Votes and Violence: Evidence from a Field Experiment in Nigeria*

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Abstract

Elections are now common in low-income societies. However, they are frequently flawed. We investigate a Nigerian election seriously marred by violence. We designed and conducted a nationwide field experiment based on anti-violence campaigning. The campaign appealed to collective action through electoral participation, and worked through town meetings, popular theaters, and door-to-door distribution of materials. We find that the campaign reduced the intensity of violence, as measured by independent sources. We also observe an increase on voter turnout, and infer that the intimidation was dissociated from incumbents. These effects are accompanied by improved perceptions of security and empowerment to counteract violence.

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This election is a do-or-die affair.
President Olusegun Obasanjo, February 10, 2007

Citizens are generally expected to use their vote or even lobby (Becker, 1983) to further their interests. However, there may be imperfections. Besley (2006) reviews the consequences if voters have poor information about government performance. As information deteriorates, a point is reached beyond which those potential politicians who may be ill-motivated are not disciplined by the fear of losing votes and enter politics. These politicians may act in very dysfunctional ways. However, in most electoral settings analyzed in the literature, the strategies open to candidates remain confined to those prevailing in the mature, high-income democracies: mainly strategies oriented to please ordinary citizens are considered.[1] In many of the newly democratic, low-income countries the only aspect of democracy that has been introduced is elections. There are neither checks-and-balances upon the use of power, nor effective regulations for the conduct of the election itself. We contend that these elections warrant close attention.

The record of democratization has been particularly dismal in Africa.[2] Kudamatsu (2011) measures government performance by infant mortality and shows that, in Africa, elections produced no improvement except in the rare instances in which the incumbent was defeated. Other recent empirical work asserts that in developing countries elections only discipline economic policy conditional upon being well conducted (Chauvet and Collier, 2009), and that for low levels of development democracy significantly increases proneness to civil conflict (Collier and Rohner, 2008). Indeed, Africa has seen a particularly pronounced emergence of political intimidation and violence during election time. Just to focus on recent years, Kenya, Nigeria, and Zimbabwe provide examples of elections marked by thousands of deaths. We believe that by studying malfeasant electoral strategies like violence, and ways to counter them, we may begin to improve our understanding of ways to improve electoral conduct as a means of increasing political accountability in Africa.

The main contribution of this paper is to present evidence from a field experiment we designed and conducted to establish the causal effects of community campaigning against electoral

[1] Skaperdas and Grofman (1995) is a notable early exception.
[2] Note that until the 1990s the predominant African political system was autocracy. As Besley and Kudamatsu (2008) show, while in some contexts autocracy has produced good economic performance, in Africa it has consistently been dysfunctional.

violence. The context for our analysis is the 2007 Nigerian national and state-level elections: this proved to be an all-too-suitable context for our purposes as during the two days of these elections over 300 people were killed. The campaign against political violence that we study in this paper was randomized across neighborhoods and villages of six states of Nigeria. These states represent the main socio-economic regions of the country. The campaign was conducted in half of those locations before the 2007 elections by a major international NGO, ActionAid, which specializes in community participatory development. It included town meetings, popular theater, and the distribution of campaign material, standardized across all locations. It was aimed at empowering citizens to counteract local violence. Its activities were designed to reduce the costs of protest and collective action through electoral participation. In a complementary manner, it appealed to "voting against violent politicians."

Our measurement was based on the compilation of violence-event diaries by independent local journalists in the treatment and control areas of the experiment, on representative surveys, and on a behavioral measure of empowerment to counteract violence (the sense that common citizens have ways to oppose intimidation). A panel of survey respondents was interviewed both before the anti-violence campaign and after the elections, and constituted the primary focus of campaigners after the baseline. The interviews elicited a wide range of measurements of experience with, and perceptions of, violence. Subjects were also asked to report their voting behavior after the elections. We complement our survey measures of empowerment to counteract violence with an incentive-compatible (cost-inducing) behavioral measure: we asked all survey respondents to mail a postcard if they wanted to flag the problem of electoral violence in the media. We also drew 300 additional respondents at the post-election stage, in treated areas only, who were not directly approached by the campaign. This group allows the quantification of the average effects of the campaign on untargeted individuals within treated locations.

Our empirical results document a clear decrease on actual violence as reported in the journalists' diaries. Namely, we observe a 47 percent effect on the likelihood that physical violence occurs. This is evidence that the campaign was able to influence the behavior of violent politicians. We also find effects on likely mediators, namely in terms of electoral behavior, perceptions of violence, and empowerment. We found that the intervention increased voter turnout by 11 percent (gubernatorial elections), and that political intimidation was a strategy predominantly linked to non-incumbent political groups (as reflected by the impact of the campaign on vote choices). Moreover, we present evidence that the anti-violence campaign was able to increase the sense of

security to the general population, and boost empowerment to counteract electoral violence. We find spillover effects on untargeted individuals, specifically in terms of decreased perceptions of violence.

To the best of our knowledge this is the first experimental paper dedicated to analyze electoral violence. It is also one of the few papers in the emerging literature applying field experiments to the study of elections in the developing world[3]. This literature began with Wantchekon (2003), who studied clientelism in Benin through the randomization of actual political campaign platforms. Vicente (2007) analyzed a voter education campaign focusing on vote-buying in Sao Tome and Principe. Wantchekon (2009), Banerjee, Green, Green, and Pande (2010), and Banerjee, Kumar, Pande, and Su (2010) explore interventions providing voters with specific information about public-policy options, priming against ethnic voting and corruption, and offering information about politician performance and qualifications (respectively). Finally, some work has been devoted to evaluating awareness campaigns focusing on the means, namely boosting electoral participation through cell phones (Aker et al, 2010) and social networks (Gine and Mansuri, 2011).

Not many other studies are dedicated to understanding political violence in the developing world. In terms of theory, Ellman and Wantchekon (2000) provide a model where an incumbent, while controlling more violence resources (e.g., army), can more effectively use the threat of violence to influence votes. Chaturvedi (2005) and Collier and Vicente (2011), consistently with the findings in this paper, provide models where the use of political violence by a party decreases with its electoral support. Empirically, Wilkinson (2004) provides a thorough study of political violence in India. This author emphasized the idea that violence may be an important political strategy in the face of ethnic divisions. A recent paper by Chaves et al. (2009) looked at the 1922 presidential election in Colombia in order to estimate the correlations between the illicit electoral strategies of ballot fraud and coercion, and the presence of the state and of the clergy, as well as land inequality. Like us, they found support for the claim that coercion was used to prevent opposed voters from participating in the election.

[3] Experimental methods have been applied in the context of American elections. Namely, there is a large literature testing the impact of conventional election techniques, such as canvassing, phone calls, and direct mail (see for instance Gerber and Green, 2000, Gerber, 2004, and Nickerson, 2007).

In Section I we describe the Nigerian context. Section II discusses the design of the experiment, including details of the treatment, measurement, and estimation strategy. Section III presents descriptive statistics, shows the experimental results regarding actual violence, voter behavior, violence-related individual outcomes (including the behavioral measure of empowerment), and the spillover effects within treatment locations. Section IV concludes with some implications for future research and policy.

I. Background: The 2007 Nigerian Election

Nigeria is the most populous country in Africa, with an estimated 148 million inhabitants in 2007.[4] Despite being a major oil producer, with the 10th largest oil reserves in the world (35 billion barrels)[5], it ranks 150 in 190 countries in terms of GDP per capita, with 1979 USD PPP in 2007.[6] As implied by this failure to harness oil revenues for growth, the quality of governance has been low: in Transparency International's Corruption Perception Index it ranks 147 of 179 countries (2007).[7]

From 1999, with the passing of a new federal constitution, Nigeria moved to civilian rule[8] under democratic elections: these happened in 1999, 2003, and 2007. However, all of these elections were damaged by widespread electoral malfeasance. By many accounts these elections were far from being "free and fair" by any international standards.

The election of 2007, which is the focus of our study, covered four distinct contests: presidential; federal house of representatives and senate; gubernatorial; and state assembly. Under Nigeria's federal constitution, political power is particularly concentrated in the president and the state governors. The 2007 election was not contested by the incumbent president, Olusegun Obasanjo, due to a term limit. The key contestants were Umaru Yar'Adua, Muhammadu Buhari, and Atiku Abubakar. Yar'Adua was Obasanjo's chosen successor in the ruling PDP. However, he was little known because until June 2006 Obasanjo had been hoping to change the constitution to allow him a third term in office. Buhari had already been the main challenger in the 2003 election, and was standing for the All Nigeria Peoples Party (ANPP). A previous military ruler, his past regime

[4] World Development Indicators, 2009.
[5] Oil & Gas Journal, 103(47), December 19, 2005.
[6] World Development Indicators, 2009.
[7] See Smith (2007), for a thorough account of corruption in Nigeria.
[8] See Maier (2000) for a description of this transfer of power and recent political history of Nigeria.

had been noted for a public campaign against corruption. Abubakar, although the incumbent vice-president, was in serious conflict with President Obasanjo, and had been forced to switch party to the Action Congress (AC). Previously a customs officer with controversial sources of wealth, he had been indicted by the federal anti-corruption commission (EFCC) on multiple charges related to campaign fund embezzlement and bribery. At the core of the election campaign were the headlines surrounding the possible impeachment of Vice-President Abubakar, which would have debarred him from running for the presidency.

