THE ECONOMIC JOURNAL

The Economic Journal, **124** (*February*), F327–F355. Doi: 10.1111/ecoj.12109 © 2013 Royal Economic Society. Published by John Wiley & Sons, 9600 Garsington Road, Oxford OX4 2DQ, UK and 350 Main Street, Malden, MA 02148, USA.

VOTES AND VIOLENCE: EVIDENCE FROM A FIELD EXPERIMENT IN NIGERIA*

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Elections are now common in low-income societies. However, they are frequently flawed. We investigate a Nigerian election 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 theatres and door-to-door distribution of materials. We find that the campaign decreased violence perceptions and increased empowerment to counteract violence. We observe a rise in voter turnout and infer that the intimidation was dissociated from incumbents. These effects are accompanied by a reduction in the intensity of actual violence, as measured by journalists.

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

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We thank the editors, three anonymous referees, Christian Ahlin, Robert Bates, Cátia Batista, Tim Besley, Michael Bratton, David Clingingsmith, Michael Conlin, Esther Duflo, Marcel Fafchamps, Claudio Ferraz, Yvan Guichaoua, Masa Kudamatsu, Rocco Macchiavello, Alice Mesnard, Abdul Mustapha, Laura Valderrama and Eric Werker for helpful suggestions. We are particularly indebted to Ojobo Atukulu, Otive Igbuzor and Olutayo Olujide at ActionAid International Nigeria, Austin Emeanua, campaigners Nwakaudu Chijoke Mark, Gbolahan Olubowale, George-Hill Anthony, Monday Itoghor, Umar Farouk, Emmanuel Nehemiah, Henry Mang and their field teams and to the surveyors headed by Taofeeq Akinremi, Gbenga Adewunmi, the late Oluwasegun Olaniyan, and Moses Olusola: their professionalism, courage and dedication to this project were truly outstanding. We also acknowledge the kind institutional collaboration of the Afrobarometer. We thank seminar participants at the LSE-Oxford iiG meetings, CSAE, NCDE, EEA, NEUDC, CEPR, AEA/ASSA and IGC conferences and at various seminars for useful comments. We acknowledge financial support from the Department for International Development (DFID), the William and Flora Hewlett Foundation and the Open Society Institute, in the context of the iiG Consortium – 'Improving Institutions for Pro-poor Growth'. All errors are the responsibility of the authors.

[1] Skaperdas and Grofman (1995) is a notable early exception.

The record of democratisation has been especially problematic in Africa.[2] Kudamatsu (2012) 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 considerably increases proneness to civil conflict (Collier and Rohner, 2008). Indeed, Africa has seen a pronounced emergence of political intimidation and violence during times of election. 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 study is to present evidence from a field experiment we designed and conducted to establish the causal effects of community campaigning against electoral violence. The context for our analysis is the 2007 Nigerian national and state-level elections, which 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 article was randomised across neighbourhoods 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 specialises in community participatory development. It included town meetings, popular theatre and the distribution of campaign material, standardised 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'.

We based our measurement on representative surveys, on a behavioural measure of empowerment to counteract violence (the sense that common citizens have ways to oppose intimidation) and on the compilation of violence-event diaries by independent local journalists in the treatment and control areas of the experiment. 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 behaviour after the elections. We complement our survey measures of empowerment to counteract violence with an incentive-compatible (cost-inducing) behavioural 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 additional respondents at the post-election stage, in treated areas only, who were not directly approached by the campaign. This group

[2] Note that until the 1990s the predominant African political system was autocracy. As Besley and Kudamatsu (2008) show, although in some contexts autocracy has produced good economic performance, in Africa it has consistently been dysfunctional.

allows us to quantify the effects of the campaign on untargeted individuals within treated locations.

We present evidence that the anti-violence campaign was able to increase the sense of security to the general population. As an example, our measure of perceived local electoral violence induced by politicians decreased by 0.23 standard deviation units. The campaign also boosted empowerment to counteract electoral violence – the likelihood that the postcard was mailed was 8 percentage points higher for treated respondents. We found that the intervention increased voter turnout by 11 percentage points (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). All these effects on ordinary citizens may have undermined electoral violence as an electoral strategy. Indeed, we observe a clear decrease in actual violence as reported in the journalists' diaries. Namely, we detect a 47 percentage point effect on the likelihood that physical violence occurs. This is evidence that the campaign was able to influence the behaviour of violent politicians. Note that we also find effects on untargeted individuals within treated locations, which may include spillovers of the campaign, specifically in terms of decreased perceptions of violence.

