Political Violence and Social Networks: Experimental Evidence from a Nigerian Election*

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Abstract

Voter education campaigns often aim to increase political participation and accountability. We followed a randomized campaign against electoral violence sponsored by an international NGO during the 2007 Nigerian elections. This paper investigates whether the effects of the campaign were transmitted indirectly through kinship, chatting, and geographical proximity. For individuals personally targeted by campaigners, we estimate the reinforcement effect of proximity to other targeted individuals. For individuals who self-report to be untargeted by campaigners, we estimate the diffusion of the campaign depending on proximity to targeted individuals. We find evidence for both effects, particularly on perceptions of violence. Effects are large in magnitude – often similar to the average effect of the campaign. Kinship is the strongest channel of reinforcement and diffusion. We also find that geographical proximity transmits simple effects on perceptions, and that chatting conveys more

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complex effects on behavior.

1. Introduction

For democracy to deliver politicians that improve the welfare of the masses, citizens must be informed and vote to hold politicians accountable. Yet politicians often manage to secure votes by stirring up greed, rivalry, or fear. Improving democracy therefore requires that we find ways to reduce the role that greed, rivalry and fear play in the electoral process, especially in young democracies such as those in Africa.

Using field experiments in Benin and in Sao Tome and Principe, Wantchekon (2003) and Vicente (2010) study greed: they show that politicians attract more votes by using clientelistic or vote-buying electoral platforms, respectively. The study of the use of rivalry in politics has centered on ethnic tensions. Using a natural experiment in the border region of Malawi and Zambia, Posner (2004) provides evidence that ethnic identification is endogenous to political conditions. This finding is reinforced by Habyarimana, Humphreys, Posner, and Weinstein (2007) using lab experiments in Uganda, and by Eifert, Miguel, and Posner (2010) using Afrobarometer data across ten African countries. In this paper we focus on the use of fear in elections.

The fundamental question is: what can be done to reduce the role of malfeasant electoral strategies like vote-buying, ethnic polarization, or violent intimidation? Vicente (2010) shows that a campaign against vote-buying reduced its influence on the vote but also decreased turnout. Using the field experiment we exploit in this paper, Collier and Vicente (2011) show that an awareness campaign encouraging Nigerian voters to oppose electoral violence was successful in reducing the perception of local violence and in encouraging empowerment. This finding stands in contrast with those of Dellavigna and Kaplan (2007) and Dahl and Dellavigna (2009), who study the perception and behavioral effect of broadcasting information on violence and crime. Namely, Dellavigna and Kaplan (2007) find that stressing information related to terrorism appears to generate a sense of paranoia. No such effect is documented by Collier and Vicente (2011), possibly because of the very different context and nature of the treatment.

If awareness campaigns can successfully reduce the role of electoral malfeasance, this raises the question of what proportion of the population must be reached for a campaign to be successful. It is indeed onerous and, in many cases, infeasible for campaigners to visit every household. In this paper we investigate whether visiting some individuals affects other individuals as well. We do so using the same randomized field experiment as Collier and Vicente (2011). This experiment was designed not only to evaluate the average effect of the anti-violence campaign undertaken in Nigeria before the 2007 elections, but also to investigate the possible existence of peer effects of the campaign.

The experiment was organized as a randomized controlled trial. Pairs of selected locations (urban neighborhoods or villages) with similar characteristics were randomly assigned, one to treatment and the other to control. In treated locations, campaigners distributed materials (pamphlets, items of clothing) bearing an anti-violence message. They also organized town meetings and theater plays ('popular theater') aiming at boosting electoral participation and at discouraging people from voting for politicians who promote or condone electoral violence. Control locations were not visited by campaigners.

Within each treated or control location, a representative sample of 50 individuals (one per household) was randomly selected and surveyed before and after treatment. The experiment was designed so that, in treated locations, individuals surveyed at baseline were subsequently visited at their homes by the campaigners, who gave them campaign materials and invited them to attend the town meeting and popular theater. We call this sample the targeted individuals because they were the only individuals explicitly targeted by campaigners. In treatment locations we also surveyed, after the campaign was over, a randomly selected sample of individuals (one per household) who self-reported not having been visited by campaigners. We call these individuals the untargeted. Note that this group was randomly selected only if: (i) campaigners followed their protocol rigorously, i.e., they did not approach any other individuals beyond the targeted, and (ii) individuals remembered and reported correctly whether campaigners approached them. We have no way of fully verifying either of these. In any replication of this study, it would be better to draw both targeted and untargeted individuals in a random fashion from the beginning of the experiment, without relying on self-reports to code whether they were targeted by campaigners or not. We discuss in the paper how we deal with potential self-selection into the untargeted group. Individuals in control locations are referred to as control individuals. Within each control and treated location, we collected information about social links and geographical proximity between individuals. Social proximity is measured by kinship (i.e., family ties) and the frequency of social interaction (i.e., chatting). In the conclusion we discuss various ways in which the experimental design could be improved.

We are interested in the effect that a house call by campaigners to one individual, say i, has on another individual, say j, and whether this effect is stronger if i and j are close in a social or geographical sense. We distinguish between two types of effects, depending on whether j was himself/herself visited by campaigners or not. If both individuals i and j were visited by campaigners, we test whether the effect of treatment on j is stronger when j is closer, in a social or geographical sense, to other targeted individuals. We call this a reinforcement effect since it reinforces the effect of targeted treatment (i.e., house visit) on j. To test for the presence of a reinforcement effect, we observe whether, relative to controls, the effect of the campaign on the perceptions and behavior of targeted individuals is reinforced by proximity to targeted individuals in the same location.

If individual j was not visited by campaigners, j may nevertheless have experienced an indirect effect of the campaign compared to individuals in control locations. We test whether the effect of the campaign is stronger if j is socially or geographically close to targeted individuals. We call this a diffusion effect since it diffuses the effect of the campaign to untargeted individuals. To investigate diffusion effects we test whether, compared to controls, untargeted individuals show stronger effects of the campaign when they have closer social ties to targeted individuals in their location.

Collier and Vicente (2011) show that the campaign had a significant effect on decreasing the intensity of actual violence reported by independent journalists. Furthermore, in terms of homogeneous (average) effects of the campaign on individual-level outcomes, it is found that perceptions of violence were generally diminished, both in terms of targeted vs. control and in terms of untargeted vs. control groups. Behavior was altered for targeted vs. control only: Collier and Vicente (2011) observe higher levels of turnout, of voting for incumbents, and of empowerment to counteract violence, as a result of the anti-violence campaign. The bottom line is that the campaign was able to reduce perceptions of violence for both targeted and untargeted individuals, but was only able to affect the voting behavior of individuals directly targeted by the campaign.

In this paper, we find evidence of both reinforcement and diffusion heterogeneous effects. For reinforcement, we find a robust effect on decreasing respondents' perceptions of violence. What seems to matter most is kinship but geographical proximity is also significant. We observe some albeit limited reinforcement effect on behavior through chatting and kinship. For diffusion, we find robust effects on perceptions of violence and on voting behavior using a variety of estimation methods. The pattern is similar to reinforcement: kinship ties and geographical proximity to targeted individuals reduce respondents' perception of violence. Chatting and kinship ties to targeted individuals are associated with significant effects on behavior. Overall, the magnitude of estimated coefficients is similar across reinforcement and diffusion. Kinship ties were particularly effective in spreading the effect of the campaign. For instance, reinforcement and diffusion of the campaign through kinship ties led to a decrease in respondents' perceptions of political freedom and violence by 0.21-0.23 standard deviations (for an individual with average kinship). This compares to a homogeneous treatment effect of 0.34-0.39 standard deviations.

Taken together, the results indicate that geographical proximity to targeted households reduces primarily perceptions of violence. This suggests that proximity to targeted individuals increased the visibility of the campaign, possibly through the pamphlets and clothing bearing the anti-violence message that targeted individuals received. Social proximity, in contrast, appears to have been useful in spreading the more complex parts of the campaign relative to collective action since it affected behavior associated with empowerment and voting. Since network links were not experimentally assigned, we cannot completely rule out the possibility that proximity variables may be correlated with unobservables that affect susceptibility to treatment. This is a problem that affects much of the existing literature.

Our estimation of network effects in the context of a randomized field experiment relates to a recent body of literature on the role of networks in aid interventions. Kremer and Miguel (2004) launched this literature by estimating externalities of a deworming school-based program in Kenya. They estimated the impact of the treatment on control populations. Because their design features program randomization at the school level, it did not allow for an experimental estimation of individual externalities within treated schools. More recently, Angelucci, De Giorgi, Rangel, and Rasul (2010) extend the study of externalities to a conditional cash transfer program. By exploring a rich set of outcomes at the household level they are able to throw some light on specific mechanisms by which unexposed households are influenced by treatment. These authors, however, do not use explicit network information. Also in the context of a conditional cash transfer program, Macours and Vakis (2008) introduce explicit interaction among households but focus on reinforcement effects only. Angelucci, De Giorgi, Rangel, and Rasul (2010) extend the analysis to diffusion but limit their analysis to kinship links. The work by Nickerson (2008) relates closely to our study: his focus is on using randomized get-out-the-vote house visits to identify peer-effects in two-member households. Recently, Gine and Mansuri (2011) estimate spillovers of a get-out-the-vote campaign in Pakistan using geographical data. Our result that kinship proximity is more important than other measures of social interaction is similar to the results of Bandiera and Rasul (2006) who study technology adoption in Mozambique in a non-experimental setting.

The paper is organized as follows. In Section 2 we provide a description of the context in which our study takes place. Treatment, measurement, and testing strategy are presented in detail in Section 3. Subsequently, in Section 4, the empirical results are presented. We start by analyzing balance and the homogeneous effects of the campaign before focusing on reinforcement and diffusion effects. Section 5 concludes.

2. Context

Nigeria, the most populous country in Africa with an estimated population of 148 million inhabitants in 2007[1], has been challenged by persistent development problems. Despite holding the largest proven oil reserves in Sub-Saharan Africa (10th largest in the world[2]), Nigeria ranked 150 in 190 countries in terms of GDP per capita in 2007.[3] Moreover, it has been seen as an example of bad governance and has continuously featured among the most corrupt countries in the world. In the words of Chinua Achebe Achebe (1983), 'the trouble with Nigeria is simply and squarely a failure of leadership'.

From independence in 1960, Nigeria faced enormous political instability and, for most of the time, military rule. The breaking point came in 1999 when a new constitution was passed and civilian rule was

[1] World Development Indicators, 2009.
[2] Oil & Gas Journal, 103(47), December 19th, 2005.
[3] Using 1979 USD PPP, World Development Indicators, 2009.

adopted. Elections were successfully held in 1999, 2003, and 2007. However, these elections were affected by many instances of electoral misbehavior. Most observers have described these elections as being far from 'free and fair'.

We focus on the April 2007 national elections which covered all federal (president, senate, and federal house of representatives) and state-level (governors and state assemblies) political bodies. The presidential election was highly anticipated because it marked the first transfer of power from one civilian to another. Olusegun Obasanjo was stepping down as president due to a two-term limit. The main presidential contestants were Umaru Yar'Adua from the Peoples Democratic Party (PDP), Muhammadu Buhari from the All Nigeria Peoples Party (ANPP), and Atiku Abubakar from the Action Congress (AC). Yar'Adua was seen as a protégé of Obasanjo, and was clearly the front-runner due to the overwhelming influence of the PDP as ruling party. Buhari had been the main challenger in 2003, was strongly associated with the Muslim North, and had an anti-corruption track-record. Abubakar, the vice-president of Obasanjo, was a former customs official with controversial sources of wealth, and was very much on the news because of corruption accusations that almost impeded him from running. He had to switch to AC due to a conflict with Obasanjo.

PDP won the 2007 elections: Yar'Adua secured 70 percent of votes, and PDP candidates won 28 out of the 36 gubernatorial races. The elections were seriously marred by ballot-fraud and violence. Electoral observers, most notably the European Union mission and the Transition Monitoring Group (which deployed 50,000 observers), were unanimous in underlining numerous irregularities in the voting process. Both stated that the elections were not credible and fell far short of basic international standards. Human Rights Watch, in a report released in May 2007, writes

> '[...] 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 [on the election days], the body count had surpassed 300'.[4]

According to Human Rights Watch, much of the violence originated from marginalized political

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

groups.[5] It manifested itself partly in the form of assassination of known politicians. Most electoral violence, however, took the form of widespread vandalism and physical intimidation directed at voters. The violence was usually conducted by armed gangs recruited among unemployed youth from the same or a nearby community. This is the context in which we ran the field experiment that we now describe.

3. Experimental design

3.1. The campaign

In anticipation of the 2007 elections, ActionAid International Nigeria (AAIN) launched a nationwide campaign against electoral violence. AAIN is the local chapter of a major international NGO specializing in community participatory development. It is a well established NGO with an extensive field infrastructure.

The campaign was designed to induce experimental subjects to resist voter intimidation. The main mechanism was to lower the perceived threat to individual voters through collective action. The theoretical foundation for this approach is Kuran (1989)'s model of political protest. According to this theory, people who dislike their government hide their desire for change as long as the opposition seems weak, but are willing to express it when the opposition appears stronger. It predicts that an incumbent may incur a fall in support following a slight surge in the opposition's apparent size, for instance caused by a small event such as a public call for protest. AAIN's campaign is analogous as a public call for protest. In addition to trying to lower the perceived threat to individual voters, the campaign also emphasized the lack of legitimacy of the use of intimidation.