The ruling PDP duly won the election with 70 percent of votes, as did 28 of its candidates in the 36 gubernatorial elections. However, the election was deeply flawed through violence, vote-miscounting, and bribery. As illustration, we present the assessments of three well-informed independent organizations. These features make the 2007 elections well-suited for a study of electoral violence.

> *Nigeria's elections were not credible and fell far short of basic international standards. […] Elections for president, state governors and legislators were marred by violence, poor organisation, lack of transparency, significant evidence of fraud, voter disenfranchisement and bias.* European Union Electoral Observation Mission.

> *Rigging, violence and intimidation were so pervasive and on such naked display that they made a mockery of the electoral process. […] Where voting did take place, many voters stayed away from the polls. […] By the time voting ended, the body count had surpassed 300.* Human Rights Watch.

> *The irregularities were so numerous and so far-reaching that the election was a charade and did not meet the standards required for democratic elections.* Transition Monitoring Group (an NGO which deployed 50,000 Nigerian observers to the 2007 elections).

II. Experimental Design

Our experiment was based on community campaigning aimed at undermining electoral intimidation and violence. At the center of this paper is the measurement of the consequences of

the campaign on real violence and electoral behavior. We also document effects on individual perceptions of violence and empowerment using survey outcomes and a behavioral measure.

A. The Intervention

We collaborated with the Nigeria chapter of ActionAid, ActionAid International Nigeria (AAIN; http://www.actionaid-nigeria.org/), which regarded the prospect of political violence as a grave challenge to democracy and wished to counter it. AAIN's input in designing a campaign against electoral violence drew on its expertise in community participatory development and its experienced field infrastructure.[9]

The campaign was designed to induce experimental subjects to oppose voter intimidation. The main mechanism employed was to lower the perceived threat to individual voters through collective action. The analytic foundation for this method is the model of political protest of Kuran (1989). There, people who dislike their government may hide their desire for change as long as the opposition seems weak. In this context, a solid government may see its support crumble by a slight surge in the opposition's apparent size, caused by potentially insignificant events like a public call for protest. This mechanism is exemplified by McMillan and Zoido (2004), who describe the fall of an intimidation-based regime in Peru due to the release of a videotape.[10] AAIN's campaign was analogous as a public call for protest. In addition to trying to lower the perceived threat to individual voters through collective action, the campaign also emphasized the lack of legitimacy in the use of intimidation. We therefore expect that the campaign increases voter turnout and causes supporters of violent candidates to change their electoral preferences away from those candidates. All these expected impacts of AAIN's campaign imply a reduction in the effectiveness of violence and intimidation as an electoral strategy. In that view the ultimate test of the effectiveness of AAIN's campaign is whether we observe a decline in actual violence and intimidation instigated by politicians, despite the fact that they are not likely to have been directly treated by the campaigners.

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[9] AAIN is Nigeria's chapter of global ActionAid, headquartered in South Africa, with total budget USD 133m in 2005, and specializing on Community Improvement and Capacity Building – it was ranked 20th worldwide in terms of "performance" in a recent list compiled by Financial Times/Dalberg of global organizations devoted to philanthropy (above Transparency International, UNDP, and Amnesty International) - see the Financial Times, July 5, 2007, Report on "Corporate Citizenship and Philanthropy".

[10] This idea also relates to the theory of informational cascades by Bikhchandani et al. (1992) and Lohmann (1994), which was proposed to explain the behavior of masses.

The anti-violence campaign reached a set of enumeration areas, i.e., neighborhoods or villages. It was implemented during a two-week period approximately two months before the election, just after our baseline panel survey. For this campaign, AAIN worked with local state-level partner NGOs, who conducted the campaign activities in the field.[11] Because each state was allocated a different team of campaign fieldworkers, campaign activities were fully contemporaneous in all states.[12]

The campaign consisted of a clear message against electoral violence, as embedded in its main slogan: "No to political violence! Vote against violent politicians." The main guidelines of the campaign were discussed with the authors, the central AAIN officers, and the local partnering NGO representatives. Actual material and activity design was undertaken with the help of a specialized firm in Abuja. The campaign slogan was written on a wide range and large quantity of distributed campaign materials: T-shirts (3,000, i.e., on average one for every two households in treated locations), caps (3,000), hijabs for Muslim women (1,000), leaflets (5,000), posters (3,000), and stickers (3,000) – images for the leaflets, posters, and stickers are displayed in Figure 1. Note that these means of campaigning are the ones primarily chosen by politicians in Nigeria to licitly spread awareness about their candidacies. The campaign also included roadshows which featured jingles in Yoruba, Hausa, and Pidgin English.

<Figure 1 near here>

However, the campaign was designed to work mainly through the holding of town meetings and popular theater. The town meetings provided an opportunity for the grassroots to meet with local representatives to discuss ways of counteracting politically motivated violence. In line with the theory, meetings were designed to minimize the collective action problem that impedes diminishing conflict at the local level. Popular theater was based on the same script for all states (featuring one good and one bad politician, with the bad one instilling violent intimidation), and was designed to target youths (usually the ones providing labor for violent activities) and others (e.g., women) who were relatively difficult to attract to town meetings. There was at least one town meeting and one popular theater per treatment location.

[11] The authors of the paper witnessed operations in the field in four out of the six states included in this campaign.
[12] A comprehensive report of the campaign, including photographs, films, and reports for each state's campaign activities, is available from http://www.iig.ox.ac.uk/research/08-political-violence-nigeria/.

The campaigners were instructed to target the panel respondents to our surveys (i.e., baseline subjects),[13] not only in terms of distribution of materials, but also in terms of invitations to attend the campaign events, i.e., the town meetings and the popular theater. We gathered data showing that 47 percent of the panel households were represented at one of the campaign events. The panel households that attended the community meetings and popular theater were not statistically different from the remaining panel households in terms of demographic characteristics, except for some ethnic groups and income (which was lower for the attending households). Moreover, a large majority of the panel individuals recalled well the activities of the campaign during our post-election survey: 88/89/86/84 percent remembered well the distribution of materials, the roadshows, the town meetings, and the popular theater (respectively). Note however that the activities of the campaign may have reached other individuals beyond our panel households. This is natural to have happened with the roadshows, which were meant to raise general awareness about the campaign at the local level. But it is also possible to have occurred through the distribution of materials, on demand by passers-by, as sometimes it was carried out on the street with some visibility. However, the town meetings and popular theater were publicized through the less evident panel invitations and were held at specific venues, deeming small the likelihood that other individuals attended those events.

B. Sampling

Our field experiment included 24 locations/enumeration areas. These were chosen from Afrobarometer's (http://www.afrobarometer.org/) representative sample of enumeration areas in all 36 states of Nigeria, which was drawn for their 2007 pre-election survey. Their sample framework was the population census, with census enumeration areas and corresponding population weights. The Afrobarometer sample included 301 enumeration areas.

Our sampling has a non-random component as we chose two states in each of the three main regions of the country (Southwest, Southeast, and North), by looking at the recent history of politically motivated violence.[14] This process led to choosing Lagos and Oyo (Southwest), Delta and Rivers (Southeast), and Kaduna and Plateau (North). This option revealed our emphasis on

[13] One campaign representative accompanied the survey team during the baseline survey exclusively for site identification, and respondent addresses were shared with the AAIN at that stage. The surveys and the campaign were fully independent, with distinct field teams and branding.

[14] We used reports by Human Rights Watch, ActionAid International, and other independent sources. See for instance Human Rights Watch, "Testing Democracy: Political Violence in Nigeria," April 2003, "Nigeria's 2003 Elections: the Unacknowledged Violence," June 2004.

studying violence while keeping the basic diversity of the country borrowed from the ethnic predominance of Yoruba in the Southwest, Igbo in the Southeast, and Fulani/Hausa in the North.

The remainder of the sampling process was statistically representative. We began by organizing all the enumeration areas in the Afrobarometer's list (in each of the six states selected) by pairs. We paired enumeration areas by identifying closest enumeration areas that were of the same type in terms of the classification "large urban," "small urban," and "rural" (stemming from the census). We then randomly chose 12 pairs (two in each state) and randomly selected one of the enumeration areas in each of these pairs to be treated. This process led to selecting the areas shown in Figure 2.

