CSAE WPS/2009-14

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

Marcel Fafchamps
University of Oxford[†]

Pedro C. Vicente
Trinity College Dublin[‡] and CSAE

July 2009

Abstract

Political accountability and participation are taken as key ingredients for development. In this context voter education and informational campaigns are becoming popular with donors. We followed a large-scale randomized campaign against electoral violence sponsored by an international NGO during the 2007 Nigerian elections. Substantial direct effects on perceptions about violence and voting behavior are reported for this campaign. This paper is devoted to the assessment of

*We thank Oriana Bandiera, Paul Collier, Donald Green, Macartan Humphreys, Craig McIntosh, Bilal Siddiqi, and seminar participants at CERDI, CSAE-Oxford, and EGAP-Yale for helpful suggestions. Sarah Voitchovsky provided superb research assistance. We are particularly grateful to Ojobo Atukulu, Otive Igbuzor, and Olutayo Olujide at ActionAid International Nigeria, Austin Emeanua, campaigners Nwakaudu Chijoke Mark, Gbolahan Olubowale, George-Hill Anthony, Monday Itoghor, Umar Farouk, Emmanuel Nehemiah, Henry Mang and their field teams, and to the surveyors headed by Taofeeq Akinremi, Gbenga Adewunmi, Oluwasegun Olaniyan, and Moses Olusola. We also want to acknowledge the kind institutional collaboration of the Afrobarometer. We wish to acknowledge financial support from the iiG Consortium - 'Improving Institutions for Pro-Poor Growth'. All errors are our responsibility.

[†]Department of Economics, University of Oxford, Manor Road, Oxford OX1 3UQ, UK. Email: marcel.fafchamps@economics.ox.ac.uk. Fax: +44(0)1865-281447. Tel: +44(0)1865-281446.

[‡]Department of Economics, Trinity College, Arts Building, Dublin 2, Ireland. Email: vicentep@tcd.ie. Fax: +353(0)1-6772503. Tel: +353(0)1-8963478.

the network effects of this intervention. Comprehensive measurement of the links between households allows us to estimate reinforcement effects on the treated subjects in campaign locations, and diffusion effects on untreated subjects in campaign locations. These effects are derived with reference to suitable comparison groups in untreated locations. We find evidence for both network effects using different estimation techniques. Namely, we document the importance of kinship and geographical distance in spreading perceptions associated with the campaign. We do not find clear network effects on behavior.

1. Introduction

Democracy is notoriously difficult to implement in Africa. For it to deliver politicians that seek to improve the welfare of the masses, it is central that citizens are better informed and vote according to policy-accountability. Yet it is only too easy for politicians to seek votes by stirring up greed, rivalry, or fear. Using field experiments in Benin and Sao Tome and Principe respectively, Wantchekon (2003) and Vicente (2007) study greed: they show that politicians attract more votes by using clientelistic and vote-buying platforms. The study of rivalry has been centered on the use of ethnic divisions in politics. Posner (2004) uses a natural experiment in the border of Malawi and Zambia to prove 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 (2009) using Afrobarometer data across ten African countries. In this paper we focus our attention on the use of fear in elections.

In this context, the fundamental question we face is: what can be done to reduce the role of malfeasant electoral strategies like vote-buying, ethnic polarization, or violent intimidation? Vicente (2007) shows that a campaign against vote-buying is effective in reducing the effect the practice has on voting behavior. In a similar vein, Collier and Vicente (2009) use a field experiment and show that an awareness campaign encouraging Nigerian voters to oppose electoral violence was successful in reducing perceptions of local violence and margins of related behavior.[1] The campaign also affected voting behavior by increasing turnout and reducing the electoral score of non-incumbent candidates.

If awareness campaigns can successfully reduce the role of malfeasance in voting behavior, this raises other questions, such as what proportion of the population must be reached for a campaign to be successful. It is indeed onerous and, in many cases, infeasible for an awareness campaign to target everyone. One would therefore like to know whether individuals not directly exposed to an awareness campaign nevertheless report perception and behavioral changes similar to those of exposed individuals as the message diffuses through social networks. We call this a diffusion effect. It is also possible that community members directly exposed to the message of a campaign may have the impact of that campaign reinforced by interaction with their peers. We call this a reinforcement effect.

This paper provides a partial answer to these two questions using a field experiment specifically designed to evaluate the diffusion and reinforcement of an anti-violence message among voters. We study the effects of an informational campaign against political

[1] Perception and behavioral effects of broadcasting information on violence and crime are studied by Dellavigna and Kaplan (2007) and Dahl and Dellavigna (2009). The first reports on paranoia effects of stressing information related to terrorism. Perhaps due to the nature of an anti-violence campaign, the Nigerian campaign this not induce the same kind of impact.

violence, undertaken nationwide in Nigeria before the 2007 elections. It worked primarily through town meetings and popular theatres, as a way to decrease collective action costs for counteracting violence. For the estimation of our effects of interest, we collected information about social network links and geographical distance between households in targeted and control groups within treatment villages, and in groups within control villages. To test for the presence of a reinforcement effect, we examine whether the effect of the message on the perceptions and behavior of targeted households is reinforced by proximity to other exposed households. To investigate diffusion within a village, we test whether households not directly exposed to the campaign show effects similar to exposed households with whom they have close social ties.

Results provide some evidence of both reinforcement effects. Findings suggest that the impact of the campaign on perceptions of community violence and feelings of intimidation is reinforced by social and geographical proximity to other exposed households. What seems to matter most is kinship – i.e. family relationships – although geographical proximity is also significant. We however find little reinforcement effect on behavior – either in terms of voting behavior or in terms of willingness to express opposition to electoral violence.

We also find evidence of diffusion to unexposed households. For perceptions of community violence and intimidation, the diffusion effect nearly perfectly mimics the reinforcement effect: the sign, significance, and magnitude of the coefficients are similar. We find a significant externality of the campaign on households' willingness to express disapproval of electoral violence, but unclear effects on voting behavior *per se*. Because self-reported exposure to the anti-violence campaign may be subject to self-selection, we investigate the robustness of our results with respect to selection on observables or unobservables. Similar findings obtain.

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 programme in Kenya. They estimated the impact of the treatment on control populations. Because their design featured programme randomization at the school level, this did not allow for an experimental estimation of individual externalities within treated schools. More recently, Angelucci, DiGiorgi, Rangel, and Rasul (2009) extend the study of externalities to a conditional cash transfer programme. By exploring a rich set of outcomes at the household level they are able to draw light into specific mechanisms of influence of unexposed households. These authors do not use explicit network variables, however. Still in the context of a conditional cash transfer programme, Macours and Vakis (2008) extend the literature by introducing explicit interaction among households. But they only estimate reinforcement effects and do not have individual variation in networks. The work by Nickerson (2008) relates most closely to our study: his focus is on using door-to-door randomized get-out-to-vote campaigning to identify peer-effects in two-member households. 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 begin by providing a rapid description of the context in which our study takes place. The field experiment and testing strategy are presented in detail in Section 3. The data and descriptive statistics are discussed in Section 4. Subsequently, in Section 5, empirical results are presented, with corresponding robustness tests. Section 6 concludes.

2. Context

Nigeria, the most populous country in Africa with estimated 146 million inhabitants[2], has been challenged by persistent development problems. Despite holding the largest proven oil reserves in Sub-Saharan Africa (10th largest in the world[3]), Nigeria ranks 201 in 233 countries in terms of GDP per capita (1400 USD PPP in 2005[4]). Moreover, it has been seen as a textbook-example of bad governance: Nigeria has continuously featured among the most corrupt countries in the world (see Transparency International). Clearly, one can only understand this state of affairs if one deepens the study of politics in Nigeria: in the words of Chinua 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. However, in 1999, a new constitution was passed and civilian rule was adopted. Elections were run in 1999, 2003, and 2007. Despite formally marking the transfer of political power, these elections were influenced by widespread vote-buying, ballot-fraud, and violent intimidation. Most observers have seen these elections as being far from 'free and fair'.