Based on this, we expect the campaign to increase voter turnout and to cause voters to remove their support from political candidates perceived as encouraging electoral violence. Hence, AAIN's campaign is expected to reduce the effectiveness of violence and intimidation as an electoral strategy. We may then observe a decline in the actual violence and intimidation instigated by politicians.

[5] Human Rights Watch, 'Criminal Politics: Violence, 'Godfathers', and Corruption in Nigeria', October 2007. 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, known to have had links to major political figures in past elections. 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 outside the three main parties, PDP, AC, and ANPP ('A Nigerian Perspective on the 2007 Presidential and Parliamentary Elections,' August 2007).

AAIN's campaign was implemented over a two-week period approximately two months before the election. AAIN worked with local state-level partner NGOs who conducted the campaign activities in the field.[6] The campaign was organized around a slogan opposing electoral violence: 'No to political violence! Vote against violent politicians.' A poster from the campaign is shown in Figure 1. The campaign slogan was also written on distributed materials.[7] These are the same means of campaigning as those used by Nigerian politicians to licitly spread awareness about their candidacy. The campaign also included roadshows featuring jingles in Yoruba, Hausa, and Pidgin English, the main languages spoken in Nigeria.[8]

AAIN did not simply rely on the distribution of these materials for impact. The campaign was designed to work mainly through the holding of town meetings and popular theater. The town meeting provided an opportunity for voters to meet with local representatives to discuss ways of counteracting politically motivated violence. The purpose of these meetings was to minimize the collective action problem faced by those seeking to reduce political violence in their community. The popular theater followed the same basic script in all states. It featured one good and one bad politician, with the bad politician relying on violent intimidation. It targeted youths and all those not attracted by the town meetings. There was at least one town meeting and one popular theater in each treated location.

3.2. Sampling

The sampling frame for the experiment is a large representative sample of all 36 states of Nigeria drawn by Afrobarometer (http://www.afrobarometer.org/) for their pre-2007 election survey. The Afrobarometer sample includes 301 enumeration areas (EAs) randomly selected from the population census using population weights.

Sample selection for our study proceeded in three steps. First, we chose two states in each of the three main regions of the country (Southwest, Southeast, and North), based on their recent history of

[6] A comprehensive report on the campaign is available at http://www.iig.ox.ac.uk/research/08-political-violence-nigeria/. It includes photographs, films, and reports of campaign activities in each state.

[7] AAIN reported the distribution of large quantities of these materials in each covered campaign location: T-shirts (3,000); caps (3,000); hijabs for Muslim women (1,000); leaflets (5,000); posters (3,000); and stickers (3,000).

[8] A roadshow consisted in a vehicle circulating in treated locations while displaying posters of the campaign and playing campaign jingles.

political violence.[9] This process led to selecting the states of Lagos and Oyo (Southwest), Delta and Rivers (Southeast), and Kaduna and Plateau (North). These states are well suited to our emphasis on studying violence, while taking into account the basic ethnic structure of the country – Yoruba in the Southwest, Igbo in the Southeast, and Fulani/Hausa in the North.

Second, we selected 24 of Afrobarometer's EAs in the six selected states as follows. We began by organizing EAs in pairs by identifying those that were close to each other geographically and were similarly classified in the census as either 'large urban,' 'small urban,' or 'rural'. We then randomly selected 12 pairs of EA's (two in each state), randomly assigning one to treatment and the other as control – see Figure 2.

Third, we selected surveyed individuals within each of the 24 selected EAs. For baseline respondents, who constitute our main sample, we use random representative sampling within each EA. The baseline survey was performed in collaboration with Afrobarometer and our Nigerian partner Practical Sampling International (PSI) and took place from January 20 to February 3, 2007. Individuals within each EA were selected randomly using Afrobarometer's standard methodology.[10] 1,200 individuals were interviewed during the baseline survey – 50 per EA. The same individuals were re-surveyed after the electoral results had been publicized and a sense of political normalcy was re-established. The post-election survey, also conducted with PSI, took place from May 22 to June 5 and reached 1,149 or 96 percent of the baseline respondents. We also surveyed a second, smaller, sample, the selection of which is described below. Individuals in this sample were only administered the post-election survey.

[9] We used reports by Human Rights Watch, ActionAid International, and other independent sources for information on historical levels of political violence. See for instance Human Rights Watch, 'Testing Democracy: Political Violence in Nigeria,' April 2003, 'Nigeria's 2003 Elections: the Unacknowledged Violence,' June 2004.

[10] Enumerators were instructed to start from the center of the EA and to proceed walking in different directions. Each n'th house was visited. For each EA the number n was set to ensure an equal likelihood of visit to all houses within the EA, based on the number of houses and enumerators in the EA. 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 substituted by the next adjacent house. This happened in 24 percent of the cases.

Despite being a standard sampling technique, this method has imperfections. It may cluster interviews in specific directions (e.g., along main roads), which may increase intra-cluster correlation of errors. It also may lead to oversampling close to the center of the EA.

3.3. Assignment to treatment

Within each of the 12 pairs of EAs, one EA was randomly assigned to be visited by AAIN campaigners. The other was assigned to control and was not visited by campaigners. The campaign took place shortly after the baseline survey was completed.

In each treated EA, campaigners were instructed to target baseline respondents, not only in terms of distribution of materials, but were also instructed to invite respondents to attend the town meeting and the popular theater.[11] Although we have gathered information on compliance with treatment, this information is not used here to avoid self-selection bias. Throughout the analysis we regard baseline respondents as assigned to treatment irrespective of whether they were actually reached by campaigners, accepted the campaign materials, or attended the campaign events. Consequently, our analysis is measuring 'intent-to-treat' effects.

In the post-election survey, we also interviewed 300 additional individuals (one per household) in treated EAs – 25 per EA. Similar to the selection of baseline respondents, enumerators were instructed to visit each n'th house (with n depending on number of houses in the EA) along a number of directions (departing from the center of the EA). Any houses corresponding to the baseline sample, which were known to the team conducting the post-election survey,[12] were substituted by the next adjacent houses. After identifying a representative member of the household, they first asked the respondent whether he/she had been directly and individually approached by AAIN campaigners. If he/she said yes, he/she was not included in the survey and the enumerator moved to the next house. This group of respondents was then selected to be only representative of 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 this sample is to estimate the effect of the campaign on the untargeted individuals in treated locations. How this is achieved is discussed in the estimation strategy section.

[11] To ensure correct site identification, one campaign representative accompanied the survey team during the baseline survey. The addresses of baseline respondents were shared with AAIN to enable campaigners to make house calls. Importantly, the surveys and the campaign were fully independent, with distinct field teams and branding.

[12] The post-election visit to the baseline sample and the post-election visit to the new sample were conducted in the same week, by the same team. The survey team coordinated in order to make it common knowledge where the baseline houses were.

Information on compliance from the post-election survey indicates that 47 percent of the baseline households participated to at least one campaign event – i.e., town meeting or popular theater. Individuals who attended the town meetings and popular theater were not statistically different from other baseline individuals in terms of demographic characteristics, except that some ethnic groups and lower income individuals were more likely to attend. The large majority of targeted individuals recalled the AAIN campaign: 88, 89, 86, and 84 percent remembered the distribution of materials, the roadshows, the town meetings, and the popular theater, respectively.

The campaign may have reached individuals other than baseline respondents. This is despite the fact that campaigners were told to only approach (directly and individually) 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 meeting and popular theater, which are central to the campaign from a theoretical standpoint, were held at specific venues and were only publicized to baseline respondents through personal invitation. This is consistent with post-election survey data on the untargeted. The percentage of untargeted respondents who report having attended campaign events was 4 percent, compared to 47 percent among the targeted.

3.4. Outcome measures

The analysis presented in this paper rests on two types of individual outcome measures: responses to survey questions, and a behavioral measure of empowerment. Collier and Vicente (2011) also test the effect of treatment on indicators of electoral violence based on EA-specific diaries compiled by local journalists. These indicators are not used here since the focus is on heterogeneous effects, for which we need individual-level data.

The surveys asked questions on individual perceptions and experience of violence and on individual voter behavior. 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 Likert scale. Voter behavior in the April 2007 elections is reported by respondents in the post-election survey. In addition, all post-election respondents – targeted, untargeted, and control – were asked about their social links to each of the 50 baseline individuals.[13] An approximate map of each surveyed EA was also drawn with the location of each respondent's residence.

A behavioral measurement of voter empowerment was implemented in our post-election survey as follows. All respondents were given a pre-stamped postcard which they could choose to mail or not. On the card was a message demanding that more attention be paid to countering voter intimidation in the respondent's state. The postcard was addressed to the organizations involved in the experiment, who promised to raise media awareness about voter intimidation in states where enough postcards were sent. To post the card, the respondent had to make the effort of going to a post office. Our assumption is that respondents were more likely to incur this cost if they had a stronger sense that intimidation could be countered. Sending the postcard is thus an incentive-compatible measure of voter empowerment, i.e., of the sense that 'something can be done' about voter intimidation.

3.5. Estimation strategy

Our empirical approach is based on reduced form specifications. We proceed as follows. Let y_{ilt} denote a relevant outcome variable for individual i in location l at time $t = \{0, 1\}$ where 0 stands for baseline and 1 for post-election data. Further let $T_l = 1$ if location l was selected for treatment. The average treatment effect – i.e., the homogeneous effect – of the campaign is coefficient α in the following regression:

$$y_{il1} = \delta + \alpha T_l + e_{il1}, \tag{3.1}$$

or, equivalently, in:

$$y_{ilt} = \delta + \beta T_l + \gamma t + \alpha T_l t + e_{ilt}, \tag{3.2}$$

if we also use baseline data.

Given random assignment to treatment, α in either of these equations provides a consistent estimate of

[13] Because this part of the questionnaire requires knowing the name of other sampled individuals in each EA, the sampling-cum-survey method used for untargeted individuals made it impossible for them to be listed in the social links questionnaire.

the homogeneous effect of the campaign. Because of the small sample size, however, it may be preferable to include individual fixed effects u_i, which also control for time-invariant location unobservables:

$$y_{ilt} = \delta_i + \gamma t + \alpha T_l t + e_{ilt} \tag{3.3}$$

Time-invariant regressors drop out of equation (3.3) after inclusion of the fixed effects. Estimating equation (3.3) by ordinary least squares yields the standard difference-in-difference estimator. Equivalently, (3.3) can be estimated in first-differences:

$$\Delta y_{ilt} = \gamma + \alpha T_l + \Delta e_{ilt} \tag{3.4}$$

In this paper we are not primarily interested in the homogeneous effect of the campaign, which is discussed in detail in Collier and Vicente (2011). This effect can be decomposed into a direct effect – that effect stemming from the visits by door-to-door campaigners – and an indirect effect – induced by the public visibility of the campaign or by contact with those visited by door-to-door campaigners. When comparing targeted individuals in treated locations to control individuals, α in equation (3.3) or (3.4) measures the combined direct and indirect effect of the campaign, i.e., the average effect of being visited by campaigners plus the average indirect effect resulting from the campaign. When comparing untargeted individuals in treated locations to control individuals, α measures the average diffusion (spillover) effect of the campaign, since by design there is no direct effect on untargeted subjects (i.e., they were not visited by campaigners).

We are particularly interested in studying the indirect effects of the campaign. However, not all indirect effects can be ascribed to social networks. As mentioned earlier, untargeted individuals in treated locations may have seen the roadshows or sought to have campaign materials. The basis of our strategy for identifying network effects relies on the idea that, if the campaign operates at least partly through social networks, then the indirect effect of the campaign will be stronger on respondents who are more closely connected to targeted individuals.

We are interested in the effect that a house call by campaigners to one individual, say i, has on another individual, say j, and whether this effect is stronger if i and j are close in a social or geographical sense. We distinguish between two types of effects, depending on whether j was itself visited by campaigners or not. If both individuals i and j were visited by campaigners, we test whether the effect of treatment on j is stronger when j is closer, in a social or geographical sense, to other targeted individuals. We call this a reinforcement effect since it reinforces the effect of targeted treatment (i.e., house visit) on j.

If individual j was not visited by campaigners, j may still display an indirect effect of the campaign compared to individuals in control locations. We test whether the effect of the campaign is stronger if j is socially or geographically close to targeted individuals. We call this a diffusion effect since it diffuses the effect of the campaign to untargeted individuals.

Formally, let g denote a social network matrix where $g_{ij} = 1$ if i is linked to baseline individual j, and 0 otherwise. Given that all respondents in one EA are asked about the same 50 targeted individuals, we cannot distinguish whether influence comes from the number or the proportion of treated neighbors. Therefore, without loss of generality, we take as network variable the proportion of targeted individuals to whom i is directly linked, i.e., $\widetilde{n}_i \equiv \frac{1}{50} \sum_{j=1, j\neq i}^{50} g_{ij}$.

If we use only second round data, the estimated model takes the simple form:

$$y_{il1} = \delta + \alpha T_l + \tau n_i + \theta T_l n_i + e_{il1} \tag{3.5}$$

where we interact treatment with the demeaned value $n_i \equiv \widetilde{n}_i - \frac{1}{N} \sum_{j=1}^{N} \widetilde{n}_j$ (where where N is total sample size) of the network measure $\widetilde{n}_i$. The advantage of demeaning interaction variables is that coefficient α can still be interpreted as the average treatment effect – see Wooldridge (2002) for details. The parameter of interest is θ: if it is significant and positive, this can be taken as evidence of a stronger effect of the campaign on respondents who are socially linked to targeted individuals.

