

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 participation were randomized: an information campaign using SMS, an SMS hotline for electoral misconduct, and the distribution of a free newspaper. To measure impact, we look at official electoral results, reports by electoral observers, and behavioral and survey data. We find positive effects of all treatments on voter turnout. However, only the distribution of the free newspaper led to more accountability-based participation and to a decrease in electoral problems.

FRELIMO did it, FRELIMO does it.

2009 campaign slogan

I. Introduction

ECONOMISTS and development practitioners alike have long recognized the importance of political accountability for development. The hope is that once democratic institutions are in place and reflect the interests of the majority, effective development policies focusing on the poor will be implemented. Economic theory appears to support these claims: Becker (1983) shows that when political competition is secured, efficient policies arise.

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Yet developing democratic institutions that favor the general population has been difficult to achieve in many countries. These problems have often been linked to information deficiencies (Grossman & Helpman, 1996). Empirically, information provision has been linked to a reduction in the reelection of corrupt politicians (Ferraz & Finan, 2008), the strengthening of pro-poor public policies (Besley & Burgess, 2002), and improved public service delivery (Bjorkman & Svensson, 2009). The mobilization and political participation of citizens are seen as necessary conditions for these impacts of information.

In sub-Saharan Africa, the record of post-Soviet democratization in producing development has been mixed (e.g., Kudamatsu, 2012). One prominent concern is that elections do not discipline governments because of the many irregularities that have tainted their conduct (Chauvet & Collier, 2009; Collier & Vicente, 2012). However, relatively unproblematic elections, such as the Mozambican election studied in this paper, suggest that dominant incumbents may have developed (licit or illicit) mechanisms to secure clear victories well before the actual suffrage. These mechanisms may be helped by information deficiencies and by limited political participation: indeed, in Mozambique, levels of political information are low (e.g., as pointed out by the Afrobarometer), and voter turnout has decreased dramatically over the years, from 90% in 1994 to just 36% in 2004.

In this paper, we test whether the Mozambican electorate is responsive, in terms of both electoral turnout and demand for political accountability, to voter education that provides neutral electoral information and calls for political participation. While civic education campaigns have traditionally been provided by the electoral commission and civil society through door-to-door campaigns several months prior to the election, the voter education interventions we study were delivered through innovative means near the elections, including text messages on mobile phones and a free newspaper that attracted the attention of international media.¹ While political campaigns based on text messaging have become increasingly common in the United States and Europe, to the best of our knowledge, this is the first study of

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

voter education based on mobile phones in a developing country.²

We designed and conducted a field experiment during the October 2009 presidential and parliamentary elections in Mozambique. Polling locations in four provinces of the country were randomly selected to receive three interventions. The first treatment, civic education, delivered electoral information via an official leaflet and, subsequently, a range of mobile phone messages. The second treatment, a hotline, invited citizens to report electoral problems by sending text messages to an SMS hotline, which were verified by local correspondents and disseminated back to voters in the same treatment locations. The third treatment, newspaper, provided voter education information in a free and independent newspaper *@Verdade*. By prior agreement with the editors of the newspaper, *@Verdade* included weekly information on civic education and access to a national hotline, both of which were similar to our other treatments. All treatments were aimed at disseminating electoral information, as well as increasing electoral participation and the demand for political accountability. However, the civic education treatment focused on the provision of neutral information about the electoral process, whereas the hotline treatment focused on the provision of information about electoral problems and on encouraging citizens to become actively engaged in sharing this information.

To measure the effects of these voter education interventions, we employ the official electoral results and administrative records of electoral problems from electoral observation at the polling location level, as well as individual behavioral and survey data. Official electoral results allow an objective assessment of the impact of the treatments on voter turnout and voting patterns. We also collected a behavioral measure of demand for political accountability by inviting experimental subjects in all locations to send SMS proposing their policy priorities to the president-elect. Since sending a text message is a costly action, we interpret it as an incentive-compatible measure of demand for accountability.

Our results provide evidence that all three treatments increased official voter turnout by 5 percentage points as compared to the control group, without statistically significant differences between the treatments. This treatment effect magnitude represents 11% of the average turnout rate in control locations. We observe that the civic education treatment increased the vote share of the incumbent. We also report that the newspaper treatment led to higher demand for political accountability: the probability that an individual sends a text message about his or her policy priorities increased by 10 percentage points as compared to the control group. The newspaper was also the only treatment that affected the prevalence of electoral problems, as

reported by electoral observers; incidence of these occurrences was reduced by 0.58 problems. All treatments increased information about the elections. It is possible that the newspaper was particularly effective at delivering complex political information and producing local oversight, which together could explain the effects on increasing the demand for political accountability and decreasing the prevalence of electoral problems.

Recent experimental studies in developing countries have focused on voter education interventions aimed at counteracting specific illicit strategies during elections. Wantchekon (2003) and Fujiwara and Wantchekon (2013) target clientelism in Benin by studying clientelism-free political campaigning. Vicente (2014) looks at vote buying (as cash for votes) in Sao Tome and Principe by analyzing an educational campaign against that practice. Collier and Vicente (2014) examine electoral violence in Nigeria by assessing the effects of grassroots mobilization against politically motivated violence. Ichino and Schundeln (2012) study voter registration irregularities in Ghana. Other recent experimental papers focused directly on participation and accountability. Gine and Mansuri (2011) assess the impact of a voter mobilization campaign that targeted women in Pakistan and find a treatment effect on turnout comparable to ours. Banerjee et al. (2011) study the effects of the dissemination of information about candidate qualifications and legislator performance on electoral outcomes in India; they report an increase in voter participation. Chong et al. (2015) focus on the voter turnout effects of dissemination of information about corruption in Mexico; contrary to Banerjee et al., these authors find a decrease in voter participation. Humphreys and Weinstein (2012) analyze the effects of scorecards about legislator performance on both voter and politician behavior in Uganda.

This paper also relates to two other branches of the literature. First, it links to the vast array of experimental research on voter mobilization and electoral campaigning in U.S. elections. This work ranges from the assessment of different voter mobilization activities (Gerber & Green, 2000) and partisan campaigning (Gerber, 2004), generally labeled as get-out-the-vote (GOTV) interventions, to the identification of the effects of newspapers on voting behavior (Gerber, Karlan, & Bergan, 2009). We mention specifically the work of Dale and Strauss (2009), who look at the effect of text messages reminding citizens to vote in the 2006 elections. Although this literature for the United States points to the importance of personal contact in mobilizing voters to vote (canvassing in Green & Gerber, 2000, increased turnout by 10 percentage points), Dale and Strauss still find that text messages increased voter turnout by 3 percentage points. The magnitudes of the effects on voter turnout we find in this paper are broadly comparable to this literature, as we used in-person contact including the distribution of written materials, as well as follow-up text messages. Second, our paper links to the emerging literature on the effects of information and communication technology on various develop-

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

ment outcomes. Jensen (2007) looks at the use of mobile phones to improve market efficiency in a local fish market in India. Aker (2010) studies the effects of mobile phone introduction on grain market outcomes in Niger. Other recent studies focus on information campaigns relating to health or savings behavior (e.g., Pop-Eleches et al., 2011; Karlan et al., 2016).