To the best of our knowledge this is the first experimental study dedicated to analysing electoral violence. It is also one of the few studies 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 randomisation of actual political campaign platforms. Vicente (2014) analysed a voter education campaign focusing on vote buying in Sao Tome and Principe. Banerjee *et al.* (2011) and Fujiwara and Wantchekon (2013) explore interventions providing voters with specific information about public policy options and offering information about politician performance and qualifications (respectively). Finally, some work has been devoted to evaluating awareness campaigns focusing on the employed means, namely boosting electoral participation through cell phones and free newspapers (Aker *et al.*, 2012) and social networks (Giné and Mansuri, 2011). Fafchamps and Vicente (2013) analyse the same field experiment we study here but focus on social-network (heterogeneous) effects of the anti-violence campaign.

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 in which 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 (2012), consistently with the findings in this study, provide models in which 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 emphasises the idea that violence may be an important political strategy in the face of ethnic divisions. A recent study by Chaves *et al.* (2009) looked at the 1922

[3] Experimental methods have been applied in the context of North 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 (2008). Note that they find effects on voting behaviour that are comparable in magnitude with the ones we find in this study. In a recent field experiment, Dewan *et al.* (2014) distinguish between different elements of political persuasion.

presidential election in Colombia 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 opposition voters from participating in the election.

In Section 1 we describe the Nigerian context. Section 2 discusses the design of the experiment, including details of the treatment, measurement and estimation strategy. Section 3 presents descriptive statistics, and shows the experimental results regarding violence-related survey measures, the behavioural measure of empowerment, voter behaviour, actual violence and effects on untargeted individuals within treatment locations. Section 4 concludes with some implications for future research and policy.

1. 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 were held in 1999, 2003 and 2007. However, all these elections were damaged by widespread electoral malfeasance (Omotola, 2010). 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. Incumbent president, Olusegun Obasanjo, did not stand in the 2007 election, 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 People's Democratic Party (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, representing the All Nigeria Peoples Party (ANPP). A previous military ruler, his past regime 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

[4] World Development Indicators, 2009.
[5] *Oil & Gas Journal*, 103(47), 19 December 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.

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% of the votes, as did 28 of its candidates in the 36 gubernatorial elections. However, the election was deeply flawed through violence and vote miscounting. As an illustration, we present the assessments of three well-informed independent organisations. 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.[9]

> 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.[10]

> 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.[11]

2. Experimental Design

2.1. *The Intervention*

We collaborated with the Nigeria chapter of ActionAid, ActionAid International Nigeria (AAIN), 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.[12]

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 through a slight increase

[9] European Union Election Observation Mission, 'Nigeria – Final Report on the Gubernatorial and State Houses of Assembly Elections of 14 April 2007 and on the Presidential and National Assembly Elections of 21 April 2007', 2007.

[10] Human Rights Watch, 'Nigerian Debacle a Threat to Africa', May 2007.

[11] Transition Monitoring Group (an NGO which deployed 50,000 Nigerian observers to the 2007 elections) as cited by BBC News on 23 April 2007.

[12] AAIN is Nigeria's chapter of global ActionAid, headquartered in South Africa, with total budget US$ 133 mn. in 2005, and specialising 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 organisations devoted to philanthropy (above Transparency International, UNDP and Amnesty International) – see the *Financial Times*, 5 July 2007, Report on 'Corporate Citizenship and Philanthropy'.

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.[13] 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 emphasised the lack of legitimacy in the use of intimidation. We therefore expect that the campaign increased voter turnout and caused 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 these politicians are not likely to have been directly treated by the campaigners.

The anti-violence campaign reached a set of enumeration areas, i.e. neighbourhoods or villages. It was implemented during a two-week period approximately two months before the election, just after our baseline survey. For this campaign, AAIN worked with local state-level partner NGOs, who conducted the campaign activities in the field.[14] Because each state was allocated a different team of campaign fieldworkers, campaign activities were fully contemporaneous in all states.[15]

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 between the central AAIN officers and the local partnering NGO representatives. Actual material and activity design were undertaken with the help of a specialised firm in Abuja. The campaign slogan was written on a wide range and large quantity of distributed campaign materials: T-shirts (3,000), 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.

However, the campaign was designed to work mainly through the holding of town meetings and popular theatre. The town meetings provided an opportunity for the grassroots to meet with local representatives to discuss ways of counteracting politically motivated violence. Consistent with the theory, meetings were designed to minimise the collective action problem that impedes diminishing conflict at the local level. Popular theatre 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 labour 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 theatre per treatment location.

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

[14] One author of this study witnessed operations in the field in four of the six states included in this campaign.

[15] 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/.