Regression model (3.5) is best understood as derived from a general model of network effects as follows. Consider a treated location and the network effect of the campaign on individual i. Let $x_k = 1$ if another individual, say k, was targeted by the campaign and let d_{ik} be the network distance between i and k.[14]

[14] The network distance is the shortest path between two nodes. For instance, if i is linked to j who is linked to k, the distance between i and k is 2. Distance is assumed infinite if i and k are unconnected, that is, if there is no path in the network linking the two nodes (Jackson 2009).

Let ϕ_{ik} denote the effect of the campaign on individual i that stems from targeting campaign activities towards individual k. We have:

$$\phi_{ik} = h(1) g_{ik} x_k + \sum_{s \neq 1} I(d_{ik} = s) h(s) x_k \tag{3.6}$$

where s indexes network distance and $I(d_{ik} = s)$ is an indicator function equal to 1 if $d_{ik} = s$ and 0 otherwise. The first term in (3.6) is the effect of being linked to a targeted individual directly; the second term is the net effect of being indirectly linked to k through others, some of whom were surveyed, some of whom were not. We assume that $h(s)$ falls with network distance s and that $h(\infty) = 0$ – individual i is not influenced by k if he/she is not linked, directly or indirectly, to k. These assumptions are for instance satisfied if $h(s)$ is the commonly used decay function θ^{-s} with $0 < \theta < 1$. It follows that ϕ_{ik} depends negatively on the network distance d_{ik} between i and k: the more distant i and k are, the smaller the effect.

Now we average ϕ_{ik} over all 50 targeted individuals k to get the combined network effect on i. Consider the first term of (3.6). Since $x_k = 1$ only for targeted individuals in treated locations, averaging $g_{ik} x_k$ over k yields $\widetilde{n}_i$, the proportion of targeted respondents to whom i is directly linked. The total network effect ϕ_i in treated locations can thus be written as:

$$\phi_i \equiv \frac{1}{50} \sum_{k=1, k \neq i}^{50} \phi_{ik} = h(1) \widetilde{n}_i + \frac{1}{50} \sum_{k=1, k \neq i}^{50} \sum_{s \neq 1} I(d_{ik} = s) h(s) \tag{3.7}$$

In control locations, no one was targeted by the campaign, hence $\phi_i = 0$ by design.

Regression model (3.5) seeks to test whether the network effect ϕ_i is different from 0 in treated villages for targeted and for untargeted individuals. Equation (3.7) shows that ϕ_i is an increasing function of $\widetilde{n}_i$ through $h(1)$. It is also likely that $\widetilde{n}_i$ is positively correlated with the second term: individuals with more direct links to targeted individuals probably have larger networks on average, and thus smaller network distance to other targeted individuals. Since each ϕ_{ik} falls with distance, this raises ϕ_i on average. In contrast, if $\phi_i = 0$ then the effect of treatment does not depend on $\widetilde{n}_i$.

The presence of network effects can thus be investigated by testing whether the effect of treatment is stronger among individuals with a larger $\widetilde{n}_i$, i.e., whether $\theta > 0$ in (3.5).[15] Although this approach does not allow estimating function $h(s)$, it offers the advantage of not making any assumption regarding its specific functional form:[16] if we find that $\widehat{\theta} > 0$, this constitutes evidence of network effects for any positive correlation between $\widetilde{n}_i$ and $\frac{1}{50}\sum_{k=1,k\neq i}^{50}\sum_{s\neq 1} I(d_{ik}=s)h(s)$.[17]

If we include baseline information, the estimated model takes the form:

$$\begin{aligned} y_{ilt} &= \delta + \beta T_l + \gamma t + \alpha T_l t + \varphi n_i \\ &\quad + \lambda T_l n_i + \tau t n_i + \theta T_l t n_i + e_{ilt} \end{aligned} \tag{3.9}$$

Expressing the equation in first difference to get rid of individual fixed effects, we obtain:

$$\Delta y_{ilt} = \gamma + \alpha T_l + \tau n_i + \theta T_l n_i + \Delta e_{ilt} \tag{3.10}$$

We also seek to test whether indirect effects depend on geographical proximity $\widetilde{p}_{ij}$ between i and j. We set $\widetilde{p}_{ij}$ equal to minus the distance between i and j. Influence then depends on how physically close respondent i is to those targeted by campaigners. Let $\widetilde{p}_i = \frac{1}{K}\sum_{j=1}^{K}\widetilde{p}_{ij}$, where K is the number of respondents in the same EA. Like before, the variable we use is the demeaned equivalent $p_i = \widetilde{p}_i - \frac{1}{N}\sum_{j=1}^{N}\widetilde{p}_j$ where N is total sample size. We reestimate models (3.5), (3.9) and (3.10) with $\widetilde{p}_i$ and p_i in

[15] For individuals in treated locations we have

$$E[\phi_i|\widetilde{n}_i] = h(1)\widetilde{n}_i + E\left[\frac{1}{50}\sum_{k=1,k\neq i}^{50}\sum_{s\neq 1} I(d_{ik}=s)h(s)|\widetilde{n}_i\right].$$

If the second term does not depend on $\widetilde{n}_i$, then $p\lim\widehat{\theta} = h(1)$: the coefficient of $T_l n_i$ in (3.5) is a consistent estimate of the direct network effect $h(1)$. If $\widetilde{n}_i$ is positively correlated with $\frac{1}{50}\sum_{k=1,k\neq i}^{50}\sum_{s\neq 1} I(d_{ik}=s)h(s)$, let us write the linear projection $L(.)$ of the latter on $\widetilde{n}_i$ as:

$$L\left[\frac{1}{50}\sum_{k=1,k\neq i}^{50}\sum_{s\neq 1} I(d_{ik}=s)h(s)|\widetilde{n}_i\right] = a + b\widetilde{n}_i + v_i \tag{3.8}$$

with $L(v_i|\widetilde{n}_i) = 0$ by construction. We now see that $p\lim\widehat{\theta} = h(1) + b > h(1)$: the coefficient of $T_l n_i$ in (3.5) captures the direct network effect as well as b, the indirect network effects correlated with $\widetilde{n}_i$. Indirect network effects uncorrelated with $\widetilde{n}_i$, i.e., term a in (3.8), are captured by α, the average treatment effect.

[16] Estimating $h(s)$ would be difficult with our data: since we do not observe links that respondents have with non-respondents, network distance d_{ik} computed from the sample is mismeasured, and estimates of $h(s)$ from incomplete network data are known to be biased – see Chandrasekhar and Lewis (2012).

[17] In the extreme case where there is no decay in effect with network distance, i.e., when $h(s) = \overline{h}$ for all s, the total peer effect ϕ_i is the same for everyone in the same component (i.e., connected part of the network). If in addition all respondents in an EA belong to the same component, then ϕ_i is not a function of n_i and θ cannot be identified. In other words, if the diffusion of the campaign message along social networks is too rapid and too strong, our test will erroneously conclude the absence of network effects. The test should thus be seen as conservative.

lieu of $\widetilde{n}_i$ and n_i.

To test for the presence of a reinforcement effect associated with social or geographical proximity, we compare targeted to control respondents using models (3.5), (3.9) and (3.10). In these regressions, θ measures the extent to which the effect of the treatment T_l on the outcome variable y_{ilt} is magnified by proximity to other individuals targeted by the anti-violence campaign. To test whether the campaign affects, through social or geographical proximity, individuals who did not receive the campaign message directly, we compare untargeted to control respondents. In this case, θ can be regarded as measuring diffusion of the campaign through social or geographical proximity.

The comparison between targeted and control respondents poses no particular problem: selection into the sample was random and representative for both groups; hence, campaigner visits to targeted households were also randomly allocated. Comparability between untargeted and control respondents is more prone to self-selection: in other words, being selected into the untargeted sample is more likely to be correlated with respondent characteristics that also affect the outcome variable y_{ivt}.

There are two potential sources of bias. First, campaigners may have approached individuals other than baseline respondents, contrary to instructions in the campaign protocol. While we cannot entirely rule it out, this source of bias is probably small in our data due to campaigner incentives.[18] The second source of self-selection is response bias: respondents may have mistakenly reported whether they had been 'directly and individually approached' by the campaign team. It is likely that respondents mistakenly reported that they were approached by campaigners - for instance because they confused being approached by campaigners with approaching campaigners of their own initiative, which is a subtle distinction. Another possibility is that respondents mistakenly reported that they were not approached by campaigners (this would imply the first potential source of bias as well, i.e., that campaigners approached individuals other than baseline respondents) For these reasons, we investigate the sensitivity of our diffusion results to the possibility of selection on unobservables.

It is important to note that if (i) campaigners approached individuals other than baseline respondents (contrary to campaign protocol), and (ii) these specific individuals self-reported not being targeted by

[18] If campaigners did not target individuals other than baseline respondents, then, since baseline respondents were selected at random, any sample randomly selected among the remaining households should also be representative.

campaigners (because of imperfect recall or because they did not want to tell the truth) and entered our untargeted sample, an upward bias would be produced for the effects on the untargeted (in case the effects on the targeted are positive). This could shift reinforcement effects to what we label diffusion effects.

We use ordinary least squares in all our main regressions. Since observations are clustered by EA, we need to allow for within-group dependence. One possibility is to report clustered standard errors at the EA level (e.g., Moulton (1990)), as we do. The reader may however worry that inference with cluster-robust standard errors relies on the assumption that the number of clusters goes to infinity for their asymptotic justification. Bertrand, Duflo, and Mullainathan (2004) show that, with a small number of clusters, cluster-robust standard errors calculated using the Huber-White formula are likely to be downward biased. We therefore also report the p-values of relevant coefficients using the wild bootstrap approach proposed by Cameron, Gelbach, and Miller (2008).[19]

4. Empirical results

4.1. Balance

We begin by comparing targeted, untargeted, and control respondents for a wide range of observable characteristics to check whether the selection of respondents was successful in identifying comparable groups.

In Table 2a we compare respondents in terms of location and individual demographic characteristics. We find no statistically significant differences between treatment and control groups in terms of location characteristics. We also find no significant differences between targeted and control groups for individual demographic characteristics. Overall this is evidence that the randomization of the campaign was effective in identifying comparable groups of targeted and control respondents. However there are some differences between untargeted and control individuals that are significant at the 10 percent level.

[19] 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.

Although untargeted respondents do not differ from control individuals on most dimensions, they appear to be more educated, more religious, and more likely to own a radio. This suggests possible selection into the untargeted sample on the basis of these variables. A possible explanation is reporting bias: more 'average' (i.e., less schooled, less religious, poorer) respondents may have reported being targeted by campaigners when in fact they approached the campaigners themselves in order to obtain T-shirts and other materials. As a result they may have been omitted from the untargeted sample. To correct for possible selection, we control for these demographic traits in the subsequent analysis whenever using data on untargeted respondents.

Table 2a provides complete descriptive statistics for our sample of locations and respondents. Panel attrition is not a serious concern: 97 percent of control baseline respondents also answered the post-election survey; the corresponding percentage for treated locations is 95 percent.

In Table 2b we analyze the differences between comparison groups in terms of network variables and baseline outcomes. The latter include actual violence, violence-related measures reported by survey respondents, and individual electoral preferences for the 2003 elections.

Two measures of social proximity are used in this paper. For the first one, a link from i to j exists if i can identify the name of j when prompted, and i stated that he/she talks to j on a regular basis.[20] We call this variable 'chatting'. We also construct another measure of social proximity whereby a link exists from i to j if i can identify j by name and reports being related to j.[21] We call this variable kinship.[22] We display $\widetilde{n}_i$ for chatting and kinship in Table 2b. We think of these two variables as proxying for various dimensions of social proximity that are not observed. The test results presented here are not designed to provide precise identification of the exact social channel through which these effects took place – only to test whether some dimensions of social proximity picked up by our measures matter.

We also investigate the effect of geographical proximity between i and j. Each enumerator was asked to locate each respondent on an approximate EA map, and to calculate the distance between interviews. See Figure 3 for an example. To evaluate the position of each respondent on the map, we construct

[20] The question asked was 'How frequently do you calmly chat about the day events with the following individuals or members of their households? Not at all-Frequently'.

[21] The exact question used was 'Are the following individuals relatives of yours, i.e. members of your family? Yes-No'.

[22] Although we report results with both chatting and kinship, we put more weight on the kinship results given that we cannot rule out the possibility that chatting may be endogenous to the campaign.

up-down and left-right coordinates for each of them. The distance between each ij pair is then calculated from these coordinates. Because maps differ in scale, distances are re-scaled to make them comparable across all locations.[23] The result of these calculations is our variable $-\widetilde{p}_{ij}$, which is then used to compute $\widetilde{p}_i$. We display $-\widetilde{p}_i$ (average distance to targeted households) in Table 2b. This is reported in meters in Table 2b but rescaled to kilometers in all regressions to make estimated distance coefficients easier to read. Geographical proximity may proxy for social interaction with neighbors, but also for non-verbal interaction, e.g., exposure to campaign materials worn or displayed by targeted individuals. As shown in Table 2b all network measures are balanced across comparison groups. The correlation between chatting and kinship is positive (0.55) while their correlations with geographical proximity are close to 0. Note that we cannot fully rule out the possibility that the network variables are correlated with unobservable dimensions that drive our effects of interest.

