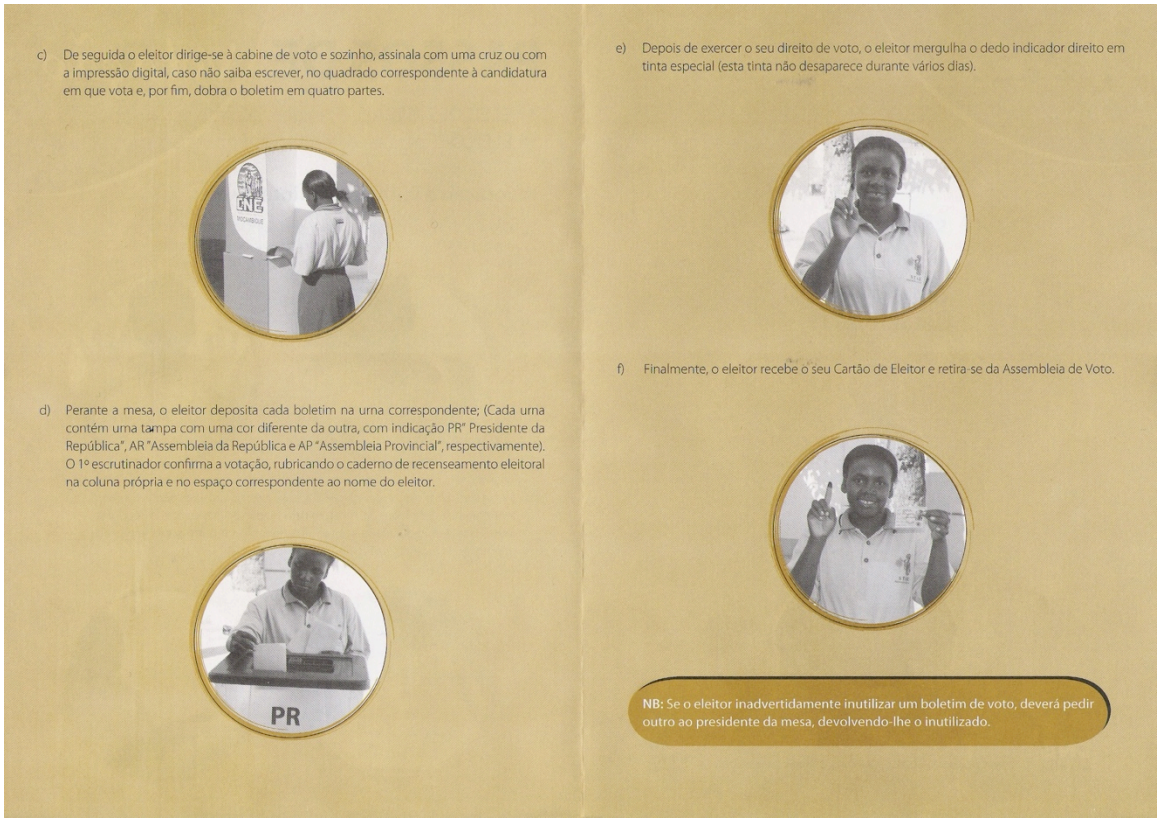
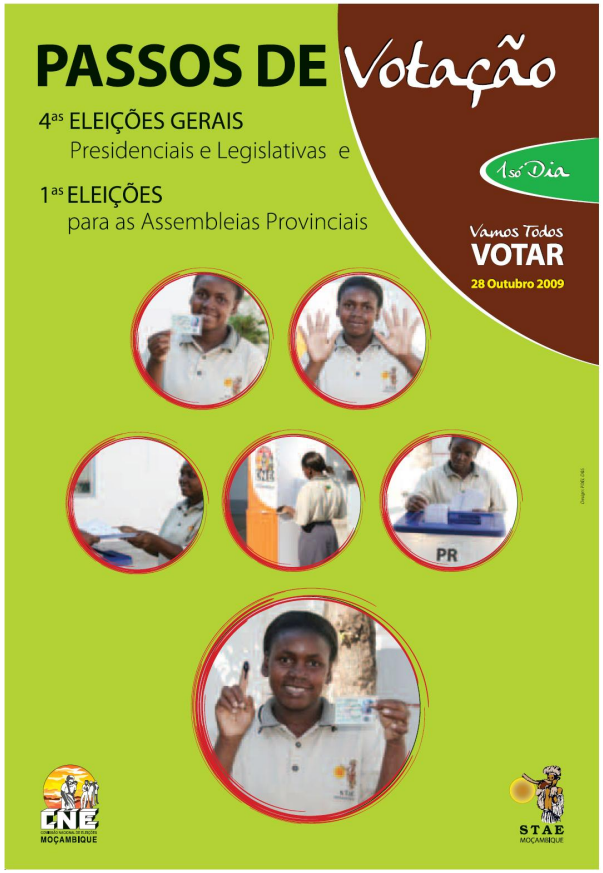


Figure 3: Hotline leaflet

Figure 4: Newspaper @Verdade (front page – election-week edition; civic education page; hotline page)



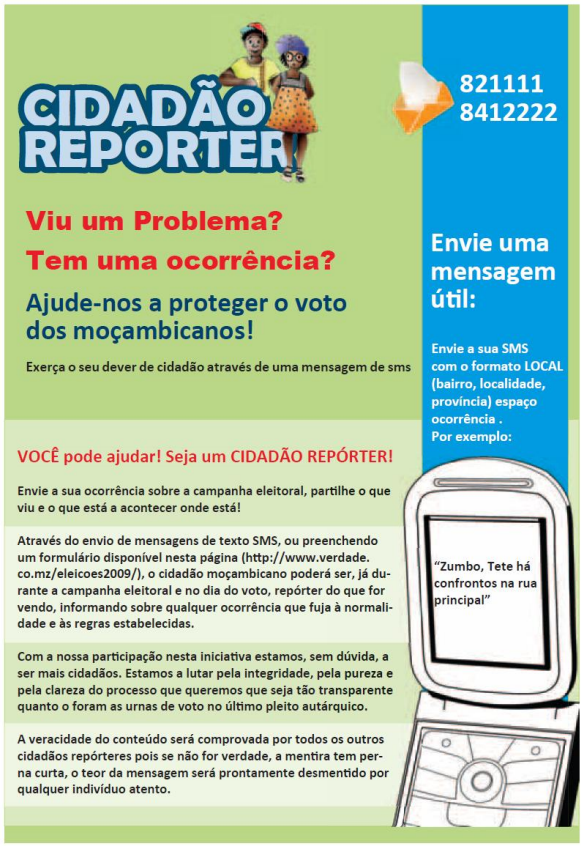


Figure 5: Experimental locations in Cabo Delgado, Gaza, Maputo-Province, and Zambezia

