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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 SMS-based information campaign, 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.
JEL Codes: D72, O55, P16.
Keywords: Voter Education, Political Economy, Cell Phones, Newspapers, Randomized Experiment, Field Experiment, Mozambique, Africa.

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'FRELIMO did it, FRELIMO does it.'
- 2009 campaign slogan

1 Introduction

The idea of political accountability has been at the center of the development debate in recent years. The hope is that once democratic institutions reflect the will of the majority, effective development policies focusing on the poor will be implemented. Economic theory supports these beliefs. Becker (1983) shows that when political competition is fully secured, efficient policies will arise. Yet developing democratic institutions that depend on the will of the general population has been particularly difficult to achieve in many countries. These problems have often been linked to information deficiencies, i.e. voters' unresponsiveness to policies (e.g. Grossman and Helpman, 1996) in theory, as well as media shortcomings (Besley and Burgess, 2002) and lack of accountable local institutions (Bjorkman and Svensson, 2009) in practice.

In Sub-Saharan Africa, the record of post-soviet democratization has been mixed (e.g. Kudamatsu, 2012). One concern is that elections do not discipline governments because of the many irregularities that have tainted their conduct (Chauvet and Collier, 2009). Electoral violence and intimidation, vote-buying, and ballot-fraud have been rampant. Collier and Vicente (2012) argue that these irregularities have been used strategically by politicians to bend electoral outcomes. 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. One observation is clear: there is no evidence that political accountability is any higher in Mozambique. Specifically, this country has seen dramatic drops in voter turnout over the years: citizen apathy and acquiescence may have reached an all-time peak. In this paper we test whether citizens are responsive to neutral electoral information and to calls for political participation delivered through innovative means during the electoral period.

Recent papers have focused on voter education interventions aimed at counteracting specific illicit strategies during elections. Wantchekon (2003) target clientelism in Benin by studying clientelism-free political campaigning. Vicente (2013) look at vote-buying (cash-for-votes) in Sao Tome and Principe by analyzing an educational campaign against that practice. Collier and

Vicente (2009) examine electoral violence in Nigeria by assessing the effects of grassroots mobilization against politically motivated violence. Other recent papers focused directly on participation and accountability. Gine and Mansuri (2011) assess the impact of a voter mobilization campaign that targeted women in Pakistan. Banerjee et al (2011) study the effects of the dissemination of information about candidate qualifications and legislator performance on electoral outcomes in India. Humphreys and Weinstein (2012) analyze the effects of scorecards about legislator performance on both voter and politician behavior. In this paper, we study the effects of broad voter-education interventions in Mozambique, aimed at mobilizing citizens to participate in elections, while providing them with better electoral information. We disseminate this information using information and communication technologies (ICT) and a free newspaper that raised the attention of international media.[1] While mobile phone-based civic education campaigns have become increasingly common in the US and Europe, to the best of our knowledge this is the first study of mobile phone-based civic education in a developing country. Cell phones are becoming important in Africa, where the take-up rate increased by 550 percent in the five years up to 2009.[2]

This paper reports the results of a field experiment conducted prior to and during the October 2009 elections in Mozambique. Three randomized interventions took place nationwide in four provinces of the country, based upon collaboration with a newspaper (@Verdade, 'The Truth') and local civil society organizations. Treatments were clustered around different polling locations. The first treatment had a civic education nature: it provided citizens with information about the election and mobilized them to vote. This intervention shared voter information via an official voter-education leaflet and followed up with a range of cell phone messages on voter education and the election. The second treatment established a mobile-phone hotline, and invited citizens to report electoral problems by sending text messages to pre-arranged numbers. After verification of the reports with local correspondents, these reports were disseminated through SMS to experimental subjects in locations where the hotline had been disseminated. The third treatment provided voter education information via free newspaper @Verdade. This is the highest-circulation newspaper in Mozambique; it is an independent newspaper. 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. While

[1] See the CNN report about the newspaper that we study (CNN Market Place on the 16th October, 2010) at: http://www.youtube.com/watch?v=UyMozYTg3tc.
[2] UNCTAD, 'Information Economy Report 2009: Trends and Outlook in Turbulent Times', 2009.

all treatments aimed at disseminating electoral information and increasing electoral participation, the first treatment emphasizes the informational component (by providing citizens with a range of details about the electoral process), and the second treatment emphasizes the coordination element (by encouraging citizens to become actively engaged in the electoral campaign); the third treatment can be interpreted as an interaction of the first two.

Subject recruitment followed a representative sampling process. 161 polling locations were randomly selected from the ones having mobile phone coverage. Within the area of a polling location, treatment targeted a specific group directly, which was randomly sampled at the level of households with cell phone access. To measure the effects of these voter education interventions, we use the official electoral results and administrative records from electoral observation (measuring electoral problems during the campaign and election-day) at the polling location level, and we employ survey and behavioral data at the individual level. Our outcome measures at the individual level are innovative in two main ways. The first relates to voter turnout. Treatments may induce respondents to assert that they voted even if they did not. To counter this bias, the post-election survey asked a comprehensive batch of questions on the election-day experience, thereby testing respondents' knowledge about the voting process, as well as checking inked fingers. The second relates to a behavioral measure of demand for accountability. Experimental subjects in all locations were invited to send cell phone SMS proposing their priorities in terms of policy measures to the president-elect. They were informed that the contents of these messages would reach the president personally. We are able to record the individuals that sent messages through cell-number matching. Since sending an SMS is a costly action, we interpret it as an incentive-compatible measure of demand for accountability. We also ask standard survey questions on information and perceptions about politics. Note that with respect to individual data we are able to contrast treatment effects on individuals that were directly targeted by the treatments to individuals that were not targeted by the treatments but live in treated locations.[3]

We find clear effects of all three treatments on increasing official voter turnout, by close to 5 percentage points. The treatment effects are not statistically different from each other. These effects are also identified in the survey data, where they are slightly larger. We observe that (mainly) the civic education and the newspaper treatments benefitted incumbents and harmed challengers in terms of electoral score. This pattern of vote shifts may be natural in a clientelistic

[3] 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).

society dominated by the ruling party: higher awareness about the elections may increase competition across locations in terms of turnout for incumbents and future benefits. We also report that the newspaper led to higher demand for accountability: the probability that an experimental subject sends a text message about his/her policy priorities increased by 10 percentage points. The newspaper was also the only treatment that affected electoral problems as reported by electoral observers. Namely, incidence of these occurrences was reduced by 0.58 problems. These findings suggest that the newspaper intervention was the most effective treatment at mobilizing citizens' demand for accountability, and at securing improvements in the electoral process. All treatments increased information about politics, as tested in survey questions. However, the different treatments induced quite different perceptions about the sponsors of the treatments and about politics. We have evidence that civic education and the newspaper increased trust in the electoral commission – an official institution that may be perceived as close to the incumbent. We also find that civic education leads respondents to demand more authority and to see improvements in terms of electoral problems, while that the hotline increases the perceived neutrality of the state and induces respondents to see worsening electoral problems. The newspaper yields a mix of these findings, consistently with the interpretation of the newspaper contents as an interaction of the other two treatments.

Apart from contributing to our knowledge of the political economy of elections in developing countries, this paper broadly 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 should mention specifically the work of Dale and Strauss (2009), who look at the effect of text messages reminding citizens to vote in 2006 elections. Note that the magnitudes of the effects on voter turnout we find in this paper are broadly comparable with the effects found in this literature for the US. Second, it links into the emerging literature on the effects of information and communication technology on various development outcomes. Jensen (2007) looks at the use of cell phones to improve market efficiency in a local fish market in India. Aker (2010) studies the effects of cell phone introduction on grain market outcomes in Niger. More closely to the information campaigns we study, Pop-Eleches et al (2011) analyze a field experiment looking at text message reminders for AIDS treatment: they find that adherence to treatment increased substantially as a result.

The paper is organized as follows. In section 2 we present the context of our field experiment, while providing a description of the recent political history of Mozambique. In section 3 we fully develop the experimental design, with treatments, sampling and assignment to treatment, measurement, and estimation strategy. The following section provides the econometric results, including balance tests, treatment effects on political behavior and information/perceptions about politics, and robustness/auxiliary findings. We conclude in section 5.

2 Context

Mozambique, a country with 22.4 million inhabitants, is one of the poorest countries in the world with GDP per capita of 838 USD in 2008 - it ranks 161 in 189 countries in terms of GDP per capita.[4] Without prominent natural resources, and with 81 percent of the population directly dependent on agriculture,[5] it is an aid-dependent country with official aid assistance accounting for 22 percent of GNI in 2008.[6]

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 massively to just 36 percent in 2004. Figure 1 depicts the main parliamentary election outcomes over the four elections.[7]

<Figure 1 near here>

[4] World Development Indicators, 2009.
[5] CIA World Factbook, 2010.
[6] World Development Indicators, 2009.
[7] Since 2000 the quality of governance has been rated annually for each of the 53 countries of Africa by the Ibrahim Index. 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.

The common factor across all national elections has been allegations of electoral irregularities (primarily ballot fraud) by FRELIMO, with clear consequences over the final results. While these claims have been made primarily by RENAMO, international observers have corroborated them on several occasions. For instance, in the aftermath of the 2004 elections, the Carter Center released a statement outlining the numerous shortcomings encountered.[8] Hanlon and Fox (2006) provide convincing statistical evidence for ballot fraud during the 2004 elections.

Armando Guebuza became FRELIMO's leader and president in 2004, succeeding Joaquim Chissano. Guebuza had an important record within FRELIMO, from the time fighting against the Portuguese to the early years as minister of the interior under Samora Machel. He became a wealthy and powerful businessman after the privatization of public companies in the 90s. 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[9] round of elections of October 28, 2009. The 2009 elections were relatively calm, with FRELIMO and Guebuza expected to win. Prior to the elections, Dhlakama had been increasingly discredited and was widely seen as an outdated leader, often referring to the possibility of taking up arms (which was widely considered as anachronistic). 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. Being from a younger generation not directly linked with the heavy references of the past (independence and civil war), Simango was becoming increasingly 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,[10] the exclusion of some parties (including MDM) by the National Electoral Commission of Mozambique (CNE) from contesting elections in several districts, occurrences of campaign violence, and many instances of intimidation and use of state resources for campaigning.

[8] Carter Center, 'Observing the 2004 Mozambican Elections' – Final Report.
[9] The provincial assembly elections happened for the first time in Mozambique during the 2009 round of elections. Information about the then newly-created provincial assemblies, namely about their candidates and very limited powers, was extremely reduced. We therefore focus in the analysis of this paper on the presidential and parliamentary elections.
[10] See De Brito (2008) for a review of voter registration problems in Mozambique.

The elections were conducted in a relatively unproblematic manner, as witnessed by national and international observers. These observers generally considered the elections to be following appropriate international standards, despite the existence of many small irregularities.[11] 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, building on the socialist-type local political institutions that remain to date and on the dependence of the majority of the population on state-allocated resources.[12]

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 a 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 political accountability observed in Mozambique may be the result of deficient channels of information dissemination, exacerbated by poverty and low education. De Brito (2007) underscores the marked decreasing trend of voter turnout, distinctive by regional standards. He highlights the role of international donors in providing incentives to Mozambican politicians, perhaps at the expense of truly strengthening Mozambique's civil society.

3 Experimental design

[11] 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'. Observatorio 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.' Observatorio Eleitoral, 'Declaration about the Presidential, Parliamentary, and Provincial Assembly Elections', 2009.

[12] For instance, our survey respondents reported that local chiefs were responsible for attributing residence documents (85 percent), essential for school attendance among other benefits, for undertaking dispute resolution (88), for allocating wells (70), land (55), public funds (43), for distributing food/seeds (29), and construction materials (19).

The main objective of this paper is to document the effects of voter education interventions on voting and political behavior, electoral problems, as well as on information and perceptions about politics. In this section, we begin by a detailed description of the interventions that were randomized for the field experiment we conducted during the 2009 elections, i.e., the civic education message, the hotline for electoral problems, and the newspaper. We then continue with the sampling design, the method used for assignment to treatments, and the details of our measurement, which makes use of official voting results and electoral observation records at the level of the polling location, and of behavioral and survey data at the level of the individual. Finally, we present our estimation approach, with the econometric specifications we employ.

3.1 Treatments

We collaborated with newspaper @Verdade (http://www.verdade.co.mz/) and a consortium of eight Mozambican NGOs, named Observatorio 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 manifest civic education and social responsibility mandate. Observatorio Eleitoral is an organization blending the specific efforts of its member organizations in the area of good electoral conduct and electoral observation. Its members are the main religious civil society representative organizations in the country (Catholic, other Christian, and Muslim), and prominent national governance NGOs.[13] 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. Like us, both organizations understood this project as an opportunity to learn about innovative means of delivering voter education in the Mozambican context. We now turn to the description of each specific intervention. Note that different interventions were allocated to different polling locations. Each intervention was directed at a specific set of experimental subjects within a location. We call these subjects the targeted individuals.

[13] Observatorio 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).

The civic education treatment was based on a set of messages providing citizens with specific information about the 2009 elections. The process was initiated with a door-to-door campaign approximately a month before the elections in 40 experimental locations. This campaign was implemented during the baseline survey and was centered on the distribution of a leaflet designed and made available by the electoral commission (CNE/STAE). The leaflet explained in detail the voting steps on the election-day. 10,000 leaflets were distributed (i.e. 250 per location) primarily to targeted individuals. It 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 (i.e., for 14 days), all targeted individuals in the civic education areas received a set of daily text messages on the cell phone number they provided during the baseline survey. Specifically, they received five messages a day. On each day, messages were chosen from a set 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 was based on the dissemination of two short-code phone numbers that were contracted with the two cell phone operators in Mozambique (Mcel and Vodacom). These short-codes constituted an electoral hotline in the sense that citizens were invited to send text messages to those numbers reporting electoral problems they observed in their locations.[14] The dissemination of this hotline happened in 40 experimental locations. During the baseline survey, we conducted a door-to-door campaign providing information on how the hotline could be used. As part of this sensitization campaign, we distributed 10,000 leaflets (250 per location) primarily to targeted individuals, providing the basic information about the hotline system: short-codes, examples of problems, format of reports to be sent - specifically, polling location name first, description of the problem second -, and the name of the sponsors of the initiative. The leaflet is

[14] 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.

depicted in Figure 3. Each leaflet was printed on both sides of one page, with each side providing different SMS examples, one for the electoral campaign, the other for the election-day. The leaflets were location-specific, so that they featured the name of the polling location corresponding to the location where the leaflets were distributed. The intention was to minimize any potential mistakes by experimental subjects when writing messages for the hotline.

<Figure 3 near here>

We promised that the contents of reports would be passed to the media for dissemination, and also shared via SMS with all other targeted individuals in the hotline locations. Before any dissemination took place, each report received on the hotline was verified with local correspondents we hired in each of the hotline locations. This process was managed online through the Ushahidi system (an open-source software - www.ushahidi.com), which allowed our viewing of received reports in real time. This is software that 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.[15]

The newspaper treatment was based on the distribution of free newspaper @Verdade in 40 locations. Despite being the highest circulation newspaper in Mozambique (with a minimum of 50,000 certified copies per week), the newspaper was only systematically distributed in the city of Maputo. We agreed with the newspaper founder and director that, specifically for this project, the newspaper would be distributed weekly in all newspaper 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 lasted until the post-election survey (November 2009). The newspapers were given primarily to targeted individuals. 5,000 copies of the newspaper were distributed each week, with a total of 125 at 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.

[15] 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 above by including a version of the CNE/STAE leaflet on the steps for voting (see middle panel of Figure 4) and by providing information on specific candidates, political parties and the election-day (similar to our civic education text messages). The newspaper also sponsored a national hotline for reporting electoral problems, serving as one of the most important decentralized sources of news during the electoral campaign and election-day in Mozambique: 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 our 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 Observatorio Eleitoral). It therefore had clear nationwide coverage, although there was probably an emphasis on province capitals.