The focus of our attention is the 2007 suffrage. In April of that year, elections were run for all the federal and state-level political bodies (president, federal house of representa-

[2] CIA World Factbook 2009.
[3] Oil & Gas Journal, 103(47), December 19th, 2005.
[4] World Development Indicators.

tives, and senate; state governors, and assemblies).[5] The election was highly anticipated because it marked the first transfer of presidential power from one civilian to another: Olesegun Obasanjo was stepping down as president due to a two-term limit, and the main contestants were Umaru Yar'Adua from PDP, Muhammadu Buhari from ANPP, and Atiku Abubakar from AC. Yar'Adua was seen as a protégé of Obasanjo, clearly the front-runner due to the overwhelming influence of the ruling party PDP. Buhari had been the main challenger in 2003, was strongly associated to the Muslim North, and had an anti-corruption track-record. Finally, Abubakar, the vice-president of Obasanjo, and a former customs official with controversial sources of wealth, was very much on the news because of corruption accusations that almost impeded him from running; he was led to switch to AC due to a conflict with Obasanjo.

PDP easily won the 2007 elections: Yar'Adua secured 70% of votes, and PDP candidates were able to sweep 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 Transition Monitoring Group (which deployed 50,000 observers) were unanimous in underlining numerous irregularities in the conduction of the suffrage. Both were clear in stating that the elections were not credible and fell far short of basic international standards. Human Rights Watch, in a report released in May 2007[6], 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

[5] Elections at the state and federal levels took place on two separate suffrage days.
[6] Human Rights Watch, 'Nigerian Debacle a Threat to Africa', May 2007.

body count had surpassed 300'. This violence was identified by Human Rights Watch to be originated from marginalized political groups, many of which dissidents formerly associated to PDP[7]. On the ground, this hostility emerged in the form of assassinations of known politicians, but mainly as locally-widespread intimidation, usually conducted by armed gangs, recruited among the young and unemployed. This is the context in which we ran our field experiment to which we now turn.

3. Experimental design

In anticipation for the 2007 elections ActionAid International Nigeria (AAIN) launched a nationwide campaign against electoral violence in February 2007. AAIN is the local chapter of a major international NGO specializing in community participatory development, with a wide and experienced field-infrastructure in the country. AAIN's campaign encouraged voters to resist intimidation and to participate in the elections. It also intended to persuade voters to punish violent candidates by voting against them. The main theoretical rationale of the campaign was to improve collective action in counteracting electoral violence at the local level. The analytic foundation for this aspect of the campaign is the model of political protest of Kuran (1989), where a public call to a common protest lowers its costs and makes it easier to resist intimidation. The campaign also worked through the provision of information about ways to counteract violence: by calling voters to deny their vote to perpetrators of violence, the campaign incited the population to reconsider the meaning of their vote and the value of the different candidates.

[7] Human Rights Watch, 'Criminal Politics: Violence, 'Godfathers', and Corruption in Nigeria', October 2007.

Campaign staff toured villages and urban neighborhoods organizing town meetings and street theatres to sensitize voters to the campaign message. They also distributed leaflets, posters, and items of clothing bearing an anti-violence message, the purpose of which was to reinforce and disseminate the message further.[8] A poster from the campaign is shown in Figure 1. Our survey shows that 89% of the households approached by campaigners received at least one item of clothing (e.g., t-shirt, cap, hijab), 83% received at least one written material (e.g., leaflet, sticker, poster), and 71% attended a public event organized by the campaign (e.g., community meeting, popular theatre, roadshow). To avoid possible self-selection bias, data on compliance are not used in the analysis. Households that were approached by campaigners are regarded as 'treated', irrespective of whether they accepted campaign materials or participated to campaign events. Consequently, our analysis is probably best construed as measuring 'intent-to-treat' effects.

We conducted a field experiment in collaboration with AAIN, the campaign organizer, who agreed to randomize their campaign across locations. The campaign was conducted in six states covering the three main socioeconomic regions of Nigeria: Lagos and Oyo in the Southwest; Kaduna and Plateau in the Middle-Belt/North; and Delta and Rivers in the Niger Delta. These states were chosen because they have a history of recent political violence.[9] Within each of the selected states, two pairs of nearby enumeration areas (EA) were selected randomly from a large and representative sample assembled for the 2007 Afrobarometer survey of Nigeria.[10] We started by selecting twelve villages and urban

[8] For details on this campaign, see http://www.iig.ox.ac.uk/research/08-political-violence-nigeria/default.htm.

[9] Selection is based on reports from earlier elections, such as Human Rights Watch, 'Testing Democracy: Political Violence in Nigeria', April 2003.

[10] The Afrobarometer sample is itself drawn from census data.

neighborhoods for treatment. We then identified twelve control EA's that are located nearby and have the same official urban-rural classification. By design, treatment and control EA's are broadly comparable. Their approximate location is shown in Figure 2.

In both treatment and control EA's, 50 randomly selected households were interviewed for the baseline survey in January 2007 – immediately before the campaign. The total number of respondents is 1200. The same respondents were resurveyed in May 2007, shortly after the elections. We refer to these households as *panel* respondents. To facilitate the evaluation of the direct impact of the campaign, all panel respondents in treated EA's were individually targeted by campaigners, i.e., they were offered campaign materials and were invited to town meetings and popular theatres organized by the campaign.

To study diffusion, in each treated location we interviewed an *oversample* of 25 households not directly exposed to the anti-violence campaign.[11] These were only interviewed in the second survey round. The total number of oversample respondents is 300. They are representative of the population that was not approached by campaigners. Both surveys were designed and supervised in the field by the authors, using original questionnaires pre-tested for the purpose of the experiment. Data collection was undertaken in direct collaboration with Afrobarometer and its Nigerian partner (Practical Sampling International).

Since, by design, the experiment is meant to influence voting behavior primarily by reducing the perceived threat of violence, the surveys were designed to gather information about perceptions and voting behavior. Questions on perceived violence were asked in the

[11] In May 2007 randomly selected respondents were first asked if they had been approached by AAIN campaigners. Only those who answered no to this question were included in the oversample survey.

baseline prior to the campaign, focusing on a reference period ('the last year'). Similar questions were again asked in the second survey, focusing on what had happened just before and during the elections. In the baseline, questions on voting focus on intentions; in the ex post survey, questions refer to actual voting behavior at the April 2007 elections, as reported by respondents. In addition, all respondents to the May 2007 survey – panel and oversample – were asked about their social links to each of the 50 baseline households. An approximate map of each surveyed EA was also draw with the location of each respondent's residence.

3.1. Testing strategy

We are interested in estimating the reinforcement and diffusion effects of the anti-violence campaign. We proceed as follows. Let y_{ivt} denote a relevant outcome variable for individual i in location/village v at time $t = \{0, 1\}$ where 0 stands for baseline and 1 for the post-election survey. Further let $w_{iv} = 1$ if village v was selected for treatment and let $T_{ivt} = 1$ at $t = 1$ and 0 otherwise. The average treatment effect of the campaign is coefficient α in the following regression:

$$y_{iv1} = \delta + \alpha w_{iv} + e_{iv1}, \tag{3.1}$$

or, equivalently:

$$y_{ivt} = \delta + \alpha w_{iv} T_{ivt} + \beta w_{iv} + \gamma T_{ivt} + e_{ivt}, \tag{3.2}$$

if we include baseline data. Given randomization, α in either of these equations provides a consistent estimate of the average treatment effect. Because of the small sample size, however, it may be preferable to include individual fixed effects u_{iv}, which also control for time-invariant village unobservables:

$$y_{ivt} = \alpha w_{iv} T_{ivt} + \gamma T_{ivt} + u_{iv} + e_{ivt} \tag{3.3}$$

Note that 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-differences estimator. Equivalently, (3.3) can be estimated in first-difference:

$$\Delta y_{ivt} = \alpha w_{iv} + \gamma + \Delta e_{ivt} \tag{3.4}$$

In this paper we are not primarily interested in the average treatment effect, which is discussed in detail in Collier and Vicente (2009). Our focus is on reinforcement and diffusion through social networks. Let g denote a social network matrix where $g_{ij} = 1$ if i is linked to baseline household j in a way deemed relevant for our purpose. It is important that g_{ij} be exogenous to the campaign itself. Remember that, by design, all baseline households were visited by the campaign. We therefore postulate that the influence of the campaign increases with the number of links respondents have to baseline respondents.[12] Formally, let $\widetilde{n}_i = \frac{1}{50}\sum_{j=1}^{50} g_{ij}$. Following Wooldridge (2002) regarding the estimation of heterogeneous treatment effect models, we calculate the de-meaned equivalent $n_i = \widetilde{n}_i - \frac{1}{N}\sum_{j=1}^{N} \widetilde{n}_j$ where N is total sample size.