Next we display in Table 2b EA-level variables on actual violence in the 2003 elections as reported by journalists.[24] We see no significant difference between treatment and control EAs. We then present individual-level variables relating to violence and voting behavior collected at baseline. We follow Kling, Liebman, and Katz (2007) and normalize 17 survey-based measures using z-scores, and then aggregate them into four indices using equally weighted averages. According to Kling, Liebman, and Katz (2007), such aggregation improves statistical power to detect effects that go in the same direction within a domain.[25] In the normalization we also change the sign of each measure so that more beneficial outcomes (less violence, more empowerment) have higher scores. Table 1 presents each individual variable with its original scale, and the way we group them to form indices. Table 2b shows averages for those variables collected at baseline, i.e., the indices for 'local electoral violence – from the top', 'local empowerment – from the bottom', and 'crime – perceptions and experience'. We do not observe any statistically significant difference between them.[26] Finally, we display in Table 2b the average electoral behavior of respondents

[23] This is accomplished by using the subset of pairwise distances, i.e., distance between interviews, reported by enumerators.

[24] Independent observers compiled diaries of violent events through the period covered by the experiment (from the second semester of 2006 to the election aftermath in May 2007). Collier and Vicente (2011) explore this data in detail.

[25] 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. As in Kling, Liebman, and Katz (2007), if an individual has a valid response to at least one component of an index, we impute missing values for other components at the group mean for the corresponding survey round.

[26] The first index of Table 1, 'political freedom and conflict - general', does not have baseline data for all components. We find statistically different values across untargeted and control respondents for one of its components that has baseline

across comparison groups in the 2003 (previous) presidential and gubernatorial elections in Nigeria. We see no significant differences between control respondents and either targeted or untargeted respondents.

4.2. Homogeneous effects

The average treatment effects of AAIN's anti-violence campaign are not the focus of this paper but are explored in detail in Collier and Vicente (2011). We nevertheless start by briefly presenting the homogeneous effects for comparability with the heterogeneous effects that follow.

Collier and Vicente (2011) find that AAIN's campaign reduced actual violence as reported by independent journalists. Namely, the campaign led to a 47 percent reduction in reports of physical violence. This is the ultimate impact of the campaign, which was aimed at undermining the effectiveness of intimidation as an electoral strategy for local politicians.

For ease of comparability we report full regression results from Collier and Vicente (2011) for the outcomes of interest in our paper. These homogeneous effects are presented in columns 1 and 5 of Tables 3, 7, 8 and 9 and in columns 1,2, 9, and 10 of Tables 4 to 6. In Tables 3 to 6 the dependent variables are the constructed indices reported in Table 2b. Since the indices are presented as z-scores, coefficient estimates are expressed in terms of standard deviation units, i.e., a coefficient of +1 means a one standard deviation unit increase in the index. In Tables 7 to 9 the dependent variables are binary. OLS coefficients therefore represent a change in percentage points. Cluster-robust standard errors are reported for all coefficients; wild-bootstrap p-values are reported for the homogeneous effects at the bottom of the relevant columns.

The reduction in actual violence is matched by consistent changes in respondents' subjective perceptions relating to violence. We see that the index 'political freedom and conflict – general' is 0.39 standard deviation units lower among targeted than control respondents (column 1, Table 3), and 0.34 standard deviation units lower among untargeted than control individuals (column 5, Table 3). For the index 'local electoral violence – from the top', the corresponding figures are 0.23 and 0.26 (Table 4). For the index 'local empowerment – from the bottom', the treatment effect is only significant for targeted vs. control, and corresponds to a reduction of 0.22 standard deviation units (Table 5). In contrast, the index

data available.

measuring general crime ('crime – perceptions and experience') shows no significant change associated with treatment (Table 6).

Regarding behavior, the campaign only affected targeted respondents, who were 8 percentage points more likely to send the complaint postcard (Table 7), 9 percentage points more likely to turn out for the presidential and gubernatorial elections (Table 8), and 11 percentage points more likely to vote for incumbents in the presidential and gubernatorial races (Table 9).[27]

The general interpretation in terms of individual outcomes is thus that the campaign was successful in decreasing perceived intimidation, and in generating a sense of empowerment among targeted respondents. Moreover, Collier and Vicente (2011) find significant effects of the campaign on behavior, but only for the targeted. This leaves unanswered the question of whether some untargeted individuals may nevertheless have benefitted from the campaign through their contacts or proximity with targeted individuals. We also do not know whether the effect of the campaign on targeted individuals is magnified by contact and proximity among them. To this we now turn.

4.3. Heterogeneous reinforcement effects

In this section we investigate the presence of reinforcement effects through social networks. Since we are focusing on reinforcement, we compare targeted to control respondents. Results are presented in columns 2 to 4 of Tables 3, 7, 8 and 9 and in columns 3 to 8 of Tables 4 to 6.

We begin with the four violence-related indices. For 'political freedom and violence – general' (Table 3) we estimate a single-difference model (3.5) since we do not have baseline data on all the variables composing this index. For the other three indices, we estimate difference-in-difference regressions without or with individual fixed effects, i.e., models (3.9) and (3.10). The main parameter of interest is θ, the coefficient of the interaction between treatment T_l and either social network n_i (chatting or kinship) or geographical proximity p_i. Wild bootstrap p-values for θ are reported at the bottom of the relevant table columns to check the robustness of our inference. As explained in the testing strategy section,

[27] Numerous reports emphasize that non-incumbent groups, often marginal to mainstream politics, tend to be associated with much of the electoral violence in this Nigerian election. Collier and Vicente (2011) also report a negative effect of the campaign on voting for presidential candidate Atiku Abubakar. This may be related to inflammatory declarations he made during the run-up to the election, when he was almost struck from the race.

the coefficient α of the treatment dummy T_l captures not only the direct effect of the campaign but also indirect effects that were not transmitted through the network variable we employ. All regressions without individual fixed effects – e.g., models (3.5) and (3.9) – include controls.[28]

In Table 3 we find a statistically significant θ for kinship and geographical proximity, indicating that the effect of the campaign on targeted individuals is stronger among individuals that are socially or geographically close to other targeted individuals. The magnitudes are 3.29 and 0.59 standard deviation units, and the coefficients are significant at the 5 and 10 percent levels, respectively, when employing cluster-robust inference. This means that an individual that has kinship ties to 3.5 of the 50 targeted households in the EA (i.e., 7 percent, the sample average for the control group – see Table 2b) experiences, because of the campaign, a reduction in the index of 0.23 standard deviations when compared to a targeted individual with no kinship ties to other targeted households. This is large relative to an average treatment effect of 0.39 standard deviation units. The reinforcement effect of geographic proximity is also large in magnitude: a respondent that is located on average 300 meters (0.3 kilometers, the average distance in the control group – see Table 2b) from other targeted households experiences, because of the campaign, a 0.18 standard deviation reduction in the index relative to a respondent located on average one kilometer from other targeted households. We do not find a significant effect when employing the social network variable 'chatting' (column 2). Parameter α, which represents direct effects plus other indirect effects of the campaign, remains positive and significant except in the regression concerning geographical proximity where it loses statistical significance. Parameter τ of the network variable itself does not have a robust sign across the different regressions indicating that, among control individuals, average social or geographical proximity to other baseline individuals is not associated with systematic differences in perception.[29]

Turning to the results on 'local electoral violence – from the top' (Table 4), we find a similar pattern for network heterogeneous effects. Coefficient θ is positive and significant for kinship and geographical proximity – at the 1 percent level using cluster-robust inference, slightly less but still significant using

[28] Controls are state dummies, location controls on the existence of basic public services, and individual demographic characteristics (see Table 2a, top and middle panels).

[29] Note however the negative and significant coefficient for geographical proximity. It means that for control locations, more peripheral respondents perceive less violence. Perpetrators of electoral violence may be recruited among socially isolated individuals. Indeed, we have some evidence of that: we ran regressions of survey measures of sympathy for unlawfulness on geographical proximity; we find a clear negative effect of proximity (regressions available upon request).

the wild bootstrap method. Estimates are robust across specifications with controls or with fixed effects. The magnitude of the estimates is broadly comparable to what was reported in Table 3, but slightly lower for kinship and slightly higher for proximity. Again we do not find a statistically significant interaction coefficient between treatment and chatting. Estimates of the average treatment effect α are stable across all regressions. We also observe that the magnitude of estimated coefficients is in general similar between regression models (3.9) and (3.10), a finding that is consistent with the fact that the data come from a randomized experiment so that individual characteristics – whether observable or not – should not matter.

Table 5 shows results for the index of 'local empowerment – from the bottom'. Although the average effect of the campaign on targeted individuals is significantly positive, we find no evidence of heterogeneous social or geographical proximity effects. On the index for 'crime – perceptions and experience' we find evidence (Table 6) of a treatment effect only for those individuals linked via kinship or geographical proximity to other targeted individuals. This effect is present even though the effect of the campaign is, on average, not significant. The kinship coefficient, with a magnitude of 3.73 standard deviations, is significant using both cluster-robust (at the 5 percent level) and wild bootstrap (at the 10 percent level) inference. The geographical proximity effect is significant only when using the wild bootstrap, at the 10 percent level.

We take these results as evidence of a reinforcement effect of the campaign on perceptions related to violence, and that reinforcement happens mainly through kinship and geographical proximity to other targeted individuals. Since the campaign also reduced actual violence, these findings are unlikely to be purely driven by a conformity response bias on the part of targeted respondents. We do not find evidence of reinforcement for the index of perceived empowerment, suggesting that the main effect of the campaign on perceptions of empowerment of the population is through direct exposure to treatment.

We now turn to outcomes measuring the behavior of respondents. We begin with the postcard variable, which takes value 1 in case the respondent sent the postcard (Table 7). We interpret this variable as an incentivized measure of empowerment to counteract electoral violence since the respondent should only mail the postcard if he/she believes that electoral violence can be countered. We find a significant reinforcement effect through chatting when using cluster-robust inference, but only significant at the 12 percent level with the wild bootstrap. The magnitude of the effect is large: an individual with the mean value of the chatting variable for the control group (0.04) is 5 percentage points more likely (because of the campaign) to mail the postcard compared to an individual that did not chat with any targeted households. This is the same order of magnitude as the homogenous treatment effect itself, which is close to 8 percentage points. In contrast, we find no significant heterogeneous effects for kinship or geographical proximity. Chatting with other targeted households therefore seems to encourage a manifestation of empowerment, even if it does not reinforce the effect of the campaign on violence-related perceptions.

Tables 8 and 9 display similar regressions for two voting variables: voter turnout and voting for an incumbent. We average across the presidential and gubernatorial elections for each individual. Since the original variables are binary, the variable remains bound between 0 and 1. We find no statistically significant reinforcement effect on voter turnout: estimates of θ are positive but small in magnitude and not statistically significant (Table 8). But we find a reinforcement effect through kinship on voting for an incumbent (Table 9). This effect is significant at the 5 percent level when using cluster-robust standard errors (at the 10 percent level with the wild bootstrap). The magnitude of the coefficient, 1.71, is very large: it means that an individual with the mean number of kinship ties to other targeted individuals has, because of the campaign, a 12 percentage-point higher likelihood of voting for an incumbent compared to an individual with no such ties. This compares to an 11 percentage-point average treatment effect of the campaign itself.

From this we conclude that the evidence regarding reinforcement effects on empowerment and voting behavior is less clear: we find some evidence of reinforcement on empowerment through chatting, and some reinforcement on voting for incumbents through kinship proximity to other targeted individuals. Other heterogeneous effects are not significant.

Before turning to diffusion effects on untargeted individuals, we investigate the robustness of our findings to correlations between the social and geographical proximity variables. The results above suggest the presence of reinforcement effects for kinship and geographical proximity when we use violence-related outcomes, and the presence of reinforcement effects for chatting and kinship when we employ behavior outcomes. This raises the question of which of the two proximity measures matters most for each outcome. To investigate this issue, we reestimate models (3.5) and (3.10) with both network measures. When considering social and geographical proximity variables together, the estimated regressions have the form:

$$y_{il1} = \delta + \alpha T_l + \tau_1 n_i + \theta_1 T_l n_i + \tau_2 p_i + \theta_2 T_l p_i + e_{il1} \tag{4.1}$$

$$\Delta y_{ilt} = \gamma + \alpha T_l + \tau_1 n_i + \theta_1 T_l n_i + \tau_2 p_i + \theta_2 T_l p_i + \Delta e_{ilt}. \tag{4.2}$$

We use specification (4.1) with outcomes for which we do not have baseline data, and specification (4.2) for difference-in-difference regressions.

For violence-related perceptions, we combine kinship and geographical proximity. Results are shown in the first 4 columns of Table 10. We find that kinship retains statistical significance (using both cluster-robust and wild bootstrap inference) for two indices: 'political freedom and conflict – general' and 'crime – perceptions and experience'. Geographical proximity is no longer significant. For 'local electoral violence -from the top', both interaction coefficients lose significance. For empowerment and voting behavior, we combine kinship and chatting. Results are presented in columns 5 to 7 of Table 10. Earlier findings are confirmed: reinforcement through chatting for sending the postcard, and through kinship when voting for incumbents.