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

II. Context

Mozambique, a country with 23.4 million inhabitants in 2009, is one of the poorest countries in the world, with a per capita GDP of US \$414 (current in 2009).³ Without prominent natural resources until recently and with 81% of the population directly dependent on agriculture,⁴ it is an aid-dependent country, with official aid assistance accounting for 21% of GNI in 2009.⁵

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, the United States. The civil war ended in 1992 with an agreement to hold multiparty elections. Four 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. FRELIMO has been consistently increasing its vote share, while voter turnout decreased from 90% in 1994 to just 36% in 2004. The marked decline of voter turnout over the past two decades is distinctive compared with other countries in the subregion and has often been referred to as the "Mozambican exception" (De Brito, 2007).⁶ The common factor across all national elections has been allegations of electoral irregularities (primarily ballot fraud) by FRE-

LIMO. While these claims have been made primarily by RENAMO, international observers have corroborated them on several occasions.⁷

Armando Guebuza became FRELIMO's leader and president in 2004, succeeding Joaquim Chissano. Guebuza had an important record within FRELIMO from before independence to the early years as minister of the interior under Samora Machel. He became a wealthy and powerful businessman after the privatization of public companies in the 1990s. In the 2009 election, he was running for reelection as president. His main opponent, Afonso Dhlakama, has been the leader of RENAMO since 1984. Dhlakama served as a guerrilla leader during the civil war and RENAMO's presidential candidate in all national elections.

In this paper, we study the presidential, parliamentary, and provincial assembly elections of October 28, 2009.⁸ The run-up to the 2009 elections was relatively calm, with Guebuza and FRELIMO expected to win. Prior to the elections, Dhlakama had been increasingly discredited and was widely seen as an outdated leader, as he often referred to the possibility of taking up arms. The third presidential candidate was the mayor of Beira (Mozambique's second largest city), Daviz Simango. He had split from RENAMO to launch MDM (Movimento Democrático Moçambicano) in early 2009. As a member of the younger generation not directly linked with the references of the past, Simango was popular among urban youth. The main issues arising in the run-up to the election were allegations of bias in the voter registration process,⁹ the exclusion of some parties (including MDM) by the National Electoral Commission of Mozambique (CNE) from contesting elections in several districts, campaign violence, and many instances of intimidation and use of state resources for campaigning.

The elections were conducted in a relatively unproblematic manner, as witnessed by national and international observers. These observers generally considered the elections to follow appropriate international standards despite many small irregularities.¹⁰ Results were unambiguous, giving 75% 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% and Simango/MDM had 9%/4% (respectively, for the presidential and parliamentary elections).

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

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

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

¹⁰ European Union, *Electoral Observation Mission—Final Report, Mozambique 2009*; Observatório Eleitoral, *Declaration about the Presidential, Parliamentary, and Provincial Assembly Elections* (2009).

³ World Development Indicators (2014).

⁴ CIA *World Factbook* (2014).

⁵ World Development Indicators (2014).

⁶ In the online appendix to this paper, we show one figure depicting the main election outcomes over the four elections in Mozambique, and one figure showing voter turnout trends in Mozambique relative to those in the Southern African Development Community neighboring countries.

Numerous sources point to the low levels of information and political accountability in Mozambique. Freedom House currently considers Mozambique a “partly free” country. Afrobarometer data (see Pereira, Davids, & Mattes, 2002, and Pereira et al., 2003) find relatively low levels of support for democracy, and characterize Mozambique as a “democracy with problems.” Citizens display a clear resistance to expressing opinions about politics, as well as difficulty in grasping the role of democracy in improving economic outcomes. Mattes and Shenga (2008) hypothesize that the very low levels of political accountability in Mozambique may be the result of deficient channels of information dissemination, exacerbated by poverty, low levels of education, and state control of the media. De Brito (2007) underscores the marked decreasing trend of voter turnout while also highlighting the role of international donors in providing incentives to Mozambican politicians, perhaps at the expense of truly strengthening Mozambique’s civil society. Consistently, the interventions in our field experiment aim to increase voter participation and strengthen the demand for political accountability.

III. Experimental Design

A. Treatments

We collaborated with the newspaper *@Verdade* (<http://www.verdade.co.mz/>) and Observatório Eleitoral to develop and implement civic education interventions during the 2009 electoral period in Mozambique. *@Verdade*, a free newspaper created in 2008, is a general-interest, privately owned paper, without a clear political leaning, but with a civic education and social responsibility mandate. Observatório Eleitoral is a consortium of eight Mozambican NGOs, including the main religious civil society organizations and governance NGOs. It blends the efforts of its member organizations in the area of good electoral conduct and electoral observation. The three interventions we study in this paper were designed and conducted with the institutional support and active collaboration of these organizations. Both organizations see the dissemination of information about the elections and the encouragement of voter participation as central to their missions.

We now turn to the description of each specific intervention. Note that different interventions were allocated to different polling locations. Each intervention, including the full set of its activities, was directed at a specific set of experimental subjects within a location. We call these subjects the targeted individuals.

The civic education treatment provided citizens with specific information about the 2009 elections. The process was initiated with a door-to-door campaign approximately a month before the elections around selected polling locations. This campaign was implemented during the baseline survey and was centered on the distribution of a leaflet designed and made available by the Mozambican electoral

commission (CNE/STAE). The leaflet explained how to vote on the day of the election. Ten thousand leaflets were distributed (i.e., 250 per location) to random houses in treated locations, with fieldworkers making sure all targeted individuals were given the leaflet.¹¹

The civic education intervention modified the typical civic education approach by adding on a mobile phone dissemination component. Two weeks prior to the election, all targeted individuals in the civic education areas received a set of daily text messages on the mobile phone number they had provided during the baseline survey. Specifically, they received five messages a day. On each day, messages were chosen from a group of ten different messages. Messages focused on the importance of voter participation, as in a typical GOTV 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 provided citizens with access to an SMS hotline to report electoral problems that they observed prior to and during the election. Two short-code phone numbers were contracted with the two mobile phone operators in Mozambique (Mcel and Vodacom) for that purpose.¹² Similar to the civic education treatment, approximately one month prior to the election (during the baseline survey), a door-to-door campaign, providing information on how the hotline could be used, was conducted around selected polling locations. As part of this campaign, 10,000 leaflets (250 per location) were distributed to random houses in these locations, with all targeted individuals receiving the leaflet. These leaflets provided basic information about the hotline system: phone numbers, examples of the types of electoral problems, format of reports to be sent, and the sponsors of the initiative.¹³ Each leaflet was locationspecific, so that it featured the name of the polling location corresponding to the location where the leaflet was distributed. The leaflets were also double-sided: one side provided an example of electoral problems prior to the election, and the other side provided an example of a problem on the day of the election. The purpose of this specificity was to minimize any potential mistakes when reporting problems.

A key feature of the hotline treatment was the dissemination of reported electoral problems. Individuals with access to the hotline were informed that their reports would be shared with the media and other individuals (targeted individuals in the hotline locations) by SMS. Before any dissemination took place, each report received on the hotline was verified with local correspondents hired for this purpose in

¹¹ The leaflet is displayed in the online appendix to this paper.

¹² The two numbers were meant to cover the users of both operators. Note that the same price was agreed to with both: 2 MZN (about 7 USD cents).

¹³ The leaflet is depicted in the online appendix to this paper.

each of the hotline locations. This process was managed online through the Ushahidi system (www.ushahidi.com), which allowed the reports to be viewed in real time.¹⁴ In addition to 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 the free newspaper *@Verdade*. Despite being the highest-circulation newspaper in Mozambique at the time (with a minimum of 50,000 certified copies per week), prior to this intervention, the newspaper was systematically distributed only in the city of Maputo. We agreed with the newspaper management that specifically for this project, the newspaper would be distributed weekly around selected polling locations, which had never received the newspaper since they all lie outside Maputo. This distribution was initiated with the baseline visit (September 2009) and continued until the postelection survey (November 2009). Five thousand copies of the newspaper were distributed each week—125 in each location. The newspapers were distributed to random houses (with all targeted individuals receiving the newspaper). Thus, this treatment was equivalent to an *@Verdade* subscription during the electoral period, offered to individuals who previously had not had systematic, if any, contact with that newspaper.