[12] Given that all respondents in one EA are asked about the same 50 treated households, we are unable to distinguish whether influence comes from the *number* or the *proportion* of treated neighbors.

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

$$y_{iv1} = \delta + \alpha w_{iv} + \theta w_{iv} n_i + \tau \widetilde{n}_i + e_{iv1} \tag{3.5}$$

The parameter of interest is θ, which measures the indirect influence on y_{iv1} of the social proximity of individual i to individuals exposed to the anti-violence campaign. Given the experimental design, these individuals can be seen as having been randomly exposed to a specific message.

If we include time 0 information, the estimated model takes the form:

$$\begin{aligned} y_{ivt} &= \delta + \alpha w_{iv} T_{ivt} + \theta w_{iv} T_{ivt} n_i + \tau n_i T_{ivt} \\ &\quad + \lambda n_i w_{iv} + \varphi \widetilde{n}_i + \beta w_{iv} + \gamma T_{ivt} + e_{ivt} \end{aligned} \tag{3.6}$$

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

$$\Delta y_{ivt} = \alpha w_{iv} + \theta w_{iv} n_i + \tau \widetilde{n}_i + \gamma + \Delta e_{ivt} \tag{3.7}$$

We also seek to test whether influence depends on geographical distance $\widetilde{d}_{ij}$ between i and j. Distance can be seen as defining a valued network. Influence now depends on how close respondent i is to other villagers, namely those exposed to the anti-violence campaign. Let $\widetilde{d}_i = \frac{1}{K}\sum_{j=1}^{K} \widetilde{d}_{ij}$, where K is the number of respondents in the same location. Like before, the variable we use is the demeaned equivalent $d_i = \widetilde{d}_i - \frac{1}{N}\sum_{j=1}^{N} \widetilde{d}_j$ where N is total sample size. We reestimate models (3.5), (3.6) and (3.7) with d_i and $\widetilde{d}_i$ in lieu of n_i and $\widetilde{n}_i$.

We conduct two different sets of comparisons. To test for the presence of a reinforcement effect associated with social or geographical proximity, we compare panel respondents from control and treatment villages using models (3.5), (3.6) and (3.7). In these regressions, the coefficient θ of the heterogeneous network effect measures the extent to which the effect of the treatment w_{iv} on the outcome variable y_{ivt} is magnified by proximity with other individuals who have also been exposed to the anti-violence campaign. This reinforcement effect can be viewed as a kind of social multiplier effect by which the message effects are strengthened for the primary targets of campaigning through communication with their networked individuals.

We also test whether the campaign message communicated directly to panel respondents affects residents of the same village who did not receive the campaign message directly. To this effect we compare respondents in control villages – who were not affected, either directly or indirectly, by the campaign – to oversample respondents in treated villages – who were not directly exposed to the campaign but were exposed indirectly through other villagers. In this case, coefficient α captures the externality that indirect exposure to the campaign generates for all unexposed individuals in treated villages while θ can be regarded as measuring diffusion of the effect of the campaign through social networks.

The comparison between panel respondents in control and treated villages poses no particular problem, given that treatment was allocated randomly to matched pairs of villages. The comparison between oversample and control respondents is potentially problematic given that (non-)exposure to the campaign message within a treated village may be correlated with respondent characteristics that also affect the outcome variable y_{ivt}.

This is more a source of concern for α – the average treatment effect – than for θ – the heterogeneous treatment effect. Furthermore, it affects models (3.5) and (3.6) more than model (3.7) where the addition of respondent fixed effects hopefully takes care of most of the problem.[13] In the next sections we deal with this issue the best we can given data constraints. We begin by testing balancedness of the different sub-samples. Whenever necessary, we introduce individual controls in models (3.5) and (3.6) to control for selection on observables. We also investigate the sensitivity of our results to the possibility of selection on unobservables.

4. Data

Balancedness is investigated in Table 1 where we report baseline values for a wide range of respondent characteristics. There is no noticeable difference between panel households in control and treated villages: only one variable is significantly different at the 10% level, a normal finding given the number of variables considered. Attrition is not a serious concern: 97% of control baseline respondents also answered the post-election survey; the corresponding percentage for treated villages is 95%.

We also compare panel households in control villages with oversample households in treated villages. Most characteristics are not significantly different between oversample and control households. There is, however, a small subset of variables for which balancedness does not hold: namely, schooling, religious intensity, and ownership of radios (all higher in the oversample). We control for these variables in the subsequent analysis.

[13] Because oversample respondents could only be identified ex-post, that is, after the campaign had taken place, another possible source of bias is recall bias. This issue is discussed in detail in the empirical section.

There is thus some evidence of selection into the oversample (respondents stating up-front that they were not approached by AAIN campaigners). A possibility is that 'more average' (e.g. less schooled) respondents over-reported campaign reach and this resulted in them being left out of the oversample.

Two measures of social distance are used in the analysis. For the first one, a link from i to j is assumed to exist if i could identify the name of j when prompted, and i stated that he/she talks to j on a regular basis.[14] We call this variable *chatting*. We also construct another measure of social proximity, whereby a link from i to j exists if i can identify j by name and reports being related to j.[15] We call this variable *kinship*.

We also investigate the effect of geographical distance between i and j, what we call *distance*. Each enumerator was asked to locate each respondent and his/her itinerary 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 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.[16] The result of these calculations is our variable $\widetilde{d}_{ij}$, which is then used to compute $\widetilde{d}_i$, the average distance to all respondents in the same location. As shown in Table 2 all network measures are balanced across treatment and control groups.

In this paper, we focus our attention on five outcome variables. This concise set is

[14] 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-Sometimes-Frequently'.

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

[16] This is accomplished by using the subset of pairwise distances reported by enumerators.

chosen because it captures the range of information collected in the survey – see Collier and Vicente (2009) for details. The first two variables focus on respondents' perceptions regarding violence. The first of these two, which we call *violence*, is the answer to the question 'In your experience, how often did violent conflicts arise between people within the community where you live? Never-Always on a 0-4 scale'. Given the timing of the surveys, it proxies for respondents' opinion of the severity of political violence within the community. The second of the two, which we dub *intimidation*, is the answer to the question 'How often, if ever, have you or anyone in your family been physically threatened? Never-Many times on a 1-4 scale'. Given its more precise wording, it can be regarded as a proxy for the level of political intimidation experienced by respondents. Variables *violence* and *intimidation* are scaled so that higher values correspond to worse outcomes.

The other three outcome variables of interest capture electoral behavior. The first, which we name *postcard*, is an experimentally generated measure of political empowerment. Each respondent in the post-election survey was given a stamped postcard with an anti-violence message, and encouraged to mail it to AAIN as manifestation of their disapproval towards electoral violence. If the respondent mailed the postcard, the variable postcard takes value 1. It was promised that if enough postcards were received from the respondent's state, AAIN would flag that state in the media as facing electoral violence problems. This process mimicked petitioning, except that it was likely perceived as anonymous. Even though it incurred no financial outlay for the respondent, sending the postcard requires some effort. This empowerment measure can therefore be regarded as incentive-compatible.