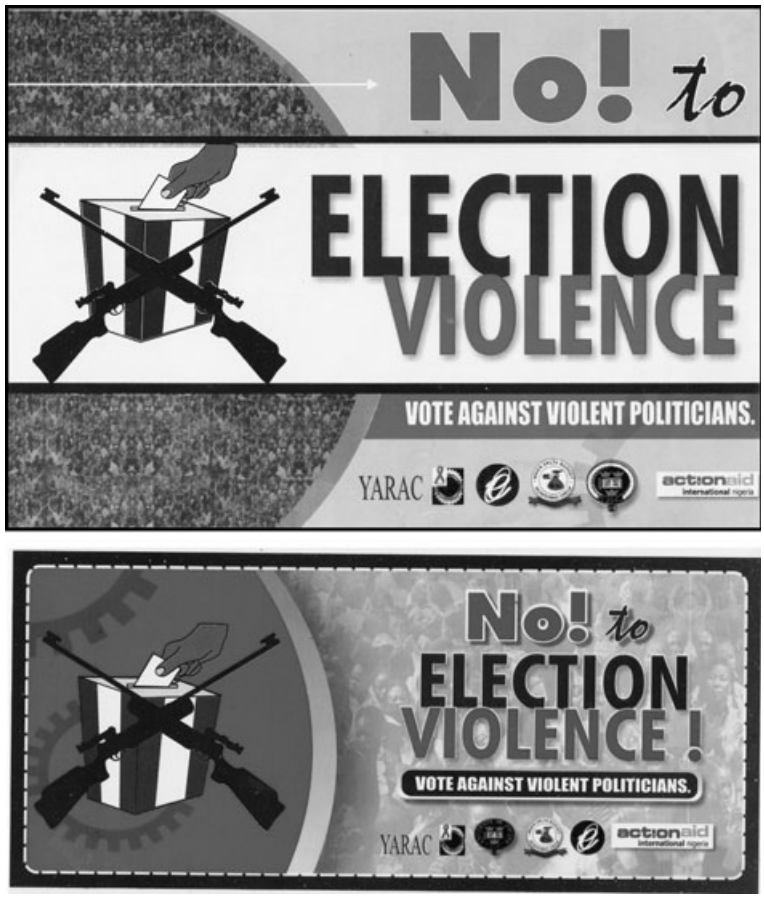

Fig. 1. *ActionAid, ActionAid International Nigeria's Campaign: Leaflet/Poster (Above) and Sticker (Below)*

2.2. *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.

We began our sample selection by choosing two states in each of the three main regions of the country (Southwest, Southeast and North). For that purpose we looked at recent history of politically motivated violence.[16] This process led to the selection of Lagos and Oyo (Southwest), Delta and Rivers (Southeast) and Kaduna and Plateau (North). This procedure revealed our emphasis on studying violence while keeping the

[16] 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', 15(9A), April 2003, 'Nigeria's 2003 Elections: the Unacknowledged Violence', June 2004.

basic diversity of the country obtained 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 organising 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

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

Fig. 2. *Sampled Enumeration Areas*

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 (with the other enumeration area serving as control). This process led to the selection of the areas shown in Figure 2.

We then selected surveyed individuals within each of the 24 selected enumeration areas. For baseline respondents, who constitute our main sample, we use random representative sampling within each enumeration area. The baseline survey was performed jointly with Afrobarometer and Nigerian partner Practical Sampling International (PSI) during the period 20 January to 3 February 2007. At that time, individuals within a census area were chosen randomly using Afrobarometer's standard techniques.[17] We reached 1,200 individuals during the baseline survey, 50 per enumeration area. The same individuals were re-surveyed after the electoral results had been publicised and a sense of political normalcy was re-established. The post-election survey, also conducted with PSI, took place from 22 May to 5 June and reached 1,149 or 96% of the baseline respondents. We also surveyed a second, smaller, sample, the selection of which is described below. Individuals in this sample were administered only the post-election survey.

2.3. *Assignment to Treatment*

We randomly assigned 12 enumeration areas to be visited by AAIN campaigners. The other 12 enumeration areas that were sampled for our study were assigned to the control group and were not visited by campaigners. In each treated enumeration area, campaigners were instructed to target baseline respondents,[18] 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 theatre.

During the post-election survey we gathered a fresh sample of 300 respondents, one per household, in treated enumeration areas (25 per enumeration area). Within the enumeration area, the selection of this group followed the same standard procedures applied to the main sample, with two exceptions:

(*i*) baseline houses were not considered; and
(*ii*) respondents were required not to have been 'directly approached' by the campaign team.

This group was then selected to be representative of only those individuals not targeted by campaigners. We refer to this sample as the 'untargeted' individuals, and by extension we refer to the baseline sample as the 'targeted' individuals. The purpose of

[17] Enumerators were instructed to start from the centre of the enumeration area and to walk in different directions. Each *n*th house was visited. For each enumeration area, the number *n* was set to ensure an equal likelihood of visit to all houses within the enumeration area based on the number of houses and enumerators in the enumeration area. Within each house, enumerators listed all individuals aged 18 and above who were of a given gender (with gender alternated). One respondent was drawn at random from the list. Empty houses, absence of selected persons and refusals were replaced by the next adjacent house. This happened in 24% of the cases.