<Figure 4 near here>

Despite the fact that all three treatments provide electoral information and attempt to mobilize voters to participate in the elections, the civic education treatment can be interpreted as focusing on the dissemination of detailed information about the elections, whereas the hotline treatment can be interpreted as centering on citizen coordination during the electoral process. Despite the fact that a newspaper is a well-identified object (which may be understood in a specific manner by experimental subjects), since its contents focused on electoral education that mirrored our civic education and hotline treatments, it can be interpreted as a blend of the first two treatments.

3.2 Sampling and assignment to treatment

The sampling framework of our experiment was constructed from the 2004 electoral map of the country (as the 2009 map only became available few weeks before the election). The unit of enumeration is the area covered by the corresponding polling station. As the use of cell phones was central to all our treatments, we eliminated all polling locations without cell phone coverage. For that purpose, we obtained detailed data from the two cell phone operators on the geographic

location of each of their antennae. These were then plotted on a map using their geographical coordinates, with a five-km coverage radius drawn for each. Any polling locations outside these balls were dropped. The remaining polling locations constitute our sampling framework. Remarkably, 60 percent of all polling locations in Mozambique were found to be covered by at least one operator. We selected 161 enumeration areas for our field experiment from our sampling framework, including 40 with civic education, 40 with the hotline, 40 with the newspaper, and 41 serving as control group (without any treatment administered). These enumeration areas are nationally representative of the population of Mozambique that has access to mobile phone coverage,[16] meaning that each registered voters in the considered universe had the same probability of having his/her enumeration area sampled. The selection of these locations is the product of two-stage clustered representative sampling, first on provinces, then on enumeration areas. The number of registered voters was used as sampling weight, based on information provided by the CNE/STAE in their publication of disaggregated electoral data for the 2004 elections. During the baseline survey, in the event that we found no cell phone coverage in any specific enumeration area, we replaced it by the closest polling location with cell phone coverage. That happened in seven locations.[17]

The project took place in four provinces, Cabo Delgado, Zambezia, Gaza, and Maputo-Province. The allocation of the treatment and control groups to the full set of enumeration areas (our experimental locations) followed a standard randomization procedure by which (i) clusters of four closest enumeration areas were formed in each province, based upon geographic data on the polling locations; and (ii) each treatment was randomly allocated to one enumeration area in each cluster (using the same probability for all). The final full sample of experimental locations, with each treatment represented, is depicted in the map of Figure 5.

<Figure 5 near here>

In each of the enumeration areas we conducted two face-to-face surveys, one before the elections, and one after.[18] Sampling within each enumeration area followed standard random procedures

[16] This was estimated at approximately 44 percent of the population in 2008 (GSM Association, 2009).
[17] We have 41 locations in the control group: this is due to the fact that we surveyed in one substitute location that was a posteriori discovered not to be needed. Results were found not to depend on the inclusion of this enumeration area.
[18] 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

during the baseline survey: namely, enumerators starting from the center of the enumeration area, typically the polling location, sought the nth houses. However, selection of the household was conditional on 'having access to a cell phone' for receiving or sending calls/messages. This criterion included households that did not own a cell phone, but had access to one via a neighbor or family member within the enumeration area.[19] Moreover, enumerators selected household heads or their spouses, and so we do not have representativeness within the household. The baseline survey included 1,766 households/respondents, 11 per enumeration area. 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] To check for selective attrition in survey data, we verify ahead whether observable characteristics vary systematically across treatments for the post-election sample. We also run our main survey results using a multiple imputation technique to account for missing observations.

Treatments were also randomized across individuals within each treated enumeration area. Of the 11 individuals interviewed at baseline per treated enumeration area, 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 last sub-section.

3.3. Measurement

Since the main objective of the treatments was to increase electoral participation, it is of particular importance to analyze the official results for the presidential and parliamentary elections of 2009 at the level of the ballot station. These were made available by the CNE/STAE almost three years after the elections. Polling locations in the disaggregated results were matched with the enumeration areas in our experiment, which as mentioned were defined by polling

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handhelds. At least one of authors was in the field at all stages of the project and directly managed operations.

[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 cell 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.

locations themselves. Apart from voter turnout, these data include voting for specific candidates/parties, blank and null votes.

Another outcome of interest is electoral problems. We have available a rich dataset of informal and formal electoral observation in the provinces of Cabo Delgado, Zambezia, Gaza, and Maputo-Province. 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. 75 locations in the experimental provinces were reported to have had problems during the electoral campaign and election-day through the newspaper's hotline. The problems reported are quite diverse, as there was no set structure for their classification. Second, we were given access to the campaign observation sheets filled by the formal national electoral observers of Observatorio Eleitoral. 157 polling locations were reported to have had problems during the electoral campaign. These observation sheets were structured as a questionnaire. It asked mainly about the use of public resources for campaigning, vandalism and intimidation; it also asked about specific instances of violation of the electoral law (e.g., breach of noise limits when campaigning). Note however that the questionnaire did not include questions about all types of campaign problems (e.g., vote-buying). Third, we were given access to the election-day observation sheets filled by the formal national electoral observers of Observatorio Eleitoral. 92 polling locations were reported to have had problems during the election-day. 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 consulted 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. 36 polling locations were reported to have problems during the election-day. These sheets were structured as a questionnaire, which asked about violence and intimidation, and about procedural deficiencies of the voting.

We matched the reported polling locations with the 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 dead people.

Apart from the use of the above administrative data sources at the level of the polling location, we base an important part of our analysis on data collected at the individual level. Since the main objectives were to measure electoral behavior (participation and voting patterns), the degree of information, and perceptions about politics (namely about electoral problems), both survey instruments were designed to elicit evidence on each of these dimensions.[21]

The voting data that we employ to derive the impact of the treatments are based on self-reported information gathered in the post-election survey. We tried to be particularly careful with our measurement of voter turnout during this survey.[22] We dedicated a module of the questionnaire to asking questions about all details of the election-day experience of the respondent. We construct five alternative measures of individual turnout. The first is self-reported turnout. The second is an indicator of whether the respondent showed without hesitation his/her inked finger to the enumerator – 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 is a composite index measuring how well the respondent answered all the questions on the election-day experience – each answer is coded according to how convincing the answer was relative to the likely voter turnout experience. The fourth is a composite index focusing on questions that tested the respondents' knowledge about ballot station facts: apart from the inked-finger measurement, these include questions about the format of the ballot papers and of the ballot boxes. The fifth is a final enumerator assessment on whether the respondent voted or not – enumerators were trained to watch body language. The 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.

<Table 1 near here>

Still relating to individual political participation, 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 161

[21] The survey instruments in Portuguese are available upon request.

[22] 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.

experimental locations, which invited them to send SMS messages proposing policy priorities to the president-elect for his new mandate. We were clear in conveying the limited extent of the initiative (a small number of experimental localities in the whole of Mozambique), and promised that the contents of these messages would reach the President in person. As with the hotline, each message sent by experimental subjects had a small monetary cost. Sending the message therefore represents a clear costly action. It was observable to us, as all cell phone numbers that sent messages were recorded and matched with those of the experimental subjects. We interpret the sending of an open letter message as an incentive-compatible measure of demand for political accountability - arguably this is a better measure of demand for political accountability than any survey question aimed at capturing the same concept. 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.

<Figure 6 near here>

Our survey data also includes information on a variety of individual measures of information about the elections, and perceptions about politics (namely about electoral problems). Specifically, we constructed measures for cell phone use, knowledge and perceptions about the sponsors of the treatments, interest about the elections, information about the elections, confusion between state and ruling party, call for authority, electoral problems in general, vote miscounting, vote-buying, and electoral violence and intimidation. Most of these measures are indices. Only a subset of the survey questions behind these measures was asked at the baseline. 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.[23] Some measures of perceptions about electoral problems are broadly comparable with the administrative data on verified occurrences reported by the electoral observers. 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),

[23] 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.

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.[24]

We also changed 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 responses about politics to whatever they perceived to be the views of the sponsors of the experiment. 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, 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>

3.4 Estimation strategy

Our empirical approach is based on estimating treatment effects on the variety of outcome variables that we have available on voting behavior and political participation, electoral problems, information, and perceptions about politics. We now describe the main econometric specifications we employed for the estimation of these parameters.

Our design allowed us to estimate average treatment effects in different ways. Most simply, the effect of interest (β) could be estimated through the specification:

$$Outcome_{i,l,post} = \alpha + \beta T_l + \varepsilon_{l,i,post}, \quad (1)$$

[24] 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 random assignment group mean.

where $Outcome$ is an outcome of interest, $l, i, post$ are identifiers for locations, individuals (in case the data are at the level of the individual), and time - specifically, $post$ represents the post-election measurement -[25], and T_l is a vector of three dummy variables representing the three treatments (civic education, hotline, and newspaper) with value 1 for treated units.

When employing data at the level of the individual, we are interested on direct treatment effects on the targeted individuals, in which case we contrast targeted individuals in treated locations to individuals in control locations; we are also interested on 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 at targeted individuals primarily, general awareness about the contents of campaigning at the enumeration area level was difficult to restrict as fieldworkers distributing materials attracted attention.

In this setting, because of limited sample size, we add geographical dummies, location-level controls, and individual-level controls to compose our main specification. 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. We then have the following core specification:

$$Outcome_{i,l,post} = \alpha + \gamma Y_l + \delta Z_l + \theta X_i + \beta T_l + \varepsilon_{l,i,post}, \qquad (2)$$

where Y_l is a province-dummy vector, Z_l is a vector of enumeration-area controls, and X_i is a vector of individual (demographic) controls.

[25] 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. In terms of individual survey measurements, we have some baseline data available, but that is limited to a subset of individual survey questions. We ran difference-in-difference regressions on these outcomes and find, as expected, similar results to the simple-difference ones shown in the paper.

For ease of interpretation and transparency, we employ OLS estimations throughout the paper. We cluster standard errors at the level of the enumeration area in all regressions at the individual level.

4 Econometric results

In this section we present our empirical results. We begin with standard balance tests, checking whether the randomization was effective in selecting comparable treatment and control groups. We then focus on our main results: the effects of the interventions on voter turnout and voting patterns, as given by official results at the polling location level. We analyze individual political behavior, including turnout, our behavioral measure of demand for accountability, and voting choices. We then explore the data on electoral problems made available by electoral observers. Subsequently we quantify the effects of the treatments on information and perceptions about politics, as given by individual survey data. We finally present a set of robustness and auxiliary results: we employ a multiple imputation method to take into account attrition for survey data; and we look at heterogeneous effects (the effects of the treatments interacted with demographic variables).

4.1 Balance

Tables 2 display means for the control group and differences between control and treatment groups in our experiment. The statistical significance of the differences is tested to 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. 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. They also distinguish between targeted and untargeted groups within treatment groups. 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 sponsors of the campaign, interest about the elections, and electoral problems. Like for individual demographics we present statistics for full and post-election, targeted and untargeted, 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. For individual characteristics, we display a large number of estimates. However, for the baseline sample, only age for the newspaper, household size for the hotline, and three occupational dummies for the civic education (mainly for the untargeted individuals) exhibit significant differences. The picture is slightly changed when the post-election sample is considered: only household size for the hotline, married or in a union for the newspaper, and two occupational dummies for the civic education and the newspaper show statistically significant differences when the targeted sample is considered; for the untargeted a few more variables/groups show significant differences, i.e., schooling for the hotline, Chironga ethnicity for the newspaper, Catholic for the hotline, and three occupational dummies for the civic education. 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.[26]

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 sewage. 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

[26] Overall we compute 483 differences across comparison groups and find 10 statistically significant differences at the 10 percent level, 2 at the 5 percent level, and 5 at the 1 percent level. We compute 171 joint significance tests and find 9 statistically significant ones. This pattern is generally consistent with what we would expect from randomized assignment.

of the households owned a cell 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.

We now look at baseline outcome individual variables across treatment and control locations. Looking at the baseline sample, we see some but limited evidence of first reactions in terms of the intention to turn out to vote (civic education for the targeted and newspaper for the untargeted), trusting @Verdade (newspaper for the targeted), and expectations about vote-buying in 2009 (newspaper for the untargeted). In terms of evidence for a conformity bias, i.e., regarding reports about the past, we can only mention that targeted respondents in newspaper areas are less likely to state that they voted in 2004. For the post-election survey, results are only slightly different: for the targeted, only trusting @Verdade is statistically significant in newspaper areas; for the untargeted, voting intentions, views about the electoral commission, and expectations about vote-buying yield some statistically significant differences, particularly for the newspaper. These results lead us to conclude that we may have had some (limited) first reactions, primarily to the distribution of newspapers. We seem not to find much evidence in favor of conformity biases when considering reports about the past: most of these reports do not change with the distribution and discussion of the campaign materials.

4.2 Official voting results

We now turn to our main treatment effects. We start by analyzing the official voting results at the level of the polling location. Tables 3 display the effects of the interventions on voter turnout and the scores of the main candidates and parties at the presidential and parliamentary elections. These tables also show the treatment effects on the shares of blank and null votes in both elections. For each outcome variable we first control for provincial dummies only, and then add location controls.[27]

<Tables 3 near here>

In the presidential election, we find clear effects of all three treatments on increasing voter turnout. These effects are all close to 5 percentage points, significant at the 5 percent level when

[27] 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.

including controls (Table 3a, column 2). We find similar estimates for the parliamentary election: only the newspaper effect is slightly larger, close to 6 percentage points, when including controls (Table 3b, column 2). These effects on voter turnout are not statistically different across the different treatments.

Regarding the scores of the presidential candidates, 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 increases Guebuza's score by 5 percentage points (significant at the 5 percent level) and decreases Dhlakama's score by 3 percentage points (significant at the 5 or 10 percent level); the newspaper increases the score of the incumbent by 4 percentage points (significant at the 10 percent level), and seems to decrease Simango's score by 1 percentage point (only significant with controls at the 10 percent level). Again, we have a similar picture for the parliamentary elections. However, only the civic education treatment is significant: the positive effect on the score of FRELIMO is 4 percentage points (significant at the 10 percent level), and the negative effect on the score of RENAMO is 4 percentage points (significant at the 5 percent level). Note that for the parliamentary election, MDM was not allowed to run in our experimental provinces.

An additional note on blank and null votes: we do not observe significant effects of the treatments on the share of blank votes, but we identify negative effects of civic education (parliamentary elections) and the newspaper (presidential elections) on the share of null votes. These effects are positive and close to 1 percentage point.

We conclude that the voter education interventions we study in this paper achieved a clear impact on voter participation in the elections of October 2009, by close to 5 percentage points for all treatments. We also have some evidence that incumbents were benefitted and challengers harmed in their electoral scores, even though statistical significance can only be documented for the civic education and (less so) for the newspaper.

A possible interpretation for these results may be that the voter education interventions increased the marginal benefit of participating in the election. This may have worked through raising extrinsic incentives related to the importance of the election and its outcome, or through increasing intrinsic incentives related to civic-mindedness. The vote shift from Dhlakama/RENAMO to Guebuza/FRELIMO may be explained by the overwhelming dominance

of the incumbents. Since electoral competition was very limited, 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: higher turnouts would be rewarded with post-election benefits at the local level. Then voter education could mobilize voting for the incumbents. An alternative interpretation for this vote shift may come from the fact that it was caused by the civic education treatment (and by the newspaper to less extent), 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 negative messages reporting electoral problems. These differences across treatments in terms of how they were perceived may then have produced the changes in voting that we observe. We provide below consistent evidence on survey perceptions about the interventions.

4.3 Individual voter turnout

Tables 4 report on our regressions estimating the effects of the treatments on individual (survey-based) voter turnout. We employ the five different proxies of individual turnout, based upon the survey module on the election-day experience. These variables are the simple self-reported turnout measure, the measure based on whether the respondent showed the right (inked) finger, the composite of all questions on the election-day experience (coded in terms of how likely it was that the individual turned out to vote), the measure focusing on specific knowledge of the ballot station facts (the number of ballot papers, whether there were photos of the candidates, the number of ballot boxes, whether they were transparent, and whether they were coloured), and the measure based on the interviewer's assessment of the likelihood that the respondent voted after asking all the related questions. Note that individual turnout in the control group using these five turnout versions is: 88 (self-reported), 81 (finger), 79 (average questions), 76 (average questions - ballot facts only), and 75 percent (interviewer assessment). Note the 13 percentage-point difference 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.[28] For each

[28] Note that 75 percent turnout in our control group is still much higher than the 44 percent average turnout in the control polling locations (see Tables 3). This difference may be due to the fact that we sampled heads of households and spouses who had access to cell phones. These individuals are more active politically than the full pool of voters, as verified in Afrobarometer data for Mozambique. Namely, in Afrobarometer Round 4 for Mozambique, we find that heads of households, mid-age respondents (30-50 years), and individuals using cell-phones are more likely to have voted in 2004 and to be interested in public affairs.

outcome we show one regression with province dummies and one regression with location controls and individual demographic controls[29] in addition to the province dummies. Table 4a compares targeted individuals in treated locations to control individuals, and Table 4b compares untargeted individuals in treated locations to control individuals.