Taking all the reinforcement results together, kinship comes out as the strongest, most consistent reinforcement channel – although it does not affect all outcome variables equally. It makes sense that kinship is so prominent. Kinship relationships are close and usually imply frequent interaction, particularly when both households live in the same neighborhood or village. Close interaction means mutual visibility but also frequent oral communication. It therefore does not come as a surprise that geographical proximity has a reinforcement effect on violence-related perceptions, and that chatting has a reinforcement effect on empowerment. The first may be due to observing campaign materials displayed by targeted households. To account for the second, it is possible that (i) behavior related to empowerment requires more complex communication, and/or (ii) coordination is required before engaging in behavior that is, at least potentially, observable and (as a consequence) dangerous; hence the need for oral communication and the role of chatting.

4.4. Heterogeneous diffusion effects

We now turn to social and geographical proximity effects on untargeted individuals in locations visited by the campaign – what we call diffusion. All results are displayed on the right hand side of Tables 3 to 10. The focus is on comparing untargeted and control respondents. We begin by assuming that once we condition on controls or individual fixed effects, untargeted and control individuals are comparable, i.e., there is no selection on unobservables. We investigate the possibility of selection on unobservables at the end of the section.

We begin with the four survey-based violence-related indices. As for reinforcement, we find a clear effect of kinship and geographical proximity in diffusing lower perceptions of 'political freedom and violence – general' (Table 3). Interaction coefficient estimates are significant at the 5 percent level using cluster-robust standard errors (at the 10 percent level using wild bootstrap). Their magnitudes are 3.07 and 0.53 standard deviation units, very close to the ones we estimated for reinforcement. Chatting is once again not significant. A similar pattern is estimated for the perception index of 'local electoral violence – from the top' (Table 4), but only diffusion through kinship is statistically significant. The magnitude of the effect is 1.27 standard deviation units (when including fixed effects) and is significant at the 10 percent level for both inference methods. As for reinforcement, we find no statistically significant diffusion effects for 'local empowerment – from the bottom' (Table 5). For memory, for this outcome variable we also found no significant average effect of the campaign on untargeted individuals (Table 5, columns 9 and 10). For the fourth perception index, 'crime – perceptions and experience' we find a significant diffusion effect of kinship (Table 6). The effect is large, i.e., 3.29 standard deviations (with fixed effects) – and is significant at the 5 or 10 percent level, depending on whether we use clustered standard errors or wild bootstrap. From this we conclude that kinship is particularly important for the diffusion of the effect of the campaign on the perceptions of untargeted individuals, and that diffusion effects largely mirror reinforcement effects.

Next we turn to measures of behavior. We start with the mailing of the postcard. As for reinforcement, we find a significant diffusion effect through chatting (Table 7). With a coefficient of 1.26, the effect is once again large in magnitude, and it is statistically significant at the 5 percent level. We also observe a significant diffusion effect associated with kinship, albeit the magnitude of the effect is smaller (0.39) and the coefficient is significant at the 10 percent level. Unlike in the case of targeted individuals, voter turnout shows clear chatting and kinship diffusion effects on untargeted individuals (Table 8). Note that individuals untargeted by campaigners show no average effect of the campaign itself on turnout: α is not statistically different from zero. The diffusion coefficients θ are 0.27 and 1.6 for chatting and kinship, respectively, with statistical significance at the 5 percent level using cluster-robust inference (10 percent level for wild bootstrap). These effects translate into 1 and 13 percentage-point increases in voter turnout, respectively through chatting and kinship (for the respective average network links in the control group). Finally, voting for incumbents again shows chatting and kinship effects (Table 9). This is true even though the average effect of the campaign on untargeted individuals' voting for incumbents is not significant. The chatting diffusion effect (0.19) is only significant when employing the wild bootstrap method (at the 10 percent level). The kinship effect (1.8) is significant at the 5 percent level with either method. An untargeted individual with the average number of kinship links is, because of the campaign, 12 percentage points more likely to vote for an incumbent when compared to an individual with no kinship links. Looking at these results on behavior, we can conclude that both chatting and kinship are important channels of diffusion. Unlike in the case of reinforcement, we find network diffusion effects on all our behavior outcomes.

In Table 10 we reproduce the diffusion results combining network variables, to help disentangle which specific network effect dominates. Results are presented in columns 8 to 14 of Table 10. As for reinforcement, kinship remains statistically significant for most outcomes – namely: for the indices of 'political freedom and conflict – general' and 'crime – perceptions and experience'; and for voting for the incumbent. For the postcard, chatting remains significant according to the wild bootstrap p-value.

To summarize, we find diffusion effects on perception indices to be similar in significance and magnitude to reinforcement effects, but diffusion effects to be stronger on behavior. The magnitude of the estimated effects is often large, especially for kinship ties.

Before concluding, we further investigate the robustness of these findings to the possibility of self-selection. As explained earlier, untargeted respondents were identified after the campaign among individuals that had not been directly targeted by campaigners. In Table 2a we noted that untargeted and control respondents differ along certain dimensions, raising the possibility of selection bias. So far we have dealt with this possibility by including additional controls or fixed effects. But this cannot correct for all sources of selection bias. We therefore subject the diffusion regressions to additional robustness checks.

We begin by checking whether the homogeneous effects of the campaign on untargeted respondents are affected by the use of linear regressions. To this effect, we reestimate the average effect of the campaign on the untargeted using a matching method. This approach ensures that untargeted respondents are only compared to control individuals that are sufficiently similar to them in terms of observables. The purpose of the procedure is to investigate whether results are sensitive to the linear approximation embedded in OLS. To implement this approach, we rely on the nearest-neighbor matching procedure proposed by Abadie and Imbens (2006).[30] This non-parametric approach bypasses the difficulties associated with propensity score matching – especially issues regarding balance of an a priori set of observables. The results shown in Table 11 indicate the presence of an average effect on untargeted individuals: the impact is positive and significant for all violence-related indices. This is a stronger result than that suggested by regression analysis, which was only significant for the first two indices (see Tables 3 and 4). In line with regression results presented in Tables 7 to 9, we find no significant average effect on mailing the postcard or voting behavior.

In our last robustness check we seek to instrument selection into the untargeted sample. The main concern is the possibility that untargeted respondents differ in meaningful but unobserved ways from control respondents, and that this may cause spurious estimates of heterogeneous diffusion effects. Our ability to deal with this concern is limited by the available data. We need variables that predict selection into the untargeted sample but have no effect on outcome variables except through being untargeted. To this effect, we assume that individuals more geographically or socially isolated from other residents are

[30] This estimator is implemented in Stata using the nnmatch command.

less likely to have been incorrectly visited by campaigners, and less likely to have approached them during the campaign. Consequently, they are more likely to have been selected into the untargeted sample, or less likely to have self-selected out of the untargeted sample. We use two variables to capture isolation. One is taken from a question on membership in local institutions.[31] The other is a measure of physical isolation, namely, the individual's distance to the mean coordinates of targeted respondents who, by design, are a representative sample of the EAs population. The exclusion restriction assumes that social and geographical isolation does not have a direct impact on the outcomes that is distinct from having self-selected into the untargeted.

Results are presented in Table 12. We start by investigating the effect of self-selection using a standard two-step Heckman regression model. We first estimate a selection regression into the untargeted sample (comparing to control respondents). The two instruments discussed above are selection variables excluded from the second step. Using the inverse Mills ratio from the first step as control function, we estimate selection-corrected regressions of outcome variables using only individuals in the untargeted sample. Coefficients of the inverse Mills ratio serve as a test of selection bias and are reported in the first line of Table 12. We find no evidence of a clear self-selection pattern. The only statistically significant coefficient concerns the index of local empowerment from the bottom, where the sign of the Mills ratio is negative, suggesting an underestimation of the real effect of the campaign.

Instrumental variable results are presented in the rest of Table 12. We begin by discussing homogeneous effects, i.e., coefficient α. We find statistically significant effects of the campaign for the indices of political freedom and conflict, and 'local electoral violence – from the top', i.e., just as in Collier and Vicente (2011). The joint F-test of the instruments in the instrumenting regression is 17, which is above the threshold for weak instruments. Since we have two instruments, we can calculate an overidentification test (Hansen J statistic), which is reported underneath the α coefficient estimates. The validity of the exclusion restriction is not rejected.

We then turn to the diffusion network effects. We report three separate IV regressions – for chat-

[31] The specific question used was: 'I am going to read out a list of groups that people join or attend. For each one, could you tell me whether in January you were an official leader, an active member, an inactive member, or not a member? A religious group (e.g., church, mosque); a trade union or farmers association; a professional or business association; a community development or self-help association; a neighborhood watch ("vigilante") committee.'.

ting, kinship, and geographical proximity. We show the coefficient of interest θ. As recommended by Wooldridge (2002), Chapter 18, the estimated propensity score $\widehat{T_l}$ from the instrumenting regression is used as instrument for T_l in (3.5) and (3.10), while $\widehat{T_l}n_i$ is used as instrument for $T_l n_i$ and $\widehat{T_l}p_i$ is used as instrument for $T_l p_i$. We find significant interaction effects using kinship for all outcomes except the index of 'local empowerment – from the bottom' – for which we did not find any evidence of diffusion effect in Table 5. We find significant diffusion effects using geographical proximity for two of the violence indices, e.g., 'political freedom and conflict – general' and 'crime – perceptions and experience'. As in Tables 7 and 8, instrumented results suggest a diffusion effect through chatting for mailing the postcard and voter turnout. Overall these results are similar in terms of significance and magnitude to those obtained assuming selection on observables or using fixed effects. This suggests that selection on time-varying unobservables is probably unimportant. While these instrumental variable results should be taken with a grain of salt, they constitute additional evidence in support of the presence of diffusion effects.

5. Conclusion

In this paper we have reported results from a field experiment designed to evaluate the reinforcement and diffusion effects of a campaign to counteract electoral violence. Information was collected on social networks and geographical proximity between individuals within treatment and control locations. To test for the presence of a reinforcement effect, we examined whether the impact of the campaign on perceptions and behavior among targeted subjects was reinforced by proximity to other targeted subjects. To investigate diffusion to untargeted individuals in treated locations, we test whether there was an impact of the campaign on these subjects that was stronger when they were closer – in a social or spatial sense – to targeted subjects.

Results provide evidence of both reinforcement and diffusion effects. For perceptions related to violence, we find reinforcement effects that are significant and large in magnitude. The corresponding diffusion effects on untargeted subjects are similar. These findings are generally in line with the homogeneous effect of the campaign. For behavioral outcomes such as our measure of empowerment (mailing the postcard) and voting behavior, the evidence of reinforcement effects is less strong, but we find significant evidence of diffusion effects to untargeted individuals. This is despite the fact that, on average, the behavior of untargeted respondents is unaffected by the campaign.

Reinforcement and diffusion effects are mostly associated with kinship. Geographical proximity is associated with significant reinforcement and diffusion effects for perceptions, but tends to lose statistical significance when social network effects are included as well. Chatting seems a possible reinforcement and diffusion channel for mailing the postcard, and for voter turnout among the untargeted. This pattern of effects suggests that the visibility of campaign materials (as proxied by geographical proximity) may suffice for perception change, while behavior requires oral interaction (as proxied by chatting). The latter is also consistent with political economy models (e.g., Kuran (1989)) that emphasize the need for communication to achieve coordination in protest.

The findings presented in this paper suggest that part of the effect of the anti-violence campaign can be attributed to reinforcement and diffusion effects among individuals that are socially or geographically close. This is reassuring because it indicates that a campaign such as this one produces indirect effects that go beyond direct interaction with campaigners. In the setting of this paper, social and geographical proximity are taken as given and remain outside the control of the policy maker. Our findings nevertheless suggest that it may be possible to increase the effect of the campaign by fostering the formation of links among targeted people, as well as between targeted and untargeted people. This can potentially be achieved by mobilizing civil society through churches and local organizations, and having them relay the campaign message through canvassing neighborhoods and villages. Further investigation is needed on this topic.

There are several dimensions along which experiments of this kind can be improved. First and most important as an improvement of the design in this paper, the sample of targeted and untargeted individuals should be constructed differently. The sample of targeted individuals was assigned randomly and then canvassed by campaigners. However, the sample of untargeted individuals was drawn by returning to EAs and asking individuals to self-report whether they were visited by campaigners. If the campaign protocol was not followed or respondents falsely reported, this causes problems for the interpretation of the results presented in this paper. The sample of untargeted individuals should then be randomly selected before the treatment. Second, the number of individuals targeted by the campaign can be exogenously varied across locations to facilitate identification of peer effects. This is the approach recently adopted, for instance, by Gine and Mansuri (2011). Third, an effort can be made to exogenously create social links among experimental subjects. This is most easily done in experiments that rely in some way on IT technology (e.g., Centola (2010)). Fafchamps and Quinn (2012) use a public competition to create new social links among experimental subjects and use these to study the diffusion of business practices among entrepreneurs in Africa.