The second behavior variable, which we call *voting*, takes value 1 if the respondent voted for Atiku Abubakar, AC's presidential candidate. This candidate was generally associated with political instability. The third variable is voter turnout at the gubernatorial elections, which we name *turnout*. We focus on state-level elections because they are arguably the most associated with political violence at the local level.

In Table 2 we present average perceptions of violence reported by treatment and control households. For panel households, the data come from the baseline survey; for oversample households, the data come from retrospective questins. We find no statistically significant difference between treatment and control panel households. We do however find a significant difference in the level of violence reported by oversample households, who report a lower level of political violence at the time of the baseline. This may reflect recall bias – respondents underestimate their own perception of violence prior to treatment. We do not, however, find evidence of a similar recall bias for other retrospective questions, suggesting that recall bias may not be the reason. Alternatively, it may reflect self-selection out of treatment: households who perceived less violence avoided campaigners.

To address the possibility of recall bias, in the next Section we present regression results based on second-round data only. Possible self-selection out of treatment among the oversample households is addressed in the usual way, e.g., by including control variables in the analysis to control for selection on observables, and by instrumenting selection into the oversample to control for selection on unobservables. For comparison purposes, we also include a measure of actual violence compiled by independent journalists.[17] This

[17] These measures were gathered through the deployment of an independent journalist in each experimental location. These 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). See Collier and Vicente (2009) for details.

shows no significant difference in political violence between treatment and control EA's prior to the campaign.

At the bottom of Table 2 we compare underlying electoral preferences across treatment and control. The first four variables report electoral behavior at the 2003 elections. We see no significant difference between control households and either panel or oversample households. Baseline interest in politics also appears similar across both comparison groups.

5. Empirical results

Table 3 presents our first set of results from model (3.5), which we reproduce here for memory:

$$y_{iv1} = \delta + \alpha w_{iv} + \theta w_{iv} n_i + \tau \widetilde{n}_i + e_{iv1}$$

The outcome variable is *violence* – i.e., respondents' perception of community violence. The estimator is ordinary least squares. Reported t-values are clustered by enumeration area. We are primarily interested in θ, the parameter of the interaction term between treatment w_{iv} and either social network n_i (chatting and kinship) or geographical distance d_i. Because isolation falls with n_i but rises with d_i, we expect coefficients to have opposite signs when handling each of these two variables.

Two sets of results are presented. The first set, shown in columns 1 to 4, compares panel respondents in the treated and control villages. Here the interpretation of θ is that of a reinforcement effect. The second set of results, shown in columns 5 to 8, compares oversample respondents in treated villages to panel households in control villages. A significant θ is evidence of diffusion effect. The campaign may also affect unexposed villagers in ways other than diffusion through social networks.[18] This is captured by the coefficient α of the treatment village dummy w_{iv} which measures the total indirect effects of the campaign on individuals who were not directly exposed to it. Individual controls are included in all regressions.[19]

Results show that the perception of community violence is significantly lower in treated villages. Coefficient α is negative and significant whether the question was answered by individuals directly exposed to the campaign, or individuals who were not directly affected by it. This is consistent with the campaign having beneficial externalities on individuals not directly exposed to it. Turning to θ, we find no evidence of beneficial social network effects from chatting or kinship, either in the sense of reinforcement (columns 2 and 3) or in the sense of diffusion (columns 6 and 7). Coefficient θ is significant for chatting when comparing oversample to control, but with a sign contrary to soothing effects of the campaign.

In contrast, geographical distance is strongly significant for both the reinforcement and diffusion models (columns 4 and 8): the coefficient θ of the distance-treatment interaction term is strongly positive, while τ, the coefficient of distance alone, is strongly negative. We also see that α is now no longer significant. Coefficient estimates are basically identical whenever we compare control respondents either to panel respondents or to oversample respondents.

[18] Or via diffusion through social networks that we do not observe.

[19] These controls are selected from the variables displayed in Table 1 (demographics) and the bottom part (baseline political preferences) of Table 2. The exact same control variables are used in all regressions where individual controls are reported to be included.

Coefficient τ captures the way in which perceptions of community violence vary systematically with distance from same-location respondents. Since respondents were randomly selected, distance from other households basically measures the 'peripherality' of each household: more centrally located households have a smaller distance, while those located at the periphery of the village have a larger distance d_i. The significantly negative τ coefficient we observe in the results implies that households that live at the periphery of control villages have lower perception of community violence. Conversely, those living in the center of those villages perceive higher community violence. This negative relationship between distance and perceptions of violence disappears in treated villages. This means that perceptions of violence fall among centrally located households but they increase for those at the periphery. Since most people live close to center, the perception of violence falls on average, as shown in columns 1 and 5.[20]

Next we examine the impact of the campaign on feelings of intimidation. Results from model (3.5) are shown in Table 4. Judging from the average treatment effects on the exposed (column 1) and unexposed (column 5), the campaign seems to have no beneficial effect on intimidation: the treatment dummy has the anticipated sign but is not significant. Network effects, however, are significant with the expected sign for reinforcement effects (columns 3 and 4). In control villages, panel respondents with more relatives among panel households on average feel more intimidated. But this difference vanishes

[20]Perpetrators of electoral violence may be recruited among socially isolated individuals. By making the community more assertive in its resistance towards violence, the campaign makes perpetrators of violence feel less secure. Indeed, we have some evidence of that: we ran regressions of survey measures of sympathy for unlawfulness on our measures of networks; we find a clear positive effect of geographical distance (regressions available upon request). Another possibility is that most respondents believe that sympathizers of electoral violence are found among individuals who are socially isolated. By strengthening the resolve of the majority, the anti-violence campaign may have made isolated individuals feel threathened, whether or not they personally condone violence.

in treated villages. A similar pattern is observed for distance with, as expected, the sign reversed. Coefficients of a similar sign and magnitude are estimated for diffusion (columns 7 and 8), but the interaction coefficient θ is never statistically significant. As in Table 3, the diffusion interaction term with chatting is significant but with the unanticipated sign.

In Table 5 we show similar results for the *postcard*. Here y_{ivt} takes value 1 if the respondent household sent the postcard provided by the enumerators, and 0 otherwise. The estimator is logit. The campaign by itself appears to have a positive effect on the likelihood that respondents households return the postcard - even though statistical significance is not achieved in the shown table of results. But interaction terms with both social proximity variables, when estimating diffusion effects, are positive and significant at the 10% level (columns 6 and 7). We observe coefficients of a similar magnitude for reinforcement (columns 2 and 3) but the effect is not significant.

Results for voting behavior are presented in Tables 6 and 7. In Table 6 the dependent variable takes value 1 if the respondent declared voting for AC as presidential candidate, and 0 otherwise. In general the campaign had a negative effect on this variable; however, the average treatment effect is not significant either on respondents exposed to the campaign (column 1) or on their unexposed co-villagers (column 5). Distance, however, matters. In treated villages, respondents who live further away from other respondents – i.e., those who live at the outskirts of the village – are more likely to vote for the opposition. This is true for both the reinforcement and the diffusion effect which, once again, are seen to operate in similar fashion. In Table 7 the binary dependent variable takes value 1 for respondents who reported voting in the gubernatorial elections. We do not find significant network effects on *turnout* either in terms of reinforcement or in terms of diffusion.

Taken together, these results offer some evidence in favor of both reinforcement and diffusion effects. What is most reassuring is that, in many cases, results for reinforcement and diffusion are similar: they nearly always have the same sign and often are significant at the same time. Physical distance from the center of the village – measured by the total distance to panel respondents – seems to play a more important role than chatting with friends, which is hardly ever significant, and at times appears with an unexpected sign.