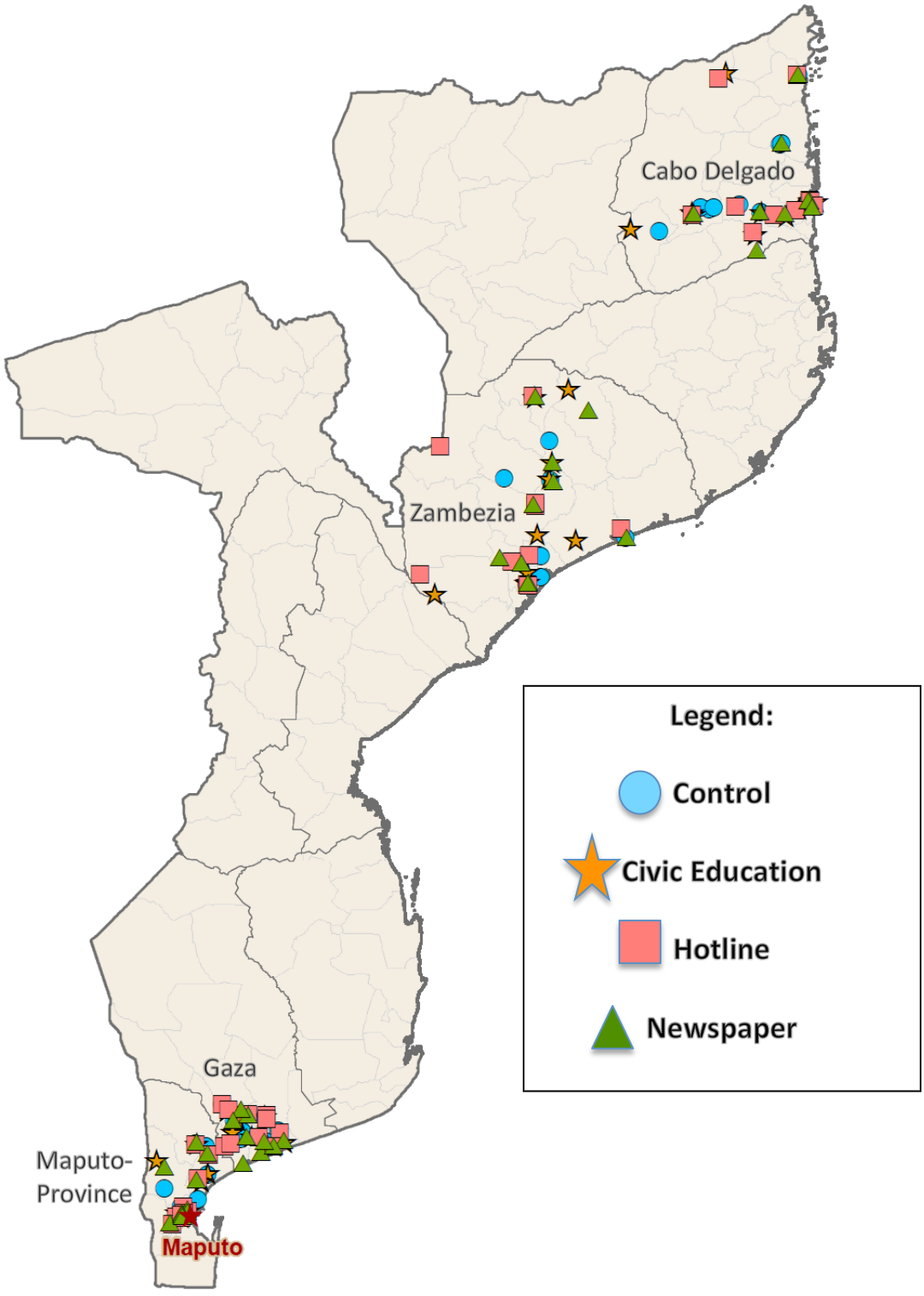


Figure 6: Open letter leaflet

Figure 7: The timing of the experiment

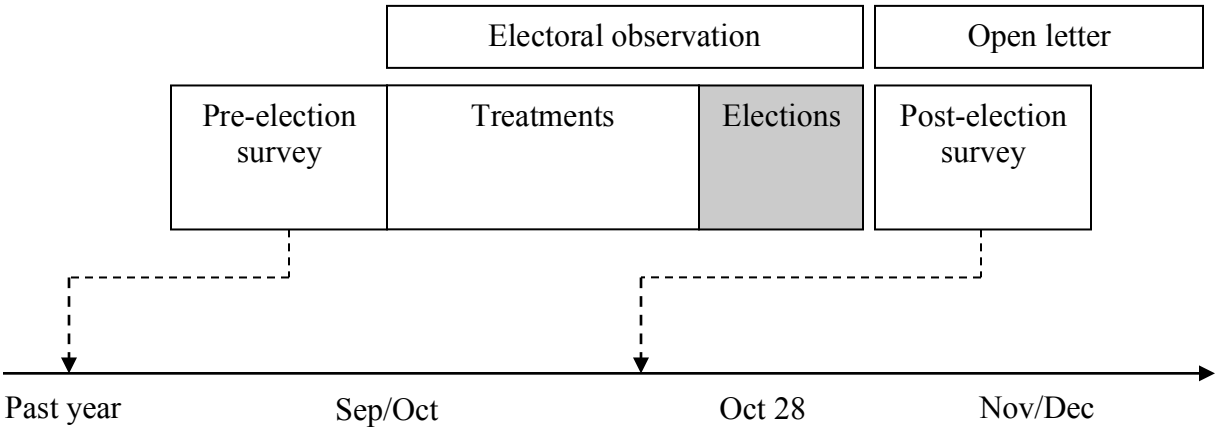


Table 1a: Individual turnout survey measures

	measures	description of the question/measurement	original scale	coding
turnout measures	self-reported	Which of the following sentences best describes your situation during the 2009 Elections?	(1) not a registered voter and not interested in voting/ (2) not a registered voter but would have liked to vote/ (3) registered voter and chose not to vote/ (4) registered voter but unable to vote/ (5) voted	missing if (1) or (2); 0 if (3) or (4); 1 if (5)
	finger knowledge	Which finger was marked with indelible ink after voting?	showed index finger without hesitation/index finger without showing/wrong finger/does not know	used variable is 0-1; based on self-report if abstention; turnout if respondent showed index finger without hesitation
	inked finger			used variable is 0-1; based on self-report if abstention; turnout if respondent's index finger was marked with ink
	interviewer assessment	(Question for the enumerator) How likely do you found that the respondent voted? (after asking all questions 1-19 below)	not likely/very likely (1-7)	used variable is 0-1; based on self-report if abstention; turnout given by the assessment of the enumerator
auxiliary survey questions	1	With whom did you go to vote on the election day?	spouse/son/daughter/other person in household/neighbor/other/does not know	
	2	Indicate the name of the polling location and how can one reach that location.	answer/does not know	
	3	What did you do during the election day, before and after voting?	answer/does not know	
	4	How long did you take from your house to the polling location on the election day?	hours:minutes/does not know	
	5	At what time did you arrive at the polling location on the election day?	hours:minutes/does not know	
	6	Was there more than one polling table in your polling location?	yes/no/does not know	
	7	How difficult was to find your polling table?	easy/a bit difficult/very difficult/does not know	
	8	How long were you queuing to vote?	hours:minutes/does not know	
	9	What happened when you were queuing to vote?	showed voting card/was assigned a number to mention at the table/there was discussion/other/does not know	
	10	We have heard that in your polling location a lady attacked with a 'catana' another lady. Do you remember having witnessed this episode?	does not remember/remembers vaguely/remembers well/does not know	
	11	How many people were sitting at your polling table?	number/does not know	
	12	Did you know anyone from the people that were sitting at your polling table?	yes/no/does not know	
	13	What happened when you reached your polling table?	showed voting card/mentioned the number assigned while queuing/your name was read in loud voice by the chair/other/does not know	
	14	How many ballot papers did you have to fill?	number/does not know	
	15	Were there photos on the ballot papers?	yes/no/does not know	
	16	Could you see anything outside from the cabin where you filled your ballot papers?	yes/no/does not know	
	17	How many ballot boxes there were at your polling table?	number/does not know	
	18	Were you able to see the ballot papers inside the ballot boxes, i.e., were the ballot boxes transparent?	yes/no/does not know	
	19	Were the different ballot boxes colored diferently?	yes/no/does not know	