<Tables 4 near here>

We observe clear effects of the civic education and the hotline treatments on almost all individual turnout measures when considering targeted respondents (Table 4a). The size of the effects is 4-8 percentage points for the civic education treatment, and 5-9 percentage points for the hotline treatment. The interviewer assessments yield the largest effects among the different individual turnout indicators – these are significant at the 1 percent level, even without controls. The newspaper treatment also yields positive effects on voter turnout for the targeted, but they are not statistically significant at standard levels. Note however that we do not find statistically significant differences across the treatments. Turning to untargeted respondents (Table 4b), we find clear effects on all individual turnout measures for the hotline and the newspaper. These effects are 7-10 percentage points for the hotline, and 6-12 percentage points for the newspaper. The civic education treatment also yields positive effects, but they are not significant. We should note the larger effects found for the untargeted relative to the targeted when considering the newspaper treatment: a possibility is that not receiving the newspaper on one's hands raised additional interest about the newspaper received by neighbors. We conclude that all interventions seem to have had a considerable impact on voter turnout, as measured in the survey: the civic education mostly had direct effects, the hotline had both direct and indirect effects, and the newspaper mostly had indirect effects. If we believe that the effects estimated from polling location official records should be an average of the effects on the targeted and the untargeted individuals, the results at the individual level are generally consistent with the official ones.[30]

We should also mention that it is also possible that the baseline survey per se induced respondents to turn out to vote.

[29] These include gender, age, household characteristics, marital status, schooling, ethnicity, religion, occupation, assets and expenditure.

[30] For instance, if we bundle together untargeted and untargeted individuals as treated, and contrast them to control individuals, all treatment effects are then positive and significant for most our measures of individual turnout – results available upon request. A note of caution is however required for this aggregation exercise, as our survey is not representative of voters. That may explain why our estimates at the polling location level roughly correspond to the lower bound of the range of significant effects we find for our survey sample.

4.4 Individual behavioral demand for accountability

We now turn to the effects of the interventions on respondents' demand for political accountability, as measured by the sending of SMS messages under our open letter system. Respondents were instructed to outline their policy priorities (via SMS) to the new president-elect. Any message sent was costly and therefore can reasonably be interpreted as representing demand for political accountability. We matched the cell numbers of the 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 to the open letter system. We run regressions with and without location and individual controls, both for targeted and untargeted individuals. The results are displayed in Table 5.

<Table 5 near here>

We first note that 15 percent of the experimental subjects in the control group sent at least one message to the open letter. This represents a clear degree of adherence to the initiative. We find positive effects of the civic education and newspaper treatments on the sending of messages for the open letter. However, we only find a statistically significant impact for the newspaper treatment when considering the targeted individuals, which is 10 percentage points (significant at the 5 percent level with controls). The difference between the newspaper effect and the effects of the hotline is statistically significant. We may then conclude that only the newspaper clearly increased the demand for political accountability as measured by our open letter. That was the case for the targeted individuals. It is possible that the treatment substance relating to political accountability was relatively complex, and that it required detailed information (as in the newspaper) and direct contact (as for the targeted).

4.5 Individual voting patterns

We now analyze the effects of the interventions on self-reported voting. In Tables 6 we depict the effects of each treatment on voting for the three presidential candidates and for the two main parties, i.e., FRELIMO and RENAMO. We display regressions with province dummies, and regressions with location and individual controls in addition to province dummies. We also distinguish between effects on the targeted and effects on the untargeted.

<Tables 6 near here>

Starting with targeted individuals vs. control individuals, we find that civic education and the hotline increased voting for Guebuza and FRELIMO. These effects are 5-7 percentage points (significant at the 5 or 10 percent level). Civic education also has a 1 percentage-point negative impact on Dhlakama's voting, but only in the specification with controls (significant at the 10 percent level). Concerning the untargeted vs. control comparison, we find effects for civic education and the newspaper. The pattern is the same as for the targeted: a positive effect of the newspaper on voting for Guebuza and FRELIMO, on the 8-9 percentage-point range, and negative effects of civic education and the newspaper on voting for Dhlakama and RENAMO, close to 1 percentage point (all these effects are significant at the 10 percent level).[31] We conclude that, as observed in the official electoral results at the level of the polling location, Guebuza/FRELIMO seem to have been benefitted and Dhlakama/RENAMO seem to have been harmed by the voter education interventions that we study. We have evidence of effects through both targeted and untargeted individuals. As discussed above, this vote shift may be related to the overwhelming dominance of Guebuza/FRELIMO, which may have turned the election into a turnout contest for incumbents. The relatively positive messages in the civic education and the newspaper may have also been associated to the incumbent through CNE/STAE.[32]

4.6 Electoral problems reported by electoral observers

Table 7 presents treatment effects on electoral problems as reported by electoral observers during the electoral period of October 2009. We had access to four administrative sources of data for electoral problems. The first is the hotline of newspaper @Verdade, which was disseminated nationwide. Through this hotline, citizens reported problems during the electoral campaign and election-day through SMS (analogously to the hotline treatment). The second is the campaign observation sheets of Observatorio Eleitoral, which were filled by formal electoral observers: the

[31] We also ran regressions of changes from pre-election intentions to reported electoral behavioral in the 2009 elections. For the parliamentary elections, targeted respondents, we find that the hotline increased voting for FRELIMO and decreased abstention for those who stated an intention to vote for FRELIMO. Civic education decreased abstention for those intending to abstain. Civic education and the hotline increased abstention for those who stated an intention to vote for RENAMO.

[32] Another specific interpretation for the vote shift is the violence perpetrated close to some of our experimental locations in Cabo Delgado by the main RENAMO convoy during the electoral campaign: increased awareness about the elections in treated locations (which we prove below) may then have mediated the changes in voting. Note that these occurrences were specifically reported through the hotline treatment, which may explain the voting effects of the hotline but only for the targeted.

questions asked in these sheets relate mainly to the use of public resources for campaigning and intimidation. The third is the election-day observation sheets of Observatorio Eleitoral, filled by their formal observers deployed to ballot stations during the election-day: the questions asked in these sheets relate mainly to violence and intimidation, as well as electoral procedural deficiencies. The fourth is the election-day observation sheets of UNDP Mozambique, filled by their international electoral observers: again, the focus is violence and intimidation, as well as procedural problems at the ballot stations. Each problematic polling location in our experiment was classified in terms of 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 our polling locations. 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 have 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 (significant at the 10 percent level). 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, and – this effect is statistically significant at the 10 percent level. 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, we find that there seems to be a negative impact of all treatments on campaign misconduct, and violence and intimidation. For election-day misconduct all estimates are particularly close to 0 (considering the much larger size of the corresponding standard errors). 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 5 or 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. Election-day misconduct seems to be unaffected by our treatments. Indeed, our voter education interventions, which happened primarily during the campaign period, were more centered on participation and voter behavior than on legal procedures at the ballot station.

4.7 Individual survey measures of information and perceptions about politics

Why do we observe these changes in voter turnout, electoral preferences, and the demand for accountability? In this section we turn to an analysis of survey outcomes on respondents' knowledge of and perceptions about politics, which may constitute mediators for our main outcomes on behavior. Specifically, in Tables 8, we look at measures for cell phone use, knowledge and perceptions about the sponsors of the treatments, interest about the elections, information about the elections, confusion between state and ruling party, call for authority, perceived electoral problems in general, and perceptions about vote miscounting, about vote-buying, and about electoral violence and intimidation. All survey-question measures are normalized as z-scores. Some are aggregated in indices 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. We also distinguish between effects for targeted and untargeted individuals.

<Tables 8 near here>

We find that both the hotline and the newspaper increased reported cell phone use. The effects for the targeted are 0.22-0.23 standard deviation units (significant at the 5 percent level with controls). Slightly lower effects are found for the untargeted (only significant at the 10 percent level). This is easily understood as both the hotline and the newspaper treatments asked individuals to use cell phones (for the respective hotlines).

As expected, when considering targeted individuals, knowledge and trust related to @Verdade increased significantly for the newspaper treatments, but not for the other treatments. These effects are 0.61-1 standard deviation units (significant at the 1 percent level). When considering untargeted individuals, only trusting @Verdade increased: the magnitude (0.25 standard deviation units) and significance (at the 10 percent level) of this effect are smaller. All treatments increased

trust in the electoral commission, even if for the hotline the only significant effect concerns the targeted and arises when employing controls. Estimates are 0.16-0.27 (significant at the 5 or 10 percent levels). The robust effects of civic education and the newspaper may be explained by the fact that these treatments distributed information sponsored by the CNE/STAE. As a consequence, civic education and newspaper may be more frequently associated with incumbents. Note however that only the hotline treatment increased the perceived neutrality of the electoral commission (only for the targeted). The size of the effect was 0.17 standard deviation units (significant at the 5 percent level). A possibility is that some citizens still viewed the hotline as sponsored by the electoral commission: the fact that electoral problems were disseminated by the hotline treatment (some of them reporting misbehavior by the supporters of the incumbent) may have made citizens perceive the electoral commission as more neutral.

We now turn to interest and information about the elections. Interest about the elections was only affected, positively, by civic education, and only for the targeted. The estimate is 0.12 standard deviation units (significant at the 10 percent level with controls). However, for targeted individuals, all treatments increased our composite of testable information questions about the elections, which includes whether respondents knew about which elections happened on the 28th of October, the duration of a presidential mandate, the names of the candidates in the presidential election, the names of the parties in the parliamentary election, and the meaning of the word abstention. The magnitude of these effects is 0.16-0.17 standard deviation units (significant at the 1 or 5 percent levels). For untargeted individuals, only the hotline increased information significantly (0.18 magnitude, significant at the 5 percent level). We can then prove that our treatments were successful in transmitting information about the elections to citizens – this is particularly the case for the individuals that were directly treated.

We now devote our attention to survey outcomes relating to perceptions about politics in general. Our index of confusion between state and FRELIMO is very clearly affected by the hotline and the newspaper. For the targeted, this type of confusion decreases by 0.19-0.24 standard deviation units (significant at the 1 percent level). For the untargeted, it decreases by 0.16-0.20 standard deviation units (significant at the 10 percent level). Our measure of call for authority increases for civic education, for both targeted (by 0.15 standard deviation units, significant at the 1 percent level), and untargeted (0.12 standard deviation units, significant at the 10 percent level) individuals, but also for the newspaper, for targeted individuals only (0.09 standard deviation units, significant at the 10 percent level). A possible explanation is that, as mentioned before, the

civic education treatment may have been perceived as relatively biased in favor of FRELIMO, specially when compared to the hotline: then it is natural that the hotline decreases the confusion between the state and FRELIMO, and that civic education induces added demand for strong leadership. The newspaper actually achieves both, which is in line with the interpretation of the newspaper as an interaction between the two other treatments.

Finally, we look at perceptions about electoral problems. We find that, for the targeted, the hotline increases the perception that the election was generally problematic. The magnitude of this effect is 0.19 standard deviation units (significant at the 5 percent level). On the contrary, we find that, for the untargeted, the newspaper decreased the perception that the election was problematic. The size of the effect is 0.24 standard deviation units (significant at the 10 percent level). Looking at specific types of electoral problems, we find that civic education leads respondents to see less vote miscounting (only for the targeted), and violence and intimidation (for both the targeted and the untargeted individuals); the newspaper leads respondents to see more vote-buying (for the targeted), but less vote-miscounting (for the untargeted), and violence and intimidation (for both targeted and untargeted). Comparing these results to the ones generated by administrative data from electoral observation, we find that the hotline seems to produce an unrealistic deterioration in perceptions (about electoral problems in general), and that civic education seems to produce an unrealistic improvement in perceptions (about vote-miscounting, and violence and intimidation). The comparison that concerns the newspaper yields a more complex pattern: while most perception changes are improvements (the one relating to vote-buying is the exception)[33], and we actually see an improvement in the administrative data, the match on specific problems is imperfect and points to exaggerated improvements in perceptions (about vote-miscounting, and violence and intimidation). We conclude that the different treatments may have induced quite different perception biases in terms of electoral problems. We interpret them in light of a simple characterization of the treatments: civic education conveys an overall positive tone, since it focused on how the electoral system is organized; in contrast, the hotline is focused entirely upon violations of the system; the newspaper combines generic information on how the system works, with reports of violations through news and the availability of the national hotline – and so may be understood as an interaction of the two other treatments.

[33] Note 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.

4.8 Robustness and auxiliary tests

We now turn to robustness and auxiliary exercises. We begin by reporting robustness tests for panel attrition – these regard our main survey results. We then explore heterogeneous effects of the different treatments, through the estimation of the effects of the interaction of the interventions with subject characteristics.[34]

In Tables 9 we display the results for our main survey outcomes, at the individual level, when employing multiple imputation by chained equations. This method assumes data are missing at random. We found this assumption to be reasonable in our case.[35] We display results for all individual turnout measures, the open letter, voting for the three presidential candidates, and voting for the two main parties in the parliamentary election. All regressions include province dummies, and location and individual controls. We distinguish between regressions on the targeted and the untargeted individuals. We find that most measures of individual turnout seem to increase for civic education and the hotline when considering targeted individuals, and for the hotline and the newspaper when considering untargeted individuals. Estimates for the measure using interviewer assessments are 6 percentage points (significant at the 5 percent level) for the targeted, and 7-9 percentage points (significant at the 5 or 10 percent levels) for the untargeted. We observe an effect of the newspaper on the sending of the open letter (6 percentage points, significant at the 10 percent level). We also find positive effects of the hotline (for the targeted) and of the newspaper (for the untargeted) on voting for Guebuza/FRELIMO – these are 5 and 8 percentage points, respectively, significant at the 10 percent level. The newspaper also yields a negative effect (for the untargeted) on voting for Dhlakama – this is 3 percentage points, significant at the 10 percent level. The majority of these estimates yield slightly lower numbers than the benchmark estimates in Tables 4-6. Still, we can conclude that most survey results are maintained when employing the multiple imputation technique.

[34] We also check the possibility of treatment contamination to nearby enumeration areas. We regressed our main outcomes on distance to closest treatment enumeration area (distinguishing by treatment), while employing observations from control locations only – results available upon request. We find that the hotline and the newspaper may have affected political participation in nearby control locations. It is then possible that these treatment effects are underestimated.

[35] We first observe that attrition rates are not statistically different across treatment and control groups. We also verify the characteristics of the panel drops. The only significant demographic characteristics are household size (negative effect on being dropped), having a job (positive effect on being dropped), and owning a house (negative effect on being dropped) – results available upon request. We also verify that these characteristics do not correspond to the few new unbalanced characteristics across treatments and control in the post-election sample.

<Tables 9 near here>

In Tables 10 we depict heterogeneous effects of the different interventions, by using individual demographic characteristics, or location baseline voting, interacted with the treatment variables. We focus on the main outcomes in the paper, at the individual level: turnout, as measured by the interviewer's assessment, and the sending of the open letter. All regressions include province dummies, apart from the explanatory variables shown in the tables. Only targeted respondents are considered in treatment locations. We find some interesting patterns. Male subjects are more likely to send the open letter in the presence of the hotline treatment – this may be due to the fact that men are more likely to hold the cell phone and, hence, receive the hotline messages. We also find that the newspaper increases turnout particularly for older individuals. Civic education and the newspaper are more effective at increasing turnout when considering less educated and poorer (as measured by owning cattle) respondents – these individuals may be easier to influence by positive interventions with an official nature. Respondents staying at home are more likely to vote if living in a newspaper location: this indicates that the newspaper may have been particularly effective with less central individuals, who may not hold cell phones, and who may have read the newspaper after others. We also find that artisans convey a larger effect on turnout, when considering civic education and the newspaper, and a larger effect on the open letter, when considering the newspaper. Most treatments are less effective for public officials and for farmers. Finally, experimental subjects living in locations with stronger support for Guebuza in 2004 are more likely to vote in newspaper areas.