The editors of the newspaper took a strictly independent approach to the electoral process, focusing on voter education. More specifically, the newspaper published the contents of the civic education treatment by including a version of the CNE/STAE leaflet about the voting process on election day and by providing information on the scheduled date for the elections, the types of elections taking place, specific candidates, political parties, and how to vote (similar to our civic education text messages). The newspaper also sponsored a national hotline for reporting electoral problems: its website, featuring an Ushahidi interface, was very popular during that period (<http://www.verdade.co.mz/eleicoes2009>). The newspaper's hotline was essentially a replica of our hotline treatment, albeit branded with a different slogan and different phone numbers to enable the identification of a control group for our hotline treatment. The newspaper's hotline was disseminated through the newspaper itself, over the Internet, and through networks of civil society organizations (including Observatório Eleitoral). It had clear nationwide coverage.¹⁶

¹⁴ Ushahidi 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 available at www.protegemosovoto.org.

¹⁵ In effect, the standard Ushahidi software was tailored in our case to enable the management of the messages for us to send to experimental subjects, not only for the hotline (reminder messages and dissemination of received reports) but also for the civic education messages.

¹⁶ In the online appendix to this paper, we show the front page of the newspaper during the week of the elections, as well as the newspaper version of the official CNE/STAE leaflet about the voting process on election day and the description of the national hotline (sponsored by *@Verdade*) included in the newspaper.

B. Sampling and Assignment to Treatment

Since treatments were clustered around polling locations, the sampling frame of our experiment was constructed from the electoral map of the country. Because the use of mobile phones was central to all our treatments, we eliminated all polling locations without mobile phone coverage. For that purpose, we obtained detailed data from the two mobile phone operators on the GIS coordinates of each of their towers. Eligible polling locations were those classified as having a mobile phone tower within a 5-kilometer radius. Remarkably, 60% of all polling locations in Mozambique were covered by at least one operator. We selected 160 polling locations for our field experiment from our sampling frame. The registered voters corresponding to these polling locations were representative of Mozambican registered voters who had access to mobile phone coverage,¹⁷ meaning that each registered voter in the considered universe had the same probability of having his or her polling location sampled. The selection of these locations was the product of two-stage clustered representative sampling, first on provinces (four provinces) and then on polling locations (forty per province). The number of registered voters was used as sampling weight, based on information provided by the CNE/STAE. If, during the baseline survey, we found no mobile phone coverage in any specific polling location, we replaced it by the closest polling location with mobile phone coverage. That happened in seven locations.¹⁸

The allocation of treatment and control conditions to the full set of experimental polling locations (our primary randomization unit) followed a standard randomization procedure by which (a) clusters of four closest polling locations were formed within each province and (b) each of the three treatments was randomly assigned to one polling location in each cluster, with the remaining polling location serving as control. The project took place in the provinces of Cabo Delgado, Gaza, Maputo-Province, and Zambezia.¹⁹

We conducted two face-to-face surveys in the catchment areas of the experimental polling locations: one before the elections and one after.²⁰ While first-best random sampling for these surveys would have been based on voter registration lists at the individual level, Mozambican law does not allow the CNE/STAE to make these lists public. For this rea-

¹⁷ This was estimated at approximately 44% of the population in 2008 (GSM Association statistical data, 2009).

¹⁸ We have one additional polling location in our control group (the reason we have 161 and not 160 polling locations in our experimental data). This is due to the fact that we surveyed in one substitute location that was found to be unnecessary a posteriori. Results are robust to the exclusion of this polling location.

¹⁹ The final full sample of experimental locations, with each treatment represented, is depicted in a map in the online appendix.

²⁰ The fieldwork was undertaken by four teams, contemporaneously in each of the four provinces, with one supervisor per team and 31 enumerators in total. The surveys were administered mainly using electronic handhelds. The survey instruments in Portuguese are available on request. At least one of us was in the field at all stages of the project and directly managed operations.

son, sampling in the catchment area of each polling location, which we refer to as an enumeration area (EA), followed a random walk during the baseline survey: enumerators started from the center of the EA (typically the polling location itself) and sought houses with a predetermined interval between them. To find this predetermined interval, the procedure was as follows. We first divided the number of registered voters in the polling location by the average number of adult household members in Mozambique (three). This provided us with the approximate number of households in the corresponding polling location. We divided the corresponding number by five, which was the number of directions in which the survey team had to walk from the center of the EA. We next divided the resulting number by two (an approximation since we aimed for interviewing eleven individuals or households per location), which gave us the predetermined interval: when counting houses, the number where enumerators stopped to recruit a respondent. An additional condition for the selection of households was that each sampled household owned a mobile phone or had access to one owned by a friend or neighbor within the EA.²¹ Moreover, within each household, enumerators selected either the household head or his or her spouse. If it was not possible to meet both of these conditions of having access to mobile phones and being a household head or spouse, enumerators substituted the next house for it. Note that these conditions imply that we do not have a representative sample of adults at the EA level. The baseline survey had 1,766 households or respondents, 11 per EA on average, and took place from mid-September to mid-October 2009.

In addition to the randomization at the polling location level, treatments were randomized across individuals within each treated EA. Of the individuals interviewed at baseline per treated EA, two were, on average, randomly selected not to receive the treatment. We call these experimental subjects the untargeted individuals. The remaining sampled individuals in treatment locations are the targeted individuals, who were the main targets of the treatment activities, as already described. Note that leaflets (for the civic education and hotline treatments) and newspapers were distributed in much larger numbers than the ones corresponding to targeted individuals or houses. As noted, this distribution followed a random procedure. This was similar to the sampling rule used for the baseline survey. However, it deliberately left out untargeted individuals or houses.

Given our design of sampling and assignment to treatment, we perform analysis at both the polling location and individual levels in this paper.

C. Hypothesized Mechanisms

While all three treatments aimed to increase the levels of electoral information, mobilize voters to participate in the

electoral process, and strengthen their demand for political accountability, they are likely to produce different mechanisms of impact.

The civic education treatment focused on the dissemination of detailed and neutral information about the electoral process. If voters do not fully understand or trust the electoral process or they are forgetful, they may decide not to participate in the political process. Thus, receiving electoral information may increase voters' trust in the electoral process and increase their likelihood of voting. Receiving reminders in the form of SMS may make people more attentive to the election and increase political participation as well. It is possible that since all the information conveyed through the civic education treatment is neutral (likely to reassure voters about the reliability of the electoral process) and conveyed partially by CNE/STAE (through the official leaflet), which is closely linked to the incumbent party, voting for the incumbent candidate or party increases and perceptions about electoral problems improve. It is unclear whether neutral information about the elections can affect the prevalence of electoral problems.

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

The newspaper treatment is more difficult to define, as it is a multifaceted object, from a well-identified source institution, including different types of information beyond politics, which may trigger many different mechanisms of impact. Still, its political message focused on voter education, while including civic education and hotline treatments. Hence, we expect to find similar impacts on information and political participation. The potential impacts on voting for specific candidates or parties, trust in the electoral process, and perceptions of electoral problems are more ambiguous, even though, as with the civic education, the CNE/STAE sponsored some of the information. Since the newspaper provides more complex and detailed political information than the other treatments and its frequent distribu-

²¹ We verify that only 3% of our house calls in the baseline survey were unsuccessful because the corresponding households had no access to a mobile phone.

tion may be understood as local presence, we may expect a stronger impact on the demand for political accountability. It is possible that the prevalence of electoral problems decreases because of the additional scrutiny the presence of the newspaper and its hotline brings to treatment locations.²²

IV. Measurement

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

A. Measures at the Level of the Polling Location

Official electoral outcomes. All treatments aimed at increasing political participation in the 2009 elections. As a result, it is of particular importance to analyze the official results for the presidential and parliamentary elections of 2009. To this end, we obtained from CNE/STAE the electoral results, at the level of the polling location, from the 2009 elections, including data on the number of votes for each candidate and party, as well as blank and null votes. These data allow us to establish the impact of the interventions we study on voters' electoral behavior at the polls.