Table 1b: Individual survey measures

indices	variables	phrasing of the question	original scale
information about the elections	elections	Do you know which elections took place on the 28th October?	presidential/parliamentary/provincial (1-3)
	mandate	What is the duration of a presidential mandate?	2-5 years (0-1)
	candidates	Do you know the names of the candidates in the Presidential elections of the 28th October?	names of the candidates (0-1)
	parties	Can you name 5 parties running in the Parliementary elections of the 28th October?	party names (0-2)
	understand abstention	Do you know what electoral abstention means?	interviewer assesses understanding (0-2)
trust electoral commission		You trust the following institutions. CNE: Electoral Commission. Agree or disagree?	disagree/agree (1-5)
neutralilty of electoral commission		CNE, the Electoral Commission, is independent, i.e., it is neutral relative to the parties. Agree or disagree?	disagree/agree (1-5)
confusion between state and ruling party	schools	Tell us if the following happened in your community: school construction/improvement. If yes, who was responsible for it?	state/frelimo (0-1)
	clinics	Tell us if the following happened in your community: clinic construction/improvement. If yes, who was responsible for it?	state/frelimo (0-1)
	electricity	Tell us if the following happened in your community: expansion of electricity network. If yes, who was responsible for it?	state/frelimo (0-1)
	jobs	Tell us if the following happened in your community: job creation. If yes, who was responsible for it?	state/frelimo (0-1)
problematic elections		Generally, to what extent were the October 2009 elections free and fair?	free and fair/neither free nor fair (1-4)
vote miscounting		To what extent do you think the counting process of the October 2009 elections was fair?	fair/unfair (1-7)
vote-buying	vote-buying in elections	To what extent were the October 2009 elections free and fair in terms of vote-buying by parties and candidates?	free and fair/neither free nor fair (1-4)
	vote-buying - difference	Comparing to 2 months ago when we last visited, to what extent people in your community have been offered money, food, or presents in exchange for their votes?	less/more (1-5)
electoral violence and intimidation	careful about politics - difference	Comparing to 2 months ago when we last visited, how often people have to be careful about what they say about politics?	less/more (1-5)
	political conflict	In this country, during the electoral campaign of october 2009, how often has competition between political parties produced violent conflicts?	never/always (1-4)
	political conflict - difference	Comparing to 2 months ago when we last visited, how often has competition between political parties produced violent conflicts?	less/more (1-5)
	destruction - difference	Comparing to 2 months ago when we last visited, how often have people purposely destroyed campaign materials?	less/more (1-5)
	violence in elections	To what extent were the October 2009 elections free and fair in terms of use of violence by parties and candidates?	free and fair/neither free nor fair (1-4)
	intimidation	During the eelectoral campaign of October 2009, how often someone threatened people in your community with negative consequences unless they voted in a certain way?	never/very often (1-4)
	intimidation by frelimo	Which part was behind these threats? Frelimo.	no/yes (0-1)

Table 2a: Location characteristics - differences across treatments and control

	control	civic education	hotline	newspaper	joint F-stat p-value
school	0.976	0.000 (0.034)	-0.001 (0.035)	-0.001 (0.035)	1.000
police	0.512	-0.024 (0.112)	-0.012 (0.113)	-0.102 (0.112)	0.805
electricity	0.488	0.049 (0.112)	0.137 (0.111)	0.025 (0.113)	0.636
piped water	0.317	-0.000 (0.104)	-0.117 (0.098)	-0.009 (0.105)	0.594
sewage removal	0.220	-0.049 (0.088)	-0.070 (0.087)	-0.040 (0.090)	0.876
health center	0.732	-0.171 (0.105)	-0.057 (0.103)	-0.219** (0.107)	0.160
recreation facility	0.732	0.024 (0.098)	-0.057 (0.103)	-0.065 (0.104)	0.781
temple	0.902	0.000 (0.066)	-0.002 (0.067)	-0.031 (0.071)	0.965
meeting room	0.317	0.024 (0.105)	0.033 (0.106)	-0.086 (0.101)	0.656
paved road	0.268	-0.098 (0.092)	-0.043 (0.097)	0.039 (0.102)	0.525
panel drops	0.361	-0.025 (0.030)	-0.046 (0.035)	-0.012 (0.035)	0.506

Note: Standard errors of the differences reported in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2b: Individual characteristics - differences across treatment and control groups; for both baseline and post-election samples