<Figure 2 near here>

The households/subjects that were the targets of the campaign, i.e., the ones primarily offered the campaign materials and the ones invited to town meetings and popular theater, were chosen in a statistically representative manner at the location level during our baseline survey. That survey was performed jointly with Afrobarometer and Nigerian partner Practical Sampling International (PSI) during the period January 20 to February 3, 2007. At that time, households within a census area were chosen randomly using Afrobarometer's standard techniques (by choosing the *n*th house). We reached 1,200 households during the baseline survey, 50 per enumeration area.

C. Measurement

Our impact measurement depended on three sources of information: violence journals at the level of the enumeration area, individual survey-based measurements, and a behavioral measure of empowerment.

First, we contracted independent local journalists (one per enumeration area) to report/describe in a diary violent events that affected the neighborhood or village, through direct witnessing and through consultation with local bodies (town meetings, police) – 131 events were identified in total, in the period before and after ActionAid's campaign. We coded each event according to its characteristics. The journalists in charge of the violence diaries collected information on violence from the second semester of 2006 and until two weeks after the last April election-day. The journalists' data serve the purpose of enabling identification of the effects of the campaign on the

behavior of the instigators/perpetrators of violence, i.e., the ultimate test of the impact of a campaign whose immediate objective was to reduce the effectiveness of violence through the perceptions and behavior of the potential victims.

Second, we conducted a panel survey in the 24 enumeration areas, with rounds prior to the campaign (as mentioned) and after the election when results were known and post-election stability was achieved. In each household, we interviewed one representative member 18 years of age or older. The post-election survey, conducted with PSI, resurveyed 1,149 panel respondents (96 percent of the baseline subjects) in the period May 22 to June 5, 2007. The surveys were designed to elicit evidence on individual voter behavior[15] and measures (e.g., perceptions) related to violence.[16] The questions on violence were generally asked both prior to the campaign, focusing on a reference period ("the last year"), and after the campaign/elections, focusing on what had happened just before and during the elections (i.e., "from January," when the baseline survey was in the field). The majority of these questions featured a subjective scale.

During the post-election survey, we gathered a fresh sample of 300 respondents in treated enumeration areas (25 per enumeration area), what we refer to as the treatment post-election oversample. The only additional requirement for this group's sampling procedure was that they were "not directly approached" by the campaign team (in principle this criterion leaves in all but the panel households). These data allowed estimating the effect of the treatment on the untargeted individuals in treated locations, i.e., the spillover of the campaign within treated locations.

Finally, we note the specific behavioral measurement that was implemented during our post-election survey. We created an incentive-compatible individual measure of empowerment to counteract violence, which we refer to as the "postcard" variable. It was based on an observable and costly action against violence which was proposed to all respondents in our survey. All were given a prestamped postcard which they could choose whether to mail or not– the main side of

[15] On electoral behavior, disaggregated official 2007 electoral results have been to date completely unavailable. Indeed, we have reports that it is unlikely that they exist for many of our experimental locations. Results were announced in terms of the overall totals in a process that appears to have bypassed the need to aggregate actual votes. In many locations, due to massive ballot fraud, our post-election survey may provide a better approximation of the will of the voters than any official results. Note that Vicente (2007) was able to contrast self-reports to disaggregated electoral results in Sao Tome and Principe's 2006 presidential election, without significant differences encountered. Although different, Nigeria and Sao Tome and Principe are neighboring countries, which gives us some assurance that self-reports may be an adequate source of voting data in that region of Africa.

[16] All questionnaires are available upon request to the authors.

the postcard is shown in Figure 3. On the card there was a message demanding that more attention be paid to countering voter intimidation in the subject's state. The postcard was addressed to the organizations involved in the experiment, which promised to raise media awareness about the problem in states where enough postcards were sent. Because in order to post the card the respondent had to make the effort of going to a post office, we have a clear, implied costly action (which we were able to record individually through numbering the postcards and matching with survey respondents). The respondent was more likely to incur this cost the stronger was his/her sense that intimidation could be countered.

<Figure 3 near here>

D. Estimation Strategy

Our empirical approach is based on reduced form specifications. We estimate the effects of the intervention on actual violence (as measured by the journals), individual voting behavior (as measured in our survey), violence-related individual outcomes (as measured in our survey), and the behavioral measure of empowerment against violence. We now describe the main econometric specifications we employed, exemplifying with survey data at the individual level.

We are interested in investigating the effect of the anti-violence campaign on voting behavior outcomes and outcomes related to violence, as measured in our survey. Our design allowed us to estimate average treatment effects in different ways. Most simply, the effect of interest (f) could be estimated through the specification:

$$Outcome_{il1} = a + fT_l + \varepsilon_{il1}, \qquad (1)$$

where $Outcome$ is a voting behavior or violence-related outcome, $i,l,t = 1$ are identifiers for individuals, locations, and time (specifically, 1 represents the post-election measurement), and T_l is a dummy variable with value 1 for treated locations.

In this setting, because of small sample size, we can also add location and individual-level control variables to compose one of our main specifications. This is in line with Duflo et al. (2007), who argued that even though controls do not generally change the estimate for the average treatment

effect, they usually help to explain the dependent variable, and therefore typically lower the standard error of the coefficient of interest. We then have the following specification:

$$Outcome_{il1} = a + bY_l + cX_i + fT_l + \varepsilon_{il1}, \quad (2)$$

where Y_l is a location-level vector of controls, and X_i is a vector of individual demographic controls.

Specification (2) does not use the time dimension. In any event this may not be possible in some cases as we do not have repeated measurement for all outcomes. This is the case of actual voting behavior in the April election which is only available for the post-election survey. However, when possible, it may be relevant to control for differing pre-intervention levels of the outcome across treatment and control groups. In this case, specification (3) below uses the pre-intervention data in a classic difference-in-differences regression:

$$Outcome_{ilt} = a + bY_l + cX_i + dt + eT_l + ft * T_l + \varepsilon_{ilt}, \quad (3)$$

where $t = 0$ before the intervention and $t = 1$ after the anti-violence campaign.

For transparency and ease of interpretation, we run OLS regressions for all estimations in this paper.

Since the data we use is clustered by enumeration area, we allow for within-group dependence in estimating standard errors of treatment effects by estimating cluster-robust standard errors through the use of the Huber-White variance estimator (see Moulton, 1990, for a defense of the use of corrected standard errors). Note however that a practical limitation of inference with cluster-robust standard errors is that the asymptotic justification assumes that the number of clusters goes to infinity. Bertrand et al (2004) show that with a small number of clusters (smaller than 50) the cluster-robust standard errors are likely to be downward biased. In our case we have 24 clusters, which is indeed a small number.

Two solutions were proposed to solve this problem, namely in calculating p-values of treatment effects that account for a small number of clusters. We use both methods in our analysis below.

First, we employ the wild bootstrap approach proposed by Cameron et al (2008). Second we use the randomization inference approach discussed by Rosenbaum (2002) and recommended by Duflo et al (2007). See Bhushan et al (2007) for a recent application of randomization inference.

Cameron et al (2008) recommend continuing to use the standard OLS estimator with the cluster-robust (Huber-White) variance estimator. However, they prescribe bootstrapping to obtain bootstrap critical values that provide an asymptotic refinement when there are few clusters. Bootstrap methods generate a number of pseudo-samples from the original sample; for each pseudo-sample they calculate the treatment effect; and use the distribution of the treatment effect across pseudo-samples to infer the distribution of the actual treatment effect. Wild bootstrap uses the fact that we are assuming additive errors and holds regressors constant across the pseudo-samples, while resampling the residuals at the level of the cluster, which are then used to construct new values of the dependent variable. Note that Cameron et al (2008) advise that Rademacher weights (+1 with probability 0.5 and -1 with probability 0.5) are used when resampling residuals, and that the null hypothesis of zero treatment effect is imposed. We follow both recommendations.

Randomization inference involves generating placebo random assignment of the treatment to clusters, and estimating the associated treatment effects for hypothesis testing. This method then takes into account the specific randomization procedure that was used. Following our treatment assignment structure composed of 12 pairs of enumeration areas, we have 4096 unique random assignments, which are all equally likely to occur and define our specific placebo assignments. We perform hypothesis testing by checking whether the actual measured treatment effect is in the tails of the distribution of the placebo treatment effects. Since the placebo assignments only vary across clusters, this method takes intra-cluster correlations into consideration. According to Bhushan et al (2007), the drawback of hypothesis testing based on randomization inference is that it has low power relative to more parametric approaches when the true effect is large because it puts not even minimal structure on the error term. We therefore take this method as a conservative one.

A final note goes to survey-based measures of violence-related outcomes. We follow Kling et al (2007) in that we normalize 17 survey-based measures using z-scores, and aggregate them in four indices using equally weighted averages of the normalized individual variables. Table 1 displays all individual variables with original scales, as well as the four groups. Note that the

normalization also changed the sign of each measure so that more beneficial outcomes (less violence, more empowerment) have higher scores. According to Kling et al (2007), this aggregation improves statistical power to detect effects that go in the same direction within a domain. The z-scores are calculated by subtracting the control group mean and dividing by the control group standard deviation. Thus, each component of the index has mean 0 and standard deviation 1 for the control group.[17]

<Table 1 near here>

III. Econometric Results

In this section we begin by displaying randomization tests alongside descriptive statistics. We then turn to our core analysis: the effect of the campaign on actual violent events, voting outcomes, violence-related perceptions, and postcard. We then assess the effects of the campaign on the individuals that were not targeted by the intervention in treated locations.