<Tables 10 near here>

5 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 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 were effective in increasing voter turnout, while providing

information about politics. We note that the free newspaper was particularly effective in increasing the demand for political accountability and in decreasing electoral problems. We find diverse effects of the three treatments on voter perceptions about politics. While civic education increased the demand for authority and the perception of electoral problems, the hotline decreased the confusion between state and ruling party, and increased the perception of electoral problems. Consistent with the interpretation of the newspaper as an interaction of the contents of the other two interventions, the distribution of the newspaper yielded mixed results on perceptions. These findings are consistent with the idea that civic education embedded a positive message with an official bend, while the hotline focused on 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 way to increase competition and 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 (For Online Publication)

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

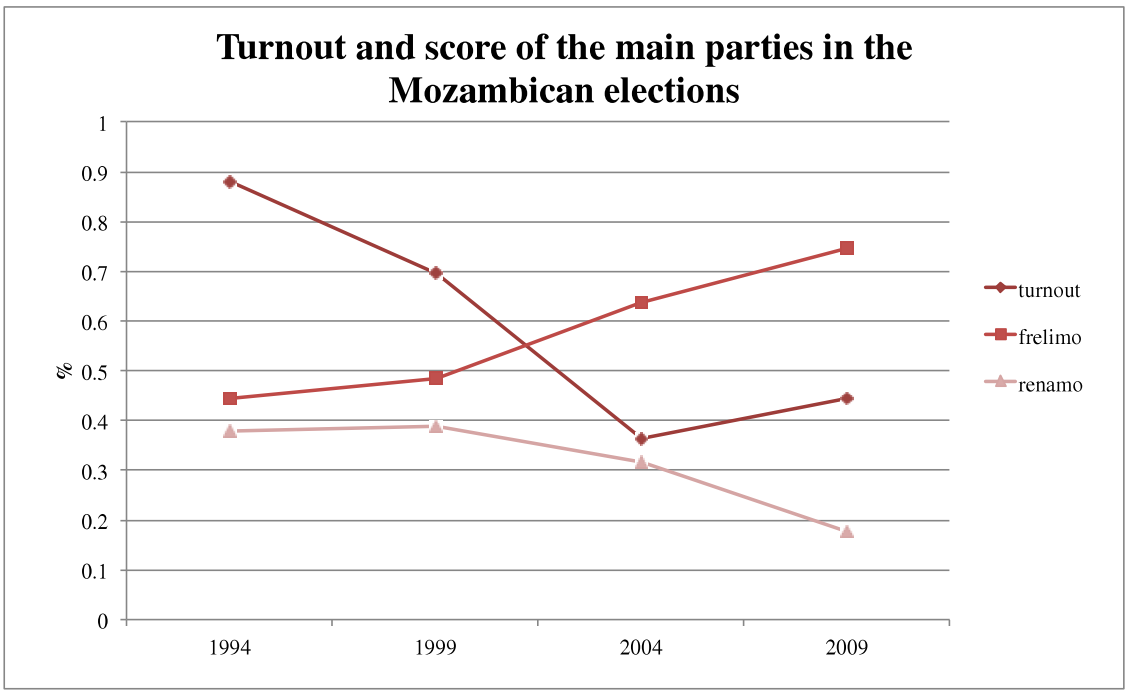
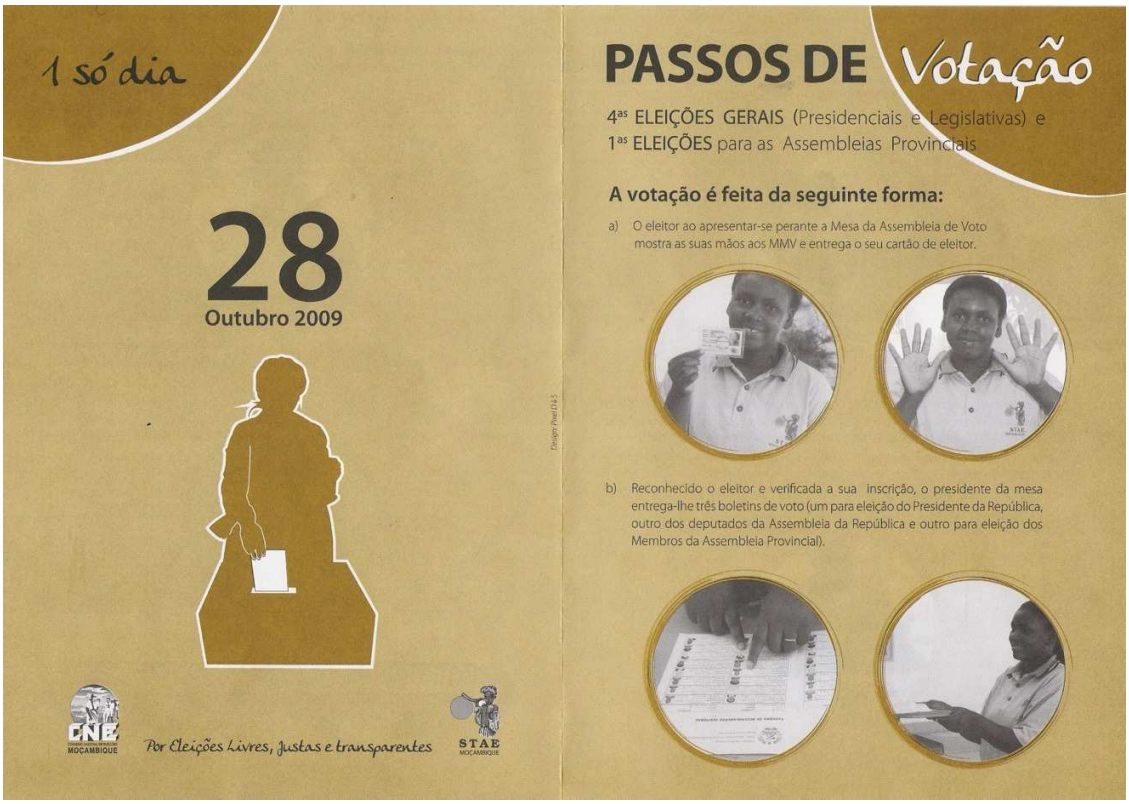


Figure 2: Civic education leaflet by CNE/STAE



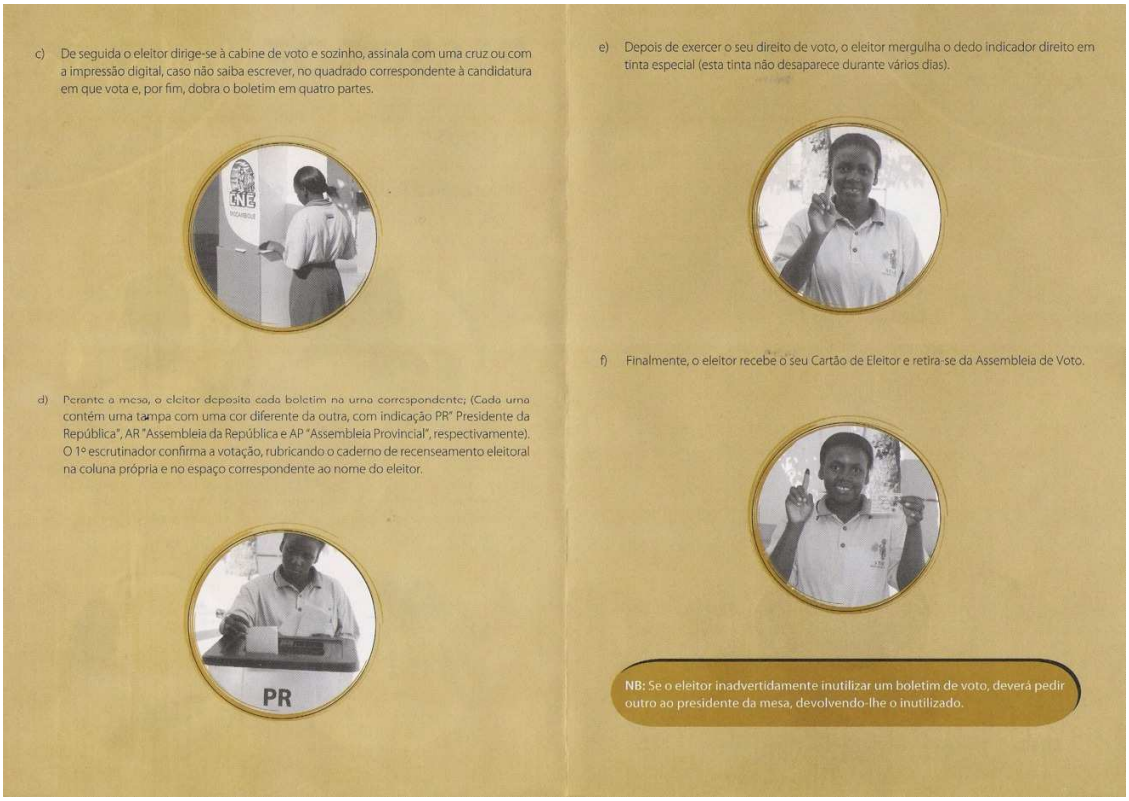


Figure 3: Hotline leaflet

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

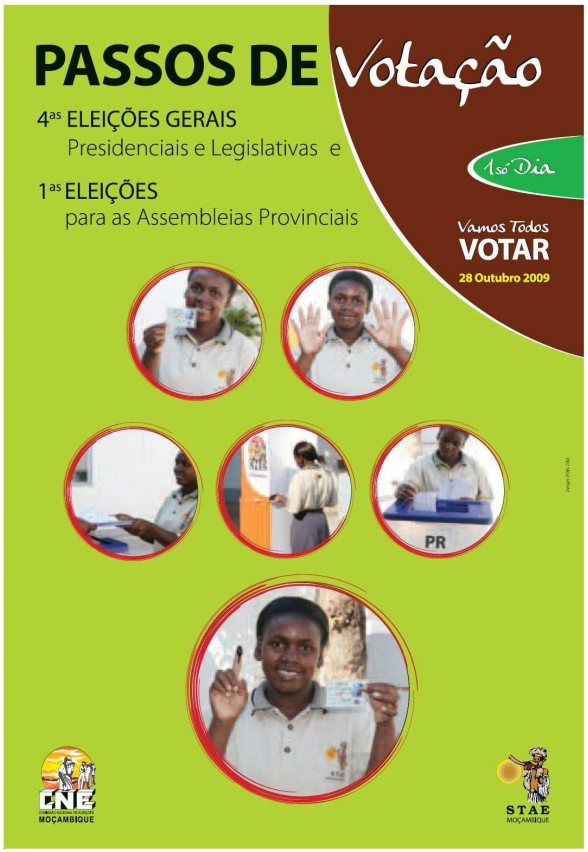


Figure 5: Experimental locations in Cabo Delgado, Zambezia, Gaza and Maputo-Province

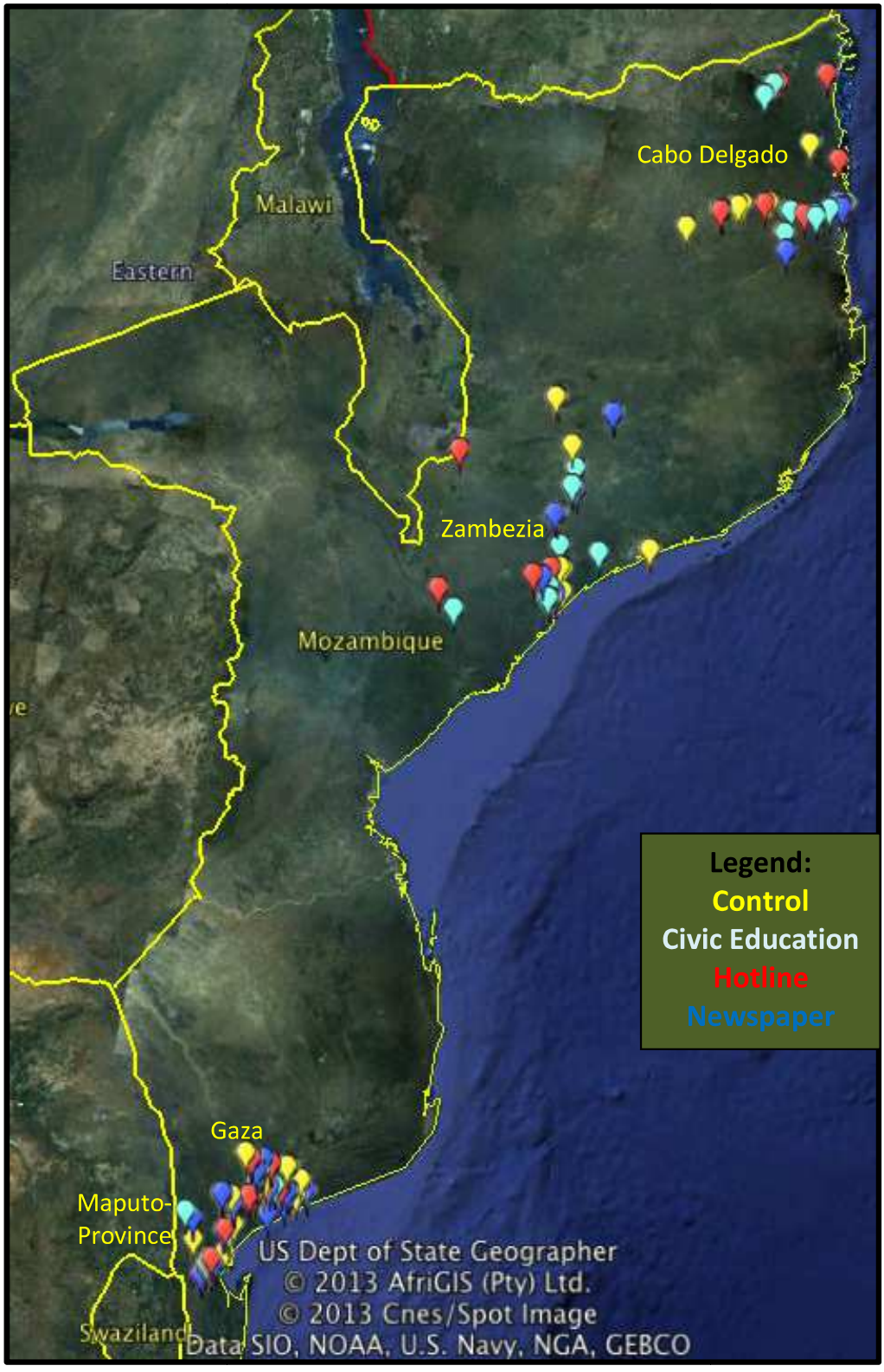


Figure 6: Open letter leaflet

CARTA ABERTA AO NOVO PRESIDENTE!
DIGA AO PRESIDENTE O QUE DEVE SER FEITO NO PAÍS DURANTE O NOVO MANDATO
NÓS COMPROMETEMO-NOS A FAZER-LHE CHEGAR A CARTA EM MÃO!
A SUA LOCALIDADE É UMA de 160 LOCALIDADES ONDE ESTA INICIATIVA DECORRE
ENVIE MENSAGENS SMS FORMATO
SEU-PONTO-DE-VOTAÇÃO espaço O-QUE-FAZER
POR EXEMPLO
"EPC Quelimane mais escolas e hospitais"
PARA
82 1112 OU 84 13333
ATÉ
15 DE DEZEMBRO DE 2009
@Verdade
csae
vodacom
mcel
APOIO:
OBSERVATÓRIO ELEITORAL
AMODE, CCM, CEDE, CEM, CISLAMO, FECIV, LDH, OREC
AMODE (Associação Moçambicana para o Desenvolvimento)
CCM (Conselho Cristão de Moçambique)
Comissão Episcopal de Justiça e Paz da Igreja Católica
LDH (Liga Moçambicana dos Direitos Humanos)
CEDE (Centro de Estudos de Democracia e Desenvolvimento)
CISLAMO (Conselho Islâmico de Moçambique)
FECIV (Instituto de Educação Cívica)
OREC (Organização para Resolução de Conflitos)