		baseline - full sample					post-election sample				
		control	civic education	hotline	newspaper	joint F-stat p-value	control	civic education	hotline	newspaper	joint F-stat p-value
basic demographics	male	0.454	-0.028	0.007	0.013	0.557	0.437	-0.061	0.022	0.039	0.073
			(0.031)	(0.030)	(0.031)			(0.040)	(0.042)	(0.041)	
	age	38.321	-1.182	-0.083	-1.664	0.398	36.957	0.025	1.863	-0.315	0.398
			(1.216)	(1.288)	(1.201)			(1.262)	(1.408)	(1.398)	
	household head	0.748	-0.008	-0.005	0.017	0.789	0.742	-0.017	-0.007	0.043	0.312
			(0.032)	(0.033)	(0.032)			(0.043)	(0.043)	(0.044)	
	household size	5.657	0.331	0.352	0.177	0.380	5.789	0.176	0.399	0.168	0.477
			(0.261)	(0.220)	(0.233)			(0.268)	(0.253)	(0.270)	
	single	0.164	0.017	0.020	0.018	0.894	0.186	-0.025	0.005	-0.029	0.684
			(0.027)	(0.027)	(0.030)			(0.035)	(0.038)	(0.038)	
	married or in a union	0.717	0.000	0.008	0.024	0.866	0.692	0.033	0.030	0.075*	0.338
			(0.034)	(0.035)	(0.032)			(0.044)	(0.045)	(0.041)	
	no schooling	0.197	0.024	-0.017	-0.033	0.388	0.179	0.042	0.006	-0.025	0.298
			(0.039)	(0.037)	(0.030)			(0.041)	(0.040)	(0.033)	
	informal schooling	0.071	-0.002	-0.009	0.002	0.962	0.082	-0.025	-0.008	-0.007	0.708
			(0.020)	(0.020)	(0.020)			(0.024)	(0.025)	(0.026)	
	literate	0.803	-0.024	0.017	0.033	0.388	0.821	-0.042	-0.006	0.025	0.298
			(0.039)	(0.037)	(0.030)			(0.041)	(0.040)	(0.033)	
	primary school	0.283	0.013	-0.030	0.023	0.393	0.276	0.009	-0.017	0.048	0.420
			(0.034)	(0.030)	(0.035)			(0.041)	(0.040)	(0.043)	
	incomplete secondary school	0.164	-0.010	0.011	-0.021	0.757	0.168	-0.011	-0.000	-0.025	0.881
			(0.031)	(0.031)	(0.032)			(0.034)	(0.035)	(0.035)	
ethnicity	changana	0.342	0.028	0.009	0.029	0.984	0.355	0.009	0.011	0.016	0.999
			(0.088)	(0.088)	(0.091)			(0.091)	(0.093)	(0.094)	
	macua	0.231	-0.030	-0.006	-0.030	0.971	0.244	-0.038	-0.009	-0.039	0.952
			(0.081)	(0.081)	(0.079)			(0.085)	(0.087)	(0.083)	
	lomue	0.104	-0.008	-0.031	0.003	0.882	0.118	-0.007	-0.048	0.000	0.683
			(0.056)	(0.051)	(0.060)			(0.067)	(0.058)	(0.071)	
	chuabo	0.093	0.012	-0.002	0.000	0.992	0.100	0.004	-0.020	-0.010	0.965
			(0.050)	(0.051)	(0.055)			(0.056)	(0.052)	(0.058)	
	chironga	0.064	-0.022	-0.021	-0.015	0.857	0.061	-0.021	-0.021	-0.018	0.825
			(0.027)	(0.027)	(0.029)			(0.025)	(0.024)	(0.027)	
	maconde	0.040	0.009	-0.003	-0.003	0.985	0.018	0.029	0.019	0.018	0.640
			(0.034)	(0.033)	(0.028)			(0.029)	(0.032)	(0.019)	

Note: Standard errors of the differences reported in parenthesis; standard errors are corrected by clustering at the location (enumeration area) level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2c: Individual characteristics - differences across treatment and control groups; for both baseline and post-election samples

		baseline - full sample					post-election sample				
		control	civic education	hotline	newspaper	joint F-stat p-value	control	civic education	hotline	newspaper	joint F-stat p-value
religion	catholic	0.398	-0.051	-0.054	-0.046	0.689	0.401	-0.012	-0.076	-0.072	0.438
			(0.050)	(0.055)	(0.050)			(0.058)	(0.062)	(0.057)	
	protestant	0.341	0.033	0.006	0.007	0.949	0.319	0.027	0.043	0.036	0.916
			(0.062)	(0.065)	(0.062)			(0.063)	(0.069)	(0.064)	
	muslim	0.206	-0.001	0.035	0.032	0.920	0.215	-0.017	0.030	0.036	0.868
			(0.061)	(0.068)	(0.068)			(0.065)	(0.073)	(0.071)	
occupation	has a job	0.259	-0.036	0.014	-0.007	0.650	0.240	-0.035	0.012	-0.014	0.741
			(0.037)	(0.041)	(0.035)			(0.042)	(0.047)	(0.039)	
	agriculture	0.343	0.022	-0.024	-0.035	0.728	0.351	0.011	-0.016	-0.039	0.838
			(0.056)	(0.060)	(0.058)			(0.063)	(0.069)	(0.065)	
	retail informal sector	0.033	0.011	0.015	0.002	0.650	0.043	0.000	0.007	-0.004	0.953
			(0.014)	(0.014)	(0.014)			(0.018)	(0.019)	(0.020)	
	artisan	0.044	-0.018	0.004	0.005	0.204	0.050	-0.034**	-0.003	0.011	0.009
			(0.012)	(0.013)	(0.014)			(0.015)	(0.017)	(0.020)	
	unskilled worker	0.056	-0.011	0.016	0.013	0.378	0.054	-0.014	0.003	0.011	0.535
			(0.017)	(0.018)	(0.017)			(0.018)	(0.020)	(0.020)	
	wage employee	0.029	-0.011	0.003	-0.001	0.454	0.032	-0.009	-0.006	-0.018	0.520
			(0.011)	(0.012)	(0.013)			(0.014)	(0.015)	(0.014)	
	teacher	0.044	0.000	0.022	0.010	0.754	0.047	-0.006	0.010	0.004	0.885
			(0.014)	(0.025)	(0.016)			(0.015)	(0.026)	(0.019)	
	public official	0.020	0.027*	0.012	0.006	0.341	0.025	0.032*	0.008	-0.000	0.375
			(0.015)	(0.012)	(0.012)			(0.019)	(0.014)	(0.016)	
	student	0.031	0.011	0.001	0.011	0.757	0.040	0.007	-0.009	-0.004	0.848
			(0.015)	(0.013)	(0.013)			(0.019)	(0.018)	(0.017)	
	stays at home	0.137	-0.006	-0.022	-0.016	0.804	0.147	-0.003	-0.033	-0.018	0.645
			(0.024)	(0.025)	(0.025)			(0.030)	(0.030)	(0.031)	
assets and expenditure	house	0.847	0.002	0.004	-0.022	0.712	0.853	0.013	0.016	-0.018	0.639
			(0.029)	(0.027)	(0.027)			(0.030)	(0.030)	(0.032)	
	land	0.608	-0.008	0.018	-0.018	0.889	0.652	-0.055	-0.045	-0.047	0.771
			(0.051)	(0.047)	(0.051)			(0.056)	(0.057)	(0.059)	
	cattle	0.255	-0.001	-0.009	0.013	0.972	0.254	-0.006	0.007	0.004	0.994
			(0.040)	(0.043)	(0.044)			(0.045)	(0.052)	(0.049)	
	cell phone	0.710	-0.001	0.051	0.034	0.749	0.706	0.022	0.056	0.022	0.856
			(0.059)	(0.058)	(0.055)			(0.065)	(0.065)	(0.064)	
	expenditure	127.203	3.526	-0.860	3.431	0.987	122.452	1.162	8.942	4.192	0.955
			(16.270)	(15.926)	(16.008)			(16.730)	(17.000)	(16.157)	