[18] 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.

this sample is to estimate the effect of the campaign on the untargeted individuals in treated locations.

We gathered data showing that 47% of the panel households were represented at one of the campaign events. Moreover, a large majority of the panel individuals recalled the activities of the campaign well during our post-election survey: 88/89/86/84% remembered the distribution of materials, the roadshows, the town meetings and the popular theatre, respectively. Note, however, that the activities of the campaign may have reached other individuals beyond our baseline respondents. This is despite the fact that campaigners were told to approach (directly and individually) only the 50 baseline respondents at their homes. The roadshows were by nature designed to raise local awareness without the need for much personal contact with campaigners. Some passers-by approached campaigners to receive campaign materials because their presence in the streets attracted attention. However, the town meetings and popular theatre were publicised through the less evident invitations (to baseline respondents) and were held at specific venues, making it unlikely that other individuals attended those events.

2.4. *Measurement*

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

First, the surveys were designed to elicit evidence on individual voter behaviour[19] and measures (e.g. perceptions) related to violence.[20] Most questions on violence were asked both prior to the campaign and after the election. In the baseline survey, the year preceding the survey is the reference period; in the post-election survey, the reference period is the time elapsed since the baseline survey until the elections, that is, between January and April 2007. The majority of the violence questions use a subjective scale.

Second, we note the specific behavioural 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 pre-stamped postcard which they could choose whether to mail or not – the main side of 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 organisations involved in the experiment, which promised to raise media awareness about the problem in states where enough postcards were sent. Because to post the

[19] On electoral behaviour, disaggregated official 2007 electoral results are not available. 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 (2014) was able to contrast self-reports with disaggregated electoral results in São Tomé and Príncipe's 2006 presidential election, without significant differences encountered. Although different, Nigeria and São Tomé and Príncipe are neighbouring countries, which gives us some assurance that self-reports may be an adequate source of voting data in that region of Africa.

[20] All questionnaires are available from the authors upon request.

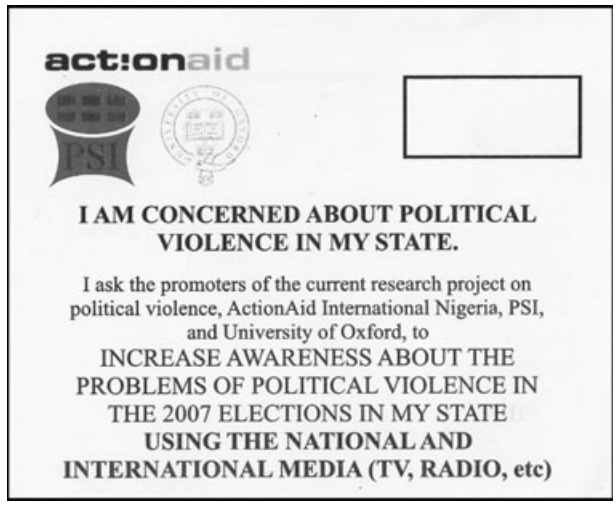

Fig. 3. *Postcard*

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.

Finally, we contracted independent local journalists (one per enumeration area) to report/describe, in a diary, violent events that affected the neighbourhood 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 AAIN'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 behaviour 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 behaviour of the potential victims.

2.5. *Estimation Strategy*

Our empirical approach is based on reduced form specifications. We estimate the effects of the intervention on individual outcomes as measured in our survey and through the postcard and on actual violence as measured by the journals. We now describe the main econometric specifications we employed, exemplifying with data at the individual level.

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

$$Outcome_{il1} = \alpha + \beta T_l + \varepsilon_{il1}, \quad (1)$$

where *Outcome* is a violence-related or voting behaviour outcome, i, l and $t = 1$ 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 consistent with Duflo *et al.* (2007), who argued that even though controls do not generally change the estimate for the 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:

$$Outcome_{il1} = \alpha + \boldsymbol{\gamma} Y_l + \boldsymbol{\delta} X_i + \beta T_l + \varepsilon_{il1}, \quad (2)$$

where $\boldsymbol{Y}_l$ is a location-level vector of controls, and $\boldsymbol{X}_i$ 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 for our measurements of voting behaviour in the April elections, which are 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:

$$Outcome_{ilt} = \alpha + \boldsymbol{\gamma} Y_l + \boldsymbol{\delta} X_i + \rho t + \theta T_l + \beta t T_l + \varepsilon_{ilt}, \quad (3)$$

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 estimation in this study.