Figure 7: The timing of the experiment

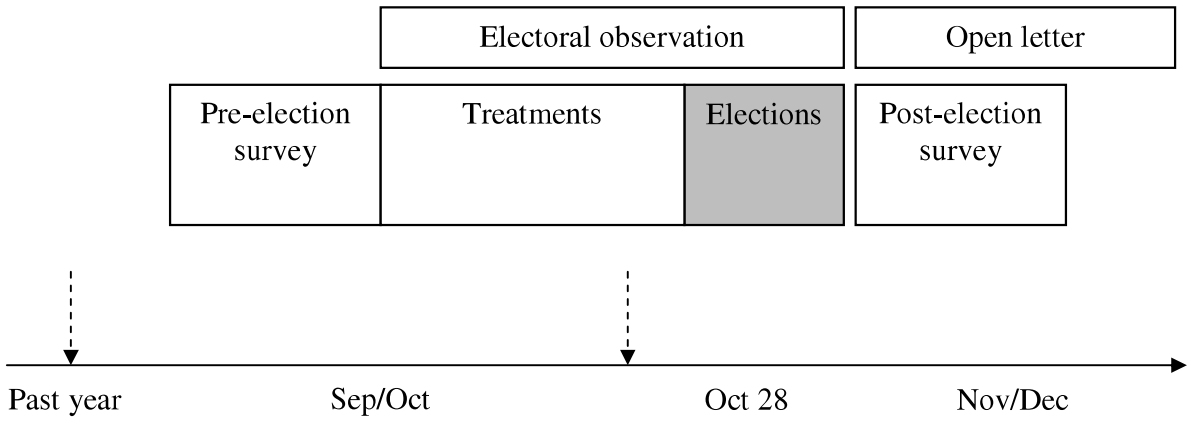


Table 1a: Individual turnout survey measures

	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	Which finger was inked after voting?	showed inked finger without hesitation/right finger without showing/wrong finger/does not know	used variable is 0-1; based on self-report if abstention; turnout if showed inked finger without hesitation
	average questions	Composite index (simple average) calculated from questions on circumstances and events during the election day (finger question plus all questions below, 1-19)		used variable is 0-1; based on self-report if abstention; turnout given by composite index
	average questions (ballot facts only)	Composite index (simple average) calculated from questions that focus on ballot station facts (finger question plus questions 14-19 below)		used variable is 0-1; based on self-report if abstention; turnout given by composite index
	interviewer assessment	(Question for the enumerator) How likely do you found that the respondent voted?	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	'does not know.' coded as possible abstention
	2	Indicate the name of the polling location and how can one reach that location.	answer/does not know	wrong polling station coded as possible abstention
	3	What did you do during the election day, before and after voting?	answer/does not know	'nothing' coded as possible abstention
	4	How long did you take from your house to the polling location on the election day?	hours:minutes/does not know	'does not know.' coded as possible abstention
	5	At what time did you arrive at the polling location on the election day?	hours:minutes/does not know	'does not know.' coded as possible abstention
	6	Was there more than one polling table in your polling location?	yes/no/does not know	'does not know.' coded as possible abstention
	7	How difficult was to find your polling table?	easy/a bit difficult/very difficult/does not know	'does not know.' coded as possible abstention
	8	How long were you queuing to vote?	hours:minutes/does not know	'does not know.' coded as possible abstention
	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	'does not know.' coded as possible abstention
	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	'remembers' coded as possible abstention
	11	How many people were sitting at your polling table?	number/does not know	'does not know.' coded as possible abstention
	12	Did you know anyone from the people that were sitting at your polling table?	yes/no/does not know	'does not know.' coded as possible abstention
	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	'does not know.' coded as possible abstention
	14	How many ballot papers did you have to fill?	number/does not know	wrong number (different from three) coded as possible abstention
	15	Were there photos on the ballot papers?	yes/no/does not know	'no' coded as possible abstention
	16	Could you see anything outside from the cabin where you filled your ballot papers?	yes/no/does not know	'yes' coded as possible abstention
	17	How many ballot boxes there were at your polling table?	number/does not know	wrong number (different from three) coded as possible abstention
	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	'no' coded as possible abstention
	19	Were the different ballot boxes colored diferently?	yes/no/does not know	'no' coded as possible abstention

Table 1b: Individual survey measures

indices	variables	phrasing of the question	original scale
cell phone use	receive SMS	How often do you undertake the following actions with cell phones? Receive SMS.	never/everyday (1-5)
	send SMS	How often do you undertake the following actions with cell phones? Send SMS.	never/everyday (1-5)
	receive phone calls	How often do you undertake the following actions with cell phones? Receive phone calls.	never/everyday (1-5)
	call	How often do you undertake the following actions with cell phones? Call.	never/everyday (1-5)
	send beep	How often do you undertake the following actions with cell phones? Send beep.	never/everyday (1-5)
know verdade	know verdade	Have you heard about newspaper A Verdade?	no/yes (0-1)
	know verdade price	Do you know the price of newspaper A Verdade?	no/yes (0-1)
	reading verdade	How often do you read newspaper A Verdade?	never/several times a week (1-4)
trust verdade		You trust the following institutions. Newspaper A Verdade. Agree or disagree?	disagree/agree (1-5)
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)
interest about the elections	interest in presidential	How interested were you in the Presidential elections of October 2009?	not interest/very interested (1-4)
	interest in parliamentary	How interested were you in the Parliamentary elections of October 2009?	not interest/very interested (1-4)
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)
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)
call for authority	sole party	There are many ways to govern a country. Only one party is authorized to run in elections and to govern. Agree or disagree?	disagree/agree (1-5)
	help from local chief	In this location to whom people recur to ask for help, or to solve a problem? Local chief.	never/always (1-4)
	help from wizard	In this location to whom people recur to ask for help, or to solve a problem? Wizard.	never/always (1-4)
	help from religious leader	In this location to whom people recur to ask for help, or to solve a problem? Religious leader.	never/always (1-4)
	power of the local chief	In this community does the local chief decide on the allocation of public funds (e.g., 7-million program)?	no/yes-always (1-4)
	party of local chief	Do you know the party the following people prefer? Local chief.	no/yes (0-2)
	party of local religious leader	Do you know the party the following people prefer? Local religious leader.	no/yes (0-2)
	day-to-day leaders	To what extent your day-to-day life has been depending on the decisions of local leaders?	nothing/very much (1-3)
	day-to-day leaders - difference	Comparing to 2 months ago when we last visited, to what extent your day-to-day life has been depending on the decisions of local leaders?	less/more (1-5)
	angry about different vote	To what extent would you feel angry if someone in your household voted differently from yourself?	not agry/angry (1-7)
	met the local chief	On the 28th of October (election-day), did you meet the following people? Local chief.	no/yes (0-1)
	met frelimo's local representative	On the 28th of October (election-day), did you meet the following people? Frelimo's local representative.	no/yes (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	-0.001	-0.001	1.000
		(0.034)	(0.035)	(0.035)	
police	0.512	-0.024	-0.012	-0.102	0.805
		(0.112)	(0.113)	(0.112)	
electricity	0.488	0.049	0.137	0.025	0.636
		(0.112)	(0.111)	(0.113)	
piped water	0.317	0.000	-0.117	-0.009	0.594
		(0.104)	(0.098)	(0.105)	
sewage	0.220	-0.049	-0.070	-0.040	0.876
		(0.088)	(0.087)	(0.090)	
health center	0.732	-0.171	-0.057	-0.219**	0.160
		(0.105)	(0.103)	(0.107)	
recreation facility	0.732	0.024	-0.057	-0.065	0.781
		(0.098)	(0.103)	(0.104)	
temple	0.902	0.000	-0.002	-0.031	0.965
		(0.066)	(0.067)	(0.071)	
meeting room	0.317	0.024	0.033	-0.086	0.656
		(0.105)	(0.106)	(0.101)	
paved road	0.268	-0.098	-0.043	0.039	0.525
		(0.092)	(0.097)	(0.102)	

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-targeted, treatment-untargeted, and control groups; for both baseline and post-election samples

		baseline - full sample									post-election sample								
			targeted in treated locations				untargeted in treated locations					targeted in treated locations				untargeted in treated locations			
		control	civic education	hotline	newspaper	joint F-stat p-value	civic education	hotline	newspaper	joint F-stat p-value	control	civic education	hotline	newspaper	joint F-stat p-value	civic education	hotline	newspaper	joint F-stat p-value
basic demographics	male	0.454	-0.021	0.022	0.014	0.548	-0.058	-0.047	0.009	0.490	0.437	-0.060	0.024	0.031	0.149	-0.067	0.016	0.071	0.322
			(0.031)	(0.031)	(0.031)		(0.047)	(0.047)	(0.050)			(0.041)	(0.044)	(0.044)		(0.059)	(0.057)	(0.061)	
	age	38.321	-0.993	-0.367	-2.176*	0.263	-2.055	0.993	0.578	0.445	36.957	0.280	1.713	-0.811	0.391	-1.145	2.411	1.587	0.439
			(1.297)	(1.321)	(1.209)		(1.687)	(1.759)	(1.858)			(1.329)	(1.461)	(1.394)		(1.945)	(2.027)	(2.077)	
	household head	0.748	-0.006	-0.014	0.006	0.940	-0.019	0.032	0.065	0.521	0.742	-0.017	-0.024	0.037	0.466	-0.020	0.055	0.065	0.588
			(0.033)	(0.035)	(0.036)		(0.060)	(0.053)	(0.050)			(0.044)	(0.044)	(0.049)		(0.077)	(0.059)	(0.063)	
	household size	5.657	0.321	0.414*	0.175	0.304	0.374	0.118	0.187	0.763	5.789	0.068	0.483*	0.227	0.296	0.665	0.094	-0.060	0.533
			(0.274)	(0.228)	(0.238)		(0.372)	(0.377)	(0.355)			(0.286)	(0.261)	(0.282)		(0.467)	(0.439)	(0.367)	
	single	0.164	0.007	0.025	0.011	0.858	0.059	0.001	0.049	0.651	0.186	-0.039	0.006	-0.042	0.422	0.036	0.001	0.024	0.958
			(0.029)	(0.029)	(0.030)		(0.063)	(0.045)	(0.051)			(0.036)	(0.041)	(0.037)		(0.080)	(0.055)	(0.065)	
	married or in a union	0.717	0.011	0.014	0.034	0.767	-0.050	-0.014	-0.017	0.896	0.692	0.050	0.039	0.092**	0.142	-0.044	-0.004	0.010	0.959
			(0.035)	(0.035)	(0.032)		(0.068)	(0.054)	(0.056)			(0.044)	(0.047)	(0.040)		(0.085)	(0.061)	(0.073)	
	no schooling	0.197	0.020	-0.017	-0.036	0.448	0.038	-0.019	-0.022	0.837	0.179	0.038	0.010	-0.034	0.225	0.062	-0.007	0.014	0.795
			(0.039)	(0.039)	(0.033)		(0.061)	(0.051)	(0.050)			(0.042)	(0.044)	(0.034)		(0.065)	(0.060)	(0.056)	
	informal schooling	0.071	-0.011	-0.004	-0.002	0.951	0.040	-0.026	0.017	0.373	0.082	-0.033	0.003	-0.015	0.365	0.010	-0.051*	0.023	0.185
			(0.020)	(0.022)	(0.022)		(0.039)	(0.025)	(0.033)			(0.024)	(0.027)	(0.027)		(0.043)	(0.029)	(0.044)	
	literate	0.803	-0.020	0.017	0.036	0.448	-0.038	0.019	0.022	0.837	0.821	-0.038	-0.010	0.034	0.225	-0.062	0.007	-0.014	0.795
			(0.039)	(0.039)	(0.033)		(0.061)	(0.051)	(0.050)			(0.042)	(0.044)	(0.034)		(0.065)	(0.060)	(0.056)	
	primary school	0.283	0.032	-0.048	0.021	0.094	-0.073	0.039	0.029	0.371	0.276	0.031	-0.053	0.063	0.050	-0.091	0.115*	-0.013	0.086
			(0.035)	(0.031)	(0.040)		(0.055)	(0.056)	(0.050)			(0.043)	(0.041)	(0.047)		(0.061)	(0.067)	(0.061)	
	incomplete secondary school	0.164	-0.009	0.014	-0.017	0.822	-0.016	0.003	-0.039	0.837	0.168	-0.013	0.003	-0.019	0.930	-0.002	-0.012	-0.046	0.862
			(0.032)	(0.033)	(0.033)		(0.045)	(0.047)	(0.045)			(0.037)	(0.037)	(0.038)		(0.056)	(0.051)	(0.054)	
ethnicity	changana	0.342	0.038	0.003	0.017	0.972	-0.017	0.031	0.083	0.790	0.355	0.018	0.013	0.012	0.998	-0.034	0.005	0.031	0.952
			(0.089)	(0.087)	(0.091)		(0.094)	(0.099)	(0.100)			(-0.092)	(-0.092)	(-0.095)		(0.099)	(0.106)	(0.107)	
	macua	0.231	-0.025	-0.008	-0.036	0.968	-0.056	-0.000	-0.006	0.894	0.244	-0.035	-0.013	-0.054	0.914	-0.055	0.006	0.019	0.887
			(0.081)	(0.081)	(0.078)		(0.083)	(0.087)	(0.085)			(-0.085)	(-0.085)	(-0.081)		(0.093)	(0.098)	(0.096)	
	lomue	0.104	-0.015	-0.026	0.008	0.915	0.021	-0.049	-0.017	0.534	0.118	-0.016	-0.046	0.008	0.710	0.033	-0.056	-0.031	0.504
			(0.056)	(0.051)	(0.061)		(0.061)	(0.051)	(0.058)			(-0.066)	(-0.058)	(-0.072)		(0.075)	(0.061)	(0.068)	
	chuabo	0.093	0.015	-0.001	-0.001	0.984	-0.006	-0.005	0.007	0.996	0.100	0.010	-0.023	-0.014	0.927	-0.025	-0.007	0.005	0.950
			(0.051)	(0.051)	(0.054)		(0.051)	(0.053)	(0.058)			(0.060)	(0.051)	(0.058)		(0.053)	(0.062)	(0.062)	
	chironga	0.064	-0.035	-0.033	-0.013	0.469	0.036	0.023	-0.027	0.447	0.061	-0.028	-0.027	-0.011	0.591	0.015	0.002	-0.043*	0.265
			(0.026)	(0.025)	(0.031)		(0.046)	(0.045)	(0.031)			(0.024)	(0.022)	(0.031)		(0.042)	(0.042)	(0.026)	
	maconde	0.040	0.006	0.001	0.000	0.997	0.023	-0.018	-0.015	0.772	0.018	0.023	0.021	0.023	0.589	0.058	0.013	-0.000	0.716
			(0.032)	(0.034)	(0.028)		(0.042)	(0.030)	(0.032)			(0.026)	(0.032)	(0.020)		(0.052)	(0.033)	(0.021)	