Electoral problems reported by electoral observers. We also obtained data on electoral problems at the polling location level. These were compiled from a rich data set of informal and formal electoral observation in the provinces of Cabo Delgado, Gaza, Maputo-Province, and Zambezia. We had access to four sources of data. First, we use the data on electoral problems received at the national hotline of @Verdade relating to both the campaign period and the day of the elections. Second, we obtained access to the campaign observation sheets filled by the formal national electoral observers of Observatório Eleitoral. These observation sheets were structured as a questionnaire, which asked mainly about the use of public resources for campaigning, vandalism, and intimidation. Third, we got access to the election day observation sheets filled by the formal national

electoral observers of Observatório Eleitoral. These observation sheets were also structured as a questionnaire: they asked mainly about violence and intimidation and about procedural deficiencies in the ballot stations. Fourth, we gained access to the election day observation sheets filled by the formal international electoral observation mission organized by UNDP Mozambique. Diplomatic personnel from a number of local embassies formed this mission. These sheets were structured as a questionnaire, asking about violence or intimidation and procedural problems of the voting process.²³

After matching the electoral problems with our experimental locations, we coded each of the problematic locations as having had election day misconduct, campaign misconduct, or violence and intimidation. We also constructed 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 nonviolent occurrences, including campaign misconduct and election day problems; 3 corresponds to occurrences leading to physical intimidation, including vandalism; 4 corresponds to occurrences resulting in injured people; and 5 corresponds to occurrences resulting in deaths. These data allow us to measure the impact of the treatments on the incidence and intensity of electoral problems.

B. Measures at the Level of the Individual

Behavioral measure of demand for political accountability. In the interest of measuring political participation at the individual level, we also designed a behavioral measure of demand for political accountability, which we refer to as the open letter. During the postelection survey, the survey team explained and distributed a leaflet to all survey respondents in all experimental locations, which invited them to send text messages proposing policy priorities to the president-elect for his new mandate.²⁴ Like the hotline leaflet, it had two sides with two different examples of possible messages. It also included phone numbers, format of the message, and sponsors. We were clear in conveying the limited extent of the initiative (a small number of polling locations in all of Mozambique) and promised that the contents of these messages would reach the president in person.²⁵ As with the hotline, each message sent had a small monetary cost. Sending the message therefore represents a costly action. It was observable to us, as

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

²⁴ The leaflet is depicted in the online appendix.

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

²² In view of this discussion of mechanisms, we summarize the main characteristics of the treatments in a table in the online appendix and grade the treatments along those dimensions.

all mobile phone numbers that sent messages were recorded and matched with those of the experimental subjects. We interpret the sending of an open letter message as an incentive-compatible measure of demand for political accountability.

Survey outcomes and survey design. The survey outcomes we employ in our analysis at the individual level come from the postelection survey we conducted, which sought the same respondents as the baseline survey, around the polling locations in our experiment. The postelection survey started after the election results were announced in early November, for a period of time similar to the baseline survey. It reached 1,154 respondents, clearly below the number of respondents at the baseline.²⁶ We checked for the impact of attrition in the individual data employing several methods. We report on these results in sections VI and VIII.

Our survey data include information for several potential mediators for the main treatment effects on behavior.²⁷ We constructed indices of individual questions for information about the elections; perceptions about the electoral commission; perceived confusion between state and ruling party; perceived electoral problems in general; and perceived vote miscounting, vote buying, and electoral violence and intimidation, in particular.²⁸

We normalize all survey question measures using z -scores, calculated by subtracting the control group mean and dividing by the control group standard deviation. The indices are constructed following the approach of Kling, Liebman, and Katz (2007).²⁹ We aggregate survey question measures using equally weighted averages of the normalized variables.³⁰ Note that for index components, the

normalization also changed the sign of individual measures in order to make them consistent with the corresponding index label. According to Kling et al. (2007), this aggregation improves statistical power to detect effects that go in the same direction within a domain.

A note is also due to the fact that we adapted our survey design in order to offer evidence of possible first reactions to the treatments and of conformity biases. Experimental subjects could change their views immediately after the treatments were initiated and could in principle adapt their survey reports about politics to whatever they perceived to be the expectations of the sponsors of the treatments.³¹ 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. In this way, we are able to measure whether there were first reactions to the treatments (the leaflets and newspaper) by contrasting treatment and control groups for baseline values of our outcomes of interest. Importantly, differences in past behavior or perceptions about the past can be taken as evidence of conformity.³²

V. Estimation Strategy

Our main objective is to estimate the impact of the voter education treatments on a variety of outcomes. The effect of interest (β) can be estimated through the following specification, exemplifying with data at the individual level:

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

where the variable $Y_{i,l,p}$ represents the outcome of interest of individual i in location l and province p in the postelection period, $T_{l,p}$ is a vector of three binary variables representing the three treatments (civic education, hotline, and newspaper) with value 1 for treated units, W_p is a vector of province binary variables (the level of stratification), $Z_{l,p}$ is a vector of location characteristics, and $X_{i,l,p}$ is a vector of individual or household demographic characteristics.³³ The coefficients of interest (β) capture the average impact of the different treatments as compared to the control group. As being assigned to a treatment does not necessarily imply

²⁶ The main specific reason for attrition in the postsurvey period was reported to be the agricultural season. The rainy season in Mozambique, requiring work in the fields (*machambas*), occurs from November to January each year. Agricultural workers often temporarily migrate for this reason.

²⁷ We also gathered measurements of individual electoral behavior during the postelection survey. We were particularly careful with our measurement of voter turnout in view of existing concerns with the standard direct question on voter turnout from Afrobarometer surveys in Mozambique, which consistently overestimate actual voter turnout. We dedicated a module of the questionnaire to collecting data about all details of the respondent's election day experience. We construct alternative measures of individual turnout, including a measure of inked fingers—dipping one finger in indelible ink was part of the official voting procedure as a way to prevent people from voting multiple times. The analysis of individual voting measures is shown in the online appendix.

²⁸ 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. For example, the question on the fairness of the vote count was, "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 or unfair.

²⁹ As in Kling et al. (2007), if an individual has a valid response to at least one component measure of an index, then we impute any missing values for other component measures at the assignment group mean.

³⁰ We display in a table in the online appendix all individual variables with original scales, as well as the corresponding aggregation.

³¹ Note that conformity biases can be seen as a specific type of demand or reporting effect. The demand, or Hawthorne, effect is commonly defined as a form of reactivity whereby subjects modify an aspect of their behavior being measured simply in response to the fact that they know they are being studied. This is a source of bias that is potentially present in any study with human subjects as long as subjects know they are being observed. Our individual measurements have this feature: subjects know someone is seeing their responses. On top of these demand effects on behavior, we may have biases in reporting. These may have happened specifically in the surveys conducted. Report biases are a particular source of concern for reported voting behavior, given its sensitivity.

³² In a figure in the online appendix, we show the sequence of the experiment, including treatments and measurement.

³³ This is in line with Duflo, Glennerster, and Kremer (2007), who argue that although controls do not generally change the estimate for the average treatment effect, they can help explain the dependent variable, and therefore typically lower the standard error of the coefficient of interest.

that the treated individual actually received the full treatment contents, we interpret these coefficients as intent-to-treat parameters. We cluster standard errors at the level of the EA in all regressions at the individual level. For ease of interpretation and transparency, we employ OLS estimations throughout the paper.

At the individual level, our experimental design allows us to distinguish between direct treatment effects on the targeted individuals (in which case we contrast targeted individuals in treated locations to individuals in control locations), and indirect treatment effects on the untargeted individuals (in which case we contrast untargeted individuals in treated locations to individuals in control locations). The indirect treatment effects on untargeted individuals may be the likely product of social network interactions with targeted individuals.³⁴ Given that the polling location was our primary unit of treatment randomization and the emphasis on polling location (administrative) data in our paper, in the main analysis that follows, we pool together targeted and untargeted individuals within each EA to estimate intent-to-treat effects. We do, however, distinguish between effects on the targeted and effects on the untargeted when discussing the mechanisms of impact in our experiment.