As the data we use are 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 (for a defence of the use of corrected standard errors, see Moulton (1990)). 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 (as in our case) the cluster-robust standard errors are likely to be downward biased.

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 randomisation inference approach discussed by Rosenbaum (2002) and recommended by Duflo *et al.* (2007). See Bhushan *et al.* (2007) for a recent application of randomisation 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 hold regressors constant across the pseudo-samples, while re-sampling 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 re-sampling residuals and that the null hypothesis of zero treatment effect is imposed. We follow both recommendations.

Randomisation 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 randomisation procedure that was used. Following our treatment assignment structure composed of 12 pairs of enumeration areas, we have 4,096 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. As the placebo assignments vary only across clusters, this method takes intra-cluster correlations into consideration. According to Bhushan *et al.* (2007), the drawback of hypothesis testing based on randomisation 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 is on survey-based measures of violence-related outcomes. We follow Kling *et al.* (2007) in that we normalise 17 survey-based measures using z-scores, and aggregate them in four indices using equally weighted averages of the normalised individual variables. Table 1 shows all individual variables with original scales, as well as the four groups. Note that the normalisation 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.[21]

3. Econometric Results

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

3.1. *Balance*

We begin by evaluating whether the randomised selection of treated locations was successful in identifying comparable treatment and control groups. We document

[21] 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 random assignment group mean for the corresponding time period.

Table 1

Violence-related Survey Measures – Questionnaire Phrasing and Scales

Variable	Phrasing of the question	Original scale
Political freedom and conflict – general		
Change in freedom to vote freely	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? Freedom to choose who to vote for without feeling pressured. Worse–better	1–5
Change in 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	1–5
Free and fair 2007 elections – general	On the whole, how free and fair were April 2007 elections? Not free and fair-free and fair	1–4
Conflict within local community	In your experience, how often did violent conflicts arise between people: Within the community where you live? Never–always	0–4
Local electoral violence – from the top		
Security	How secure against violence originated by politicians has been your neighbourhood or village? Insecure–secure	1–7
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	0–3
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	1–7
Politicians advocating violence	How supportive of violence, in terms of openly advocating violence, have been political representatives in your area? Unsupportive–supportive	1–7
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	1–7
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'	1–7
Standing against violence	How clearly have the people in your neighbourhood or village been standing against violence originated by politicians? Unclear –clear	1–7
Empowerment against violence	How empowered to defend against violence originated by politicians have the people been feeling in your neighbourhood or village? Disempowered–empowered	1–7
Knowledge of ways to counteract violence	How knowledgeable have been the people in your neighbourhood or village on ways to resist violence originated by politicians? Not Knowledgeable–knowledgeable	1–7
Crime – perceptions and experience		
Vandalism (perception)	How frequently have you heard about purposely made damages (vandalism) to property in your area? Infrequent–frequent	1–7

Table 1
(*Continued*)

Variable	Phrasing of the question	Original scale
Vandalism (experience)	How frequently, if ever, have you or anyone in your family had some property purposely damaged (vandalised)? Never–many times	1–4
Physical intimidation (perception)	How frequently have you heard about physical threats/intimidation in your area? Infrequent–frequent	1–7
Physical intimidation (experience)	How often, if ever have you or anyone in your family been physically threatened? Never–many times	1–4

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 (individual survey-based measures related to violence using the indices of Table 1 when available for the baseline, individual electoral preferences for the 2003 elections and actual violence). 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.

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 randomisation 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 2 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.

3.2. *Violence-related Survey 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 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'

Table 2

Differences Across Treatment and Control Groups

	Control	Treatment	
		Level	Difference (to control)
(*a*) Location characteristics, individual demographics and attrition			
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)

Table 2
(*Continued*)

	Control	Treatment: Level	Treatment: Difference (to control)
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)
Mobile 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)
Attrition			
Panel re-surveying	0.967	0.948	−0.018
			(0.013)
(*b*) Baseline outcomes			
Violence (*survey*)			
Local electoral violence – from the top (z-score)	0.000	0.011	0.011
			(0.081)
Local empowerment – from the bottom (z-score)	0.000	0.252	0.252
			(0.210)
Crime – perceptions and experience (z-score)	0.000	0.114	0.114
			(0.102)
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)
Actual violence (*Journals*)			
Physical violence (0–1)	0.425	0.659	0.234
			(0.150)
Violence intensity score (1–5)	2.694	2.929	0.234
			(0.291)

Notes. *Significant at 10%. **Significant at 5%. ***Significant at 1%. These results come from OLS regressions. 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.