Note: Standard errors of the differences reported in parenthesis; standard errors are corrected by clustering at the location (enumeration area) level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2d: Baseline electoral results - differences across treatments and control

	control	civic education	hotline	newspaper	joint F-stat p-value
registered voters 2004	4,768.415	-201.878 (613.452)	69.185 (720.236)	230.380 (722.124)	0.947
presidential turnout 2004	0.407	-0.019 (0.027)	-0.006 (0.034)	-0.028 (0.030)	0.819
guebuza 2004	0.714	0.038 (0.046)	0.007 (0.047)	-0.012 (0.048)	0.736
dhlakama 2004	0.188	-0.028 (0.041)	-0.007 (0.042)	0.019 (0.043)	0.725
presidential null 2004	0.035	-0.004 (0.003)	0.002 (0.005)	-0.002 (0.003)	0.654
presidential blank 2004	0.032	-0.005 (0.005)	-0.001 (0.006)	-0.004 (0.005)	0.768
parliamentary turnout 2004	0.414	-0.027 (0.027)	-0.005 (0.036)	-0.032 (0.029)	0.694
frelimo 2004	0.673	0.040 (0.046)	0.016 (0.046)	-0.010 (0.047)	0.714
renamo 2004	0.179	-0.029 (0.037)	-0.013 (0.037)	0.014 (0.040)	0.694
parliamentary null 2004	0.039	-0.007 (0.004)	0.002 (0.007)	-0.003 (0.004)	0.486
parliamentary blank 2004	0.055	-0.005 (0.008)	-0.005 (0.009)	-0.004 (0.008)	0.895
registered voters 2009	4,013.146	42.293 (554.263)	-49.671 (541.481)	378.110 (631.748)	0.896

Note: Standard errors of the differences reported in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2e: Individual outcomes at the baseline - differences across treatment and control groups; for both baseline and post-election samples

		baseline - full sample					post-election sample				
		control	civic education	hotline	newspaper	joint F-stat p-value	control	civic education	hotline	newspaper	joint F-stat p-value
voting intentions 2009	turnout	0.975	0.013	0.008	-0.004	0.284	0.974	0.008	0.012	0.007	0.751
			(0.009)	(0.009)	(0.011)			(0.013)	(0.011)	(0.012)	
	guebuza	0.866	0.020	0.022	0.004	0.859	0.860	0.012	0.025	0.031	0.805
			(0.031)	(0.032)	(0.032)			(0.036)	(0.040)	(0.033)	
	dhlakama	0.016	0.002	0.007	0.006	0.903	0.009	0.010	0.008	0.013	0.625
			(0.009)	(0.012)	(0.010)			(0.010)	(0.014)	(0.012)	
	simango	0.037	-0.006	-0.014	-0.009	0.785	0.043	0.003	-0.014	-0.017	0.609
			(0.016)	(0.015)	(0.015)			(0.022)	(0.021)	(0.020)	
	frelimo	0.903	0.029	0.025	-0.012	0.300	0.912	-0.000	0.018	-0.003	0.924
			(0.021)	(0.023)	(0.025)			(0.028)	(0.032)	(0.027)	
	renamo	0.017	-0.003	0.001	-0.002	0.973	0.009	0.007	0.008	0.004	0.893
			(0.008)	(0.012)	(0.009)			(0.010)	(0.015)	(0.010)	
past voting 2004	turnout	0.968	-0.016	0.001	-0.021	0.337	0.970	-0.022	-0.006	-0.017	0.606
			(0.016)	(0.013)	(0.014)			(0.019)	(0.018)	(0.017)	
	guebuza	0.807	0.002	-0.009	-0.011	0.970	0.823	-0.024	-0.010	-0.009	0.926
			(0.031)	(0.031)	(0.032)			(0.036)	(0.038)	(0.034)	
	dhlakama	0.017	0.000	0.003	-0.002	0.980	0.008	0.010	0.007	0.004	0.786
			(0.009)	(0.011)	(0.008)			(0.011)	(0.013)	(0.008)	
	frelimo	0.785	0.015	0.000	0.001	0.961	0.796	-0.005	0.001	0.005	0.993
			(0.033)	(0.033)	(0.032)			(0.038)	(0.039)	(0.036)	
	renamo	0.017	-0.002	0.003	-0.001	0.966	0.008	0.010	0.011	0.004	0.723
			(0.009)	(0.011)	(0.008)			(0.011)	(0.013)	(0.008)	
survey outcomes	trust electoral commission	-0.000	0.020	-0.060	0.010	0.852	-0.036	0.014	-0.058	0.093	0.595
			(0.095)	(0.103)	(0.098)			(0.113)	(0.119)	(0.113)	
	neutrality of electoral commission	0.000	0.057	0.064	0.079	0.846	-0.032	0.102	0.118	0.169	0.433
			(0.092)	(0.095)	(0.095)			(0.107)	(0.108)	(0.103)	
	problematic elections 2004	0.000	-0.054	0.039	0.018	0.821	-0.016	-0.032	0.095	-0.032	0.714
			(0.103)	(0.113)	(0.106)			(0.123)	(0.128)	(0.115)	
	vote miscounting 2009	0.000	-0.067	0.041	-0.051	0.591	0.051	-0.091	-0.073	-0.138	0.646
			(0.081)	(0.090)	(0.085)			(0.108)	(0.116)	(0.109)	
	vote-buying in elections 2009	-0.000	-0.048	-0.033	-0.097	0.670	-0.003	-0.042	-0.031	-0.107	0.788
			(0.074)	(0.071)	(0.079)			(0.101)	(0.093)	(0.107)	
	violence in elections 2009	-0.000	-0.044	0.009	-0.101	0.557	-0.006	-0.043	-0.046	-0.081	0.891
			(0.078)	(0.079)	(0.081)			(0.100)	(0.100)	(0.103)	

Note: Standard errors of the differences reported in parenthesis; standard errors are corrected by clustering at the location (enumeration area) level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3: Official ballot station outcomes (presidential elections)

dependent variable ------>		presidential elections											
		turnout		guebuza		dhlakama		simango		blank votes		null votes	
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
civic education	coefficient	0.047*	0.053**	0.049**	0.046**	-0.030*	-0.032**	-0.013	-0.012	-0.001	0.001	-0.004	-0.003
	standard error	(0.025)	(0.025)	(0.020)	(0.020)	(0.016)	(0.016)	(0.008)	(0.008)	(0.010)	(0.009)	(0.004)	(0.004)
hotline	coefficient	0.049*	0.053**	0.022	0.022	-0.010	-0.012	-0.006	-0.004	-0.001	-0.001	-0.005	-0.005
	standard error	(0.025)	(0.025)	(0.020)	(0.020)	(0.016)	(0.016)	(0.008)	(0.008)	(0.010)	(0.009)	(0.004)	(0.004)
newspaper	coefficient	0.047*	0.054**	0.040**	0.041**	-0.015	-0.016	-0.013	-0.014*	-0.005	-0.004	-0.007*	-0.007*
	standard error	(0.025)	(0.025)	(0.020)	(0.020)	(0.016)	(0.016)	(0.008)	(0.008)	(0.010)	(0.009)	(0.004)	(0.004)
mean dep. variable (control)		0.440	0.440	0.723	0.723	0.114	0.114	0.069	0.069	0.057	0.057	0.036	0.036
r-squared adjusted		0.373	0.383	0.664	0.670	0.565	0.569	0.281	0.410	0.296	0.438	0.110	0.171
number of observations		161	161	161	161	161	161	161	161	161	161	161	161
h0: civic education = hotline	F-stat p-value	0.949	0.989	0.186	0.233	0.216	0.236	0.399	0.283	0.977	0.845	0.873	0.647
h0: civic education = newspaper	F-stat p-value	0.991	0.981	0.674	0.807	0.345	0.362	0.968	0.833	0.715	0.595	0.453	0.331
h0: hotline = newspaper	F-stat p-value	0.942	0.970	0.374	0.358	0.775	0.803	0.383	0.209	0.696	0.735	0.556	0.600
h0: all treatments = 0	F-stat p-value	0.143	0.079	0.079	0.094	0.293	0.272	0.344	0.226	0.962	0.957	0.306	0.271
controls		no	yes	no	yes	no	yes	no	yes	no	yes	no	yes