We now investigate the robustness of our results by estimating models (3.6) and (3.7). Model (3.6) is estimated with individual controls; model (3.7) controls for individual fixed effects. In contrast with the results reported so far which only use post-election responses, models (3.6) and (3.7) make use of information on the value of the dependent variable y_{ivt} at time 0. By design the postcard was only distributed after the treatment and thus only exists for period 1. Hence models (3.6) and (3.7) cannot be estimated for *postcard*. Using the baseline data has the *a priori* advantage of controlling for differential time trends across treatment and control groups. But, as mentioned earlier, for some regressions information about y_{ivt} comes from recall questions asked to respondents after the elections. This introduces the possibility of recall bias, which was not a concern in the results reported in Tables 3 to 7. For this reason, we are mostly interested in results that are robust under both specifications.

Results for dependent variable *violence* are reported in Table 8. The coefficient of $T_{ivt}w_{iv}$ (time-treat) gives α, the average treatment effect. The coefficient of $n_iT_{ivt}w_{iv}$ (network-time-treat) gives θ, the heterogeneous effect of social links. In the distance regressions, n_i is simply replaced with d_i as before. The individual fixed-effect model

(3.7) is estimated in first differences.[21] The estimator is ordinary least squares with errors clustered by village.

We find strong evidence of both reinforcement and diffusion effects with respect to kinship links and geographical distance: estimated coefficients for the triple interaction term $n_i T_{ivt} w_{iv}$ (network-time-treat) are negative and significant for kinship, and positive and significant for distance. The magnitude of estimated coefficients is in general very similar between models (3.6) and (3.7), 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. Note that, even though chatting-network effects remain positive, they lose statistical significance. These findings reinforce and broaden earlier findings from Table 3.

Next we look at perceptions of intimidation. Estimation results, reported in Table 9, are very similar to those shown in Table 8: coefficients for the triple interaction terms are significant with the anticipated sign in the kinship and distance regressions. This confirms the presence of both reinforcement and diffusion effects of the campaign on respondents' perceptions.

We find no evidence of an impact of the campaign on behavior, however. Results for *voting (for AC)* and *turnout* are reported in Tables 10 and 11. Model (3.7) is estimated using fixed-effects logit. None of the triple interaction terms is significant, and some take unlikely – albeit non-significant – values. This may be because the dependent variables are dichotomous and hence contains little information: indeed, when estimating (3.7) all observations with identical values of y_{ivt} over time are dropped, dramatically reducing

[21] To facilitate comparison, we have aligned coefficients according to their meaning in model (3.6).

sample size.

To summarize, results suggest the presence of reinforcement and diffusion effects for kinship and distance, in particular when we consider perception outcomes. The two network measures may be correlated, however. This raises the question of which of the two matters. To investigate this issue, we reestimate model (3.7) with both interaction terms combined:

$$\Delta y_{ivt} = \alpha w_{iv} + \theta_1 w_{iv} n_i + \tau_1 n_i + \theta_2 w_{iv} d_i + \tau_2 d_i + \gamma + \Delta e_{ivt} \tag{5.1}$$

Results are shown in Table 12. For the *postcard* regression, we report results for a one-period version of (5.1). For *violence* and *intimidation*, the model is estimated in first difference. For *voting (for AC)* and *turnout*, estimates are obtained using fixed-effects logit. We find that the strongest and most consistent results are obtained for kinship: it is significant in 5 of the 8 regressions reported in Table 10. This confirms earlier findings. When we control for kinship, physical distance to panel respondents no longer matters – except for the reinforcement regression for the *violence* outcome where it remains significant.

5.1. Robustness

Collier and Vicente (2009) run a variety of robustness checks on the average treatment effect of the campaign using the same data as this paper. They look for evidence of conformity bias that would stem from respondents answering survey questions in a way that artificially adapts to expected effects of the treatment. However no such evidence is found.

Changes in perceptions of violence in the respondent's location do not differ significantly when comparing subjects who were confronted with the whole experimental activities and those who only responded to the post-election questionnaire and are therefore less prone to conformity. Furthermore, the intensity of violence measured by independent journalists in each EA is also reported to have decreased. Finally, in the spirit of Nickerson (2008) who finds that voting behavior is 'contagious', network-correlated individual voting behavior (i.e. voting behavior explained by that of each individual's closest subjects) is regressed on the treatment. This exercise assumes that any voting mismeasurement (conformity) is not correlated within networks. Average treatment effects are found to be maintained.

Collier and Vicente (2009) also test whether control locations were contaminated by the AAIN's campaign. To this effect, they regress outcome variables in control EA's on the distance to the nearest treatment EA. They find no clear effects and conclude that contamination is not of overall concern. They also point out that, if present, contamination would result in underestimated average treatment effects. Please refer to Collier and Vicente (2009) for details.

Turning to the main focus of this paper on network effects, we subjected our results to various robustness checks for the diffusion effect. As explained earlier, oversample respondents were identified *after* the campaign among households that had not been directly exposed to it. Comparing oversample and control households in Table 2 led us to consider the possibility of selection bias. So far we have dealt with this possibility by including additional controls – i.e., individual characteristics or fixed effects. But there remain other sources of concern.

One is that, in the presence of heterogeneous effects, the average treatment effect is mismeasured. To investigate this possibility, we reestimate the average diffusion effect using a matching method. This approach ensures that control households are only compared to oversample households that are sufficiently similar to them in terms of observables. We use the nearest-neighbor matching procedure proposed by Abadie and Imbens (2006).[22] This non-parametric approach bypasses the difficulties associated with propensity score matching – especially issues regarding balancedness of an *a priori* set of observables. Results shown in Table 13 confirm the presence of a diffusion impact on households not directly exposed to the anti-violence campaign: the impact is positive and significant for perceptions of community violence and for the postcard. The campaign also has reduced voting for the opposition and increased voter turnout. These findings lend clear credibility to the homogeneous effects we estimated before.

Our last set of robustness checks seeks to instrument (the absence of) treatment for oversample households. Our main concern is the possibility that oversample households differ in meaningful but unobserved ways from control households, and that this causes spurious estimates of heterogeneous diffusion effects. Our ability to deal with this concern is limited by the available data. We use two instruments for oversample households: an average of questions about membership in village institutions[23] and a measure of physical isolation (distance to the mean coordinates of the panel respondents). The rationale for this choice of instruments is that oversample households may have avoided exposure to the campaign because they are socially and geographically isolated. These instruments satisfy

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

[23] 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 neighbourhood watch ("vigilante") committee.'.

the inclusion restriction: they are jointly significant in the instrumenting regression with an F-statistic well in excess of 10. As recommended by Wooldridge (2002), Chapter 18, estimated propensity scores $\widehat{w}_{iv}$ from the instrumenting regression are used as instruments for w_{iv} in (5.1), while $\widehat{w}_{iv}n_i$ is used as instrument for $w_{iv}n_i$ and $\widehat{w}_{iv}d_i$ is used as instrument for $w_{iv}d_i$. Results are presented in Table 14. We find significant interaction effects for distance in the *violence* and *intimidation* regressions, which confirm the existence of diffusion effects. But the interaction term with *kinship* is no longer significant. We also confirm a significant *kinship* interaction effect in the *postcard* regression while a positive *kinship* effect on *turnout* emerges for the first time. While they should be taken with a grain of salt, these results nevertheless constitute additional evidence in support of diffusion effects.

6. Conclusion

In this paper we have reported results from a field experiment designed to evaluate the reinforcement and diffusion effects of a campaign to discourage electoral violence. Information was collected on social networks and geographical distance between households targeted by an awareness campaign. To test for the presence of a reinforcement effect, we examined whether the impact of the campaign on perceptions and behavior among treated households is reinforced by proximity to other treated households. To investigate diffusion to unexposed households, we test whether households not directly exposed to the campaign show effects that are similar to exposed households and whether the impact is stronger when they are closer – in a social or spatial sense – to other households.

Results provide evidence of both diffusion and reinforcement effects. Findings suggest that the impact of the campaign on perceptions of violence is reinforced by social (kinship) and geographical proximity to other households. We however find little reinforcement effect on behavior. For perceptions of violence, the diffusion effect nearly perfectly mimics the reinforcement effect. We find a significant externality of the campaign on households' willingness to express disapproval of electoral violence, but unclear effects on voting behavior *per se*.