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 normalised as z-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 3. 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 have only a post-election measurement). The first specification we show for each different dependent variable includes only state dummies,[22] with the second adding location and individual demographic controls (as in (2) and (3) above). Note that location controls and individual demographic controls are depicted in panel (*a*) of Table 2.

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% levels using cluster-robust standard error inference (statistical significance is maintained when using the other methods of statistical inference we adopt in our study). 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.[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.

3.3. *Behavioural Empowerment against Violence: Postcard*

We turn now to our behavioural 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, consistent 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 behavioural aspect of this measure (respondents had to incur a cost to send the postcard) seeks to be an improvement on equivalent survey questions, which may be more vulnerable to report biases. Because we have only post-election levels for the

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

[23] Note that we have found a significant decrease on perceptions of police-induced problems as a result of the campaign, 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 favour of the incumbent (who controls the police), as one would expect from the independent nature of the campaign sponsor (international NGO ActionAid).

Table 3

Regressions of Individual Violence-related 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 randomisation 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 dependent 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

Notes. *Significant at 10%. **Significant at 5%. ***Significant at 1%. 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 2, panel (*a*)). 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 1,000 replications. Randomisation inference uses all 4,096 placebo treatment vectors.

postcard variable, we use variations in specification (2) above, with state dummies only and with added location and individual controls. These results are shown in Table 4.

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 wish to identify those cases where the attitude referred to 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 4 uses as dependent variable the postcard dummy only if the perceived change in empowerment was positive. Otherwise it takes the value 0, as if the respondent had not sent the postcard.

We first note that 37% of the subjects returned the postcard in the mail, which implies that the initiative had a remarkably high degree of adherence. Treated respondents were found to send the postcard 8 percentage points more frequently than their control counterparts. However, statistical significance can be observed only when adding controls, at the 5 and 10% levels for inference based on the cluster-robust standard errors and for wild bootstrap (respectively). Statistical significance does not emerge when using randomisation inference, our most conservative method. 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% (randomisation inference) 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 achieve a positive change in empowerment for the targeted population.

Table 4

Regressions of Behavioural Empowerment Against Violence (Postcard)

Dependent variable:	Postcard		Postcard if Δ empowerment > 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 randomisation inference	0.566	0.412	0.096*	0.087*
Number of observations	1,149	1,131	1,149	1,131
Mean dependent variable (control)	0.341	0.342	0.109	0.108
Controls	No	Yes	No	Yes

Notes. *Significant at 10%. **Significant at 5%. ***Significant at 1%. 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 2, panel (*a*)). 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 1,000 replications. Randomisation inference uses all 4,096 placebo treatment vectors.

3.4. *Voting Behaviour*

We now focus on the effects of the anti-violence campaign on the electoral behaviour of the panel of respondents, in terms of both voter turnout and voting for specific candidates/parties. Our results are in Table 5. The focus is on the estimation of treatment effects by using single-difference regressions employing post-election survey data on reported voting behaviour in the April elections. For each outcome, we begin by showing the single-difference regression with state dummies only; we then report the regression adding location and individual demographic controls (specification 2). 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 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 percentage points 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% level for cluster-robust standard errors and randomisation inference; it is significant at the 10% level for wild bootstrap. For the presidential race, this effect is significant only when using the cluster-robust standard errors and when using randomisation inference (both at the 5% level). The larger size and greater 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 are channelled to state-level budgets managed by governors.[24] We can conclude in favour 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.

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 emphasise 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.

We find that in the presidential election, the campaign increased the vote for the PDP candidate by 8 percentage points and reduced the vote for the AC candidate by 7 percentage points. The first is significant at the 1% level using the cluster-robust standard errors, 10% level using wild bootstrap and 5% level using randomisation inference (note that wild bootstrap and randomisation inference do not show significant levels for the specification without controls) and the second is significant at the 1% level using the cluster-robust standard errors and 5% level randomisation inference (note that randomisation inference does not achieve a significant level for the specification without controls). We also observe a positive effect on voting for ANPP, which nevertheless is

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

Table 5

Regressions of Voting Behaviour

	Turnout		Voting					
Dependent variable:	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 randomisation 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 dependent 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 randomisation 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 dependent 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

Notes. *Significant at 10%. **Significant at 5%. ***Significant at 1%. 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 2, panel (*a*)). 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 1,000 replications. Randomisation inference uses all 4,096 placebo treatment vectors.

significant only when using controls, for cluster-robust (at the 5% level) and randomisation inference (at the 10% level). We should recall that the AC presidential candidate was portrayed in the media as espousing instability. The reduced vote for the AC candidate provides 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 seems to have benefited the most from these vote changes.