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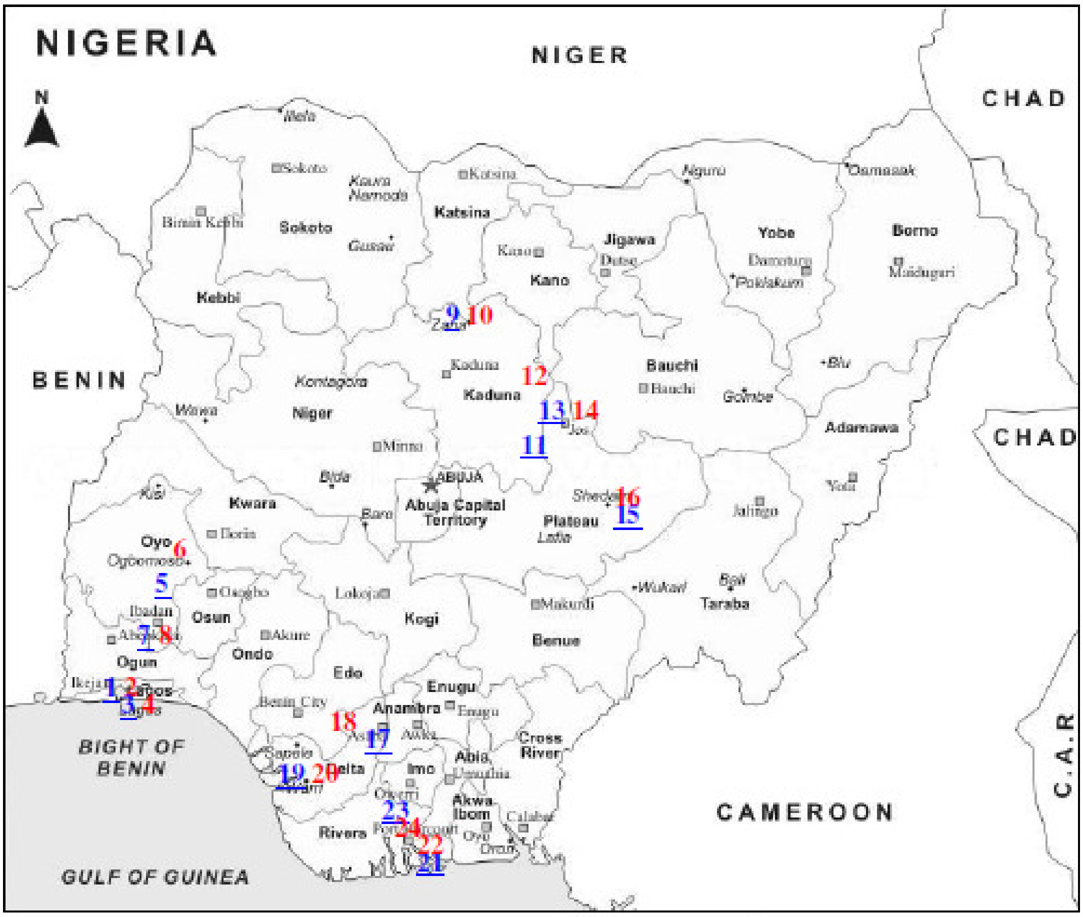
Figure 1: A poster distributed during the anti-violence campaign

Figure 2: Map of experimental locations

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: A map for an enumeration area, with enumerator itineraries

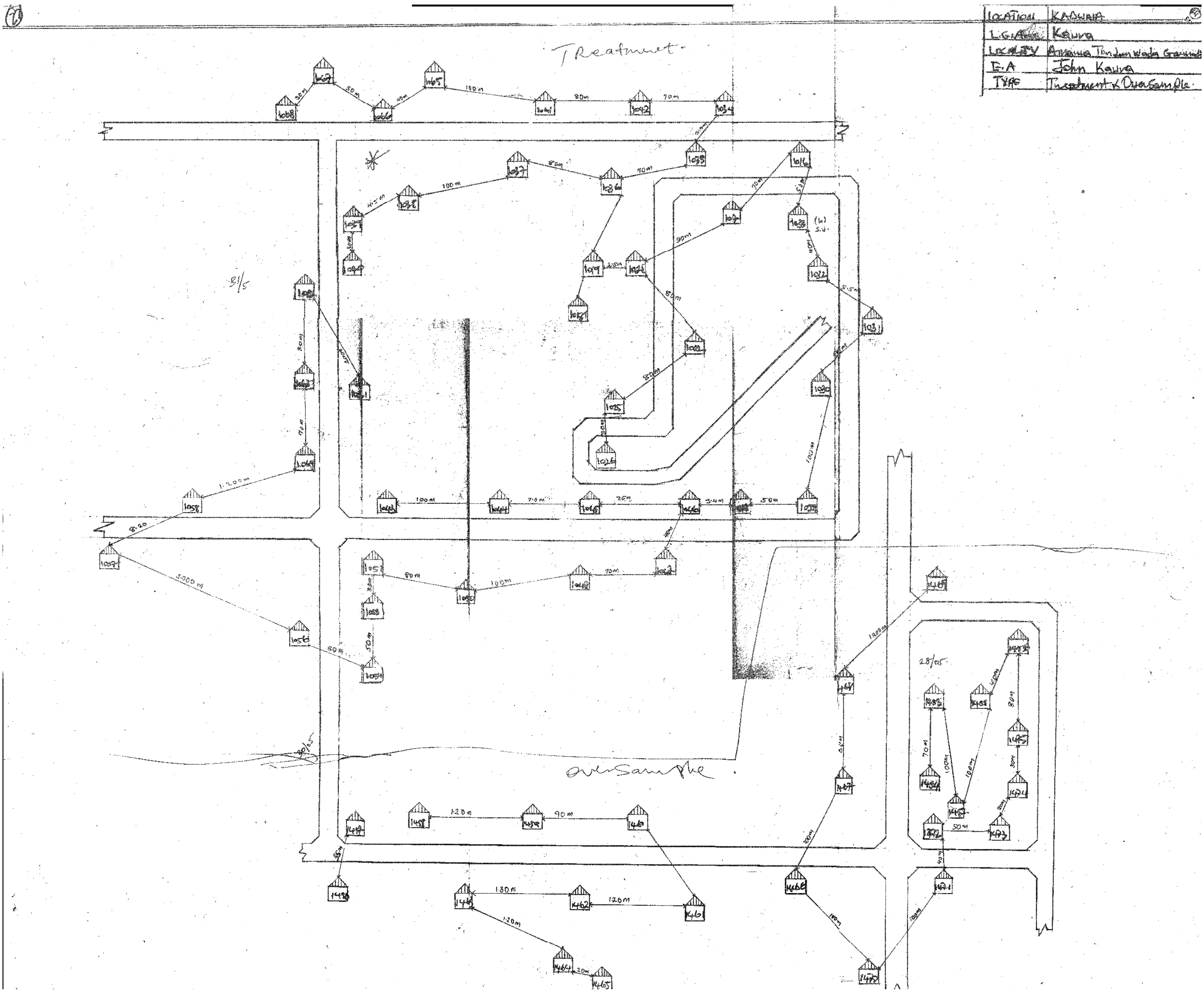


Table 1: Violence-related survey variables - questionnaire phrasing and scales

index name	variable name	phrasing of the survey 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 comparison groups - location characteristics, individual demographics, and attrition

			targeted in treated locations		untargeted in treated locations	
		control	level	difference (to control)	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 0.000	0.500	-0.000 0.006
	age	32.955	33.027	0.072 0.960	32.030	-0.925 1.373
	household size	6.430	6.332	-0.098 0.708	6.727	0.297 0.843
	married	0.581	0.585	0.004 0.046	0.490	-0.091 0.061
	completed secondary school	0.237	0.315	0.078 0.063	0.317	0.080* 0.048
ethnicity	yoruba	0.318	0.273	-0.045 0.166	0.283	-0.035 0.170
	hausa	0.157	0.102	-0.055 0.113	0.097	-0.060 0.117
	igbo	0.072	0.160	0.088 0.089	0.157	0.085 0.086
religion	christian	0.621	0.762	0.141 0.124	0.687	0.066 0.135
	muslim	0.344	0.233	-0.111 0.129	0.293	-0.051 0.141
	religious intensity (1-6)	4.764	5.022	0.258 0.206	5.190	0.426* 0.236
occupation	agriculture	0.158	0.117	-0.042 0.064	0.117	-0.042 0.072
	trader	0.125	0.118	-0.007 0.032	0.170	0.045 0.039
	artisan	0.112	0.130	0.018 0.038	0.140	0.028 0.039
	student	0.222	0.202	-0.020 0.039	0.263	0.042 0.048
	housework	0.120	0.098	-0.022 0.033	0.083	-0.037 0.044
property and expenditure	own a house	0.606	0.605	-0.001 0.107	0.512	-0.094 0.118
	own land	0.526	0.573	0.047 0.114	0.515	-0.011 0.122
	own cattle	0.329	0.327	-0.002 0.100	0.441	0.112 0.102
	own a radio	0.888	0.928	0.040 0.033	0.940	0.052* 0.031
	own a cell phone	0.512	0.608	0.096 0.116	0.542	0.030 0.130
	household expenditure (naira/month)	19,001.358	22,187.872	3,186.514 4,655.297	24,162.236	5,160.877 5,118.745
panel re-surveying		0.967	0.948	-0.018 0.013		

Note: Reported results come from OLS regressions. Standard errors reported; these are corrected for clustering at the location (enumeration area) level.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2b: Differences across comparison groups - networks and baseline outcomes

			targeted in treated locations		untargeted in treated locations	
		control	level	difference (to control)	level	difference (to control)
networks	chatting (0-1)	0.023	0.040	0.017	0.030	0.007
				0.025		0.016
	kinship (0-1)	0.069	0.100	0.031	0.083	0.014
				0.051		0.052
	distance (metres)	302.776	780.999	478.223	941.266	638.490
				331.806		455.485
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		
violence (survey)	local electoral violence - from the top (zscore)	0.000	-0.007	-0.007	0.047	0.047
				0.080		0.113
	local empowerment - from the bottom (zscore)	0.000	0.217	0.217	0.316	0.316
				0.205		0.223
	crime - perceptions and experience (zscore)	0.000	0.111	0.111	0.119	0.119
				0.104		0.105
voting 2003 (survey)	turnout (presidential)	0.728	0.678	-0.050	0.654	-0.073
				0.061		0.069
	turnout (gubernatorial)	0.737	0.686	-0.051	0.648	-0.089
				0.059		0.072
	vote for pdp (presidential)	0.471	0.501	0.030	0.473	0.001
				0.086		0.096
	vote for anpp (presidential)	0.165	0.082	-0.083	0.103	-0.062
				0.078		0.087
	vote for ac (presidential)	0.027	0.047	0.020	0.034	0.007
				0.024		0.023
	vote for pdp (gubernatorial)	0.473	0.479	0.006	0.397	-0.076
				0.080		0.095
	vote for anpp (gubernatorial)	0.134	0.086	-0.048	0.162	0.028
				0.064		0.080
	vote for ac (gubernatorial)	0.034	0.031	-0.003	0.021	-0.014
				0.023		0.023

Note: Reported results come from OLS regressions. Standard errors reported; these are corrected for clustering at the location (enumeration area) level.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3: Regressions of political freedom and violence - general

	political freedom and violence - general							
	homogeneous effect (targeted vs. control)	reinforcement effect (targeted vs. control)			homogeneous effect (untargeted vs. control)	diffusion effect (untargeted vs. control)		
		chatting	kinship	proximity		chatting	kinship	proximity
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
constant (δ)	-0.318	-0.541	-0.526	0.115	-0.675**	-0.792**	-0.844***	-0.208
	(0.381)	(0.348)	(0.349)	(0.332)	(0.334)	(0.330)	(0.327)	(0.304)
treatment (α)	**0.386*** **	0.374***	0.349***	0.160	**0.336*** **	0.335***	0.319***	0.105
	(0.123)	(0.116)	(0.124)	(0.180)	**(0.110)**	(0.106)	(0.108)	(0.142)
network (τ)		0.380**	-1.354	-0.566*		0.145	-1.605	-0.543**
		(0.191)	(1.406)	(0.336)		(0.179)	(1.464)	(0.231)
network*treatment (θ)		**0.415**	**3.289** **	**0.591* **		**0.461**	**3.062** **	**0.531** **
		(0.271)	**(1.365)**	**(0.337)**		**(0.391)**	**(1.420)**	**(0.234)**
p-value from wild bootstrap	0.068	0.220	0.048	0.220	0.080	0.332	0.052	0.096
controls	**yes**	**yes**	**yes**	**yes**	**yes**	**yes**	**yes**	**yes**
number of observations	1,130	1,130	1,130	1,050	862	862	862	823