A. Balance

We begin by evaluating whether the randomized selection of treated locations was successful in identifying comparable treatment and control groups. We document differences across these groups in terms of a wide range of observable initial characteristics. In Table 2 we contrast treatment and control groups in terms of location characteristics, individual demographics of our survey respondents, and baseline outcomes (actual violence, individual electoral preferences for the 2003 elections, and individual survey-based violence using the indices of Table 1 when available for the baseline). Because all these variables are unaffected by the intervention, and given our treatment assignment criteria, any differences between treatment and control locations should be understood as a product of chance.

<Table 2 near here>

[17] 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 for the corresponding time period.

We find no statistically significant differences (at standard levels) between treatment and control groups for the location-level variables. This is also the case for survey-based variables. Overall this is evidence that the randomization was effective in isolating similar groups of locations and respondents. The fact that observables are balanced across treatment and control makes us hope that unobservable dimensions are balanced as well. Note that the first part of Table 1 provides complete descriptive statistics for our sample of locations and respondents. Finally, panel attrition is found not to be statistically different across treatment and control locations.

B. Actual Violence

We now analyze the effects of the treatment on the intensity and incidence of violent events as reported by the independent local journalists at each experimental location. These reports were based on information gathered from direct observation and local institutions such as police and town meetings.

There were 131 violent events in total that were recorded in the journalists' diaries across all experimental locations. Each violent event was classified using a 1-5 scale, from lowest to highest seriousness. This scale uses the following objective thresholds: 5, occurrences resulting in more than five dead people; 4, occurrences resulting in dead people (although less than five casualties in total); 3, occurrences resulting in physically wounded people; 2, occurrences leading to severe intimidation; 1, petty-crime occurrences leading to intimidation. Although we use the full scale, we also consider the simple binary classification of whether physical violence has occurred (i.e., attributing value 1 to occurrences coded 3-5 and value 0 otherwise).

For intensity we average the seriousness of the occurrences per location. We measure incidence as the number of occurrences per location. We plot these variables in Figure 4 (as measured after the campaign) against our treatment. Despite the low number of observations, we find a statistically significant effect of the treatment (at the 10 percent level) on decreasing intensity of violence, but we find no effect on incidence.

<Figure 4 near here>

In Table 3 we run regressions of violence intensity taking the violent event as the unit of analysis (we cannot use that level to study incidence), which allow for the use of state dummies[18] and location controls (see first panel of Table 2a). Note that we would not be able to add controls to regressions at the level of the enumeration area given the low number of observations at that level. In order to use this degree of disaggregation to study intensity, we weigh observations so as to attribute the same importance to each experimental location in the sample. We present in Table 3 regressions for physical violence (0-1) and for the full violence intensity scale (1-5). We first display single-difference estimations (with state dummies only), in line with specification (2) above. We then show results using difference-in-differences, without any controls, with state dummies only, and with location controls (these are described in the first panel of Table 2a) added to state dummies.

We find a 47 percent reduction in the intensity of electoral violence when using the binary classification of physical violence. This effect is statistically significant at the 5 percent level using all three inference methods (Huber-White, wild bootstrap, and randomization inference). This is a very robust effect, as it displays statistical significance from the least powerful specification. We also find a significant negative effect when using the full violence intensity scale (14 percent of the 1-5 scale). This is significant at the 10 percent level when using state dummies and location controls for all three inference methods. The estimated impact on intensity constitutes evidence that there was an effect of the treatment on the instigators/perpetrators of actual violence, ultimately politicians. Indeed, the likely reduction in the effectiveness of intimidation (the direct aim of the campaign) was able to lead to a reduction in the actual level of electoral violence as politicians adjusted their strategies.

<Table 3 near here>

C. Voting Behavior

We now focus on the effects of the anti-violence campaign on the electoral behavior of the panel of respondents, both in terms of voter turnout and voting for specific candidates/parties. Our results are displayed in Table 4. The focus is on the estimation of treatment effects by using single-difference regressions employing post-election survey data on reported voting behavior in

[18] The state dummies represent not only the obvious state-level heterogeneity but also allow controlling for the fact that AAIN's campaign was conducted by a different team of fieldworkers in each state.

the April elections. For each outcome, we begin by showing the single-difference regression with state dummies only; we then display the regression with full location and individual demographic controls (specification 2). Note that location controls are described in the first panel of Table 2a, and individual demographic controls are depicted in the middle panel of Table 2a. We focus on voter turnout and voting patterns in the presidential and gubernatorial elections, these being the elections where the stakes were highest, i.e., where the executive powers are concentrated in Nigeria.

We begin by observing the effect of the treatment on voter turnout. The first main purpose of the campaign was to lower the threat of intimidation through a call for electoral participation. This implied persuading people who had decided not to vote because of intimidation to participate in the election after all. We may therefore interpret a turnout effect of the campaign as being qualitatively the opposite of the effect of electoral violence itself. We find that the proportion of registered voters who voted was 7 and 11 percent larger in the treated group than in the control group for the presidential and gubernatorial contests (respectively). The effect on turnout at the gubernatorial election is statistically significant at the 1 percent level for cluster-robust standard errors and randomization inference; it is significant at the 10 percent level for wild bootstrap. For the presidential race, this effect is only significant when using the cluster-robust standard errors and when using randomization inference (both at the 5 percent level). The larger size and higher significance of the estimate concerning the gubernatorial elections indicate that political violence may be more closely associated with local contests – indeed, a large share of the Nigerian oil revenues is channeled to state-level budgets managed by governors.[19] We can conclude in favor of a clear effect of the AAIN campaign on voter turnout, which allows inferring that electoral violence was an effective strategy in keeping voters away from the polls.

<Table 4 near here>

We now turn to the effects of the anti-violence campaign on each candidate/party's score. The second main purpose of the campaign was to emphasize the lack of legitimacy in the use of intimidation by politicians. This implied persuading voters to vote against those candidates/parties they identified as violent.

[19] The 1999 Nigerian Constitution defines at 44 percent the percentage of oil revenues accruing to states and local governments.

We find that in the presidential election the campaign increased the vote for the PDP candidate by 8 percent and reduced the vote for the AC candidate by 7 percent. The first is significant at the 1 percent level using the cluster-robust standard errors, 10 percent level using wild bootstrap, and 5 percent level using randomization inference (note that wild bootstrap and randomization inference do not display significant levels for the specification without controls), and the second is significant at the 1 percent level using the cluster-robust standard errors and 5 percent level randomization inference (note that randomization inference does not display a significant level for the specification without controls). We also observe a positive effect on voting for ANPP, which nevertheless is only significant when using controls, for cluster-robust (at the 5 percent level) and randomization inference (at the 10 percent level). We should recall that the AC presidential candidate was portrayed in the media as espousing instability. The reduced vote for the AC candidate constitutes some evidence that the complementary objective of the campaign embodied in the slogan "vote against violent politicians" also seems to have worked; people who were expected to support Abubakar decided to punish that candidate by not voting for him. Yar'Adua seemed to benefit the most from these vote changes.

Concerning the gubernatorial elections, the campaign increased very clearly the vote of the incumbent (i.e., PDP in all six but one state) by 13 percent, an effect significant at the 1 (cluster-robust standard errors) and 10 (wild bootstrap and randomization inference) percent levels, and robust to the exclusion of location and individual controls (with the exception of statistical significance provided by randomization inference). We do not find significant effects for the score of the second and the third parties.

In view of this pattern of results, i.e., incumbents as clear beneficiaries of the treatment, and after the evidence gathered for the presidential elections, we suggest that violence may be a strategy of weaker political groups.[20] This is consistent with the idea that the incumbent may have an advantage in using other more effective illicit strategies like fraud and vote-buying when

[20] Indeed, several sources point to the importance of electoral violence by marginal groups not representing the main parties. In Oyo State, Human Rights Watch underlined the role of violent groups who contested power within PDP in primary elections but were then defeated. See Omobowale and Olutayo (2007) for a description of the Oyo political setting, centered on the figure of Chief Lamidi Adedibu. For Rivers State, the same organization underlines the activities of autonomous armed gangs, who had links to major political figures in past elections. For further details, see "Criminal Politics: Violence, 'Godfathers', and Corruption in Nigeria," October 2007. In addition, the International Foundation for Electoral Systems (IFES), who implemented nationwide surveys during the 2007 Nigerian elections, considers 40 percent of the electoral violence to be originated from purely outside the main parties, PDP, AC, and ANPP ("A Nigerian Perspective on the 2007 Presidential and Parliamentary Elections," August 2007).

needed.[21] Weak political groups may be restricted to the use of electoral intimidation of opponents to maximize their vote share. Intimidation may be viable when it does not carry a substantial electoral cost (i.e., when those groups do not hold significant popular support), analogously to terrorism. Collier and Vicente (2011) propose a general theory of electoral competition with illicit strategies that formalizes this hypothesis.