VI. Main Econometric Results

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

A. Balance

We report the results on balance in the online appendix to this paper (table 3 in the online appendix). We specifically display baseline means for the control group and differences between control and treatment groups in our experiment. The statistical significance of the differences is tested to assess comparability across the different groups. We also test the joint significance of the three treatments (relative to the control group). We document these results for a wide range of observable characteristics for both locations and individuals. We observe few differences (at standard significance levels) between the treatment groups and the control group, reassuring us that the randomization pro-

cedures were effective at creating similar comparison groups of locations and respondents.³⁵ We also report on attrition for survey respondents. We observe that there are no statistically significant differences between comparison groups for attrition rates; panel attrition did not significantly change the comparability of treatment and control groups in terms of observable characteristics. Finally, we assess balance of baseline outcomes across treatment and control locations. Since the corresponding survey questions were asked before the respondents were given the materials (leaflets and newspapers) for the treatments and no differences across comparison groups were found, we conclude that no evidence in favor of either first reactions (when considering voting intentions, current views, and expectations) or conformity biases (when considering reports about the past) was found.

B. Official Voting

We now turn to our main treatment effects. We start by analyzing the impact of the interventions on voting behavior using the 2009 official voting results at the level of the polling location. Table 1 displays the effects of the interventions on voter turnout and the vote shares of the candidates at the presidential elections. The table also shows the treatment effects on the shares of blank and null votes. Results for the parliamentary elections are very similar, given the typical straight tickets for Guebuza/FRELIMO and for Dhlakama/RENAMO.³⁶ For each outcome variable, we first control for provincial dummies only and then add location controls.³⁷ For each regression, we test the null hypothesis of equality within each pair of treatments effects and the null that the three treatments effects are equal to 0.

We find clear effects of all three treatments on increasing voter turnout in the presidential election. These effects are all close to 5 percentage points and statistically significant at the 5% level (p -values between 0.034 and 0.036) when including location controls (table 4, column 2). This treatment effect magnitude represents an increase of 11% in the average turnout rate in control locations. While the voter turnout results are jointly statistically significant (including location controls),³⁸ the treatments are not sig-

³⁵ We compute 285 differences across comparison groups and find 5 statistically significant differences at the 10% level, 3 at the 5% level, and none at the 1% level. We compute 95 joint significance tests and find 2 statistically significant ones. This pattern is consistent with what we would expect from randomized assignment.

³⁶ Note that MDM did not run in the districts of our experiment. The results relating to the parliamentary elections are included in the online appendix.

³⁷ These include number of polling tables and whether the location has public infrastructure or services.

³⁸ In face of the fact that joint significance of the treatments arises only in our analysis with controls, we performed a robustness exercise employing the lasso procedure to select control variables. The p -value for joint significance of the treatments decreases slightly as a result, which reassures us about the benchmark results. This is shown in the online appendix.

³⁴ This is related to the literature on the network effects of voter mobilization and education interventions (Nickerson, 2008; Fafchamps & Vicente, 2013; Gine & Mansuri, 2011). Note, however, that our indirect treatment effects may also include visibility effects of the campaign due to the door-to-door distribution of leaflets and newspapers: although leaflets and newspapers were directed to specific houses, general awareness about the content of campaigning at the enumeration-area level was difficult to restrict as field-workers distributing materials attracted attention.

TABLE 1.—OFFICIAL BALLOT STATION OUTCOMES: PRESIDENTIAL ELECTIONS

Dependent Variable	Turnout		Guebuza		Dhlakama		Simango		Blank Votes		Null Votes	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Civic education	0.047*	0.053**	0.049**	0.046**	-0.030*	-0.032**	-0.013	-0.012	-0.001	0.001	-0.004	-0.003
SE	(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	0.049*	0.053**	0.022	0.022	-0.010	-0.012	-0.006	-0.004	-0.001	-0.001	-0.005	-0.005
SE	(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	0.047*	0.054**	0.040**	0.041**	-0.015	-0.016	-0.013	-0.014*	-0.005	-0.004	-0.007*	-0.007*
SE	(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 dependent 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 ² adjusted	0.373	0.383	0.664	0.670	0.565	0.569	0.281	0.410	0.296	0.438	0.110	0.171
Number of observations	161	161	161	161	161	161	161	161	161	161	161	161
h0: Civic education = hotline	0.949	0.989	0.186	0.233	0.216	0.236	0.399	0.283	0.977	0.845	0.873	0.647
h0: Civic education = newspaper	0.991	0.981	0.674	0.807	0.345	0.362	0.968	0.833	0.715	0.595	0.453	0.331
h0: Hotline = newspaper	0.942	0.970	0.374	0.358	0.775	0.803	0.383	0.209	0.696	0.735	0.556	0.600
h0: All treatments = 0	0.143	0.079	0.079	0.094	0.293	0.272	0.344	0.226	0.962	0.957	0.306	0.271
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

All regressions are OLS. All dependent variables are shares; for turnout, we divide by the number of registered voters; for candidate scores, blank votes, and null votes, we divide by the number of votes. Guebuza, Dhlakama, and Simango are presidential candidates. Controls are enumeration area/polling location characteristics, which include number of polling tables and whether the location has public infrastructure and services. All regressions include province dummies. Standard errors reported in parentheses. Significant at *10%, **5%, ***1%.

nificantly different from each other, suggesting that each treatment was equally effective in increasing voter turnout. Note that in section VIII, we discuss evidence pointing in the direction of positive treatment contamination of control locations. It is then possible that the average effects on voting for some of the treatments constitute lower bounds for the true effects.

Although all the treatments were candidate and party neutral, a natural question is whether the treatments affected voting patterns. We see positive effects of the treatments on the vote share of the incumbent (Guebuza) and negative effects on the vote share of the challengers (Dhlakama and Simango). Specifically, the civic education treatment increased Guebuza's score by 5 percentage points (p -value 0.023) and decreased Dhlakama's score by 3 percentage points (p -value 0.051); the newspaper increased the score of the incumbent by 4 percentage points (p -value 0.047), and decreased Simango's score by 1 percentage point (significant only with location controls, with a p -value of 0.078). The hotline does not yield statistically significant effects on voting for the candidates. Note that when considering Guebuza's vote share, the treatments are jointly different from 0 but not significantly different from each other.³⁹

When looking at the remaining voting outcomes, we do not observe statistically significant effects of the treatments on the share of blank votes, but we identify negative effects of the newspaper on the share of null votes. These effects are close to 1 percentage point (significant at the 10% level). While this is small in magnitude, it is potentially a significant economic effect, as the mean of the dependent variable in the control locations is 3.6% this suggests a 19% decrease in the share of null votes in the newspaper treatment areas.

The increase in voter participation in the October 2009 elections, by close to 5 percentage points for all treatments, indicates that the interventions increased the marginal benefit of participating in the election. This change may have worked through extrinsic incentives related to the importance of the election and its outcome or through intrinsic incentives related to civic-mindedness.⁴⁰

The vote shift from the challengers to the incumbent, induced by civic education and (less clear) the newspaper,

³⁹ Individual measures of voter turnout and of voting for the candidates or parties gathered during the postelection survey yield treatment effects that are generally consistent with the results employing administrative data at the level of the polling location. The treatment effects using individual-level data are reported in detail in the online appendix. Specifically, we show turnout results employing a measure of inked fingers, as observed during the post election survey (table 8 in the online appendix). This measure may be taken as particularly reliable if compared with measures based on survey self-reports. When using inked fingers for measuring voter turnout, all treatments effects are slightly higher in magnitude than when employing the official results: specifically, 6 (civic education), 7–8 (hotline), and 8–10 (newspaper) percentage points (all statistically significant at the 5% or 10% levels, jointly different from 0, and not significantly different from each other). This is likely due to the fact that targeted individuals, who were more exposed to the interventions than average voters in the same locations, form most of our survey sample.