Note: Regressions on targeted vs. control include observations for targeted (in treated locations) and control respondents; regressions on untargeted vs. control include observations for untargeted (in treated locations) and control respondents. All regressions are OLS. The dependent variable is an index of z-scores. The z-scores are scaled from high violence (low empowerment) to low violence (high empowerment). Controls are state dummies, 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 for clustering at the location (enumeration area) level. Wild bootstrap method follows Cameron et al (2008), with null hypothesis imposed, weights -1 and 1, and 1000 replications. Wild bootstrap p-values concern the 'treatment' coefficient in the homogeneous effect regressions, and the 'network*treatment' coefficient in the heterogeneous effect regressions. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4: Regressions of local electoral violence - from the top

	local electoral violence - from the top															
	homogeneous effect (targeted vs. control)		reinforcement effect (targeted vs. control)						homogeneous effect (untargeted vs. control)		diffusion effect (untargeted vs. control)					
			chatting		kinship		proximity				chatting		kinship		proximity	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
constant (δ)	-0.274*		-0.358**		-0.316**		-0.295*		-0.377**		-0.228		-0.307**		-0.494***	
	(0.146)		(0.150)		(0.157)		(0.179)		(0.148)		(0.196)		(0.148)		(0.175)	
time (γ)	-0.000	0.001	0.001	0.001	-0.005	-0.006	0.046	0.048	-0.000	0.001	0.001	0.001	-0.005	-0.006	0.046	0.048
	(0.091)	(0.088)	(0.090)	(0.087)	(0.092)	(0.089)	(0.084)	(0.080)	(0.091)	(0.088)	(0.091)	(0.088)	(0.092)	(0.089)	(0.084)	(0.080)
treatment (β)	0.050		0.048		0.045		0.100		0.100		0.108		0.113		0.107	
	(0.078)		(0.079)		(0.078)		(0.064)		(0.083)		(0.083)		(0.082)		(0.081)	
time*treatment (α)	**0.233****	**0.233****	0.226**	0.231**	0.229**	0.234**	0.183**	0.189**	**0.260****	**0.256****	0.258**	0.256**	0.265**	0.263**	0.216**	0.210**
	(0.102)	**(0.099)**	(0.102)	(0.100)	(0.103)	(0.101)	(0.091)	(0.087)	**(0.111)**	**(0.107)**	(0.109)	(0.106)	(0.111)	(0.108)	(0.107)	(0.103)
network (φ)			0.119		-0.287		0.336				-0.066		-0.572		0.258	
			(0.112)		(0.508)		(0.226)				(0.133)		(0.502)		(0.243)	
network*time (τ)			0.038	0.042	-1.322*	-1.265*	-0.393*	-0.422*			0.038	0.042	-1.322*	-1.265*	-0.394*	-0.422*
			(0.195)	(0.186)	(0.683)	(0.653)	(0.233)	(0.216)			(0.196)	(0.187)	(0.684)	(0.653)	(0.234)	(0.216)
network*treatment (λ)			0.029		0.139		-0.542**				-0.767		-0.155		-0.713	
			(0.138)		(0.578)		(0.250)				(0.546)		(0.737)		(0.535)	
network*time*treatment (θ)			**0.237**	**0.220**	**2.298*****	**2.206*****	**0.747*****	**0.826*****			**-0.035**	**-0.033**	**1.310***	**1.267***	**0.426**	**0.433**
			(0.273)	**(0.270)**	**(0.802)**	**(0.783)**	**(0.279)**	**(0.257)**			**(0.299)**	**(0.290)**	**(0.784)**	**(0.753)**	**(0.286)**	**(0.263)**
p-value from wild bootstrap	0.028	0.030	0.554	0.590	0.060	0.054	0.012	0.000	0.022	0.022	0.928	0.956	0.096	0.096	0.208	0.148
controls	yes	no	yes	no	yes	no	yes	no	yes	no	yes	no	yes	no	yes	no
individual fixed effects	no	yes	no	yes	no	yes	no	yes	no	yes	no	yes	no	yes	no	yes
number of observations	2,303	1,148	2,261	1,148	2,261	1,148	2,114	1,067	1,739	880	1,724	880	1,724	880	1,649	841

Note: Regressions on targeted vs. control include observations for targeted (in treated locations) and control respondents; regressions on untargeted vs. control include observations for untargeted (in treated locations) and control respondents. All regressions are OLS. The dependent variable is an index of z-scores. The z-scores are scaled from high violence (low empowerment) to low violence (high empowerment). Controls are state dummies, 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 for clustering at the location (enumeration area) level. Wild bootstrap method follows Cameron et al (2008), with null hypothesis imposed, weights -1 and 1, and 1000 replications. Wild bootstrap p-values concern the 'time*treatment' coefficient in the homogeneous effect regressions, and the 'network*time*treatment' coefficient in the heterogeneous effect regressions. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5: Regressions of local empowerment - from the bottom

	local empowerment - from the bottom															
	homogeneous effect (targeted vs. control)		**reinforcement effect (targeted vs. control)**						**homogeneous effect (untargeted vs. control)**		**diffusion effect (untargeted vs. control)**					
			chatting		**kinship**		**proximity**				**chatting**		**kinship**		**proximity**	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**	**(12)**	**(13)**	**(14)**	**(15)**	**(16)**
constant (δ)	-0.317		-0.449		-0.427		-0.255		-0.882***		-0.897***		-0.835***		-0.758**	
	(0.333)		(0.321)		(0.322)		(0.368)		(0.287)		(0.290)		(0.288)		(0.302)	
time (γ)	-0.001	-0.000	-0.000	0.000	0.003	0.004	0.012	0.012	-0.001	-0.000	-0.000	0.000	0.003	0.004	0.012	0.012
	(0.078)	(0.076)	(0.077)	(0.076)	(0.078)	(0.076)	(0.082)	(0.080)	(0.078)	(0.076)	(0.078)	(0.076)	(0.078)	(0.076)	(0.082)	(0.080)
treatment (β)	0.367***		0.356***		0.348***		0.394***		0.375***		0.375***		0.385***		0.458***	
	(0.103)		(0.108)		(0.109)		(0.105)		(0.128)		(0.130)		(0.133)		(0.122)	
time*treatment (α)	**0.221****	**0.221****	0.221**	0.221**	0.219**	0.219**	0.214*	0.216**	**0.131**	**0.130**	0.129	0.129	0.127	0.127	0.106	0.102
	(0.106)	**(0.104)**	(0.106)	(0.104)	(0.106)	(0.104)	(0.110)	(0.107)	**(0.142)**	**(0.138)**	(0.140)	(0.136)	(0.142)	(0.139)	(0.142)	(0.137)
network (φ)			0.153		-0.602		-0.218				0.009		-0.767		-0.388	
			(0.268)		(1.128)		(0.247)				(0.274)		(1.076)		(0.257)	
network*time (τ)			-0.091	-0.086	0.723	0.763	-0.025	-0.019			-0.091	-0.086	0.723	0.763	-0.025	-0.019
			(0.194)	(0.195)	(1.169)	(1.171)	(0.166)	(0.164)			(0.195)	(0.195)	(1.173)	(1.172)	(0.166)	(0.164)
network*treatment (λ)			0.305		1.795		0.738**				0.121		0.053		1.431***	
			(0.571)		(1.589)		(0.335)				(0.877)		(1.650)		(0.455)	
network*time*treatment (θ)			**0.118**	**0.105**	**-1.044**	**-1.095**	**0.120**	**0.149**			**-0.007**	**-0.013**	**-0.175**	**-0.216**	**-0.155**	**-0.212**
			(0.376)	**(0.377)**	**(1.479)**	**(1.478)**	**(0.339)**	**(0.323)**			**(0.529)**	**(0.523)**	**(1.445)**	**(1.436)**	**(0.380)**	**(0.350)**
p-value from wild bootstrap	0.046	0.042	0.790	0.808	0.560	0.544	0.714	0.652	0.394	0.394	0.950	0.952	0.942	0.912	0.718	0.610
controls	**yes**	**no**	**yes**	**no**	**yes**	**no**	**yes**	**no**	**yes**	**no**	**yes**	**no**	**yes**	**no**	**yes**	**no**
individual fixed effects	**no**	**yes**	**no**	**yes**	**no**	**yes**	**no**	**yes**	**no**	**yes**	**no**	**yes**	**no**	**yes**	**no**	**yes**
number of observations	2,260	1,148	2,260	1,148	2,260	1,148	2,100	1,067	1,724	880	1,724	880	1,724	880	1,646	841

Note: Regressions on targeted vs. control include observations for targeted (in treated locations) and control respondents; regressions on untargeted vs. control include observations for untargeted (in treated locations) and control respondents. All regressions are OLS. The dependent variable is an index of z-scores. The z-scores are scaled from high violence (low empowerment) to low violence (high empowerment). Controls are state dummies, 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 for clustering at the location (enumeration area) level. Wild bootstrap method follows Cameron et al (2008), with null hypothesis imposed, weights -1 and 1, and 1000 replications. Wild bootstrap p-values concern the 'time*treatment' coefficient in the homogeneous effect regressions, and the 'network*time*treatment' coefficient in the heterogeneous effect regressions. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6: Regressions of crime - perceptions and experience

	crime - perceptions and experience															
	homogeneous effect (targeted vs. control)		**reinforcement effect (targeted vs. control)**						**homogeneous effect (untargeted vs. control)**		**diffusion effect (untargeted vs. control)**					
			chatting		**kinship**		**proximity**				**chatting**		**kinship**		**proximity**	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**	**(12)**	**(13)**	**(14)**	**(15)**	**(16)**
constant (δ)	-0.215		-0.299		-0.253		0.051		-0.618***		-0.444*		-0.432*		-0.371**	
	(0.208)		(0.195)		(0.197)		(0.203)		(0.195)		(0.250)		(0.230)		(0.184)	
time (γ)	0.003	-0.000	0.000	-0.001	-0.012	-0.014	0.134	0.132	0.003	-0.000	0.000	-0.001	-0.012	-0.014	0.134	0.132
	(0.101)	(0.100)	(0.099)	(0.096)	(0.096)	(0.093)	(0.134)	(0.132)	(0.102)	(0.100)	(0.099)	(0.096)	(0.096)	(0.093)	(0.134)	(0.132)
treatment (β)	0.151**		0.135*		0.138*		0.057		0.134*		0.132*		0.150**		0.086	
	(0.067)		(0.075)		(0.070)		(0.088)		(0.078)		(0.071)		(0.073)		(0.087)	
time*treatment (α)	**-0.037**	**-0.035**	-0.038	-0.036	-0.030	-0.027	-0.156	-0.155	**0.062**	**0.064**	0.068	0.067	0.078	0.078	-0.069	-0.070
	(0.117)	**(0.117)**	(0.112)	(0.109)	(0.109)	(0.106)	(0.147)	(0.146)	**(0.119)**	**(0.117)**	(0.115)	(0.112)	(0.113)	(0.110)	(0.149)	(0.146)
network (φ)			-0.464		1.189		-0.155				-0.459		0.975		-0.035	
			(0.347)		(0.909)		(0.119)				(0.324)		(0.931)		(0.097)	
network*time (τ)			0.544	0.544	-2.711*	-2.670*	-0.388	-0.384			0.544	0.544	-2.711*	-2.670*	-0.388	-0.384
			(0.518)	(0.513)	(1.452)	(1.426)	(0.300)	(0.294)			(0.520)	(0.513)	(1.456)	(1.427)	(0.301)	(0.294)
network*treatment (λ)			0.609		-1.296		0.129				-0.548		-2.894**		0.021	
			(0.378)		(0.951)		(0.113)				(0.705)		(1.149)		(0.100)	
network*time*treatment (θ)			**-0.221**	**-0.225**	**3.777****	**3.730****	**0.466**	**0.466**			**-0.333**	**-0.330**	**3.316****	**3.286****	**0.384**	**0.379**
			(0.550)	**(0.545)**	**(1.558)**	**(1.533)**	**(0.301)**	**(0.294)**			**(0.599)**	**(0.591)**	**(1.510)**	**(1.480)**	**(0.301)**	**(0.294)**
p-value from wild bootstrap	0.746	0.770	0.850	0.852	0.056	0.056	0.080	0.078	0.628	0.622	0.752	0.754	0.084	0.084	0.172	0.170
controls	**yes**	**no**	**yes**	**no**	**yes**	**no**	**yes**	**no**	**yes**	**no**	**yes**	**no**	**yes**	**no**	**yes**	**no**
individual fixed effects	**no**	**yes**	**no**	**yes**	**no**	**yes**	**no**	**yes**	**no**	**yes**	**no**	**yes**	**no**	**yes**	**no**	**yes**
number of observations	2,312	1,149	2,262	1,149	2,262	1,149	2,123	1,068	1,743	880	1,724	880	1,724	880	1,651	841

Note: Regressions on targeted vs. control include observations for targeted (in treated locations) and control respondents; regressions on untargeted vs. control include observations for untargeted (in treated locations) and control respondents. All regressions are OLS. The dependent variable is an index of z-scores. The z-scores are scaled from high violence (low empowerment) to low violence (high empowerment). Controls are state dummies, 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 for clustering at the location (enumeration area) level. Wild bootstrap method follows Cameron et al (2008), with null hypothesis imposed, weights -1 and 1, and 1000 replications. Wild bootstrap p-values concern the 'time*treatment' coefficient in the homogeneous effect regressions, and the 'network*time*treatment' coefficient in the heterogeneous effect regressions. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 7: Regressions of postcard

	postcard							
	homogeneous effect (targeted vs. control)	reinforcement effect (targeted vs. control)			homogeneous effect (untargeted vs. control)	diffusion effect (untargeted vs. control)		
		chatting	kinship	proximity		chatting	kinship	proximity
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
constant (δ)	0.907***	0.401***	0.404***	0.426***	0.873***	0.036	0.016	0.063
	(0.097)	(0.133)	(0.136)	(0.147)	(0.093)	(0.112)	(0.112)	(0.109)
treatment (α)	**0.078****	0.052	0.048	0.052	**-0.008**	0.014	0.002	0.016
	(0.035)	(0.053)	(0.054)	(0.061)	**(0.059)**	(0.059)	(0.058)	(0.066)
network (τ)		-0.543**	0.114	0.080		-0.216	0.198	0.060
		(0.264)	(0.239)	(0.141)		(0.230)	(0.202)	(0.133)
network*treatment (θ)		**1.143*****	**0.295**	**-0.096**		**1.261****	**0.393***	**-0.095**
		(0.331)	**(0.239)**	**(0.249)**		**(0.520)**	**(0.217)**	**(0.224)**
p-value for wild bootstrap	0.090	0.116	0.256	0.768	0.902	0.052	0.090	0.734
controls	**yes**	**yes**	**yes**	**yes**	**yes**	**yes**	**yes**	**yes**
number of observations	1,131	1,131	1,131	1,051	863	863	863	824