The findings presented in this paper together with those reported by Collier and Vicente (2009) suggest that an anti-violence campaign of the kind implemented prior to the 2007 Nigerian elections by AAIN was effective in reducing perceptions of community violence and intimidation. It also affected respondents' willingness to express their disapproval of electoral violence. Part of the effect of the campaign (in particular for perceptions) can be attributed to reinforcement and diffusion effects among kin and neighbors. This is reassuring as it indicates that a campaign such as this one reaches more people than those directly exposed to it, and that those exposed to it probably discuss it among themselves in ways that reinforce its impact. For these same reasons, awareness campaign such as the one studied here can be expected to have less impact on socially and geographically isolated individuals. Yet these less well integrated individual – who are more likely to be disenfranchised – may themselves be a source of electoral violence, either directly or because they are manipulated by cynical politicians. A campaign directed at them may reduce the risk of electoral violence directly.

In the results reported here, social and geographical proximity between households is taken as given and remains outside the control of the researcher. Yet if proximity reinforces the impact of the campaign and diffuses its effect more widely, it may be possible to magnify campaign impact by fostering the formation of links among exposed people, as well as between exposed and non-exposed people. How this could be achieved is unclear, but one idea worth investigating is the possibility of identifying local relays for the campaign message – churches, civil society – that could magnify its effect by canvassing their neighborhood. This deserves further investigation.

References

Abadie, A., and G. Imbens (2006): "Large Sample Properties of Matching Estimators for Average Treatment Effects," *Econometrica*, 74(1), 235–267.

Achebe, C. (1983): *The Trouble with Nigeria*. Heinemann Educational Publishers.

Angelucci, M., G. DiGiorgi, M. Rangel, and I. Rasul (2009): "Insurance and Investment in Family Networks„" (mimeograph).

Bandiera, O., and I. Rasul (2006): "Social Networks and Technology Adoption in Northern Mozambique," *Economic Journal*, 116(514), 862–902.

Collier, P., and P. C. Vicente (2009): "Votes and Violence: Evidence from a Field Experiment in Nigeria," Discussion paper, Oxford University and BREAD, Working Paper.

Dahl, G., and S. Dellavigna (2009): "Does Movie Violence Increase Violent Crime?," *Quarterly Journal of Economics*, 124, 677–734.

DellaVigna, S., and E. Kaplan (2007): "The Fox News Effect: Media Bias and Voting," *Quarterly Journal of Economics*, 122, 1187–1234.

Eifert, B., E. Miguel, and D. Posner (2009): "Political Competition and Ethnic Identification in Africa," *American Journal of Political Science*, (forthcoming).

Habyarimana, J., M. Humphreys, D. N. Posner, and J. M. Weinstein (2007): "Why Does Ethnic Diversity Undermine Public Goods Provision?," *American Political Science Review*, 101(4), 709–725.

Kremer, M., and E. Miguel (2004): "Worms: Identifying Impacts on Education and Health in the Presence of Treatment Externalities," *Econometrica*, 72(1), 159–217.

Kuran, T. (1989): "Sparks and Prairie Fires: A Theory of Unanticipated Political Revolution," *Public Choice*, 61, 41–74.

Macours, K., and R. Vakis (2008): *Changing Households' Investments and Aspirations through Social Interactions: Evidence from a Randomized Transfer Program in a Low-Income Country.* World Bank, (Working Paper).

Nickerson, D. W. (2008): "Is Voting Contagious? Evidence from Two Field Experiments," *American Political Science Review*, 102(1), 49–57.

Posner, D. N. (2004): "The Political Salience of Cultural Difference: Why Chewas and Tumbukas are Allies in Zambia and Adversaries in Malawi," *American Political Science Review*, 98(4), 529–545.

Vicente, P. C. (2007): *Is Vote Buying Effective? Evidence from a Field Experiment in West Africa*. Oxford University and BREAD, (Working Paper).

Wantchekon, L. (2003): "Clientelism and Voting Behavior: Evidence from a Field Experiment in Benin," *World Politics*, 55, 399–422.

Wooldridge, J. M. (2002): *Econometric Analysis of Cross Section and Panel Data*. MIT Press, Cambridge, Mass.

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

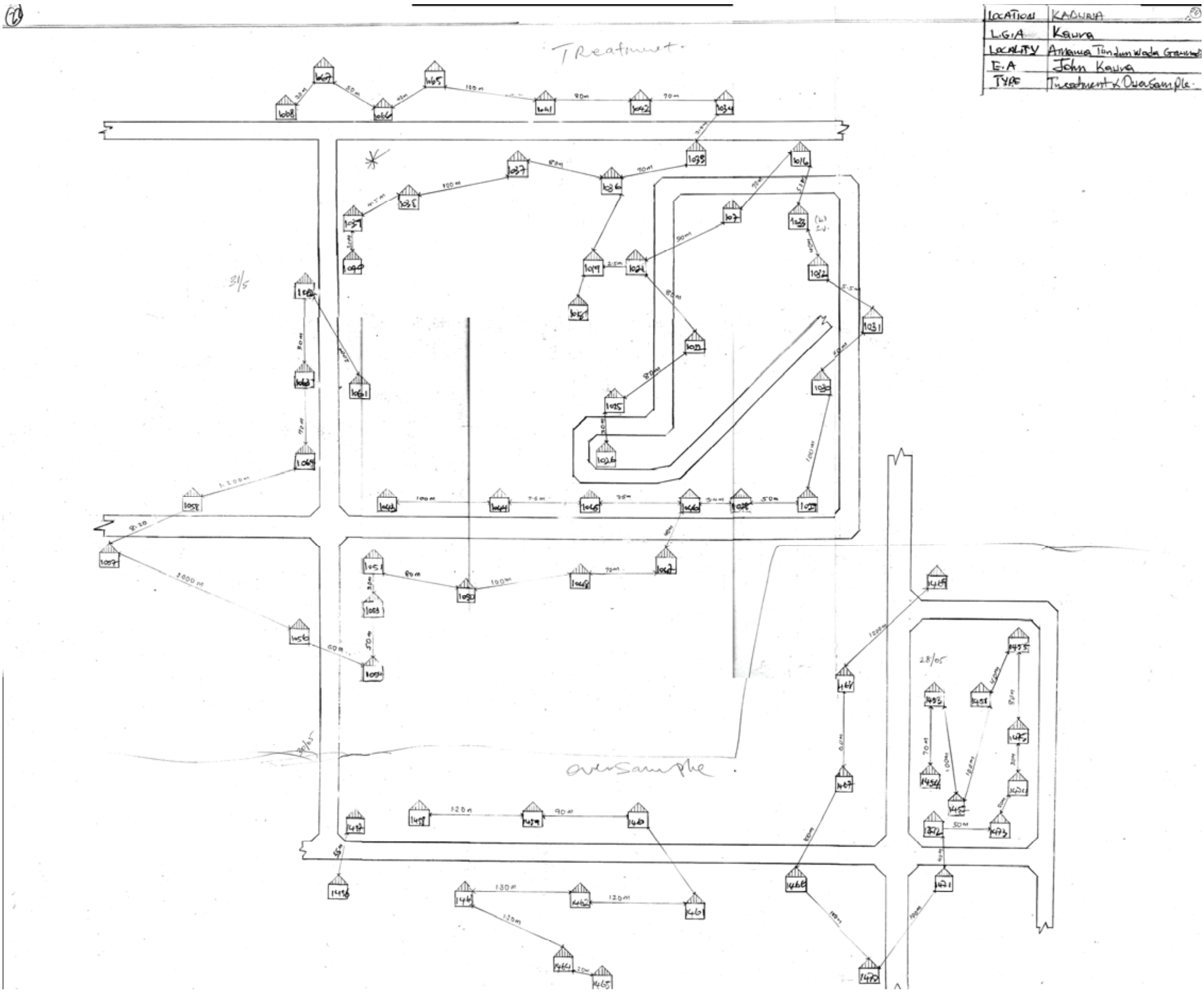

Table 1: Differences across Treatment and Control Areas - Demographics, Attrition