D. Violence-related Individual Measures

AAIN's campaign was aimed at lowering the perceived violent threat to individual voters by giving them a sense of empowerment. We measured individual perceptions and experience of intimidation, as well as individual feelings of empowerment to counteract violence. We report here on the effects of the treatment on those variables.

We begin by using a wide range of perception and experience variables from our surveys. As mentioned, we compose four indices with these variables (described in Table 1). The first index concerns general variables of political freedom, i.e., on "voting freely," on "being free from insecurity," and on the perceived fairness of elections, and relates to general measures of conflict at the local level "within the local community." The second index is dedicated to perceptions of politically motivated violence as induced by politicians (from the top). It includes "security from violence originated by politicians," political intimidation ("threatening negative consequences in order to induce voting in a certain way"), "influence of political assassinations on instilling a climate of fear" (frequent in the 2006 party primaries), politicians "openly advocating violence," and "violent gangs being active." Third, we isolate proxies of empowerment against violence at the bottom: "support for do-or-die affair," local populations "standing against violence originated by politicians," "empowerment," and "knowledge of ways to resist violence". The final index of survey violence measurements comes from a batch of standard questions (both perceptions and experience) on local crime. These are likely to be indirectly related to politics, through gang and political thugs' activities. The component variables are "purposely made damage to property (vandalism)" and "physical threats/intimidation". All variables mentioned are normalized as z-

[21] Ballot fraud is likely to advantage incumbents, as these candidates are more likely to control the vote-counting process. Vote-buying is also expected to benefit incumbents, as these politicians are expected to have more money available and to be more convincing in proposing clientelistic exchanges. Indeed, we find a positive correlation between competitive local races and the use of fraud and vote-buying (as reported in the working paper version of this paper, Collier and Vicente, 2008).

scores (with higher numbers referring to less violence and more empowerment) and averaged to compose the corresponding index.

We display results regarding our indices of survey perceptions and experience with violence in Table 5a. We use specifications with difference-in-differences when possible (the exception is the index of general political freedom and conflict, which has several components that only have a post-election measurement). The first specification we show for each different dependent variable only includes state dummies, with the second adding location and individual demographic controls (as in equations (2) and (3) above).

<Table 5a near here>

Overall, we found clear and statistically significant effects of the campaign on diminishing perceptions of political violence and increasing empowerment of the population against political violence. These effects are 0.39, 0.23, and 0.22 standard deviation units for general political freedom and conflict, local electoral violence from the top, and local empowerment from the bottom (respectively). These effects are strongly significant at the 1 or 5 percent levels using cluster-robust standard error inference (statistical significance is maintained when using the other methods of statistical inference we adopt in our paper). They are also robust to the exclusion of controls. These results reassure us that the campaign was able to lessen perceptions of intimidation and offer a sense of empowerment at the individual level for the general population.[22] [23] Regarding perceptions and experience with crime, we do not find a statistically significant effect. This may be due to the fact that the crime index was a general one, i.e., it was not referring directly to politically-motivated violence, the focus of AAIN's campaign.

[22] Note that households that are more marginal to local communities seem to be most responsive to the intervention. This is found by interacting the treatment with demographic characteristics (results shown in Collier and Vicente, 2008). This finding is consistent with the targeting of violence towards those marginalized groups. Because we may perceive these voters as less attached to specific political interests at the local level (e.g., clientelism), our findings are in line with Robinson and Torvik (2009) who asserted that political violence may be primarily pointed at swing voters. Moreover, we did not find a statistically significant effect on the statement that "violence is justified" (Collier and Vicente, 2008), which lends some support to the idea that most individuals (surveyed) were not involved in violent/intimidatory activities.

[23] Note that we have found a significant decrease on perceptions of police-induced problems as a result of the campaign (see Collier and Vicente, 2008), i.e., in the same direction as the political violence outcomes. This finding reassures us that the campaign does not seem to have been understood by respondents as biased in favor of the incumbent (who controls the police), as one would expect from the independent nature of the campaign sponsor (international NGO ActionAid).

We now turn to our behavioral measure of empowerment against electoral violence, i.e., the postcard variable. If respondents actually put the postcard in the mail, on average that means they hope that media awareness can help in undermining the phenomenon in their state, in line with the postcard contract that was conveyed to them during the post-election survey. Indeed, increasing the sense of empowerment to counteract intimidation was a primary objective of AAIN's campaign. Moreover, the behavioral aspect of this measure (respondents had to incur a cost to send the postcard) aims to be an improvement on equivalent survey questions which may be more vulnerable to report biases. Because we only have post-election levels for the postcard variable we use variations of specification (2) above, with state dummies only and with added location and individual controls. These results are displayed in Table 5b.

We also explicitly address the hypothesis that the postcard variable is particularly useful in skimming those cases where the respondent reports a sense of increased empowerment in the surveys. In other words, by using the postcard we want to identify those cases where the referred attitude is not just "cheap talk." However, we also worry about different interpretations of the postcard by respondents despite our efforts. For that reason, we use the information from the survey question on empowerment against violence (see Table 1) to skim erroneous interpretations of the postcard. The second set of regressions in Table 5b uses as dependent variable the postcard dummy only if the perceived change in empowerment was positive. Otherwise it takes value 0, as if the respondent had not sent the postcard.

We first note that 37 percent of the subjects returned the postcard in the mail, which implies the initiative had a remarkably high degree of adherence. Treated respondents were found to send the postcard 8 percent more frequently than their control counterparts. However statistical significance can only be observed when adding controls, at the 5 and 10 percent levels for inference based on the cluster-robust standard errors and for wild bootstrap (respectively). Statistical significance does not emerge when using randomization inference. We should therefore take the postcard result with caution. We then proceeded with the regressions of our hybrid version of empowerment. There, we find clearer treatment effects of the same size, significant at the 1 (cluster-robust and wild bootstrap) or 10 (randomization inference) percent levels with full controls, but also significant without controls when using any of the inference methods. Thus, we feel relatively confident that the anti-violence campaign was able to carry a positive change in empowerment for the targeted population.

E. Effects on Untargeted Individuals

We now evaluate the effects of the anti-violence campaign on our treatment oversample of untargeted individuals. The subjects of the oversample were approached only for the second round of the survey and were not approached by campaigners, although they may still have seen the street activities and have been generally aware (through their social network) of the campaign. We contrast in Table 6 the treatment oversample with the control individuals, regarding the relevant outcomes analyzed above at the individual level. Our general hypothesis is that the effects of the campaign may have gone beyond the panel of respondents. Table 6 displays for each outcome the same specifications used in Tables 4 and 5 when adding full controls (but using the treatment oversample instead of the treated panel individuals). It also repeats the point estimates from those tables for comparison purposes.

<Table 6 near here>

We find no significant results for voting behavior, but we encounter clear effects on violence and intimidation perceptions. The size of the estimates is comparable to the effects we have found for the panel treated respondents, namely for general political freedom and conflict, and local electoral violence from the top. These are significant typically at the 1 or 5 percent levels using all three inference methods. Note that effects on local empowerment are less clear: the survey-based index yields a significant treatment effect only for randomization inference, and the postcard does not show statistical significance. We can infer from these results that the spillovers of the campaign within treatment locations are likely to have been concentrated on perceptions of violence and intimidation, not on behavior. Fafchamps and Vicente (2010) explore different social network channels of influence that may have contributed to these effects.

IV. Conclusion

Since the fall of the Soviet Union there has been a proliferation of elections in societies with weak governance, many of them in Africa. These elections have often produced serious levels of violence. The Nigerian election of 2007 was one of the largest African elections to date, and it brought many instances of electoral violence. In this paper we have provided an in-depth analysis of community-based campaigning against electoral violence in Nigeria. We have found that the anti-violence campaign we studied decreased the intensity of real violent events, implying that the

behavior of politicians who use intimidation as an electoral strategy was influenced. We suggest that the campaign has worked through boosted voter participation and electoral penalization of candidates perceived to use intimidation (violence was dissociated from incumbents). It also led to increasing perceptions of local safety and to rising empowerment of the population.

The findings in our paper are optimistic regarding the role of community-based campaigning in counteracting electoral violence. Like in Kuran (1989), relatively insignificant but targeted events can indeed mobilize citizens to collective action. Specifically, more participation at the polls together with improved security and empowerment of the population may be mutually reinforcing, in a context where violence is associated to small political groups. Anti-violence campaigns may then constitute a particularly effective form of voter education, mainly working as a coordination mechanism, and relatively undemanding on the amount of information that is passed to voters. However, we would like to underline that future empirical research should not lose sight of the likely joint determination of the different electoral strategies of politicians. These may include other types of illicit behavior like vote-miscounting and vote-buying. In the same vein and policy-wise, an anti-violence campaign cannot be the sole remedy to problematic elections; attention should be devoted to political accountability and to all illicit strategies in an integrated manner. It is in this context that voter education, broadly construed, and electoral observation may be invaluable policy tools for the improvement of elections and democracy in the developing world.