Note: Standard errors 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-targeted, treatment-untargeted, and control groups; for both baseline and post-election samples

		baseline - full sample								post-election sample								
			targeted in treated locations				untargeted in treated locations					targeted in treated locations				untargeted in treated locations			
		control	civic education	hotline	newspaper	joint F-stat p-value	civic education	hotline	newspaper	joint F-stat p-value	control	civic education	hotline	newspaper	joint F-stat p-value	civic education	hotline	newspaper	joint F-stat p-value
religion	catholic	0.398	-0.056	-0.045	-0.052	0.666	-0.028	-0.091	-0.023	0.609	0.401	-0.024	-0.060	-0.077	0.566	0.043	-0.136*	-0.051	0.255
			(0.049)	(0.055)	(0.052)		(0.075)	(0.067)	(0.072)			(0.056)	(0.062)	(0.060)		(0.093)	(0.077)	(0.081)	
	protestant	0.341	0.026	-0.004	0.015	0.958	0.067	0.044	-0.028	0.686	0.319	0.029	0.036	0.050	0.889	0.014	0.072	-0.021	0.798
			(0.060)	(0.065)	(0.064)		(0.082)	(0.077)	(0.071)			(0.063)	(0.071)	(0.067)		(0.088)	(0.086)	(0.072)	
	muslim	0.206	0.003	0.035	0.038	0.916	-0.021	0.036	0.007	0.927	0.215	-0.018	0.020	0.037	0.882	-0.011	0.066	0.031	0.856
			(0.061)	(0.067)	(0.069)		(0.068)	(0.077)	(0.072)			(0.064)	(0.071)	(0.073)		(0.079)	(0.088)	(0.083)	
occupation	has a job	0.259	-0.047	0.028	-0.007	0.357	0.013	-0.039	-0.009	0.870	0.240	-0.052	0.029	-0.033	0.333	0.038	-0.053	0.058	0.502
			(0.037)	(0.042)	(0.036)		(0.058)	(0.052)	(0.056)			(0.042)	(0.049)	(0.041)		(0.068)	(0.059)	(0.067)	
	agriculture	0.343	0.002	-0.033	-0.062	0.633	0.114	0.009	0.082	0.429	0.351	0.001	-0.026	-0.063	0.711	0.056	0.024	0.052	0.903
			(0.057)	(0.060)	(0.059)		(0.078)	(0.079)	(0.079)			(0.065)	(0.069)	(0.066)		(0.089)	(0.091)	(0.089)	
	retail informal sector	0.033	0.018	0.013	0.007	0.646	-0.021	0.022	-0.021	0.221	0.043	0.006	0.008	0.002	0.975	-0.025	0.004	-0.025	0.537
			(0.016)	(0.014)	(0.015)		(0.015)	(0.025)	(0.016)			(0.019)	(0.019)	(0.022)		(0.023)	(0.030)	(0.022)	
	artisan	0.044	-0.017	0.002	0.010	0.228	-0.020	0.011	-0.019	0.550	0.050	-0.030*	0.001	0.017	0.036	-0.050***	-0.019	-0.015	0.001
			(0.013)	(0.014)	(0.015)		(0.019)	(0.025)	(0.020)			(0.016)	(0.019)	(0.022)		(0.013)	(0.025)	(0.028)	
	unskilled worker	0.056	-0.015	0.014	0.022	0.177	0.006	0.021	-0.030	0.364	0.054	-0.013	0.006	0.018	0.517	-0.017	-0.007	-0.018	0.899
			(0.017)	(0.018)	(0.020)		(0.029)	(0.033)	(0.021)			(0.020)	(0.021)	(0.023)		(0.030)	(0.029)	(0.029)	
	wage employee	0.029	-0.007	0.006	-0.006	0.692	-0.029***	-0.007	0.022	0.001	0.032	-0.004	-0.007	-0.023*	0.161	-0.032***	-0.001	0.003	0.010
			(0.012)	(0.013)	(0.013)		(0.009)	(0.018)	(0.026)			(0.015)	(0.015)	(0.013)		(0.012)	(0.025)	(0.027)	
	teacher	0.044	-0.009	0.025	0.007	0.536	0.042	0.011	0.019	0.570	0.047	-0.022	0.013	-0.002	0.296	0.064	0.000	0.025	0.443
			(0.015)	(0.027)	(0.016)		(0.032)	(0.026)	(0.029)			(0.015)	(0.027)	(0.019)		(0.042)	(0.029)	(0.037)	
	public official	0.020	0.023*	0.015	0.003	0.340	0.042	0.002	0.018	0.532	0.025	0.032	0.013	-0.003	0.339	0.030	-0.010	0.011	0.667
			(0.014)	(0.013)	(0.013)		(0.032)	(0.017)	(0.023)			(0.020)	(0.015)	(0.018)		(0.032)	(0.018)	(0.027)	
	student	0.031	0.021	-0.002	0.015	0.443	-0.031***	0.013	-0.006	0.000	0.040	0.018	-0.014	0.001	0.592	-0.040***	0.007	-0.022	0.004
			(0.017)	(0.014)	(0.014)		(0.009)	(0.023)	(0.020)			(0.022)	(0.019)	(0.018)		(0.013)	(0.030)	(0.022)	
	stays at home	0.137	-0.004	-0.027	-0.011	0.735	-0.014	-0.005	-0.037	0.836	0.147	-0.008	-0.032	-0.016	0.776	0.020	-0.038	-0.024	0.795
			(0.025)	(0.026)	(0.026)		(0.042)	(0.045)	(0.041)			(0.031)	(0.032)	(0.032)		(0.059)	(0.048)	(0.046)	
assets and expenditure	house	0.847	-0.005	0.011	-0.028	0.603	0.030	-0.023	0.003	0.885	0.853	0.008	0.027	-0.015	0.598	0.036	-0.025	-0.028	0.805
			(0.030)	(0.028)	(0.030)		(0.047)	(0.058)	(0.055)			(0.033)	(0.031)	(0.034)		(0.054)	(0.056)	(0.066)	
	land	0.608	-0.018	0.044	-0.023	0.500	0.034	-0.080	0.005	0.677	0.652	-0.062	-0.028	-0.044	0.753	-0.023	-0.105	-0.056	0.638
			(0.053)	(0.048)	(0.051)		(0.073)	(0.080)	(0.082)			(0.059)	(0.060)	(0.059)		(0.088)	(0.084)	(0.090)	
	cattle	0.255	0.003	-0.017	0.011	0.949	-0.020	0.020	0.020	0.951	0.254	-0.000	-0.011	0.016	0.973	-0.032	0.074	-0.044	0.611
			(0.041)	(0.044)	(0.046)		(0.062)	(0.061)	(0.073)			(0.047)	(0.052)	(0.052)		(0.072)	(0.077)	(0.073)	
	cell phone	0.710	-0.008	0.046	0.018	0.814	0.031	0.071	0.103	0.448	0.706	0.007	0.046	0.006	0.897	0.090	0.091	0.083	0.562
			(0.060)	(0.059)	(0.055)		(0.074)	(0.069)	(0.067)			(0.068)	(0.068)	(0.065)		(0.081)	(0.083)	(0.080)	
	expenditure	127.203	-6.942	-5.576	4.611	0.862	51.242	17.605	-2.103	0.596	122.452	-15.275	3.641	4.816	0.363	77.759	28.799	1.572	0.523
			(16.118)	(15.166)	(16.601)		(42.386)	(28.712)	(20.031)			(14.056)	(15.622)	(17.013)		(60.203)	(35.099)	(22.193)	

Note: Standard errors 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 (2004 elections) - differences across treatments and control

	control	civic education	hotline	newspaper	joint F-stat p-value
number of polling tables	5.488	-0.415	0.012	0.128	0.894
		(0.629)	(0.744)	(0.736)	
presidential turnout	0.407	-0.019	-0.006	-0.028	0.819
		(0.027)	(0.034)	(0.030)	
guebuza	0.714	0.038	0.007	-0.012	0.736
		(0.046)	(0.047)	(0.048)	
dhlakama	0.188	-0.028	-0.007	0.019	0.725
		(0.041)	(0.042)	(0.043)	
presidential null	0.035	-0.004	0.002	-0.002	0.654
		(0.003)	(0.005)	(0.003)	
presidential blank	0.032	-0.005	-0.001	-0.004	0.768
		(0.005)	(0.006)	(0.005)	
parliamentary turnout	0.414	-0.027	-0.005	-0.032	0.694
		(0.027)	(0.036)	(0.029)	
frelimo	0.673	0.040	0.016	-0.010	0.714
		(0.046)	(0.046)	(0.047)	
renamo	0.179	-0.029	-0.013	0.014	0.694
		(0.037)	(0.037)	(0.040)	
parliamentary null	0.039	-0.007	0.002	-0.003	0.486
		(0.004)	(0.007)	(0.004)	
parliamentary blank	0.055	-0.005	-0.005	-0.004	0.895
		(0.008)	(0.009)	(0.008)	

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-targeted, treatment-untargeted, and control groups; for both baseline and post-election samples

		baseline - full sample									post-election sample								
			targeted in treated locations				untargeted in treated locations					targeted in treated locations				untargeted in treated locations			
		control	civic education	hotline	newspaper	joint F-stat p-value	civic education	hotline	newspaper	joint F-stat p-value	control	civic education	hotline	newspaper	joint F-stat p-value	civic education	hotline	newspaper	joint F-stat p-value
voting intentions 2009	turnout	0.975	0.017*	0.010	-0.011	0.057	0.000	0.002	0.025***	0.002	0.974	0.013	0.013	0.003	0.576	-0.011	0.010	0.026***	0.012
			(0.009)	(0.010)	(0.013)		(0.019)	(0.017)	(0.007)			(0.011)	(0.011)	(0.013)		(0.028)	(0.018)	(0.009)	
	guebuza	0.866	0.035	0.038	-0.005	0.435	-0.047	-0.033	0.045	0.451	0.860	0.019	0.045	0.014	0.742	-0.023	-0.041	0.098**	0.038
			(0.030)	(0.034)	(0.035)		(0.055)	(0.055)	(0.045)			(0.035)	(0.041)	(0.036)		(0.066)	(0.068)	(0.039)	
	dhlakama	0.016	-0.003	0.002	0.008	0.770	0.026	0.023	-0.001	0.729	0.009	0.005	0.002	0.018	0.646	0.032	0.028	-0.009	0.165
			(0.008)	(0.010)	(0.011)		(0.031)	(0.029)	(0.016)			(0.010)	(0.010)	(0.014)		(0.029)	(0.036)	(0.006)	
	simango	0.037	-0.006	-0.019	-0.010	0.603	-0.009	0.002	-0.007	0.970	0.043	0.004	-0.021	-0.016	0.499	-0.002	0.012	-0.021	0.798
			(0.015)	(0.015)	(0.016)		(0.023)	(0.025)	(0.024)			(0.022)	(0.021)	(0.020)		(0.033)	(0.035)	(0.027)	
	frelimo	0.903	0.033	0.035	-0.018	0.102	0.008	-0.012	0.017	0.949	0.912	0.004	0.027	-0.007	0.723	-0.019	-0.014	0.018	0.934
			(0.021)	(0.022)	(0.027)		(0.043)	(0.044)	(0.036)			(0.027)	(0.030)	(0.029)		(0.058)	(0.055)	(0.042)	
	renamo	0.017	-0.004	-0.006	-0.002	0.928	-0.002	0.024	-0.001	0.882	0.009	0.005	0.002	0.008	0.898	0.012	0.032	-0.009	0.251
			(0.009)	(0.009)	(0.009)		(0.016)	(0.031)	(0.017)			(0.010)	(0.010)	(0.011)		(0.022)	(0.041)	(0.006)	
past voting 2004	turnout	0.968	-0.011	0.001	-0.027*	0.337	-0.043	0.004	0.004	0.706	0.970	-0.019	-0.010	-0.019	0.705	-0.042	0.010	-0.010	0.685
			(0.016)	(0.015)	(0.016)		(0.038)	(0.021)	(0.021)			(0.022)	(0.020)	(0.019)		(0.041)	(0.022)	(0.030)	
	guebuza	0.807	0.014	0.005	-0.026	0.718	-0.054	-0.063	0.054	0.266	0.823	-0.006	0.004	-0.015	0.953	-0.109	-0.060	0.017	0.415
			(0.032)	(0.031)	(0.034)		(0.069)	(0.053)	(0.047)			(0.037)	(0.037)	(0.035)		(0.080)	(0.065)	(0.055)	
	dhlakama	0.017	-0.002	-0.001	-0.001	0.996	0.010	0.018	-0.003	0.882	0.008	0.006	0.001	0.007	0.882	0.033	0.026	-0.008	0.254
			(0.009)	(0.009)	(0.009)		(0.028)	(0.026)	(0.015)			(0.009)	(0.009)	(0.010)		(0.041)	(0.034)	(0.005)	
	frelimo	0.785	0.027	0.012	-0.013	0.708	-0.041	-0.040	0.063	0.418	0.796	0.012	0.006	0.001	0.989	-0.082	-0.016	0.024	0.717
			(0.034)	(0.032)	(0.035)		(0.068)	(0.054)	(0.052)			(0.038)	(0.038)	(0.038)		(0.080)	(0.065)	(0.064)	
	renamo	0.017	-0.005	-0.001	-0.001	0.933	0.010	0.018	-0.003	0.882	0.008	0.006	0.006	0.007	0.857	0.033	0.026	-0.008	0.254
			(0.008)	(0.009)	(0.009)		(0.027)	(0.026)	(0.015)			(0.009)	(0.010)	(0.010)		(0.041)	(0.034)	(0.005)	
survey outcomes	neutrality of electoral commission	0.000	0.042	0.037	0.077	0.898	0.125	0.172	0.091	0.515	-0.032	0.062	0.109	0.147	0.564	0.275*	0.152	0.265*	0.187
			(0.097)	(0.100)	(0.101)		(0.147)	(0.123)	(0.151)			(0.114)	(0.113)	(0.108)		(0.164)	(0.138)	(0.153)	
	trust verdade	0.000	0.032	0.005	0.233*	0.227	-0.135	-0.067	0.166	0.677	-0.085	0.085	0.195	0.400***	0.044	-0.293	-0.146	0.220	0.316
			(0.118)	(0.139)	(0.126)		(0.251)	(0.199)	(0.181)			(0.139)	(0.153)	(0.147)		(0.251)	(0.219)	(0.210)	
	trust electoral commission	-0.000	-0.001	-0.049	-0.003	0.960	0.110	-0.101	0.068	0.589	-0.036	-0.042	-0.038	0.090	0.639	0.244*	-0.135	0.105	0.167
			(0.100)	(0.107)	(0.103)		(0.135)	(0.140)	(0.131)			(0.120)	(0.123)	(0.117)		(0.141)	(0.169)	(0.143)	
	interest about the elections	-0.000	0.073	0.096	-0.019	0.537	-0.093	-0.098	-0.020	0.867	0.006	0.079	0.024	0.020	0.875	-0.011	-0.071	-0.012	0.976
			(0.089)	(0.088)	(0.092)		(0.152)	(0.145)	(0.153)			(0.100)	(0.103)	(0.105)		(0.177)	(0.155)	(0.165)	
	problematic elections 2004	0.000	-0.016	0.039	0.037	0.943	-0.220	0.041	-0.067	0.373	-0.016	-0.000	0.086	0.004	0.888	-0.174	0.131	-0.184	0.305
			(0.108)	(0.113)	(0.107)		(0.136)	(0.163)	(0.149)			(0.125)	(0.129)	(0.119)		(0.159)	(0.198)	(0.139)	
	vote miscounting 2009	0.000	-0.098	0.020	-0.057	0.580	0.067	0.119	-0.024	0.793	0.051	-0.098	-0.110	-0.156	0.591	-0.063	0.054	-0.076	0.910
			(0.087)	(0.096)	(0.091)		(0.146)	(0.135)	(0.133)			(0.114)	(0.118)	(0.117)		(0.165)	(0.177)	(0.159)	
	vote-buying in elections 2009	-0.000	-0.097	-0.064	-0.066	0.678	0.167	0.090	-0.226*	0.145	-0.003	-0.072	-0.064	-0.054	0.896	0.091	0.084	-0.301**	0.122
			(0.084)	(0.079)	(0.082)		(0.189)	(0.169)	(0.118)			(0.107)	(0.100)	(0.109)		(0.232)	(0.191)	(0.139)	
	violence in elections 2009	-0.000	-0.073	-0.010	-0.098	0.610	0.088	0.077	-0.111	0.741	-0.006	-0.038	-0.071	-0.064	0.894	-0.070	0.037	-0.146	0.810
			(0.088)	(0.086)	(0.082)		(0.202)	(0.155)	(0.137)			(0.109)	(0.099)	(0.103)		(0.203)	(0.181)	(0.167)	