		Control	Treatment (panel)	Difference	Number of Observations	Treatment (oversample)	Difference	Number of Observations
Basic Demographics	female	0.500	0.500	0.000	1,200	0.500	-0.000	900
							0.006	
	age	32.955	33.027	0.072	1,198	32.030	-0.925	897
				0.960			1.373	
	household size	6.430	6.332	-0.098	1,200	6.727	0.297	900
				0.708			0.843	
	single	0.383	0.392	0.009	1,149	0.473	0.091	880
				0.049			0.064	
	married	0.581	0.585	0.004	1,149	0.490	-0.091	880
				0.046			0.061	
	schooling (0-9)	4.308	4.673	0.365	1,200	5.140	0.832***	900
				0.374			0.288	
Ethnicity and Religion	yoruba	0.318	0.273	-0.045	1,200	0.283	-0.035	900
				0.166			0.170	
	hausa	0.072	0.160	0.088	1,200	0.157	0.085	900
				0.089			0.086	
	igbo	0.157	0.102	-0.055	1,200	0.097	-0.060	900
				0.113			0.117	
	christian	0.621	0.762	0.141	1,199	0.687	0.066	899
				0.124			0.135	
	muslim	0.344	0.233	-0.111	1,199	0.293	-0.051	899
				0.129			0.141	
	religious intensity (1-6)	4.764	5.022	0.258	1,185	5.190	0.426*	889
				0.206			0.236	
Occupation	job stability (0-3)	1.363	1.378	0.015	1,200	1.483	0.120	900
				0.157			0.198	
	agriculture	0.158	0.117	-0.042	1,200	0.117	-0.042	900
				0.064			0.072	
	industry/services: trader	0.125	0.118	-0.007	1,200	0.170	0.045	900
				0.032			0.039	
	housework	0.120	0.098	-0.022	1,200	0.083	-0.037	900
				0.033			0.044	
Property and Expenditure	house	0.606	0.605	-0.001	1,199	0.512	-0.094	896
				0.107			0.118	
	land	0.526	0.573	0.047	1,199	0.515	-0.011	894
				0.114			0.122	
	cattle	0.329	0.327	-0.002	1,198	0.441	0.112	896
				0.100			0.102	
	radio	0.888	0.928	0.040	1,199	0.940	0.052*	899
				0.033			0.031	
	cell phone	0.512	0.608	0.096	1,197	0.542	0.030	897
				0.116			0.130	
	household expenditure (naira/month)	19,001	22,188	3,186.514	1,003	24,162	5,161	770
				4,655.297			5,119	
Panel Attrition	panel re-surveying	0.97	0.95	-0.02	1200			
				0.01				

Note: Standard errors reported; these are corrected by clustering at the location (census area) level.

* significant at 10%; ** significant at 5%; *** significant at 1%. These results come from OLS regressions.

Table 2: Differences across Treatment and Control Areas (Networks, Baseline Violence and Political Preferences)

		Control	Treatment (panel)	Difference	Number of Observations	Treatment (oversample)	Difference	Number of Observations
Networks	chatting (0-1)	**0.023**	**0.040**	**0.017** 0.025	1,149	**0.030**	**0.007** 0.016	880
	kinship (0-1)	**0.069**	**0.100**	**0.031** 0.051	1,149	**0.083**	**0.014** 0.052	880
	distance (metres)	**302.776**	**780.999**	**478.223** 331.806	1,089	**941.266**	**638.490** 455.485	846
Baseline Violence	violence (0-4)	**1.175**	**1.283**	**0.108** 0.176	1,184	**0.380**	**-0.795***** 0.141	884
	intimidation (1-4)	**1.142**	**1.089**	**-0.053** 0.040	1,195	**1.100**	**-0.042** 0.041	898
	actual (journals) (1-5)	**2.720**	**2.730**	**0.010** 0.240	57			
Baseline Politics	turnout 2003	**0.699**	**0.699**	**-0.000** 0.064	1,174	**0.656**	**-0.043** 0.071	873
	pdp 2003	**0.453**	**0.483**	**0.030** 0.085	1,149	**0.460**	**0.007** 0.095	880
	anpp 2003	**0.159**	**0.079**	**-0.080** 0.075	1,149	**0.100**	**-0.059** 0.085	880
	ac 2003	**0.026**	**0.046**	**0.020** 0.023	1,149	**0.033**	**0.007** 0.022	880
	interest in public affairs (0-3)	**1.835**	**1.810**	**-0.025** 0.117	1,189	**1.779**	**-0.056** 0.133	892
	discuss politics (0-2)	**1.079**	**1.133**	**0.054** 0.062	1,188	**0.976**	**-0.103** 0.073	892

Note: Standard errors reported; these are corrected by clustering at the location (census area) level. Baseline party scores refer to presidential elections.
* significant at 10%; ** significant at 5%; *** significant at 1%. These results come from OLS regressions.

Table 3: Regressions of 'Conflict within Community'

Dependent Variable ----->		Homogeneous Effect (Panel vs. Control)	Reinforcement Effect (Panel vs. Control)			Homogeneous Effect (Oversample vs. Control)	Diffusion Effect (Oversample vs. Control)		
		Conflict within Community							
			Chatting	Kinship	Distance		Chatting	Kinship	Distance
		OLS							
Main Explanatory Variables	treated village	-0.325***	-0.320***	-0.334***	-0.032	-0.414***	-0.409***	-0.423***	-0.133
		(-2.912)	(-2.847)	(-2.942)	(-0.245)	(-3.743)	(-3.852)	(-3.825)	(-1.073)
	network		-0.735	0.526	-0.900***		-0.602	0.591	-0.904***
			(-1.015)	(0.571)	(-2.747)		(-0.798)	(0.635)	(-3.235)
	network*treat		**1.080**	**-0.356**	**0.943*** **		**2.069** **	**-0.136**	**0.958*** **
			(1.423)	**(-0.388)**	**(2.868)**		**(2.466)**	**(-0.149)**	**(3.362)**
Demographic/Political Controls		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared		0.099	0.099	0.099	0.131	0.122	0.127	0.125	0.160
Number of Observations		971	971	971	900	744	744	744	708

Note: t-stats reported; these are corrected by clustering at the location (enumeration area) level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4: Regressions of 'Physical Intimidation'

Dependent Variable ----->		Homogeneous Effect (Panel vs. Control)	Reinforcement Effect (Panel vs. Control)			Homogeneous Effect (Oversample vs. Control)	Diffusion Effect (Oversample vs. Control)		
		Physical Intimidation							
			Chatting	Kinship	Distance		Chatting	Kinship	Distance
		OLS							
Main Explanatory Variables	treated village	-0.019	-0.019	-0.025	0.028	-0.047	-0.045	-0.055*	-0.002
		(-0.658)	(-0.655)	(-0.831)	(0.722)	(-1.535)	(-1.508)	(-1.763)	(-0.050)
	network		0.026	0.442**	-0.139*		-0.162	0.531***	-0.134*
			(0.073)	(2.236)	(-1.807)		(-0.403)	(2.918)	(-1.682)
	network*treat		**-0.005**	**-0.435** **	**0.152***		**0.929** **	**-0.245**	**0.132**
			(-0.013)	**(-2.065)**	**(1.915)**		**(2.339)**	**(-1.284)**	**(1.558)**
Demographic/Political Controls		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared		-0.007	-0.009	-0.005	-0.006	0.010	0.017	0.023	0.008
Number of Observations		978	978	978	906	747	747	747	711

Note: t-stats reported; these are corrected by clustering at the location (enumeration area) level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5: Regressions of 'Postcard'