Concerning the gubernatorial elections, the campaign very clearly increased the vote count of the incumbent (i.e. PDP in all but one state) by 13 percentage points, an effect significant at the 1% (cluster-robust standard errors) and 10% (wild bootstrap and randomisation inference) levels, and robust to the exclusion of location and individual controls (with the exception of statistical significance provided by randomisation 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.[25] This is consistent with the idea that the incumbent may have an advantage in using other more effective illicit strategies such as fraud and vote buying when needed.[26] Weak political groups may be restricted to the use of electoral intimidation of opponents to maximise 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 (2012) propose a general theory of electoral competition with illicit strategies that formalises this hypothesis.

3.5. *Actual Violence: Journals*

We now analyse the effects of the treatment on the intensity 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,

[25] Indeed, several sources point to the importance of electoral violence perpetrated 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, centred on the figure of Chief Lamidi Adedibu. For Rivers State, the same organisation 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, who implemented nationwide surveys during the 2007 Nigerian elections, considers that 40% of the electoral violence originated purely from outside the main parties, PDP, AC and ANPP ('A Nigerian Perspective on the 2007 Presidential and Parliamentary Elections', August 2007).

[26] Ballot fraud is likely to favour 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 measured by perceptions).

occurrences resulting in more than five dead people; 4, occurrences resulting in dead people (although fewer than five casualties in total); 3, occurrences resulting in physically wounded people; 2, occurrences leading to severe intimidation and 1, petty crime occurrences leading to intimidation. Although we use the full scale as an intensity score, 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).

In Figure 4 we average the seriousness of the occurrences per location and plot both the physical violence indicator and the violence intensity score (using post-campaign data only) against our treatment. We find that violence decreased as a result of the treatment. Despite the low number of (location-level) observations, we find a statistically significant effect of the treatment at the 10% level on decreasing physical violence.

To confirm these results, in Table 6 we run regressions for physical violence (0–1) and for the full violence intensity score (1–5) taking the violent event as the unit of analysis. This approach allows for the use of state dummies and location controls (see panel (*a*) of Table 2). 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. To use this degree of disaggregation to study violence intensity, we weight observations so as to attribute the same importance to each experimental location in the sample. We show results using difference in differences with state dummies only, and with location controls added to state dummies.

We find a 47 percentage point reduction in the likelihood that physical violence occurs. This effect is statistically significant at the 5% level using all three inference methods (Huber–White, wild bootstrap and randomisation inference). This is a robust effect, as it displays statistical significance without location controls. We also find a

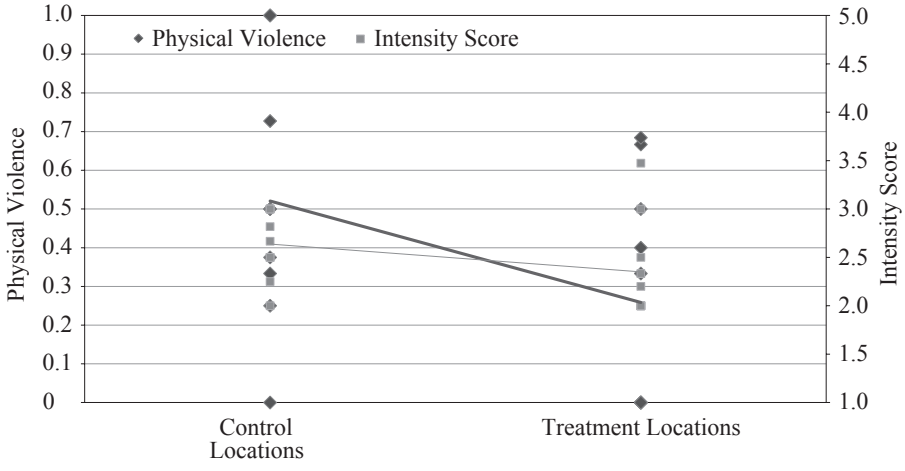

Fig. 4. *Actual Violence from Journals versus Treatment – Averages per Location and Post-campaign Data Notes.* Each datapoint represents average violence for an experimental location. Physical violence is between 0 and 1. Those occurrences where physical violence happened were coded 1; otherwise they were given value 0. The intensity score is between 1 and 5, from lowest to highest intensity.