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 3a: 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	0.047*	0.051**	0.025	0.025	-0.013	-0.015	-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	0.048*	0.055**	0.039*	0.040*	-0.014	-0.015	-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		0.375	0.389	0.666	0.673	0.574	0.582	0.281	0.411	0.297	0.439	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	0.983	0.919	0.231	0.282	0.281	0.296	0.401	0.278	0.957	0.850	0.886	0.651
h0: civic education = newspaper	F-stat p-value	0.972	0.929	0.624	0.739	0.290	0.294	0.963	0.816	0.707	0.600	0.448	0.332
h0: hotline = newspaper	F-stat p-value	0.956	0.850	0.485	0.473	0.989	0.978	0.381	0.198	0.670	0.735	0.539	0.598
controls		no	yes	no	yes	no	yes	no	yes	no	yes	no	yes

Note: All regressions are OLS. All dependent variables are vote shares. 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 3b: Official ballot station outcomes (parliamentary elections)

dependent variable ---->		parliamentary elections									
		turnout		frelimo		renamo		blank votes		null votes	
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
civic education	coefficient	0.046*	0.052**	0.040*	0.039*	-0.036**	-0.038**	0.003	0.005	-0.006*	-0.006*
	standard error	(0.025)	(0.025)	(0.021)	(0.021)	(0.015)	(0.015)	(0.012)	(0.011)	(0.003)	(0.003)
hotline	coefficient	0.050**	0.053**	0.021	0.023	-0.017	-0.017	0.002	0.001	-0.003	-0.004
	standard error	(0.025)	(0.025)	(0.021)	(0.021)	(0.015)	(0.015)	(0.013)	(0.011)	(0.003)	(0.003)
newspaper	coefficient	0.049**	0.057**	0.033	0.034	-0.018	-0.020	-0.009	-0.008	-0.003	-0.002
	standard error	(0.025)	(0.025)	(0.021)	(0.021)	(0.016)	(0.016)	(0.013)	(0.012)	(0.003)	(0.003)
mean dep. variable (control)		0.438	0.438	0.722	0.722	0.136	0.136	0.081	0.081	0.028	0.028
r-squared adjusted		0.368	0.377	0.640	0.666	0.628	0.637	0.211	0.356	0.127	0.145
number of observations		161	161	161	161	161	161	161	161	161	161
h0: civic education = hotline	F-stat p-value	0.892	0.939	0.356	0.432	0.205	0.194	0.941	0.747	0.416	0.474
h0: civic education = newspaper	F-stat p-value	0.911	0.834	0.718	0.820	0.233	0.259	0.353	0.262	0.368	0.290
h0: hotline = newspaper	F-stat p-value	0.982	0.893	0.580	0.589	0.948	0.887	0.394	0.421	0.926	0.720
controls		no	yes	no	yes	no	yes	no	yes	no	yes

Note: All regressions are OLS. All dependent variables are vote shares. 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 4a: Individual turnout (targeted)

dependent variable ---->		individual turnout (targeted)									
		self-reported		finger		average questions		average questions (ballot facts only)		interviewer assessment	
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
civic education	coefficient	0.038	0.034	0.055*	0.046	0.042*	0.041*	0.050**	0.050**	0.073***	0.076***
	standard error	(0.027)	(0.027)	(0.029)	(0.029)	(0.025)	(0.024)	(0.026)	(0.025)	(0.028)	(0.027)
hotline	coefficient	0.064**	0.058**	0.063**	0.049*	0.072***	0.065***	0.078***	0.070***	0.093***	0.088***
	standard error	(0.025)	(0.026)	(0.026)	(0.027)	(0.024)	(0.024)	(0.024)	(0.025)	(0.025)	(0.024)
newspaper	coefficient	0.021	0.020	0.014	0.013	0.027	0.030	0.033	0.036	0.038	0.043
	standard error	(0.030)	(0.028)	(0.040)	(0.036)	(0.033)	(0.030)	(0.033)	(0.029)	(0.037)	(0.031)
mean dep. variable (control)		0.877	0.876	0.807	0.805	0.788	0.788	0.757	0.756	0.753	0.752
r-squared adjusted		0.011	0.030	0.014	0.027	0.023	0.053	0.036	0.069	0.026	0.046
number of observations		953	943	953	943	953	943	953	943	953	943
h0: civic education = hotline	F-stat p-value	0.272	0.305	0.774	0.921	0.182	0.276	0.266	0.407	0.423	0.633
h0: civic education = newspaper	F-stat p-value	0.522	0.607	0.310	0.386	0.653	0.719	0.610	0.657	0.328	0.328
h0: hotline = newspaper	F-stat p-value	0.101	0.129	0.206	0.300	0.150	0.207	0.163	0.234	0.103	0.127
controls		no	yes	no	yes	no	yes	no	yes	no	yes

Note: All regressions are OLS. All dependent variables are between 0 and 1. 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 4b: Individual turnout (untargeted)

dependent variable ------>		individual turnout (untargeted)									
		self-reported		finger		average questions		average questions (ballot facts only)		interviewer assessment	
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
civic education	coefficient	0.024	0.022	0.057	0.046	0.049	0.043	0.059	0.047	0.039	0.021
	standard error	(0.043)	(0.043)	(0.048)	(0.051)	(0.039)	(0.040)	(0.039)	(0.039)	(0.044)	(0.045)
hotline	coefficient	0.102***	0.096***	0.090**	0.074*	0.080**	0.074**	0.085**	0.078**	0.101***	0.093**
	standard error	(0.026)	(0.028)	(0.043)	(0.042)	(0.032)	(0.032)	(0.033)	(0.033)	(0.038)	(0.037)
newspaper	coefficient	0.070*	0.061*	0.143***	0.122***	0.085**	0.070**	0.092***	0.075**	0.146***	0.119***
	standard error	(0.036)	(0.037)	(0.036)	(0.036)	(0.034)	(0.034)	(0.034)	(0.035)	(0.038)	(0.036)
mean dep. variable (control)		0.877	0.879	0.807	0.811	0.788	0.790	0.757	0.758	0.753	0.756
r-squared adjusted		0.013	0.020	0.015	0.031	0.021	0.038	0.039	0.058	0.028	0.048
number of observations		437	431	437	431	437	431	437	431	437	431
h0: civic education = hotline	F-stat p-value	0.067	0.092	0.569	0.650	0.482	0.482	0.563	0.479	0.224	0.153
h0: civic education = newspaper	F-stat p-value	0.347	0.424	0.111	0.185	0.424	0.553	0.472	0.547	0.035	0.052
h0: hotline = newspaper	F-stat p-value	0.364	0.376	0.282	0.325	0.902	0.919	0.870	0.932	0.328	0.555
controls		no	yes	no	yes	no	yes	no	yes	no	yes

Note: All regressions are OLS. All dependent variables are between 0 and 1. 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 5: Behavioral measure of political accountability (open letter)

dependent variable ------>		open letter			
		targeted		untargeted	
		(1)	(2)	(3)	(4)
civic education	coefficient	0.043	0.053	0.106	0.108
	standard error	(0.048)	(0.046)	(0.070)	(0.068)
hotline	coefficient	-0.036	-0.017	0.007	0.022
	standard error	(0.035)	(0.033)	(0.047)	(0.044)
newspaper	coefficient	0.088*	0.099**	0.065	0.088
	standard error	(0.050)	(0.050)	(0.066)	(0.065)
mean dep. variable (control)		0.153	0.151	0.153	0.151
r-squared adjusted		0.013	0.029	0.015	0.047
number of observations		973	957	449	442
h0: civic education = hotline	F-stat p-value	0.094	0.125	0.199	0.240
h0: civic education = newspaper	F-stat p-value	0.454	0.452	0.649	0.818
h0: hotline = newspaper	F-stat p-value	0.012	0.016	0.434	0.341
controls		no	yes	no	yes

Note: All regressions are OLS. The dependent variable is binary. 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 6a: Individual self-reported voting (targeted)

dependent variable ------>		individual voting (targeted)									
		guebuza		dhlakama		simango		frelimo		renamo	
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
civic education	coefficient	0.043	0.054*	-0.007	-0.014*	0.008	0.009	0.044	0.053*	-0.003	-0.012
	standard error	(0.036)	(0.032)	(0.008)	(0.008)	(0.016)	(0.016)	(0.033)	(0.030)	(0.009)	(0.010)
hotline	coefficient	0.059*	0.056*	0.008	0.004	-0.008	-0.008	0.074**	0.071**	0.013	0.008
	standard error	(0.032)	(0.032)	(0.012)	(0.011)	(0.014)	(0.015)	(0.030)	(0.029)	(0.012)	(0.013)
newspaper	coefficient	-0.011	0.003	0.012	0.007	-0.014	-0.017	0.004	0.008	0.007	0.001
	standard error	(0.040)	(0.039)	(0.014)	(0.013)	(0.014)	(0.014)	(0.040)	(0.039)	(0.012)	(0.012)
mean dep. variable (control)		0.819	0.822	0.012	0.012	0.028	0.028	0.821	0.824	0.012	0.012
r-squared adjusted		0.037	0.051	0.003	0.025	0.002	0.008	0.033	0.062	0.004	0.019
number of observations		872	862	872	862	872	862	886	876	886	876
h0: civic education = hotline	F-stat p-value	0.604	0.966	0.128	0.073	0.289	0.277	0.294	0.516	0.170	0.095
h0: civic education = newspaper	F-stat p-value	0.179	0.176	0.125	0.092	0.128	0.092	0.288	0.224	0.363	0.274
h0: hotline = newspaper	F-stat p-value	0.056	0.143	0.783	0.862	0.629	0.511	0.050	0.068	0.660	0.576
controls		no	yes	no	yes	no	yes	no	yes	no	yes

Note: All regressions are OLS. All dependent variables are binary. 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 6b: Individual self-reported voting (untargeted)

dependent variable ------>		individual voting (untargeted)									
		guebuza		dhlakama		simango		frelimo		renamo	
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
civic education	coefficient	0.037	0.033	-0.012*	-0.011*	0.015	0.011	0.037	0.027	0.009	0.010
	standard error	(0.062)	(0.066)	(0.007)	(0.006)	(0.028)	(0.031)	(0.062)	(0.065)	(0.020)	(0.021)
hotline	coefficient	0.009	0.000	0.006	0.005	0.049	0.052	0.016	0.027	0.023	0.022
	standard error	(0.052)	(0.054)	(0.018)	(0.018)	(0.035)	(0.035)	(0.049)	(0.047)	(0.025)	(0.023)
newspaper	coefficient	0.082*	0.085*	-0.013*	-0.013*	-0.007	-0.018	0.065	0.077*	-0.012	-0.016*
	standard error	(0.047)	(0.045)	(0.007)	(0.008)	(0.022)	(0.025)	(0.047)	(0.041)	(0.007)	(0.009)
mean dep. variable (control)		0.819	0.820	0.012	0.012	0.028	0.029	0.821	0.823	0.012	0.012
r-squared adjusted		0.014	0.004	-0.006	-0.016	0.020	0.039	0.006	0.048	0.000	0.001
number of observations		408	402	408	402	408	402	414	408	414	408
h0: civic education = hotline	F-stat p-value	0.697	0.673	0.301	0.331	0.418	0.352	0.767	0.996	0.626	0.694
h0: civic education = newspaper	F-stat p-value	0.522	0.475	0.593	0.439	0.488	0.421	0.701	0.472	0.270	0.231
h0: hotline = newspaper	F-stat p-value	0.230	0.170	0.279	0.265	0.139	0.078	0.419	0.348	0.139	0.116
controls		no	yes	no	yes	no	yes	no	yes	no	yes

Note: All regressions are OLS. All dependent variables are binary. 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 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	-0.345	-0.056	-0.061	-0.003	-0.010	-0.350	-0.366	-0.076	-0.070
	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	-0.090	0.102	0.110	0.001	0.008	-0.243	-0.217	-0.022	0.015
	standard error	(0.310)	(0.312)	(0.230)	(0.236)	(0.133)	(0.134)	(0.265)	(0.263)	(0.135)	(0.136)
newspaper	coefficient	-0.588*	-0.576*	-0.399*	-0.468*	0.058	0.010	-0.566**	-0.505*	-0.170	-0.164
	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	0.243	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	0.416	0.496	0.470	0.972	0.895	0.687	0.573	0.690	0.540
h0: civic education = newspaper	F-stat p-value	0.404	0.473	0.140	0.096	0.651	0.887	0.419	0.608	0.490	0.503
h0: hotline = newspaper	F-stat p-value	0.157	0.133	0.033	0.019	0.677	0.989	0.231	0.290	0.281	0.207
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)

dependent variable ------>		individual survey measures (targeted)													
		cell phone use		know verdade		trust verdade		trust electoral commission		neutrality of electoral commission		interest about the elections		information about the elections	
		(1)	(2)	(3)	(4)	(5)	6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
civic education	coefficient	0.009	0.140	-0.055	0.029	0.094	0.136	0.190**	0.182**	0.127	0.102	0.092	0.121*	0.066	0.159**
	standard error	(0.107)	(0.097)	(0.074)	(0.075)	(0.101)	(0.115)	(0.086)	(0.081)	(0.091)	(0.090)	(0.066)	(0.071)	(0.066)	(0.063)
hotline	coefficient	0.147	0.219**	0.057	0.096	-0.050	-0.052	0.133	0.168**	0.169**	0.168**	0.117	0.103	0.151**	0.167***
	standard error	(0.098)	(0.096)	(0.086)	(0.084)	(0.105)	(0.106)	(0.087)	(0.082)	(0.085)	(0.078)	(0.087)	(0.081)	(0.062)	(0.056)
newspaper	coefficient	0.148	0.231**	0.964***	1.003***	0.620***	0.614***	0.155*	0.156*	0.141	0.119	-0.030	-0.031	0.122*	0.159**
	standard error	(0.105)	(0.101)	(0.101)	(0.094)	(0.107)	(0.109)	(0.084)	(0.082)	(0.088)	(0.091)	(0.086)	(0.088)	(0.067)	(0.064)
mean dep. variable (control)		-0.000	0.008	-0.000	-0.007	0.000	-0.008	-0.000	-0.004	0.000	0.000	-0.000	0.004	0.000	0.000
r-squared adjusted		0.054	0.158	0.277	0.338	0.078	0.095	0.069	0.087	0.085	0.092	0.102	0.147	0.080	0.243
number of observations		977	966	978	967	746	737	907	896	878	869	971	960	976	965
h0: civic education = hotline	F-stat p-value	0.178	0.398	0.198	0.437	0.188	0.098	0.527	0.870	0.621	0.448	0.754	0.816	0.163	0.881
h0: civic education = newspaper	F-stat p-value	0.202	0.361	0.000	0.000	0.000	0.000	0.692	0.750	0.879	0.851	0.122	0.064	0.395	0.996
h0: hotline = newspaper	F-stat p-value	0.995	0.904	0.000	0.000	0.000	0.000	0.797	0.890	0.724	0.566	0.133	0.154	0.647	0.886
controls		no	yes	no	yes	no	yes	no	yes	no	yes	no	yes	no	yes

Note: All regressions are OLS. All dependent variables are z-scores. 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)

dependent variable ---->		individual survey measures (targeted)											
		confusion between state and ruling party		call for authority		problematic elections		vote miscounting		vote-buying		electoral violence and intimidation	
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
civic education	coefficient	-0.011	-0.091	0.173***	0.152***	-0.095	-0.032	-0.143*	-0.146*	-0.022	-0.009	-0.140***	-0.123***
	standard error	(0.068)	(0.066)	(0.039)	(0.040)	(0.084)	(0.085)	(0.080)	(0.086)	(0.063)	(0.063)	(0.043)	(0.043)
hotline	coefficient	-0.195***	-0.238***	0.059	0.038	0.158*	0.187**	-0.044	-0.049	0.031	0.036	-0.055	-0.051
	standard error	(0.063)	(0.059)	(0.043)	(0.041)	(0.091)	(0.088)	(0.086)	(0.091)	(0.067)	(0.068)	(0.048)	(0.048)
newspaper	coefficient	-0.134**	-0.193***	0.113**	0.089*	0.053	0.078	-0.042	-0.055	0.164**	0.189***	-0.080*	-0.064
	standard error	(0.068)	(0.061)	(0.046)	(0.049)	(0.092)	(0.090)	(0.088)	(0.092)	(0.070)	(0.073)	(0.041)	(0.044)
mean dep. variable (control)		0.000	0.005	-0.000	-0.000	0.000	0.004	0.000	-0.005	0.000	0.003	0.000	0.004
r-squared adjusted		0.042	0.079	0.099	0.128	0.018	0.027	0.013	0.009	0.009	0.015	0.030	0.041
number of observations		696	690	978	967	948	938	935	925	960	949	973	962
h0: civic education = hotline	F-stat p-value	0.003	0.026	0.007	0.006	0.013	0.033	0.207	0.208	0.445	0.519	0.060	0.109
h0: civic education = newspaper	F-stat p-value	0.064	0.123	0.181	0.150	0.142	0.269	0.208	0.256	0.012	0.007	0.106	0.107
h0: hotline = newspaper	F-stat p-value	0.323	0.469	0.268	0.296	0.323	0.297	0.979	0.942	0.083	0.052	0.579	0.760
controls		no	yes	no	yes	no	yes	no	yes	no	yes	no	yes