⁴⁰ See Dhillon and Peralta (2002) and Feddersen (2004) for reviews of the literature on the fundamentals of voter turnout.

TABLE 2.—BEHAVIORAL MEASURE OPEN LETTER: TARGETED PLUS UNTARGETED

Dependent Variable		Open Letter (targeted plus untargated)	
		(1)	(2)
Civic education	Coefficient	0.055	0.065
	SE	(0.045)	(0.044)
Hotline	Coefficient	−0.027	−0.014
	SE	(0.034)	(0.033)
Newspaper	Coefficient	0.083*	0.091*
	SE	(0.048)	(0.049)
Mean dependent variable (control)		0.153	0.151
R^2 adjusted		0.013	0.034
Number of observations		1,147	1,127
h0: Civic education = hotline	F p -value	0.065	0.065
h0: Civic education = newspaper	F p -value	0.613	0.644
h0: Hotline = newspaper	F p -value	0.021	0.020
h0: All treatments = 0	F p -value	0.068	0.046
Controls		No	Yes

All regressions are OLS. The dependent variable is binary: it takes value 1 if the individual sent an open letter message. Controls are enumeration area or polling location characteristics, which include whether the location has public infrastructures and services; and individual demographic characteristics. All regressions include province dummies. Standard errors reported in parentheses, corrected by clustering at the location (enumeration area) level. Significant at *10%, **5%, ***1%.

may be explained in different ways. First, given the limited electoral competition and the overwhelming dominance of the incumbent, in a clientelistic setting like the one in Mozambique, it is possible that the election became a turnout contest for Guebuza/FRELIMO across polling locations, with higher turnouts rewarded through postelection benefits at the local level. Then, broad voter education could mobilize voting for the incumbent.⁴¹ Second, we note that the shifts were driven by the civic education intervention and the newspaper treatment, while there is no significant effect of the hotline. The civic education, and the newspaper to lesser extent, focused on positive messages about the election, which could be associated with the incumbent via the CNE/STAE-sponsored information. On the contrary, the hotline focused on messages reporting electoral problems, without a state sponsor behind them. These differences across treatments may then have produced the changes in voting that we observe. This is supported by survey evidence on perceptions related to the elections. Third, it is possible that the treatments were not implemented in a fully neutral way, since field-workers may have their own views about the election and may have shown a bias in favor of the incumbent (without our knowledge).⁴²

⁴¹ Recent theoretical foundations for voter turnout, as in Myatt (2015), predict an underdog effect: lower turnout by the supporters of the incumbent and likely winner of the election than for the supporters of the challenger and underdog. A campaign analogous to voter education that increases the marginal benefit of voting can then lead to higher turnout by incumbent supporters. The same result would arise if the increase in the marginal benefit of voting happens only for incumbent supporters. This is in line with the simple clientelistic setting we allude to. Clientelism is pervasive in Mozambique, particularly in rural areas. Our survey respondents reported that local chiefs (typically appointed by the incumbent) were responsible for obtaining residence documents (85%), essential for school attendance, among other benefits; for undertaking dispute resolution (88%); for allocating wells (70%), land (55%), and public funds (43%); and for distributing food or seeds (29%) and construction materials (19%).

⁴² As a check of this, we estimate regressions for survey-based voting reports using enumerator dummies as controls. These results are shown in the online appendix. We do not find significant differences in the treatment effects stemming from the inclusion of these enumerator controls.

Finally, the fact that the newspaper decreased the share of null votes can be easily explained as a direct effect of voter education. Information about the voting process is likely to have decreased the number of errors by voters when voting. Since null votes are often taken as a proxy for ballot fraud, as votes can be nullified during the counting process (see Hanlon & Fox, 2006, for evidence suggesting this practice in Mozambique), we can also understand the negative impact of the newspaper on the share of null votes as a monitoring effect the presence of the newspaper may have induced in treated locations.

C. Demand for Political Accountability

While the previous results show how the treatments affected voting behavior, with an emphasis on voter turnout and voting for the candidates, we are also interested in analyzing treatment effects on the demand for political accountability. We employ data gathered from our open letter initiative to compose a behavioral measure of the demand for political accountability. As noted, all respondents in our postelection survey were invited to send their policy priorities via SMS, a costly action, to the new president-elect. After matching the mobile phone numbers that sent these SMS to those recorded for the survey respondents, we constructed a binary variable equal to 1 for individuals who sent a message through our open letter system and 0 otherwise. We run regressions with and without location and individual controls.⁴³ The results are displayed in table 2.

We first note that 15% of the experimental subjects in the control group sent at least one message through the open letter.⁴⁴ This fact represents a clear degree of adherence to the initiative. When looking at the difference between the

⁴³ Individual controls include gender, age, marital status, schooling, ethnicity, religion, occupation, and living conditions.

⁴⁴ Forty-eight percent of the messages received on our open letter system were matched to respondents' mobile phone numbers, as reported in the surveys.

TABLE 3.—ELECTORAL PROBLEMS

Dependent Variable		Any Problem				Election Day Misconduct (Incidence)		Campaign Misconduct (Incidence)		Violence and Intimidation (Incidence)	
		Incidence		Intensity (0-5)		(5)	(6)	(7)	(8)	(9)	(10)
		(1)	(2)	(3)	(4)						
Civic education	Coefficient	−0.326	−0.346	−0.056	−0.061	−0.003	−0.010	−0.350	−0.366	−0.076	−0.069
	SE	(0.308)	(0.311)	(0.229)	(0.235)	(0.132)	(0.134)	(0.264)	(0.262)	(0.134)	(0.136)
Hotline	Coefficient	−0.141	−0.089	0.102	0.109	0.001	0.006	−0.243	−0.215	−0.022	0.015
	SE	(0.310)	(0.312)	(0.230)	(0.236)	(0.133)	(0.134)	(0.265)	(0.263)	(0.135)	(0.137)
Newspaper	Coefficient	−0.588*	−0.578*	−0.399*	−0.466*	0.058	0.011	−0.566**	−0.508*	−0.170	−0.163
	SE	(0.312)	(0.317)	(0.232)	(0.240)	(0.134)	(0.136)	(0.267)	(0.267)	(0.136)	(0.139)
Mean dependent variable (control)		0.951	0.951	0.819	0.819	0.390	0.390	0.756	0.756	0.341	0.341
R^2 adjusted		0.153	0.159	0.202	0.177	0.443	0.445	0.215	0.242	0.108	0.109
Number of observations		161	161	161	161	161	161	161	161	161	161
h0: Civic education = hotline	F p -value	0.551	0.414	0.496	0.473	0.972	0.902	0.687	0.568	0.690	0.540
h0: Civic education = newspaper	F p -value	0.404	0.471	0.140	0.097	0.651	0.878	0.419	0.601	0.490	0.504
h0: Hotline = newspaper	F p -value	0.157	0.131	0.033	0.019	0.677	0.974	0.231	0.282	0.281	0.207
h0: All treatments = 0	F p -value	0.271	0.261	0.163	0.101	0.964	0.999	0.202	0.264	0.607	0.570
Controls		No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

All regressions are OLS. Electoral problems are coded from four sources in the four provinces covered in the experiment: @Verdade's national hotline, Observatorio Eleitoral's campaign observation, Observatorio Eleitoral's election day observation, and 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 in terms of intensity of violence: 0, no occurrences. Controls are enumeration area/polling location characteristics, which include number of polling tables, and whether the location has public infrastructures/services. All regressions include province dummies. Standard errors reported in parentheses. Significant at *10%, **5%, ***1%.

treatment and control groups, we find that individuals in both the civic education and newspaper treatment locations were more likely to send a text message to the open letter system. These effects are not statistically different from each other. However, we find a statistically significant impact only for the newspaper treatment, whose magnitude is 9 percentage points (p -value 0.063). We conclude that only the newspaper clearly increased the demand for political accountability as measured by our open letter. It is likely that the treatment substance relating to political accountability was relatively complex and required detailed information, as provided by the newspaper. It is also possible that the frequent newspaper distribution created a local presence, which facilitated the demand for political accountability.