Note: Regressions on targeted vs. control include observations for targeted (in treated locations) and control respondents; regressions on untargeted vs. control include observations for untargeted (in treated locations) and control respondents. All regressions are OLS. The dependent variable is binary. Controls are state dummies, 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 for clustering at the location (enumeration area) level. Wild bootstrap method follows Cameron et al (2008), with null hypothesis imposed, weights -1 and 1, and 1000 replications. Wild bootstrap p-values concern the 'treatment' coefficient in the homogeneous effect regressions, and the 'network*treatment' coefficient in the heterogeneous effect regressions. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 8: Regressions of voter turnout - presidential and gubernatorial elections

	voter turnout - presidential and gubernatorial elections							
	homogeneous effect (targeted vs. control)	reinforcement effect (targeted vs. control)			homogeneous effect (untargeted vs. control)	diffusion effect (untargeted vs. control)		
		chatting	kinship	proximity		chatting	kinship	proximity
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
constant (δ)	0.614***	0.483***	0.526***	0.753***	0.870***	0.664***	0.662***	0.787***
	(0.068)	(0.077)	(0.080)	(0.101)	(0.112)	(0.083)	(0.091)	(0.110)
treatment (α)	**0.091*****	0.075***	0.069**	0.007	**-0.025**	-0.024	-0.032	-0.123**
	(0.028)	(0.027)	(0.029)	(0.048)	**(0.053)**	(0.048)	(0.049)	(0.061)
network (τ)		0.303***	-0.097	-0.144		0.361***	-0.582	-0.188*
		(0.082)	(0.696)	(0.095)		(0.082)	(0.651)	(0.105)
network*treatment (θ)		**0.059**	**0.698**	**0.143**		**0.272****	**1.604****	**0.130**
		(0.135)	**(0.803)**	**(0.097)**		**(0.136)**	**(0.694)**	**(0.105)**
p-value for wild bootstrap	0.056	0.634	0.458	0.300	0.672	0.052	0.060	0.410
controls	yes	yes	yes	yes	yes	yes	yes	yes
number of observations	1,127	1,127	1,127	1,047	859	859	859	820

Note: Regressions on targeted vs. control include observations for targeted (in treated locations) and control respondents; regressions on untargeted vs. control include observations for untargeted (in treated locations) and control respondents. All regressions are OLS. The dependent variable is the average of two binary variables for turnout (regarding the presidential and the gubernatorial elections). Controls are state dummies, 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 for clustering at the location (enumeration area) level. Wild bootstrap method follows Cameron et al (2008), with null hypothesis imposed, weights -1 and 1, and 1000 replications. Wild bootstrap p-values concern the 'treatment' coefficient in the homogeneous effect regressions, and the 'network*treatment' coefficient in the heterogeneous effect regressions. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 9: Regressions of voting for the incumbent - presidential and gubernatorial elections

	voting for the incumbent - presidential and gubernatorial elections							
	homogeneous effect (targeted vs. control)	reinforcement effect (targeted vs. control)			homogeneous effect (untargeted vs. control)	diffusion effect (untargeted vs. control)		
		chatting	kinship	proximity		chatting	kinship	proximity
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
constant (δ)	0.470***	0.288***	0.290***	0.321***	0.532***	0.303***	0.299***	0.235**
	(0.065)	(0.071)	(0.073)	(0.073)	(0.090)	(0.084)	(0.085)	(0.093)
treatment (α)	**0.105*** **	0.107***	0.098***	0.118***	**-0.007**	-0.011	-0.010	0.006
	(0.024)	(0.020)	(0.022)	(0.032)	**(0.025)**	(0.039)	(0.039)	(0.049)
network (τ)		0.188	-0.957	0.034		0.205	-0.961	0.037
		(0.155)	(0.594)	(0.093)		(0.144)	(0.661)	(0.082)
network*treatment (θ)		**0.163**	**1.709** **	**-0.042**		**0.188**	**1.809** **	**-0.001**
		(0.194)	**(0.646)**	**(0.103)**		**(0.190)**	**(0.646)**	**(0.086)**
p-value for wild bootstrap	0.016	0.462	0.056	0.794	0.794	0.052	0.032	0.410
controls	**yes**	**yes**	**yes**	**yes**	**yes**	**yes**	**yes**	**yes**
number of observations	1,127	1,127	1,127	1,047	859	859	859	820

Note: Regressions on targeted vs. control include observations for targeted (in treated locations) and control respondents; regressions on untargeted vs. control include observations for untargeted (in treated locations) and control respondents. All regressions are OLS. The dependent variable is the average of two binary variables for voting for the incumbent (regarding the presidential and the gubernatorial elections). Controls are state dummies, 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 for clustering at the location (enumeration area) level. Wild bootstrap method follows Cameron et al (2008), with null hypothesis imposed, weights -1 and 1, and 1000 replications. Wild bootstrap p-values concern the 'treatment' coefficient in the homogeneous effect regressions, and the 'network*treatment' coefficient in the heterogeneous effect regressions. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 10: Regressions including several network variables at the same time

	all outcomes													
	reinforcement effect (targeted vs. control)							diffusion effect (untargeted vs. control)						
	political freedom and conflict - general	local electoral violence - from the top	local empowerment - from the bottom	crime - perceptions and experience	postcard	voter turnout	voting for the incumbent	political freedom and conflict - general	local electoral violence - from the top	local empowerment - from the bottom	crime - perceptions and experience	postcard	voter turnout	voting for the incumbent
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
constant	-0.407	0.128	0.016	-0.006	0.417***	0.487***	0.264***	-0.771**	0.128	0.016	-0.006	0.025	0.669***	0.292***
	(0.352)	(0.140)	(0.082)	(0.082)	(0.132)	(0.074)	(0.065)	(0.334)	(0.140)	(0.082)	(0.083)	(0.112)	(0.079)	(0.084)
treatment	0.359***	0.100	0.213**	-0.037	0.049	0.083***	0.078**	0.337***	0.130	0.100	0.058	0.007	-0.020	-0.007
	(0.119)	(0.146)	(0.108)	(0.100)	(0.051)	(0.030)	(0.032)	(0.119)	(0.154)	(0.138)	(0.098)	(0.056)	(0.044)	(0.038)
network (chatting)					-0.586**	0.345***	0.276**					-0.272	0.436***	0.292***
					(0.266)	(0.078)	(0.108)					(0.258)	(0.093)	(0.091)
network (chatting)*treatment					**0.936*** **	**0.123**	**0.033**					**0.736**	**0.233**	**-0.236**
					(0.326)	**(0.219)**	**(0.270)**					**(0.469)**	**(0.212)**	**(0.313)**
network (kinship)	-0.517	0.832	1.074	-2.604*	0.557	-0.606	-1.369**	-0.887	0.832	1.074	-2.604*	0.608	-1.133*	-1.376**
	(1.157)	(0.979)	(1.234)	(1.374)	(0.568)	(0.708)	(0.625)	(1.204)	(0.979)	(1.235)	(1.375)	(0.501)	(0.669)	(0.691)
network (kinship)*treatment	**2.364** **	**0.197**	**-1.304**	**3.702** **	**0.313**	**0.248**	**1.478***	**2.402** **	**-0.834**	**-0.609**	**3.150** **	**0.582**	**0.994**	**2.153** **
	(1.085)	**(1.037)**	**(1.483)**	**(1.479)**	**(0.644)**	**(0.920)**	**(0.836)**	**(1.194)**	**(1.050)**	**(1.520)**	**(1.440)**	**(0.522)**	**(0.813)**	**(0.914)**
network (proximity)	0.028	-0.310	0.012	-0.009				0.042	-0.310	0.012	-0.009			
	(0.162)	(0.334)	(0.155)	(0.306)				(0.173)	(0.334)	(0.155)	(0.306)			
network (proximity)*treatment	**0.234**	**0.358**	**0.116**	**0.018**				**0.370**	**0.313**	**-0.225**	**-0.172**			
	(0.206)	**(0.334)**	**(0.320)**	**(0.360)**				**(0.415)**	**(0.334)**	**(0.363)**	**(0.352)**			
p-value for wild bootstrap: chatting					0.136	0.630	0.888					0.074	0.224	0.588
p-value for wild bootstrap: kinship	0.042	0.858	0.508	0.054	0.620	0.842	0.120	0.062	0.426	0.754	0.086	0.296	0.276	0.044
p-value for wild bootstrap: proximity	0.308	0.304	0.728	0.998				0.442	0.418	0.594	0.622			
controls	Yes	No	No	No	Yes	Yes	Yes	Yes	No	No	No	Yes	Yes	Yes
individual fixed effects	No	Yes	Yes	Yes	No	No	No	No	Yes	Yes	Yes	No	No	No
number of observations	1,050	1,067	1,067	1,068	1,131	1,127	1,127	823	841	841	841	863	859	859

Note: Regressions on targeted vs. control include observations for targeted (in treated locations) and control respondents; regressions on untargeted vs. control include observations for untargeted (in treated locations) and control respondents. All regressions are OLS. The dependent variables are as in the previous tables: they are indices of z-scores for violence perceptions, and binary for the postcard, turnout, and voting. Controls are the same as the ones used in the previous tables. Standard errors reported; these are corrected for clustering at the location (enumeration area) level. Wild bootstrap method follows Cameron et al (2008), with null hypothesis imposed, weights -1 and 1, and 1000 replications. Wild bootstrap p-values concern the 'network*treatment' coefficients. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 11: Robustness for diffusion: matching results for homogeneous effects

	all outcomes						
	diffusion effect (untargeted vs. control)						
	political freedom and conflict - general	local electoral violence - from the top	local empowerment - from the bottom	crime - perceptions and experience	postcard	voter turnout	voting for the incumbent
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
treatment	**0.440*****	**0.321*****	**0.482*****	**0.248*****	**-0.005**	**-0.008**	**-0.011**
	(0.074)	**(0.080)**	**(0.083)**	**(0.090)**	**(0.047)**	**(0.038)**	**(0.041)**
number of observations	862	862	862	862	863	859	859

Note: All regressions use a matching estimator proposed by Abadie and Imbens (2006) and implemented in Stata through the nnmatch command. The dependent variables are as in the previous tables: they are indices of z-scores for violence perceptions, and binary for the postcard, turnout, and voting. Controls are the same as the ones used in the previous tables. Standard errors reported. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 12: Robustness for diffusion: selection and instrumental variables

		all outcomes					
		diffusion effect (untargeted vs. control)					
	political freedom and conflict - general	local electoral violence - from the top	local empowerment - from the bottom	crime - perceptions and experience	postcard	voter turnout	voting for the incumbent
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
inverse mills ratio untargeted	**0.094**	**-0.032**	**-0.125***	**-0.056**	**0.054**	**-0.113**	**-0.067**
	(0.113)	**(0.064)**	**(0.076)**	**(0.082)**	**(0.060)**	**(0.079)**	**(0.062)**
IV homogeneous effect	**0.261***	**0.407*****	**0.173**	**0.079**	**-0.075**	**-0.020**	**0.048**
	(0.150)	**(0.152)**	**(0.174)**	**(0.186)**	**(0.071)**	**(0.078)**	**(0.064)**
p-value overidentification	0.150	0.702	0.392	0.845	0.831	0.494	0.770
number of observations	816	834	834	834	817	813	813
IV chatting	**0.043**	**0.371**	**-0.613**	**0.241**	**0.900***	**0.459****	**0.264**
	(0.356)	**(0.802)**	**(0.560)**	**(0.542)**	**(0.500)**	**(0.202)**	**(0.180)**
number of observations	816	816	816	816	817	813	813
IV kinship	**2.866***	**4.187***	**-1.599**	**2.980***	**0.707***	**1.524***	**2.070****
	(1.716)	**(2.526)**	**(2.163)**	**(1.749)**	**(0.374)**	**(0.813)**	**(0.818)**
number of observations	816	816	816	816	817	813	813
IV proximity	**0.557****	**0.152**	**-0.369**	**0.519***	**0.356**	**0.146**	**-0.019**
	(0.281)	**(0.435)**	**(0.453)**	**(0.300)**	**(0.385)**	**(0.105)**	**(0.130)**
number of observations	816	816	816	816	817	813	813
controls	yes	no	no	no	yes	yes	yes
individual fixed effects	no	yes	yes	yes	no	no	no

Note: The top panel displays the coefficient of the inverse Mills ratio in the regression of the outcome on controls using untargeted respondents within treatment areas only. Selection is for the untargeted relative to control respondents - excluded variables are distance to mean panel coordinates of baseline respondents and membership in local organizations. In the rest of the table, each coefficient estimate is taken from a separate regression using instrumental variables. When estimating heterogeneous effects, we include only one network measure at a time. Instruments are distance to mean panel coordinates of baseline respondents and membership in local organizations (see text for details). The dependent variables are as in the previous tables: they are indices of z-scores for violence perceptions, and binary for the postcard, turnout, and voting. Controls are the same as the ones used in the previous tables. The p-value of overidentication is for the Hansen J statistic. Standard errors reported; these are corrected for clustering at the location (enumeration area) level. * significant at 10%; ** significant at 5%; *** significant at 1%.