Note: All regressions are OLS. All dependent variables are shares: for turnout, we divide by the number of registered voters; for candidate scores, blank votes, and null votes, we divide by the number of votes. Guebuza, Dhlakama, and Simango are presidential candidates. Controls are enumeration area/polling location characteristics, which include number of polling tables, whether the location has a school, a police station, electricity, piped water, sewage, a health center, a recreational facility, a temple, a meeting room, and paved road access. All regressions include province dummies. Standard errors reported in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4: Individual turnout (targeted plus untargeted)

dependent variable ------>		individual turnout (targeted plus untargeted)							
		self-reported		finger knowledge		inked finger		interviewer assessment	
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
civic education	coefficient	0.036	0.036	0.055**	0.055**	0.052	0.057*	0.066**	0.074***
	standard error	(0.026)	(0.026)	(0.026)	(0.026)	(0.032)	(0.034)	(0.026)	(0.025)
hotline	coefficient	0.072***	0.065***	0.068***	0.053**	0.084**	0.073*	0.094***	0.087***
	standard error	(0.024)	(0.024)	(0.026)	(0.025)	(0.039)	(0.038)	(0.024)	(0.023)
newspaper	coefficient	0.030	0.032	0.041	0.046	0.084**	0.096**	0.059*	0.066**
	standard error	(0.030)	(0.027)	(0.038)	(0.033)	(0.040)	(0.042)	(0.034)	(0.029)
mean dep. variable (control)		0.877	0.876	0.807	0.805	0.238	0.240	0.754	0.753
r-squared adjusted		0.013	0.038	0.014	0.036	0.039	0.054	0.030	0.056
number of observations		1,121	1,106	1,121	1,106	1,121	1,106	1,121	1,106
h0: civic education = hotline	F-stat p-value	0.080	0.162	0.582	0.933	0.400	0.661	0.201	0.557
h0: civic education = newspaper	F-stat p-value	0.832	0.871	0.691	0.779	0.413	0.324	0.828	0.784
h0: hotline = newspaper	F-stat p-value	0.095	0.162	0.442	0.818	0.997	0.624	0.265	0.429
h0: all treatments = 0	F-stat p-value	0.017	0.049	0.062	0.115	0.084	0.085	0.003	0.002
h0: civic education targ = untarg	F-stat p-value	0.730	0.733	0.969	0.271	0.387	0.217	0.461	0.862
h0: hotline targ = untarg	F-stat p-value	0.109	0.097	0.500	0.480	0.923	0.656	0.817	0.763
h0: newspaper targ = untarg	F-stat p-value	0.038	0.003	0.000	0.000	0.957	0.760	0.000	0.000
province dummies		yes	yes	yes	yes	yes	yes	yes	yes
controls		no	yes	no	yes	no	yes	no	yes

Note: All regressions are OLS. All dependent variables are between 0 and 1. See Table 1a for the specific definitions of the dependent variables. Controls are enumeration area/polling location characteristics, which include number of polling tables, whether the location has a school, a police station, electricity, piped water, sewage, a health center, a recreational facility, a temple, a meeting room, and paved road access; and individual characteristics, which include gender, age, household characteristics, marital status, schooling, religion, ethnicity, occupation, assets and expenditure. All regressions include province dummies. Standard errors reported in parenthesis - these are corrected by clustering at the location (enumeration area) level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5: Individual self-reported voting (targeted plus untargeted)

dependent variable ------>		individual voting (targeted plus untargeted)								
		guebuza			dhlakama			simango		
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
civic education	coefficient	0.042	0.060*	0.057*	-0.008	-0.015*	-0.017*	0.009	0.009	0.010
	standard error	(0.036)	(0.034)	(0.034)	(0.008)	(0.009)	(0.010)	(0.016)	(0.016)	(0.017)
hotline	coefficient	0.048	0.059*	0.055*	0.008	-0.002	-0.003	0.004	-0.001	-0.000
	standard error	(0.032)	(0.031)	(0.031)	(0.011)	(0.010)	(0.010)	(0.014)	(0.013)	(0.014)
newspaper	coefficient	0.008	0.029	0.023	0.007	0.000	0.000	-0.013	-0.021	-0.022
	standard error	(0.038)	(0.038)	(0.039)	(0.012)	(0.012)	(0.012)	(0.013)	(0.014)	(0.015)
mean dep. variable (control)		0.819	0.822	0.822	0.012	0.012	0.012	0.028	0.028	0.028
r-squared adjusted		0.035	0.050	0.069	0.002	0.020	0.015	0.007	0.028	0.020
number of observations		1,031	1,017	1,014	1,031	1,017	1,014	1,031	1,017	1,014
h0: civic education = hotline	F-stat p-value	0.830	0.957	0.949	0.072	0.083	0.091	0.720	0.483	0.521
h0: civic education = newspaper	F-stat p-value	0.358	0.389	0.362	0.130	0.104	0.086	0.108	0.044	0.041
h0: hotline = newspaper	F-stat p-value	0.223	0.371	0.339	0.980	0.838	0.755	0.127	0.092	0.071
h0: all treatments = 0	F-stat p-value	0.362	0.209	0.239	0.155	0.114	0.125	0.291	0.158	0.144
h0: civic education targ = untarg	F-stat p-value	0.924	0.832	0.959	0.335	0.768	0.822	0.802	0.612	0.771
h0: hotline targ = untarg	F-stat p-value	0.302	0.701	0.692	0.911	0.121	0.151	0.124	0.177	0.351
h0: newspaper targ = untarg	F-stat p-value	0.037	0.019	0.046	0.041	0.046	0.155	0.773	0.861	0.820
location and individual controls		no	yes	yes	no	yes	yes	no	yes	yes
interviewer dummies		no	no	yes	no	no	yes	no	no	yes