D. Electoral Problems

We now turn to electoral problems as reported by electoral observers during the October 2009 electoral period. Table 3 presents these results. Using the data on electoral problems, we classified each problematic polling location in our experiment as having had election day misconduct, campaign misconduct, or violence and intimidation, or some combination of these. We are thus able to count reports for each type of problem at the level of the polling location. This is the way we compose incidence measures for each type of problem. We also employ a measure of the intensity of problems by classifying each problematic polling location in terms of the most serious problem that it had: we apply the scale of 1 to 5 we described above, from minor problems to occurrences resulting in homicides, with polling locations that had no electoral problems given a score of 0. We display a specification using province dummies and one specification that adds location controls to the province dummies.

When looking at the control polling locations, we can report on average almost one problem per location (0.95). However, the overall average intensity is fairly low (0.82 on the scale of 0–5). The incidence of electoral problems in control polling locations was higher for campaign misconduct than for election day misconduct or violence and intimidation. We find that all treatments had negative effects on the incidence of any type of electoral problems: they decreased the number of problems. However, only the newspaper treatment effect is statistically significant: it leads to 0.58 fewer problems as compared to the control group (p -value is 0.071). We find a similar pattern for the intensity score, where, again, only the newspaper treatment is significant: it decreases the intensity of problems by 0.47 points, a 57% decrease relative to the average score in the control group (p -value is 0.054). Note that the effect of the newspaper on intensity is statistically different from those of the other two treatments. Looking at the incidence of specific problems, the only significant effect is that of the newspaper treatment, and only for the incidence of campaign misconduct. The magnitude of this effect is 0.51 problems (significant at the 10% level). Overall, we find that the newspaper decreased the incidence and intensity of electoral problems. This is particularly the case for campaign misconduct. These findings suggest that the newspaper was effective at improving politician behavior locally, possibly because of increased perceived monitoring, in line with the idea of political accountability it was able to convey.

VII. Mechanisms

Our main results suggest that the voter education interventions were equally effective at increasing voter turnout and that the civic education and (to a lesser extent) the newspaper shifted the vote toward the incumbent and against the challengers. In addition, the newspaper

TABLE 4.—INDIVIDUAL SURVEY MEASURES: TARGETED PLUS UNTARGETED

Dependent Variable	Information about the Elections		Trust Electoral Commission		Neutrality of Electoral Commission		Confusion between State and Ruling Party		Problematic Elections		Vote Miscounting		Campaign Money Misbehavior		Electoral Violence and Intimidation	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Civic education	0.074 (0.064)	0.155*** (0.058)	0.204*** (0.078)	0.197*** (0.077)	0.139 (0.086)	0.116 (0.086)	-0.028 (0.067)	-0.087 (0.064)	-0.077 (0.082)	-0.043 (0.085)	-0.139* (0.076)	-0.139* (0.081)	-0.039 (0.062)	-0.021 (0.063)	-0.148*** (0.041)	-0.136*** (0.041)
Hotline	0.158*** (0.063)	0.177*** (0.058)	0.107 (0.081)	0.142* (0.077)	0.170*** (0.085)	0.178*** (0.078)	-0.197*** (0.060)	-0.222*** (0.058)	0.171*** (0.087)	0.193*** (0.084)	-0.020 (0.079)	-0.033 (0.084)	0.048 (0.063)	0.039 (0.065)	-0.058 (0.047)	-0.065 (0.045)
Newspaper	0.123* (0.068)	0.156*** (0.063)	0.174*** (0.078)	0.170*** (0.075)	0.151* (0.086)	0.129 (0.086)	-0.158*** (0.066)	-0.213*** (0.059)	-0.009 (0.083)	0.015 (0.084)	-0.079 (0.080)	-0.081 (0.087)	0.116* (0.067)	0.141*** (0.068)	-0.088*** (0.040)	-0.073* (0.042)
Mean dependent variable (control)	0.000	0.000	-0.000	-0.004	0.000	0.000	0.000	0.005	0.000	0.004	0.000	-0.005	0.000	0.003	0.000	0.004
R ² adjusted	0.078	0.255	0.068	0.086	0.084	0.092	0.038	0.088	0.021	0.027	0.016	0.015	0.008	0.015	0.032	0.048
Number of observations	1,151	1,135	1,068	1,053	1,033	1,020	814	804	1,119	1,104	1,102	1,088	1,131	1,115	1,148	1,132
h0: Civic education = hotline	0.145	0.678	0.200	0.467	0.693	0.439	0.003	0.024	0.010	0.017	0.073	0.106	0.173	0.370	0.029	0.071
h0: Civic education = newspaper	0.440	0.981	0.674	0.704	0.871	0.872	0.041	0.033	0.453	0.527	0.372	0.408	0.022	0.015	0.072	0.057
h0: Hotline = newspaper	0.570	0.714	0.383	0.703	0.813	0.539	0.488	0.872	0.060	0.064	0.405	0.503	0.324	0.144	0.460	0.842
h0: All treatments = 0	0.078	0.016	0.052	0.057	0.206	0.155	0.002	0.000	0.070	0.073	0.190	0.253	0.118	0.084	0.004	0.009
Controls									No	Yes	No	Yes	No	Yes	No	Yes

All regressions are OLS. All dependent variables are z-scores. See OA Table 2 for the definitions of the dependent variables. Controls are enumeration area/polling location characteristics, which include whether the location has public infrastructures/services and individual demographic characteristics. All regressions include province dummies. Standard errors reported in parentheses. These are corrected by clustering at the location (enumeration area) level. Significant at *10%, **5%, ***1%.

increased the demand for political accountability, reduced null votes, and decreased the number and intensity of electoral problems (particularly during the electoral campaign). We now revisit our proposed mechanisms in view of this evidence while offering additional survey evidence.

In table 4, we assess the impact of the treatments on information about the elections; perceptions about the electoral commission; perceived confusion between state and ruling party; perceived electoral problems in general; and perceptions about vote miscounting, vote buying, and electoral violence and intimidation. All measures are normalized as z-scores. Most measures we analyze are aggregations of different survey questions. As before, for each dependent variable, we show a specification with province dummies only and one specification adding location and individual controls.

We hypothesized that the civic education treatment would increase information and trust in the electoral process. Both are conducive to increased voter turnout (as we found) through extrinsic motives related to the electoral outcomes or civic mindedness. Indeed, civic education increased our index of information about the elections by 0.16 standard deviation units (this effect is statistically significant at the 1% level). We also observe that trust in the electoral commission increased by 0.2 standard deviations (significant at the 5% level). Since the CNE/STAE is closely associated with the incumbent, the civic education may have then increased trust in the incumbent and the incumbent's vote share. This is one of the possible explanations that we proposed for the changes in voting for the candidates produced by the civic education treatment (the main other being that incumbent-related clientelism could be the main reason to vote). Since this treatment reassured voters about the reliability of the electoral process, we also anticipated that perceptions about electoral problems could improve as a result. Consistently, individuals in the civic education treatment were less likely to perceive vote miscounting, and electoral violence and intimidation, both by 0.14 standard deviations (respectively, at the 10% and 1% levels of statistical significance).