Table 6

Regressions of Actual Violence (Journals)

Dependent variable:	Physical violence		Intensity score	
Treatment effect				
Coefficient	−0.425**	−0.468**	−0.486*	−0.558*
Standard error	(0.208)	(0.198)	(0.292)	(0.287)
p-value wild bootstrap	0.068*	0.040**	0.092*	0.062*
p-value randomisation inference	0.022**	0.021**	0.112	0.091*
Number of observations	131	131	131	131
Mean dependent variable (control)	0.500	0.500	2.703	2.703
Location controls	No	Yes	No	Yes

Notes. *Significant at 10%. **Significant at 5%. ***Significant at 1%. All regressions are OLS. Each observation corresponds to an incident; observations are weighted to focus on intensity (by giving the same weight to each location). Intensity is classified on a scale between 1 and 5. First two columns consider 1–2 to be 0, and 3–5 to be 1, i.e. events involving physical confrontation take value 1. All regressions include state dummies. Location controls are indicator variables on the existence of basic public services (see panel (*a*) 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 1,000 replications. Randomisation inference uses all 4,096 placebo treatment vectors.

significant negative effect when using the full violence intensity score (14% of the 1–5 scale). This is significant at the 10% level when using state dummies and location controls for all three inference methods. The estimated impact on intensity provides 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.

3.6. *Effects on Untargeted Individuals*

We now evaluate the effects of the anti-violence campaign on untargeted individuals within treated locations. These subjects 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 7 untargeted individuals with the control individuals, regarding the relevant outcomes analysed above at the individual level. Our general hypothesis is that the effects of the campaign may have gone beyond the panel of respondents. Table 7 displays for each outcome the same specifications used in Tables 3, 4 and 5 when adding full controls (but using the untargeted respondents instead of the targeted individuals). It also repeats the point estimates from those tables for comparison purposes.

We find clear effects on violence and intimidation perceptions. The size of the estimates is comparable with the effects we have found for the targeted, namely for general political freedom and conflict, and local electoral violence from the top. These are significant, typically at the 1 or 5% levels using all three inference methods. Note

Table 7

Regressions of Main Outcomes – Untargeted Individuals

Dependent variable:	Political freedom and conflict – general	Local electoral violence – from the top	Local empowerment – from the bottom	Crime – perceptions and experience	Postcard	Turnout		Voting	
						Presidential	Gubernatorial	PDP presidential	Incumbent gubernatorial
Direct treatment effect									
Coefficient	0.386***	0.233**	0.221**	−0.037	0.078**	0.073**	0.111***	0.083***	0.128***
Spillover treatment effect									
Coefficient	0.336***	0.260**	0.131	0.062	−0.008	−0.034	−0.016	−0.020	0.004
Standard error	(0.110)	(0.111)	(0.142)	(0.119)	(0.059)	(0.052)	(0.060)	(0.030)	(0.035)
p-value wild bootstrap	0.080*	0.022**	0.394	0.628	0.902	0.566	0.792	0.598	0.968
p-value randomisation inference	0.035**	0.002***	0.013**	0.982	0.950	0.647	0.878	0.713	0.953
Number of observations	862	1,739	1,724	1,743	863	859	857	859	857
Mean dependent variable (control)	0.001	−0.005	−0.012	−0.008	0.342	0.657	0.696	0.343	0.458

Notes. *Significant at 10%. **Significant at 5%. ***Significant at 1%. All regressions are OLS. All dependent variables and specifications are as in Tables 3, 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 1,000 replications. Randomisation inference uses all 4,096 placebo treatment vectors.

that effects on local empowerment are less clear: the survey-based index yields a significant treatment effect only for randomisation inference, and the postcard does not show statistical significance. We do not find effects on voting behaviour. 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 behaviour.[27]

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

The findings in our study are optimistic regarding the role of community-based campaigning in counteracting electoral violence. Like in Kuran (1989), relatively insignificant but targeted events can indeed mobilise citizens to collective action.[28] Specifically, more participation at the polls together with improved security and empowerment of the population may be mutually reinforcing, in a context in which violence is associated with small political groups. Anti-violence campaigns may then be an especially effective form of voter education, working mainly as a coordination mechanism and relatively undemanding on the amount of information that is passed to voters. However, we would like to emphasise 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 behaviour such as vote miscounting and vote buying. In the same vein and policy-wise, an anti-violence campaign cannot be the sole remedy for problematic elections; attention should be devoted to political accountability and to all illicit strategies in an integrated manner. It is in this context

[27] A recent contribution by Ichino and Schuendeln (2012) focused attention on electoral observation at the time of voter registration in the Ghanaian national elections of 2008. They find clear effects of national observers on decreasing voter registration fraud, but also on displacing some of this fraud to nearby locations that were not observed. In our study, contamination of control areas can be tested by regressing our main outcomes of interest (violence and voting behaviour) on distance to closest treatment area, while using observations from control locations only. We do not find evidence consistent with negative contamination of neighbouring locations.

[28] Note that individuals who are more marginal to our local communities seem to be most responsive to the intervention. This is found by interacting the treatment with demographic characteristics. This finding is consistent with the targeting of violence towards those marginalised 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 pointed primarily at swing voters.

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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Additional Supporting Information may be found in the online version of this article:

Data S1.

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