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Figure 1: AAIN's campaign: leaflet/poster (above) and sticker (below)

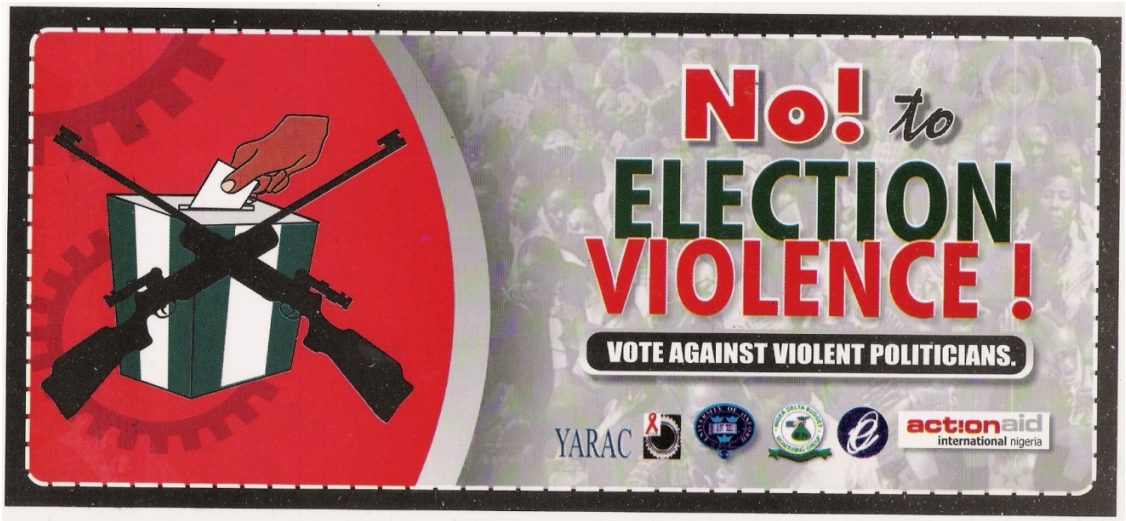
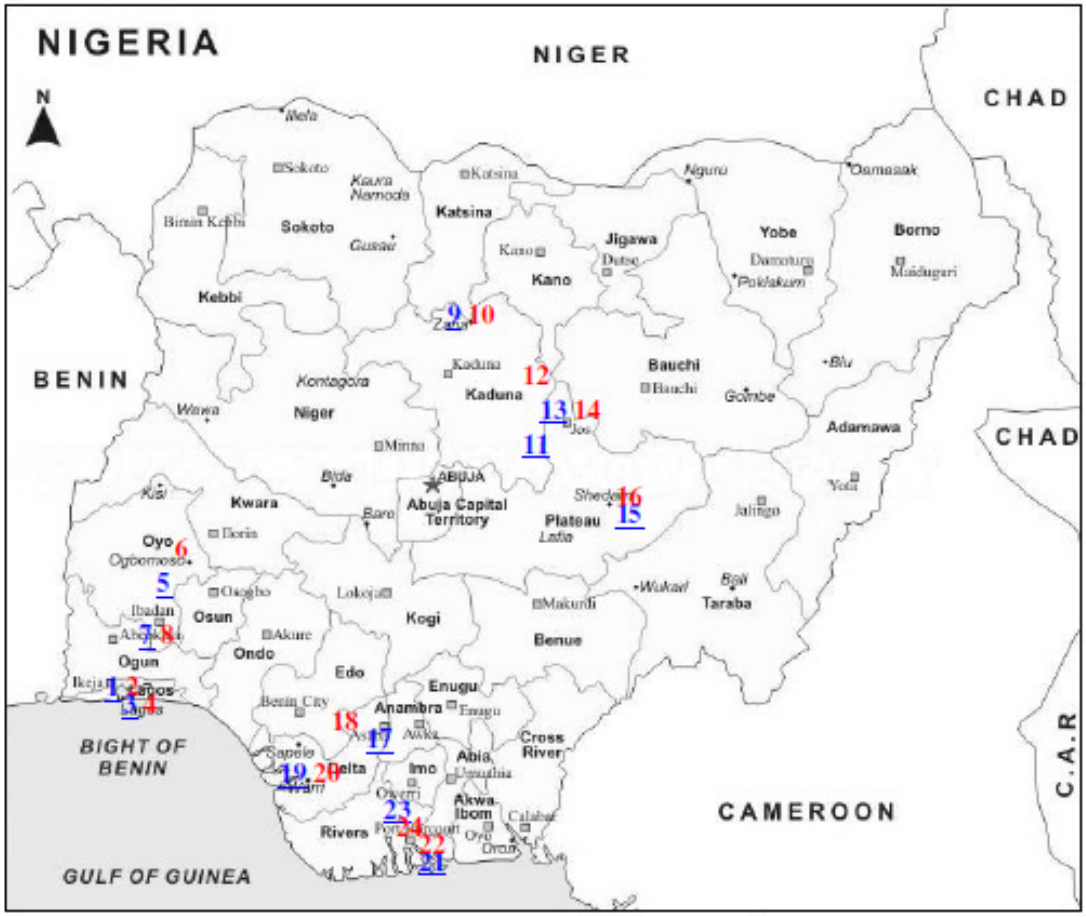


Figure 2: Sampled enumeration areas

Nigeria - Sampled Enumeration Areas



Legend: Treatment Area, Control Area; LU: Large Urban; SU: Small Urban; R: Rural

SOUTHWEST REGION	NORTH REGION	SOUTHEAST REGION
Oyo: 5. Atiba – Ajagba SU 6. Ogbomosho North – Jagun Oke. SU 7. Ibadan Southwest – Jericho LU 8. Ibadan Southwest – Ring Road LU	Kaduna: 9. Zaria – Zaria (150) LU 10. Zaria – Zaria (151) LU 11. Kaura – Amawa Tudun Wada R 12. Lere – Abadawa Laga Akwai R	Delta: 17. Oshimili North – Oko Anala R 18. Ika South – Obi Anyima R 19. Warri South – Warri (290) LU 20. Warri South – Warri (289) LU
Lagos: 1. Alimosho – Akwonjo LU 2. Alimosho – Ikotun LU 3. Lagos Mainland – Ebute Met. LU 4. Lagos Island – Lagos Island LU	Plateau: 13. Jos North – Jos (78) LU 14. Jos North – Jos (77) LU 15. Quan-Pan – Piya R 16. Quan-Pan – Pandam R	Rivers: 21. Andoni – Agama R 22. Eleme – Sime-Tai R 23. Obio/Akpor – Rukpakwolusi R 24. Gokana – Nugbe-Yeghe R

Figure 3: Postcard

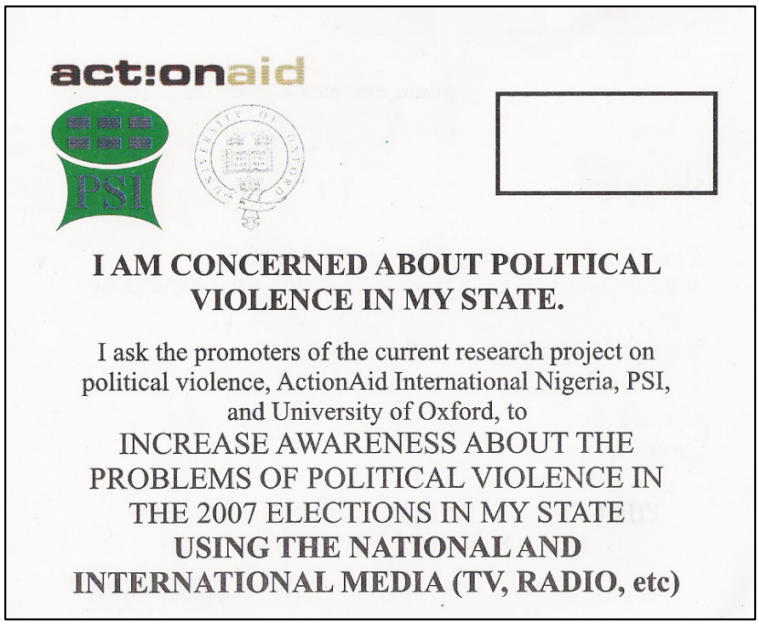


Figure 4: Intensity and incidence of violence from journals vs. treatment (averages per location, post-campaign)