Note: All regressions are OLS. All dependent variables are binary. Guebuza, Dhlakama, and Simango are presidential candidates. Controls are enumeration area/polling location characteristics, which include number of polling tables, whether the location has a school, a police station, electricity, piped water, sewage, a health center, a recreational facility, a temple, a meeting room, and paved road access; and individual characteristics, which include gender, age, household characteristics, marital status, schooling, religion, ethnicity, occupation, assets and expenditure. All regressions include province dummies. Standard errors reported in parenthesis - these are corrected by clustering at the location (enumeration area) level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6: Behavioral measure open letter (targeted plus untargeted)

dependent variable ------>		open letter (targeted plus untargeted)	
		(1)	(2)
civic education	coefficient	0.055	0.065
	standard error	(0.045)	(0.044)
hotline	coefficient	-0.027	-0.014
	standard error	(0.034)	(0.033)
newspaper	coefficient	0.083*	0.091*
	standard error	(0.048)	(0.049)
mean dep. variable (control)		0.153	0.151
r-squared adjusted		0.013	0.034
number of observations		1,147	1,127
h0: civic education = hotline	F-stat p-value	0.065	0.065
h0: civic education = newspaper	F-stat p-value	0.613	0.644
h0: hotline = newspaper	F-stat p-value	0.021	0.020
h0: all treatments = 0	F-stat p-value	0.068	0.046
h0: civic education targ = untarg	F-stat p-value	0.373	0.412
h0: hotline targ = untarg	F-stat p-value	0.270	0.320
h0: newspaper targ = untarg	F-stat p-value	0.687	0.853
province dummies		yes	yes
controls		no	yes

Note: All regressions are OLS. The dependent variable is binary - it takes value 1 if the individual sent an open letter message. Controls are enumeration area/polling location characteristics, which include number of polling tables, whether the location has a school, a police station, electricity, piped water, sewage, a health center, a recreational facility, a temple, a meeting room, and paved road access; and individual characteristics, which include gender, age, household characteristics, marital status, schooling, religion, ethnicity, occupation, assets and expenditure. All regressions include province dummies. Standard errors reported in parenthesis - these are corrected by clustering at the location (enumeration area) level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 7: Electoral problems

dependent variable ------>		electoral problems									
		any problem				election-day misconduct (incidence)		campaign misconduct (incidence)		violence and intimidation (incidence)	
		incidence		intensity (0-5)							
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
civic education	coefficient	-0.326	-0.346	-0.056	-0.061	-0.003	-0.010	-0.350	-0.366	-0.076	-0.069
	standard error	(0.308)	(0.311)	(0.229)	(0.235)	(0.132)	(0.134)	(0.264)	(0.262)	(0.134)	(0.136)
hotline	coefficient	-0.141	-0.089	0.102	0.109	0.001	0.006	-0.243	-0.215	-0.022	0.015
	standard error	(0.310)	(0.312)	(0.230)	(0.236)	(0.133)	(0.134)	(0.265)	(0.263)	(0.135)	(0.137)
newspaper	coefficient	-0.588*	-0.578*	-0.399*	-0.466*	0.058	0.011	-0.566**	-0.508*	-0.170	-0.163
	standard error	(0.312)	(0.317)	(0.232)	(0.240)	(0.134)	(0.136)	(0.267)	(0.267)	(0.136)	(0.139)
mean dep. variable (control)		0.951	0.951	0.819	0.819	0.390	0.390	0.756	0.756	0.341	0.341
r-squared adjusted		0.153	0.159	0.202	0.177	0.443	0.445	0.215	0.242	0.108	0.109
number of observations		161	161	161	161	161	161	161	161	161	161
h0: civic education = hotline	F-stat p-value	0.551	0.414	0.496	0.473	0.972	0.902	0.687	0.568	0.690	0.540
h0: civic education = newspaper	F-stat p-value	0.404	0.471	0.140	0.097	0.651	0.878	0.419	0.601	0.490	0.504
h0: hotline = newspaper	F-stat p-value	0.157	0.131	0.033	0.019	0.677	0.974	0.231	0.282	0.281	0.207
h0: all treatments = 0	F-stat p-value	0.271	0.261	0.163	0.101	0.964	0.999	0.202	0.264	0.607	0.570
location and individual controls		yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
controls		no	yes	no	yes	no	yes	no	yes	no	yes

Note: All regressions are OLS. Electoral problems are coded from four sources in the four provinces covered in the experiment: 75 problems reported by newspaper @Verdade's national hotline, 157 problems reported by Observatorio Eleitoral (campaign), 92 problems reported by Observatorio Eleitoral (election-day), and 36 problems reported by UNDP's electoral observation mission. Incidence corresponds to the number of occurrences in each location. Intensity is the average of all occurrences in each location - all occurrences are ranked from 1 to 5 (1: minor problems; 2: non-violent occurrences including campaign misconduct and election-day problems; 3: occurrences leading to physical intimidation, including vandalism; 4: occurrences resulting in wounded people; 5: occurrences resulting in dead people), 0 denotes no occurrences. Controls are enumeration area/polling location characteristics, which include number of polling tables, whether the location has a school, a police station, electricity, piped water, sewage, a health center, a recreational facility, a temple, a meeting room, and paved road access. All regressions include province dummies. Standard errors reported in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 8a: Individual survey measures (targeted plus untargeted)

dependent variable ------>		individual survey measures (targeted plus untargeted)							
		information about the elections		trust electoral commission		neutrality of electoral commission		confusion between state and ruling party	
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
civic education	coefficient	0.074	0.155***	0.204***	0.197**	0.139	0.116	-0.028	-0.087
	standard error	(0.064)	(0.058)	(0.078)	(0.077)	(0.086)	(0.086)	(0.067)	(0.064)
hotline	coefficient	0.158**	0.177***	0.107	0.142*	0.170**	0.178**	-0.197***	-0.222***
	standard error	(0.063)	(0.058)	(0.081)	(0.077)	(0.085)	(0.078)	(0.060)	(0.058)
newspaper	coefficient	0.123*	0.156**	0.174**	0.170**	0.151*	0.129	-0.158**	-0.213***
	standard error	(0.068)	(0.063)	(0.078)	(0.075)	(0.086)	(0.086)	(0.066)	(0.059)
mean dep. variable (control)		0.000	0.000	-0.000	-0.004	0.000	0.000	0.000	0.005
r-squared adjusted		0.078	0.255	0.068	0.086	0.084	0.092	0.038	0.088
number of observations		1,151	1,135	1,068	1,053	1,033	1,020	814	804
h0: civic education = hotline	F-stat p-value	0.145	0.678	0.200	0.467	0.693	0.439	0.003	0.024
h0: civic education = newspaper	F-stat p-value	0.440	0.981	0.674	0.704	0.871	0.872	0.041	0.033
h0: hotline = newspaper	F-stat p-value	0.570	0.714	0.383	0.703	0.813	0.539	0.488	0.872
h0: all treatments = 0	F-stat p-value	0.078	0.016	0.052	0.057	0.206	0.155	0.002	0.000
h0: civic education targ = untarg	F-stat p-value	0.609	0.974	0.482	0.722	0.655	0.909	0.320	0.943
h0: hotline targ = untarg	F-stat p-value	0.583	0.153	0.422	0.250	0.914	0.844	0.917	0.794
h0: newspaper targ = untarg	F-stat p-value	0.968	0.833	0.473	0.698	0.644	0.898	0.195	0.216
province dummies		yes	yes	yes	yes	yes	yes	yes	yes
controls		no	yes	no	yes	no	yes	no	yes