Note: All regressions are OLS. All dependent variables are z-scores. 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 8c: Individual survey measures (untargeted)

dependent variable ------>		individual survey measures (untargeted)													
		cell phone use		verdade		trust verdade		trust electoral commission		neutrality of electoral commission		interest about the elections		information about the elections	
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
civic education	coefficient	0.003	-0.067	-0.152	-0.149	0.008	-0.044	0.275**	0.270**	0.184	0.175	-0.051	-0.064	0.116	0.057
	standard error	(0.157)	(0.141)	(0.098)	(0.097)	(0.157)	(0.167)	(0.112)	(0.121)	(0.132)	(0.139)	(0.142)	(0.134)	(0.102)	(0.075)
hotline	coefficient	0.202	0.198*	0.013	0.001	-0.190	-0.146	0.032	0.046	0.180	0.200	0.032	0.047	0.185**	0.177**
	standard error	(0.139)	(0.112)	(0.100)	(0.103)	(0.151)	(0.155)	(0.125)	(0.121)	(0.126)	(0.125)	(0.105)	(0.103)	(0.085)	(0.078)
newspaper	coefficient	0.202	0.205*	0.161	0.147	0.212	0.244*	0.244**	0.225*	0.197	0.181	0.018	-0.053	0.125	0.071
	standard error	(0.133)	(0.119)	(0.155)	(0.150)	(0.150)	(0.140)	(0.122)	(0.117)	(0.134)	(0.126)	(0.153)	(0.145)	(0.099)	(0.082)
mean dep. variable (control)		-0.000	0.007	-0.000	-0.002	0.000	-0.012	-0.000	-0.009	0.000	-0.005	-0.000	-0.002	0.000	-0.000
r-squared adjusted		0.051	0.217	0.128	0.212	0.010	0.081	0.052	0.054	0.072	0.076	0.160	0.206	0.082	0.318
number of observations		452	445	454	447	342	337	423	417	413	408	452	445	453	446
h0: civic education = hotline	F-stat p-value	0.275	0.099	0.175	0.239	0.317	0.623	0.095	0.137	0.980	0.883	0.617	0.486	0.541	0.220
h0: civic education = newspaper	F-stat p-value	0.267	0.101	0.066	0.072	0.301	0.140	0.830	0.742	0.938	0.972	0.729	0.954	0.940	0.886
h0: hotline = newspaper	F-stat p-value	0.999	0.959	0.382	0.402	0.038	0.031	0.161	0.222	0.916	0.904	0.937	0.553	0.589	0.310
controls		no	yes	no	yes	no	yes	no	yes	no	yes	no	yes	no	yes

Note: All regressions are OLS. All dependent variables are z-scores. 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 8d: Individual survey measures (untargeted)

dependent variable ------>		individual survey measures (untargeted)											
		confusion between state and ruling party		call for authority		problematic elections		vote miscounting		campaign money misbehavior		electoral violence and intimidation	
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
civic education	coefficient	-0.109	-0.035	0.097	0.117*	0.011	-0.018	-0.126	-0.095	-0.112	-0.117	-0.182***	-0.186***
	standard error	(0.108)	(0.097)	(0.070)	(0.068)	(0.131)	(0.136)	(0.138)	(0.145)	(0.118)	(0.115)	(0.058)	(0.055)
hotline	coefficient	-0.204**	-0.161*	0.058	0.084	0.223	0.197	0.058	0.025	0.110	0.094	-0.071	-0.081
	standard error	(0.091)	(0.095)	(0.062)	(0.060)	(0.153)	(0.158)	(0.129)	(0.129)	(0.098)	(0.091)	(0.090)	(0.078)
newspaper	coefficient	-0.262**	-0.195*	0.014	0.032	-0.246**	-0.237**	-0.224*	-0.180	-0.075	-0.080	-0.119*	-0.126*
	standard error	(0.109)	(0.109)	(0.062)	(0.057)	(0.108)	(0.109)	(0.133)	(0.135)	(0.109)	(0.118)	(0.064)	(0.074)
mean dep. variable (control)		0.000	0.007	-0.000	-0.003	0.000	0.002	0.000	-0.001	0.000	0.001	0.000	0.004
r-squared adjusted		0.033	0.101	0.100	0.143	0.032	0.027	0.007	-0.002	-0.005	-0.000	0.019	0.074
number of observations		312	308	454	447	437	431	430	425	442	435	451	444
h0: civic education = hotline	F-stat p-value	0.434	0.269	0.649	0.691	0.263	0.280	0.280	0.468	0.119	0.115	0.248	0.201
h0: civic education = newspaper	F-stat p-value	0.263	0.228	0.328	0.297	0.095	0.168	0.575	0.623	0.803	0.809	0.396	0.462
h0: hotline = newspaper	F-stat p-value	0.643	0.784	0.568	0.486	0.007	0.015	0.092	0.225	0.170	0.213	0.633	0.657
controls		no	yes	no	yes	no	yes	no	yes	no	yes	no	yes

Note: All regressions are OLS. All dependent variables are z-scores. 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 9a: Multiple imputation (targeted)

dependent variable ------>		main individual outcomes (targeted)										
		self-reported	finger	average questions	average questions (ballot facts only)	interviewer assessment	open letter	guebuza	dhlakama	simango	frelimo	renamo
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
civic education	coefficient	0.021	0.044*	0.029	0.041*	0.056**	0.033	0.040	-0.006	0.002	0.034	-0.009
	standard error	(0.023)	(0.026)	(0.021)	(0.024)	(0.024)	(0.037)	(0.027)	(0.015)	(0.019)	(0.027)	(0.016)
hotline	coefficient	0.045*	0.050	0.051**	0.054**	0.062**	-0.013	0.047*	0.002	-0.006	0.050*	0.003
	standard error	(0.026)	(0.033)	(0.021)	(0.022)	(0.024)	(0.032)	(0.028)	(0.016)	(0.024)	(0.027)	(0.016)
newspaper	coefficient	0.017	0.020	0.018	0.029	0.032	0.063*	0.008	0.008	-0.006	-0.003	0.000
	standard error	(0.024)	(0.034)	(0.029)	(0.028)	(0.027)	(0.038)	(0.032)	(0.020)	(0.019)	(0.033)	(0.016)
mean dep. variable (control)		0.881	0.811	0.802	0.780	0.771	0.163	0.824	0.032	0.046	0.830	0.033
number of observations		1,514	1,514	1,514	1,514	1,514	1,514	1,514	1,514	1,514	1,514	1,514
h0: civic education = hotline	F-stat p-value	0.341	0.870	0.300	0.585	0.835	0.231	0.795	0.629	0.731	0.560	0.469
h0: civic education = newspaper	F-stat p-value	0.879	0.513	0.692	0.687	0.399	0.486	0.345	0.500	0.644	0.234	0.548
h0: hotline = newspaper	F-stat p-value	0.307	0.376	0.234	0.356	0.295	0.048	0.239	0.723	0.986	0.097	0.897

Note: All regressions are OLS, while using multiple imputation by chained equations. Dependent variables are between 0 and 1 (turnout) and binary (open letter and voting). All regressions include province dummies and controls. 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. 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 9b: Multiple imputation (untargeted)

dependent variable ------>		main individual outcomes (untargeted)										
		self-reported	finger	average questions	average questions (ballot facts only)	interviewer assessment	open letter	guebuza	dhlakama	simango	frelimo	renamo
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
civic education	coefficient	0.021	0.052	0.037	0.036	0.034	0.064	0.032	-0.011	0.003	0.025	0.004
	standard error	(0.046)	(0.048)	(0.043)	(0.044)	(0.040)	(0.056)	(0.060)	(0.021)	(0.037)	(0.052)	(0.027)
hotline	coefficient	0.070*	0.071*	0.064*	0.060	0.072*	0.001	0.010	-0.003	0.022	0.006	0.003
	standard error	(0.041)	(0.042)	(0.037)	(0.042)	(0.039)	(0.046)	(0.052)	(0.023)	(0.033)	(0.045)	(0.029)
newspaper	coefficient	0.075**	0.102**	0.071**	0.074**	0.089**	0.049	0.084*	-0.030*	-0.023	0.030	-0.014
	standard error	(0.037)	(0.045)	(0.033)	(0.034)	(0.045)	(0.057)	(0.046)	(0.016)	(0.030)	(0.043)	(0.027)
mean dep. variable (control)		0.881	0.811	0.802	0.780	0.771	0.163	0.824	0.032	0.046	0.830	0.033
number of observations		704	704	704	704	704	704	704	704	704	704	704
h0: civic education = hotline	F-stat p-value	0.314	0.747	0.584	0.642	0.431	0.348	0.754	0.778	0.626	0.752	0.986
h0: civic education = newspaper	F-stat p-value	0.281	0.422	0.486	0.455	0.289	0.844	0.465	0.468	0.498	0.949	0.606
h0: hotline = newspaper	F-stat p-value	0.918	0.528	0.884	0.767	0.745	0.471	0.287	0.330	0.210	0.670	0.532

Note: All regressions are OLS, while using multiple imputation by chained equations. Dependent variables are between 0 and 1 (turnout) and binary (open letter and voting). All regressions include province dummies and controls. 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. 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 10a: Heterogeneous effects

variable interacted with treatments ----->		var = male (individual)		var = age (individual)		var = has 12 years of schooling (individual)		var = farmer (individual)		var = stays at home (individual)	
dependent variable ----->		turnout	open letter	turnout	open letter	turnout	open letter	turnout	open letter	turnout	open letter
		(1)	(2)	(3)	(4)	(5)	6)	(7)	(8)	(9)	(10)
civic education	coefficient	0.071*	0.017	0.014	0.109	0.093***	0.049	0.081**	0.032	0.086***	0.051
	standard error	(0.040)	(0.054)	(0.085)	(0.095)	(0.029)	(0.049)	(0.039)	(0.050)	(0.029)	(0.053)
hotline	coefficient	0.093**	-0.083**	0.153*	-0.012	0.102***	-0.024	0.128***	-0.030	0.094***	-0.023
	standard error	(0.041)	(0.039)	(0.092)	(0.085)	(0.027)	(0.035)	(0.035)	(0.043)	(0.027)	(0.039)
newspaper	coefficient	0.061	0.076	-0.101	0.171	0.054	0.104**	0.093**	0.054	0.014	0.089
	standard error	(0.047)	(0.055)	(0.096)	(0.110)	(0.038)	(0.052)	(0.039)	(0.047)	(0.040)	(0.057)
var	coefficient	0.065	-0.042	0.000	-0.001	0.193***	0.165**	0.065	-0.037	-0.021	0.030
	standard error	(0.044)	(0.038)	(0.002)	(0.001)	(0.052)	(0.081)	(0.046)	(0.044)	(0.057)	(0.060)
civic education * var	**coefficient**	**0.013**	**0.064**	**0.002**	**-0.002**	**-0.274****	**0.042**	**-0.024**	**0.033**	**-0.090**	**-0.049**
	standard error	**(0.057)**	**(0.064)**	**(0.002)**	**(0.002)**	**(0.129)**	**(0.179)**	**(0.060)**	**(0.082)**	**(0.095)**	**(0.095)**
hotline * var	**coefficient**	**-0.005**	**0.105***	**-0.002**	**-0.001**	**-0.113**	**-0.137**	**-0.102***	**-0.022**	**-0.019**	**-0.103**
	standard error	**(0.062)**	**(0.062)**	**(0.002)**	**(0.002)**	**(0.079)**	**(0.114)**	**(0.056)**	**(0.059)**	**(0.091)**	**(0.078)**
newspaper * var	**coefficient**	**-0.055**	**0.028**	**0.004***	**-0.002**	**-0.204****	**-0.177**	**-0.174****	**0.109**	**0.180****	**-0.008**
	standard error	**(0.058)**	**(0.079)**	**(0.002)**	**(0.003)**	**(0.103)**	**(0.131)**	**(0.082)**	**(0.093)**	**(0.075)**	**(0.122)**
mean dep. variable (control)		0.753	0.153	0.752	0.154	0.753	0.153	0.753	0.153	0.753	0.153
r-squared adjusted		0.030	0.012	0.031	0.017	0.031	0.016	0.031	0.012	0.032	0.010
number of observations		953	973	946	966	951	971	953	973	953	973

Note: All regressions are OLS. Only targeted respondents are considered in treatment locations. The dependent variables are between 0 and 1 (turnout - interviewer assessment) and binary (open letter). 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 10b: Heterogeneous effects

variable interacted with treatments ------>		var = artisan (individual)		var = public official (individual)		var = has cattle (individual)		var = guebuza share in 2004 (location)	
dependent variable ------>		turnout	open letter	turnout	open letter	turnout	open letter	turnout	open letter
		(1)	(2)	(3)	(4)	(5)	6)	(7)	(8)
civic education	coefficient	0.061**	0.038	0.084***	0.049	0.108***	0.028	0.064	0.311
	standard error	(0.028)	(0.048)	(0.028)	(0.048)	(0.031)	(0.056)	(0.084)	(0.222)
hotline	coefficient	0.083***	-0.034	0.093***	-0.033	0.109***	-0.071*	-0.013	-0.061
	standard error	(0.026)	(0.036)	(0.025)	(0.034)	(0.030)	(0.040)	(0.091)	(0.131)
newspaper	coefficient	0.019	0.071	0.049	0.096*	0.106***	0.082	-0.258	0.177
	standard error	(0.038)	(0.049)	(0.036)	(0.049)	(0.038)	(0.058)	(0.193)	(0.181)
var	coefficient	-0.166	-0.157***	0.246***	-0.005	0.089**	-0.043	0.083	0.143
	standard error	(0.120)	(0.030)	(0.036)	(0.137)	(0.045)	(0.062)	(0.124)	(0.193)
civic education * var	**coefficient**	**0.350*** **	**0.179**	**-0.319*** **	**-0.044**	**-0.139***	**0.060**	**0.008**	**-0.366**
	standard error	**(0.124)**	**(0.180)**	**(0.099)**	**(0.173)**	**(0.073)**	**(0.083)**	**(0.112)**	**(0.272)**
hotline * var	**coefficient**	**0.200**	**0.030**	**-0.090** **	**0.000**	**-0.061**	**0.143***	**0.146**	**0.033**
	standard error	**(0.130)**	**(0.040)**	**(0.042)**	**(0.174)**	**(0.059)**	**(0.083)**	**(0.118)**	**(0.178)**
newspaper * var	**coefficient**	**0.336*** **	**0.330*** **	**-0.442*** **	**-0.228**	**-0.255*** **	**0.024**	**0.423***	**-0.126**
	standard error	**(0.127)**	**(0.093)**	**(0.147)**	**(0.146)**	**(0.065)**	**(0.098)**	**(0.247)**	**(0.246)**
mean dep. variable (control)		0.753	0.150	0.753	0.150	0.753	0.153	0.753	0.153
r-squared adjusted		0.030	0.016	0.031	0.011	0.039	0.013	0.037	0.016
number of observations		953	972	953	972	953	973	953	973

Note: All regressions are OLS. Only targeted respondents are considered in treatment locations. The dependent variables are between 0 and 1 (turnout - interviewer assessment) and binary (open letter). 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%.