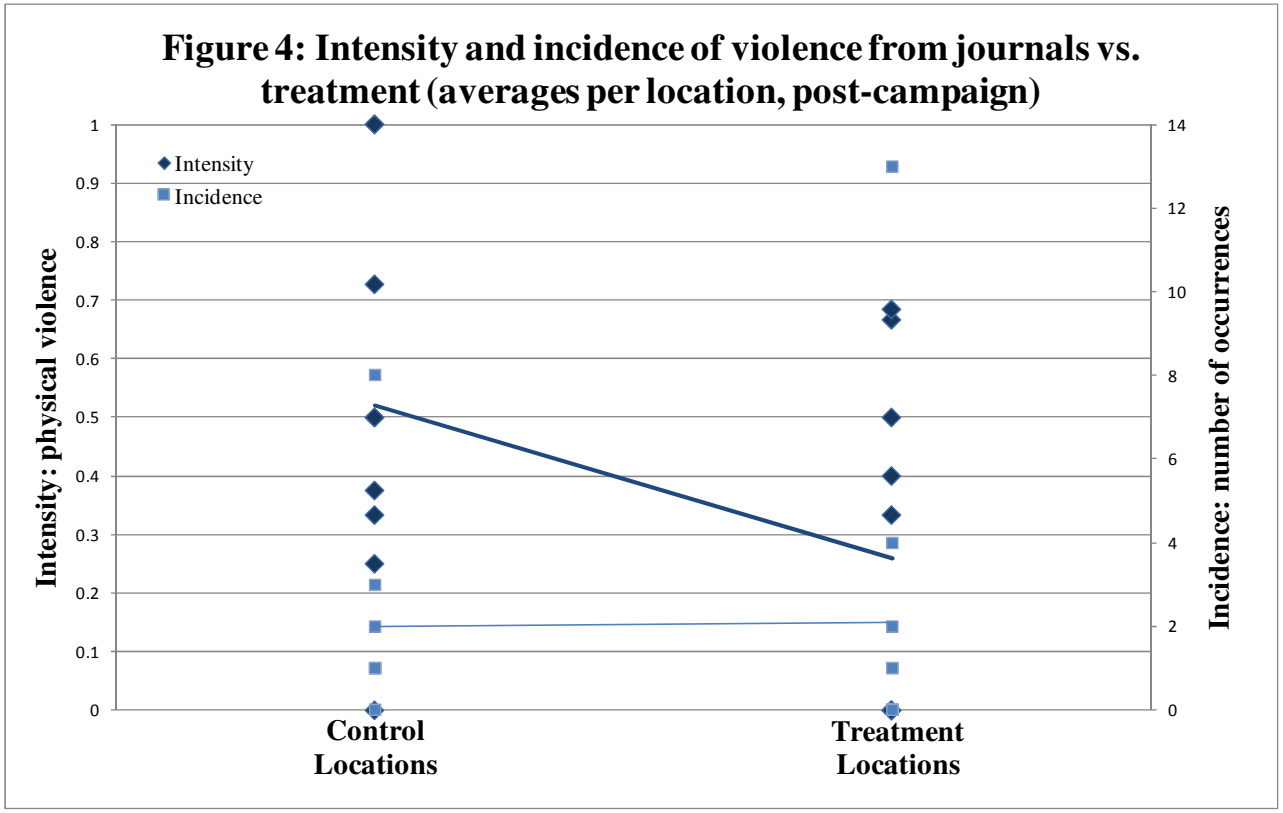


Table 1: Violence-related survey variables - questionnaire phrasing and scales

	variable	phrasing of the question	original scale
political freedom and conflict - general	change of freedom to vote freely	Please tell me if the following things are worse or better now than they were before aou January interview, or are they about the same? Freedom to choose who to vote for without feeling pressured. Worse-Better	1 to 5
	change of freedom from crime and insecurity	Please tell me if the following things are worse or better now than they were before our January interview, or are they about the same? Safety from crime and violence. Worse-Better	1 to 5
	free & fair 2007 elections - general	On the whole, how free and fair were April 2007 elections? Not free and fair-Free and fair	1 to 4
	conflict within local community	In your experience, how often did violent conflicts arise between people: Within the community where you live? Never-Always	0 to 4
local electoral violence - from the top	security	How secure against violence originated by politicians has been your neighbourhood or village? Insecure-Secure	1 to 7
	political intimidation	How often (if ever) has anyone threatened negative consequences to people in your neighbourhood or village in order to get them to vote a certain way? Never-Often	0 to 3
	influence of assassinations	How much influence have assassinations of politicians in Nigeria had on instilling a climate of fear/intimidation in your neighbourhood or village? Not Influential-Influential	1 to 7
	politicians advocating violence	How supportive of violence, in terms of openly advocating violence, have been political representatives in your area? Unsupportive-Supportive	1 to 7
	gang activity	How frequently have you heard about violent groups/gangs/area youths connected with politics being active in your neighbourhood or village? Infrequent-Frequent	1 to 7
local empowerment - from the bottom	support for 'do-or-die affair'	How much of a 'do or die affair' have the people of your neighbourhood or village considered the 2007 elections? No 'Do or die affair'-'Do or die affair'	1 to 7
	standing against violence	How clearly has the people in your neighbourhood or village been standing against violence originated by politicians? Unclear-Clear	1 to 7
	empowerment against violence	How much empowered to defend against violence originated by politicians has been the people feeling in your neighbourhood or village? Disempowered-Empowered	1 to 7
	knowledge of ways to counteract violence	How much knowledgeable has been the people in your neighbourhood or village on ways to resist violence originated by politicians? Not Knowledgeable-Knowledgeable	1 to 7
crime - perceptions and experience	vandalism (perception)	How frequently have you heard about purposely-made damages (vandalism) to property in your area? Infrequent-Frequent	1 to 7
	vandalism (experience)	How frequently, if ever, have you or anyone in your family: Had some property purposely-damaged (vandalized)? Never-Many times	1 to 4
	physical intimidation (perception)	How frequently have you heard about physical threats/intimidation in your area? Infrequent-Frequent	1 to 7
	physical intimidation (experience)	How often, if ever have you or anyone in your family: Been physically threatened? Never-Many times	1 to 4

Table 2a: Differences across treatment and control groups - location characteristics, individual demographics, and attrition

		control	treatment	
			level	difference (to control)
location characteristics	post office	0.250	0.167	-0.083
				0.172
	school	0.917	0.917	0.000
				0.118
	police	0.417	0.333	-0.083
				0.206
	electricity	0.750	0.833	0.083
				0.172
	health clinic	0.833	0.667	-0.167
				0.181
	town hall	0.333	0.417	0.083
				0.206
basic demographics	female	0.500	0.500	-0.000
				0.002
	age	32.955	32.695	-0.260
				1.005
	household size	6.430	6.463	0.033
				0.736
	married	0.581	0.552	-0.029
				0.044
	secondary school completed	0.237	0.316	0.079
				0.057
ethnicity	yoruba	0.318	0.277	-0.042
				0.167
	hausa	0.157	0.100	-0.057
				0.114
	igbo	0.072	0.159	0.087
				0.088
religion	christian	0.621	0.737	0.116
				0.126
	muslim	0.344	0.253	-0.091
				0.132
	religious intensity (1-6)	4.764	5.078	0.314
				0.204
occupation	agriculture	0.158	0.117	-0.042
				0.066
	industry/services: trader	0.125	0.136	0.011
				0.031
	industry/services: artisan	0.112	0.133	0.022
				0.032
	student	0.222	0.222	0.001
				0.039
	housework	0.120	0.093	-0.027
				0.035
property and expenditure	house	0.606	0.574	-0.032
				0.110
	land	0.526	0.554	0.028
				0.116
	cattle	0.329	0.365	0.036
				0.098
	radio	0.888	0.932	0.044
				0.029
	cell phone	0.512	0.586	0.074
				0.119
	household expenditure (naira/month)	19,001.358	22,868.778	3,867.420
				4,758.596
panel re-surveying		0.967	0.948	-0.018
				0.013

Note: These results come from OLS regressions. Note that for individual survey-based variables, we include in the treatment group oversample individuals. Standard errors reported; these are corrected by clustering at the location (census area) level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2b: Differences across treatment and control groups - baseline outcomes

		control	treatment	
			level	difference (to control)
actual violence (journals)	physical violence (0-1)	0.462	0.657	0.194
				0.134
	violence intensity score (1-5)	2.754	2.898	0.144
				0.280
voting 2003 (survey)	turnout presidential	0.728	0.669	-0.058
				0.061
	turnout gubernatorial	0.737	0.673	-0.064
				0.060
	pdp presidential	0.471	0.491	0.020
				0.087
	anpp presidential	0.165	0.089	-0.076
				0.080
	ac presidential	0.027	0.043	0.016
				0.023
	pdp gubernatorial	0.473	0.450	-0.023
				0.083
	anpp gubernatorial	0.134	0.113	-0.021
				0.069
	ac gubernatorial	0.034	0.028	-0.007
				0.023
violence (survey)	local electoral violence - from the top (zscore)	0.000	0.011	0.011
				0.081
	local empowerment - from the bottom (zscore)	0.000	0.252	0.252
				0.210
	crime - perceptions and experience (zscore)	0.000	0.114	0.114
				0.102