The hotline treatment was expected to have a positive direct impact on political participation, since it coordinated citizens' participation in the electoral process through the hotline system. We indeed found a robust effect of the hotline on voter turnout. The hotline was also anticipated to have a positive effect on the level of electoral information through nudging the importance of the election. Consistently, we find that the hotline increased individuals' information about the elections by 0.18 standard deviation units (significant at the 1% level). Effects on trust in the electoral system and on the level of perceived electoral problems could go either way, as the hotline conveyed information about electoral problems, and voters could either feel reassured that problems were being resolved through neutral media dissemination or feel the election was particularly problematic. We find that the hotline treatment increased

trust in the electoral commission by 0.14 standard deviations (significant at the 10% level with controls). Moreover, it increased the perceived neutrality of the electoral commission by 0.18 standard deviations (significant at the 5% level) and reduced the confusion between the state and the ruling party by 0.22 standard deviations (significant at the 1% level). The fact that the hotline reported misbehavior by the supporters of all candidates may have contributed to these effects. The CNE/STAE may have been associated with the hotline despite not being a sponsor. The positive effects on trust and neutrality are likely to have reinforced effects on voter turnout. Still, we find an increase in the perception that elections were problematic, by 0.19 standard deviation units (significant at the 5% level). We did not find effects of the hotline on voting for the candidates in the official results at the level of the polling location. This is likely due to the fact that the hotline brought neutrality to the electoral process, different from the civic education.

While citizens can perceive the newspaper treatment in a specific way, it included both the information conveyed by the civic education and a national hotline similar to the experimental one. We therefore expected the newspaper treatment to have impacts similar to the ones encountered for the other treatments (when they were similar among them). Consistently, we found a clear positive effect on voter turnout. The newspaper was also the only treatment to decrease the share of null votes, pointing to strong information effects. We observe that the newspaper treatment increased our index of information about the elections by 0.16 standard deviation units (significant at the 5% level). We also find that the newspaper increased trust in the electoral commission by 0.17 standard deviations (significant at the 5% level), improved the perceived neutrality of the electoral commission by 0.15 standard deviations (significant at the 10% level with province dummies only), and decreased confusion between state and ruling party by 0.21 standard deviations (significant at the 1% level). Effects on perceptions of electoral problems are mixed, like the ones we encountered for the two other treatments: while the perception of electoral violence and intimidation decreased (by 0.07 standard deviations, similar to what we found for the civic education treatment—the difference between the two effects is not statistically significant), the perception of campaign money misbehavior increased (by 0.14 standard deviations).⁴⁵ The fact that the newspaper had a positive effect on voting for the incumbent (even though less robust than the one for the civic education) may be explained by the sponsoring of some information by CNE/STAE, analo-

gous to the civic education treatment effects. Finally, improved trust and neutrality in the electoral process, together with the ability of the newspaper to convey complex messages on a frequent basis, can justify the positive impact of the newspaper on the demand for political accountability. This effect, by increasing local scrutiny, may help explain the decrease in null votes (if taken as a measure of ballot fraud) and in the prevalence of electoral problems that we find.

A final note goes to the distinction between treatment effects on targeted individuals (direct) and treatment effects on untargeted individuals (indirect) by comparing each of these subsamples in treated locations to control individuals. This exercise may help shed light on the way treatments diffused in treated locations. We observe clearer effects on the targeted than on the untargeted for the demand for political accountability (the positive effect of the newspaper treatment on the sending of the open letter is not statistically significant when considering only untargeted individuals as the treated ones). Still, we see many significant treatment effects on the untargeted for mediating outcomes.⁴⁶ For instance, the effects on information about the elections for the untargeted individuals are statistically significant for all treatments and undistinguishable from the effects on the targeted individuals. This is suggestive that information and perceptions diffused easily within treated locations, which helps explain the effects we encounter at the level of the polling location.

VIII. Robustness

There are several threats to the validity of our findings. First, while there was clear attrition in our postelection survey, this is of limited importance for our results, as it relates only to our individual-level data, which were primarily used for the open letter and for a better understanding of mechanisms at work in our experiment. Despite this fact, we undertook a number of exercises checking the robustness of our findings to panel attrition. First, we verified that attrition rates are not statistically different across treatment and control groups. We also checked the balance of baseline individual characteristics and baseline outcomes for the postelection survey sample: we found few statistically significant differences across treatment and control groups.⁴⁷ Moreover, we employ a method of sensitivity analysis proposed by Lee (2005) whereby best-case and worst-case scenarios for differential attrition are constructed by trimming the top or the bottom of the distribution of the outcome in the group with least attrition (which in our case is always

⁴⁵ While the newspaper produced the only significant effects on electoral problems reported by electoral observers, the differences in survey perceptions—improvements for violence/intimidation and deterioration for vote buying—still do not correspond to the ones encountered by electoral observers—improvements in campaign misconduct. Note, however, that electoral observation sheets did not include questions on vote buying. Hence, the perceptions about vote buying do not have an obvious comparison term in the electoral observation data that we employ.

⁴⁶ Full results distinguishing between these two types of effects are shown in the online appendix. These include not only the outcomes shown in the paper (table 10 in the online appendix) but also a wider set of individual outcomes.

⁴⁷ These results are available in the online appendix. We also identified the demographic correlates of the panel drops (refer to the online appendix). These were found to be few, at standard levels of statistical significance.

the treatment group), leading to the computation of treatment effect bounds. This exercise reassures us about the treatment effects we encounter in the main analysis of this paper: the positive effect of the newspaper on the sending of messages for the open letter appears in both Lee bounds, even though the lower bound is not statistically significant.⁴⁸ We conclude that attrition does not seem to substantially affect our results.

A second threat to the identification of our results is conformity bias. While this would primarily be a concern for our individual-level measures, we can also be concerned about the possibility that individuals living in treated polling locations felt observed and changed their electoral behavior because of that. To test for this potential source of bias, we checked the possibility of treatment contamination to nearby untreated polling locations. This is a test of conformity bias, as citizens living in untreated polling locations were not given voter education materials or sent voter education text messages. They did not have direct contact with campaigners. In this setting, no contamination of the treatment effects to control locations would be consistent with conformity. We regressed our main outcomes (turnout and voting for the candidates at the level of the polling location) on distance to closest treatment polling location or on a binary variable for treatment in the same district (distinguishing by treatment), while employing observations from untreated polling locations only.⁴⁹ We find clear evidence of positive contamination for all treatments. This suggests that our results are not driven by conformity bias and that, in fact, it is possible that some of our treatment effects are underestimated.

IX. Conclusion

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

increasing the demand for political accountability and decreasing the prevalence of electoral problems. Probably because of its official sponsorship, the civic education increased voting for the incumbent. The civic education improved and the hotline diminished perceptions about electoral problems.

Specifically for those interested in the use of mobile phones to deliver GOTV messages, we have shown that an intervention based on door-to-door distribution of a leaflet and following up with GOTV SMS (the civic education treatment) achieved an increase in turnout of 5 percentage points in treated polling locations. This is higher than the GOTV campaign using SMS reported by Dale and Strauss (2009) for the United States (which found an increase in turnout by 3 percentage points). While we do not identify first reactions to the leaflets in terms of intention to turn out to vote in the baseline survey, we cannot fully disentangle the effect of the leaflet. At the same time, it is possible that the effect on the targeted individuals (who received the SMS) is larger. Given the potential negligible cost of sending civic education SMS, this is encouraging evidence that inexpensive voter education can produce relevant increases in voter participation in developing countries.

In a moment when 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 learned 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 political accountability, may be an effective tool to improve the political incentives for development. In designing voter education, this paper has shown that the use of information and communication technologies, recently available and expanding in the African context, as well as of social enterprise innovations like free newspapers, may open new and effective avenues for long-term building of a more relevant citizenry.

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⁴⁸ These results are shown in the online appendix to this paper. We also employed multiple imputation by chained equations as an alternative robustness exercise for panel attrition (refer to the online appendix). This method assumes data are missing at random. When considering the treatment effects in our main analysis, most results are maintained when employing multiple imputation, even though we generally find lower point estimates, with lower levels of statistical significance.

⁴⁹ These results are included in the online appendix.

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