Note: All regressions are OLS. All dependent variables are z-scores. See Table 1b for the specific definitions of the dependent variables. Controls are enumeration area/polling location characteristics, which include whether the location has a school, a police station, electricity, piped water, sewage, a health center, a recreational facility, a temple, a meeting room, and paved road access; and individual characteristics, which include gender, age, household characteristics, marital status, schooling, religion, ethnicity, occupation, assets and expenditure. All regressions include province dummies. Standard errors reported in parenthesis - these are corrected by clustering at the location (enumeration area) level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 8b: Individual survey measures (targeted plus untargeted)

dependent variable ------>		individual survey measures (targeted plus untargeted)							
		problematic elections		vote miscounting		campaign money misbehavior		electoral violence and intimidation	
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
civic education	coefficient	-0.077	-0.043	-0.139*	-0.139*	-0.039	-0.021	-0.148***	-0.136***
	standard error	(0.082)	(0.085)	(0.076)	(0.081)	(0.062)	(0.063)	(0.041)	(0.041)
hotline	coefficient	0.171**	0.193**	-0.020	-0.033	0.048	0.039	-0.058	-0.065
	standard error	(0.087)	(0.084)	(0.079)	(0.084)	(0.063)	(0.065)	(0.047)	(0.045)
newspaper	coefficient	-0.009	0.015	-0.079	-0.081	0.116*	0.141**	-0.088**	-0.073*
	standard error	(0.083)	(0.084)	(0.080)	(0.087)	(0.067)	(0.068)	(0.040)	(0.042)
mean dep. variable (control)		0.000	0.004	0.000	-0.005	0.000	0.003	0.000	0.004
r-squared adjusted		0.021	0.027	0.016	0.015	0.008	0.015	0.032	0.048
number of observations		1,119	1,104	1,102	1,088	1,131	1,115	1,148	1,132
h0: civic education = hotline	F-stat p-value	0.010	0.017	0.073	0.106	0.173	0.370	0.029	0.071
h0: civic education = newspaper	F-stat p-value	0.453	0.527	0.372	0.408	0.022	0.015	0.072	0.057
h0: hotline = newspaper	F-stat p-value	0.060	0.064	0.405	0.503	0.324	0.144	0.460	0.842
h0: all treatments = 0	F-stat p-value	0.070	0.073	0.190	0.253	0.118	0.084	0.004	0.009
h0: civic education targ = untarg	F-stat p-value	0.386	0.655	0.904	0.799	0.434	0.596	0.438	0.318
h0: hotline targ = untarg	F-stat p-value	0.672	0.896	0.454	0.520	0.419	0.895	0.866	0.264
h0: newspaper targ = untarg	F-stat p-value	0.009	0.021	0.210	0.334	0.027	0.041	0.513	0.782
province dummies		yes	yes	yes	yes	yes	yes	yes	yes
controls		no	yes	no	yes	no	yes	no	yes

Note: All regressions are OLS. All dependent variables are z-scores. See Table 1b for the specific definitions of the dependent variables. Controls are enumeration area/polling location characteristics, which include whether the location has a school, a police station, electricity, piped water, sewage, a health center, a recreational facility, a temple, a meeting room, and paved road access; and individual characteristics, which include gender, age, household characteristics, marital status, schooling, religion, ethnicity, occupation, assets and expenditure. All regressions include province dummies. Standard errors reported in parenthesis - these are corrected by clustering at the location (enumeration area) level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 9: Lee bounds for main survey-based treatment effects

dependent variable ------>			main individual outcomes							
			self-reported	finger knowledge	inked finger	interviewer assessment	open letter	guebuza	dhlakama	simango
			(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
civic education	mean difference to control	coefficient	0.037	0.057*	0.052	0.066**	0.055*	0.045	-0.008	0.009
		standard error	(0.026)	(0.031)	(0.037)	(0.028)	(0.032)	(0.032)	(0.008)	(0.016)
	lower bound	coefficient	**0.032**	**0.049**	**0.010**	**0.056***	**0.010**	**0.036**	**-0.012***	**-0.028*****
		standard error	**(0.027)**	**(0.033)**	**(0.054)**	**(0.030)**	**(0.053)**	**(0.034)**	**(0.007)**	**(0.010)**
	upper bound	coefficient	**0.092***	**0.108***	**0.070***	**0.116****	**0.067***	**0.100***	**-0.008**	**0.011**
		standard error	**(0.056)**	**(0.056)**	**(0.041)**	**(0.053)**	**(0.035)**	**(0.059)**	**(0.008)**	**(0.016)**
hotline	mean difference to control	coefficient	0.074***	0.069**	0.080**	0.094***	-0.028	0.055*	0.007	0.002
		standard error	(0.023)	(0.031)	(0.038)	(0.027)	(0.029)	(0.032)	(0.011)	(0.015)
	lower bound	coefficient	**0.070*****	**0.058***	**0.021**	**0.081*****	**-0.103***	**0.044**	**-0.012***	**-0.028*****
		standard error	**(0.024)**	**(0.033)**	**(0.054)**	**(0.029)**	**(0.056)**	**(0.034)**	**(0.007)**	**(0.010)**
	upper bound	coefficient	**0.123*****	**0.145****	**0.108****	**0.163*****	**-0.017**	**0.128****	**0.009**	**0.005**
		standard error	**(0.020)**	**(0.058)**	**(0.043)**	**(0.041)**	**(0.031)**	**(0.061)**	**(0.012)**	**(0.016)**
newspaper	mean difference to control	coefficient	0.030	0.038	0.077**	0.056*	0.081**	0.003	0.008	-0.012
		standard error	(0.027)	(0.033)	(0.038)	(0.029)	(0.033)	(0.034)	(0.011)	(0.013)
	lower bound	coefficient	**0.027**	**0.033**	**0.055**	**0.050**	**0.055**	**-0.002**	**-0.012***	**-0.028*****
		standard error	**(0.028)**	**(0.034)**	**(0.054)**	**(0.031)**	**(0.053)**	**(0.036)**	**(0.007)**	**(0.010)**
	upper bound	coefficient	**0.059**	**0.065**	**0.087****	**0.082**	**0.089****	**0.029**	**0.009**	**-0.011**
		standard error	**(0.056)**	**(0.056)**	**(0.043)**	**(0.053)**	**(0.036)**	**(0.059)**	**(0.012)**	**(0.013)**
attrition control			0.405	0.405	0.405	0.405	0.392	0.449	0.449	0.449
attrition civic education			0.347	0.347	0.347	0.347	0.336	0.394	0.394	0.394
attrition hotline			0.337	0.337	0.337	0.337	0.321	0.399	0.399	0.399
attrition newspaper			0.371	0.371	0.371	0.371	0.352	0.422	0.422	0.422

Note: All dependent variables are defined as in the previous tables. Lower and upper Lee bounds displayed, as well as mean differences between treatment and control groups. Dependent variables are between 0 and 1 (turnout) and binary (open letter and voting). Standard errors reported in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1%.