Note: These results come from OLS regressions. Note that for individual survey-based variables, we include in the treatment group oversample individuals. Standard errors reported; these are corrected by clustering at the location (census area) level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3: Regressions of actual violence (journals): intensity

dependent variable ------>		physical violence				violence intensity score			
treatment effect	coefficient	**-0.248***	**-0.440****	**-0.425****	**-0.468****	**-0.201**	**-0.434**	**-0.486***	**-0.558***
	standard error	(0.147)	(0.215)	(0.208)	(0.198)	(0.222)	(0.378)	(0.292)	(0.287)
	p-value wild bootstrap	0.226	0.070*	0.068*	0.040**	0.506	0.272	0.092*	0.062*
	p-value randomization inference	0.278	0.027**	0.022**	0.021**	0.524	0.270	0.112	0.091*
number of observations		74	131	131	131	74	131	131	131
mean dep. variable (control)		0.529	0.500	0.500	0.500	2.647	2.703	2.703	2.703
time interaction		**No**	**Yes**	**Yes**	**Yes**	**No**	**Yes**	**Yes**	**Yes**
state dummies		**Yes**	**No**	**Yes**	**Yes**	**Yes**	**No**	**Yes**	**Yes**
location controls		**No**	**No**	**No**	**Yes**	**No**	**No**	**No**	**Yes**

Note: All regressions are OLS. Each observation corresponds to an incident; observations are weighted in order to focus on intensity (by giving the same weight to each location). Intensity is classified on a scale between 1 and 5. First four columns consider 1-2 to be 0, and 3-5 to be 1, i.e., events involving physical confrontation take value 1. Location controls are indicator variables on the existence of basic public services (see top panel of Table 2). Standard errors reported; these are corrected by clustering at the location (census area) level. Wild bootstrap method follows Cameron et al (2008), with null hypothesis imposed, weights -1 and 1, and 1000 replications. Randomization inference uses all 4096 placebo treatment vectors. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4: Regressions of voting behavior (survey)

dependent variable ------>		turnout		voting					
		presidential		pdp presidential		ac presidential		anpp presidential	
treatment effect	coefficient	**0.060***	**0.073****	**0.093***	**0.083*****	**-0.054***	**-0.074*****	**0.018**	**0.061****
	standard error	(0.037)	(0.031)	(0.048)	(0.032)	(0.029)	(0.021)	(0.038)	(0.028)
	p-value wild bootstrap	0.158	0.152	0.114	0.080*	0.166	0.108	0.702	0.278
	p-value randomization inference	0.096*	0.049**	0.101	0.023**	0.116	0.028**	0.574	0.059*
number of observations		1,143	1,126	1,143	1,126	1,143	1,126	1,143	1,126
mean dep. variable (control)		0.651	0.657	0.337	0.343	0.190	0.189	0.109	0.110
dependent variable ------>		**gubernatorial**		**incumbent gubernatorial**		**second party gubernatorial**		**third party gubernatorial**	
treatment effect	coefficient	**0.100****	**0.111*****	**0.103****	**0.128*****	**-0.031**	**0.034**	**0.033**	**0.004**
	standard error	(0.040)	(0.036)	(0.052)	(0.042)	(0.028)	(0.023)	(0.034)	(0.035)
	p-value wild bootstrap	0.038**	0.060*	0.084*	0.084*	0.320	0.370	0.382	0.998
	p-value randomization inference	0.066*	0.008***	0.146	0.054*	0.254	0.119	0.441	0.938
number of observations		1,143	1,125	1,143	1,125	1,143	1,125	1,143	1,125
mean dep. variable (control)		0.688	0.696	0.455	0.458	0.122	0.124	0.075	0.076
controls		No	Yes	No	Yes	No	Yes	No	Yes

Note: All regressions are OLS. All dependent variables are binary. All regressions are based on post-election observations (single-difference specifications), and include state dummies. Controls are location controls on the existence of basic public services, and individual demographic characteristics (see Table 2a, top and middle panels). Standard errors reported; these are corrected by clustering at the location (census area) level. Wild bootstrap method follows Cameron et al (2008), with null hypothesis imposed, weights -1 and 1, and 1000 replications. Randomization inference uses all 4096 placebo treatment vectors. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5a: Regressions of individual violence-related variables: survey measures

dependent variable ------>		political freedom and conflict - general		local electoral violence - from the top		local empowerment - from the bottom		crime - perceptions and experience	
treatment effect	coefficient	**0.366*** **	**0.386*** **	**0.236** **	**0.233** **	**0.221** **	**0.221** **	**-0.034**	**-0.037**
	standard error	(0.133)	(0.123)	(0.099)	(0.102)	(0.104)	(0.106)	(0.114)	(0.117)
	p-value wild bootstrap	0.024**	0.068*	0.026**	0.028**	0.042**	0.046**	0.766	0.746
	p-value randomization inference	0.050*	0.052*	0.001***	0.002***	0.012**	0.013**	1.000	0.983
number of observations		1,148	1,130	2,339	2,303	2,296	2,260	2,349	2,312
mean dep. variable (control)		-0.000	0.001	-0.000	-0.005	-0.000	-0.012	0.000	-0.008
state dummies		**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**
controls		**No**	**Yes**	**No**	**Yes**	**No**	**Yes**	**No**	**Yes**

Note: All regressions are OLS. All dependent variables are indices of z-scores. They are scaled from high violence (low empowerment) to low violence (high empowerment). All regressions include baseline observations (difference-in-difference specifications), except for political freedom and conflict - general, and include state dummies. Controls are location controls on the existence of basic public services, and individual demographic characteristics (see Table 2a, top and middle panels). Standard errors reported; these are corrected by clustering at the location (census area) level. Wild bootstrap method follows Cameron et al (2008), with null hypothesis imposed, weights -1 and 1, and 1000 replications. Randomization inference uses all 4096 placebo treatment vectors. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5b: Regressions of individual violence-related variables: behavioral measure (postcard)

dependent variable ------>		postcard		postcard if Δempowerment>0	
treatment effect	coefficient	**0.060**	**0.078** **	**0.085** **	**0.084*** **
	standard error	(0.079)	(0.035)	(0.036)	(0.015)
	p-value wild bootstrap	0.486	0.090*	0.034**	0.002***
	p-value randomization inference	0.566	0.412	0.096*	0.087*
number of observations		1,149	1,131	1,149	1,131
mean dep. variable (control)		0.341	0.342	0.109	0.108
controls		**No**	**Yes**	**No**	**Yes**

Note: All regressions are OLS. All dependent variables are binary. The second dependent variable takes value 1 if the postcard variable takes values 1 and if empowerment against violence increased from the baseline to the post-election reports. All regressions are based on post-election observations (single-difference specifications), and include state dummies. Controls are location controls on the existence of basic public services, and individual demographic characteristics (see Table 2a, top and middle panels). Standard errors reported; these are corrected by clustering at the location (census area) level. Wild bootstrap method follows Cameron et al (2008), with null hypothesis imposed, weights -1 and 1, and 1000 replications. Randomization inference uses all 4096 placebo treatment vectors. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6: Regressions of campaign spillover (survey)

dependent variable ------>		turnout		voting		political freedom and conflict - general	local electoral violence - from the top	local empowerment - from the bottom	crime - perceptions and experience	postcard
		presidential	gubernatorial	pdp presidential	incumbent gubernatorial					
direct treatment effect	**coefficient**	**0.073****	**0.111*****	**0.083*****	**0.128*****	**0.386*****	**0.233****	**0.221****	**-0.037**	**0.078****
spillover treatment effect	**coefficient**	**-0.034**	**-0.016**	**-0.020**	**0.004**	**0.336*****	**0.260****	**0.131**	**0.062**	**-0.008**
	standard error	(0.052)	(0.060)	(0.030)	(0.035)	(0.110)	(0.111)	(0.142)	(0.119)	(0.059)
	p-value wild bootstrap	0.566	0.792	0.598	0.968	0.080*	0.022**	0.394	0.628	0.902
	p-value randomization inference	0.647	0.878	0.713	0.953	0.035**	0.002***	0.013**	0.982	0.950
number of observations		859	857	859	857	862	1,739	1,724	1,743	863
mean dep. variable (control)		0.657	0.696	0.343	0.458	0.001	-0.005	-0.012	-0.008	0.342

Note: All regressions are OLS. All dependent variables and specifications are as in Tables 4 and 5, with state dummies and controls. The sample is composed of the treatment oversample and control groups. Standard errors reported; these are corrected by clustering at the location (census area) level. Wild bootstrap method follows Cameron et al (2008), with null hypothesis imposed, weights -1 and 1, and 1000 replications. Randomization inference uses all 4096 placebo treatment vectors. * significant at 10%; ** significant at 5%; *** significant at 1%.