Dependent Variable ----->		Postcard							
		Homogeneous Effect (Panel vs. Control)	Reinforcement Effect (Panel vs. Control)			Homogeneous Effect (Oversample vs. Control)	Diffusion Effect (Oversample vs. Control)		
			Chatting	Kinship	Distance		Chatting	Kinship	Distance
		Logit							
Main Explanatory Variables	treated village	0.366	0.335	0.380	0.307	0.055	0.030	0.079	0.149
		(0.902)	(0.844)	(0.942)	(0.755)	(0.129)	(0.073)	(0.200)	(0.334)
	network		0.727	-2.265	0.240		2.074	-0.696	0.041
			(0.453)	(-0.680)	(0.295)		(1.419)	(-0.264)	(0.054)
	network*treat		**2.080**	**3.939**	**-0.330**		**4.365***	**4.461***	**-0.255**
			(1.181)	**(1.177)**	**(-0.396)**		**(1.926)**	**(1.719)**	**(-0.328)**
Demographic/Political Controls		**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**
Adjusted R-squared		0.038	0.047	0.051	0.040	0.072	0.087	0.096	0.077
Number of Observations		980	980	980	908	748	748	748	712

Note: t-stats reported; these are corrected by clustering at the location (enumeration area) level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6: Regressions of 'Voting for AC (Opposition) in Presidential Elections'

Dependent Variable ----->		Voting for AC (Opposition) in Presidential Elections							
		Homogeneous Effect (Panel vs. Control)	Reinforcement Effect (Panel vs. Control)			Homogeneous Effect (Oversample vs. Control)	Diffusion Effect (Oversample vs. Control)		
			Chatting	Kinship	Distance		Chatting	Kinship	Distance
		Logit							
Main Explanatory Variables	treated village	-0.048	-0.016	-0.107	-0.237	-0.264	-0.164	-0.277	-0.980**
		(-0.117)	(-0.038)	(-0.237)	(-0.748)	(-0.359)	(-0.247)	(-0.370)	(-2.083)
	network		-0.934	1.528	-0.348		-0.542	1.642	-0.336
			(-0.808)	(1.080)	(-1.005)		(-0.458)	(1.053)	(-1.104)
	network*treat		**1.242**	**-2.258**	**0.914****		**3.633****	**-0.259**	**1.170*****
			(0.760)	**(-1.148)**	**(2.442)**		**(2.155)**	**(-0.180)**	**(3.538)**
Demographic/Political Controls		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared		0.257	0.258	0.259	0.298	0.211	0.222	0.215	0.301
Number of Observations		980	980	980	908	748	748	748	712

Note: t-stats reported; these are corrected by clustering at the location (enumeration area) level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 7: Regressions of 'Turnout in Gubernatorial Elections'

Dependent Variable ----->		Turnout in Gubernatorial Elections							
		Homogeneous Effect (Panel vs. Control)	Reinforcement Effect (Panel vs. Control)			Homogeneous Effect (Oversample vs. Control)	Diffusion Effect (Oversample vs. Control)		
			Chatting	Kinship	Distance		Chatting	Kinship	Distance
		Logit							
Main Explanatory Variables	treated village	-0.104	-0.220	-0.165	-0.518**	-0.625**	-0.730**	-0.689**	-0.855***
		(-0.414)	(-0.819)	(-0.650)	(-2.541)	(-2.116)	(-2.368)	(-2.354)	(-3.581)
	network		6.876	2.172	0.828*		8.531	3.188	0.609
			(1.226)	(1.000)	(1.661)		(1.365)	(1.595)	(1.207)
	network*treat		**-6.698**	**-1.838**	**-0.585**		**-6.006**	**-1.998**	**0.758**
			(-1.197)	**(-0.839)**	**(-1.162)**		**(-0.927)**	**(-0.984)**	**(0.667)**
Demographic/Political Controls		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared		0.139	0.142	0.141	0.162	0.169	0.176	0.174	0.205
Number of Observations		974	974	974	903	744	744	744	709

Note: t-stats reported; these are corrected by clustering at the location (enumeration area) level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 8: Regressions of 'Conflict within Community' (with two periods)

Dependent Variable ----->		Conflict within Community															
		Homogeneous Effect (Panel vs. Control)		Reinforcement Effect (Panel vs. Control)						Homogeneous Effect (Oversample vs. Control)		Diffusion Effect (Oversample vs. Control)					
				Chatting		Kinship		Distance				Chatting		Kinship		Distance	
		OLS															
Main Explanatory Variables	**constant**	1.489***		1.509***		1.450***		1.335***		1.443***		1.458***		1.414***		1.222***	
		(5.373)		(5.683)		(5.182)		(7.084)		(4.476)		(4.640)		(4.479)		(6.486)	
	time	-0.335**	-0.433***	-0.345**	-0.446***	-0.311**	-0.387***	-0.690***	-0.813***	-0.335**	-0.433***	-0.345**	-0.446***	-0.311**	-0.387***	-0.690***	-0.813***
		(-2.405)	(-2.679)	(-2.400)	(-2.734)	(-2.449)	(-2.889)	(-3.073)	(-2.962)	(-2.399)	(-2.679)	(-2.393)	(-2.733)	(-2.442)	(-2.888)	(-3.065)	(-2.961)
	treated village	0.137		0.138		0.160		0.148		-0.730***		-0.734***		-0.708***		-0.766***	
		(0.919)		(0.920)		(1.114)		(0.849)		(-5.274)		(-5.236)		(-5.581)		(-4.445)	
	time*treat	-0.502**	-0.432**	-0.500**	-0.427**	-0.535***	-0.488**	-0.182	-0.074	0.274*	0.382**	0.282*	0.397**	0.248*	0.337**	0.621***	0.760***
		(-2.407)	(-1.965)	(-2.390)	(-1.971)	(-2.687)	(-2.498)	(-0.682)	(-0.241)	(1.819)	(2.254)	(1.862)	(2.349)	(1.817)	(2.389)	(2.666)	(2.714)
	network			0.327		-1.676***		-0.023				0.506		-1.814***		0.010	
				(0.279)		(-2.726)		(-0.080)				(0.437)		(-2.870)		(0.033)	
	network*time			-1.137	-1.637	2.442***	3.087***	-0.943**	-1.051*			-1.137	-1.637	2.442***	3.087***	-0.943**	-1.051*
				(-0.545)	(-0.800)	(2.862)	(3.065)	(-2.009)	(-1.834)			(-0.544)	(-0.800)	(2.854)	(3.063)	(-2.004)	(-1.833)
	network*treat			-0.718		1.330**		-0.063				-0.412		1.763***		0.018	
				(-0.586)		(2.105)		(-0.221)				(-0.329)		(2.880)		(0.059)	
	network*time*treat			**2.083**	**2.636**	**-1.847****	**-2.405****	**1.081****	**1.195****			**2.494**	**3.022**	**-2.000****	**-2.625*****	**0.965****	**1.060***
				(0.985)	**(1.270)**	**(-2.047)**	**(-2.305)**	**(2.300)**	**(2.083)**			**(1.181)**	**(1.460)**	**(-2.323)**	**(-2.595)**	**(2.051)**	**(1.849)**
Demographic/Political 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**
Adjusted R-squared		0.159	0.027	0.159	0.033	0.167	0.057	0.180	0.057	0.150	0.022	0.152	0.027	0.161	0.060	0.173	0.065
Number of Observations		1,912	1,114	1,912	1,114	1,912	1,114	1,772	1,036	1,462	856	1,462	856	1,462	856	1,392	819

Note: t-stats reported; these are corrected by clustering at the location (enumeration area) level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 9: Regressions of 'Physical Intimidation' (with two time periods)

Dependent Variable ----->		Physical Intimidation															
		Homogeneous Effect (Panel vs. Control)		Reinforcement Effect (Panel vs. Control)						Homogeneous Effect (Oversample vs. Control)		Diffusion Effect (Oversample vs. Control)					
				Chatting		Kinship		Distance				Chatting		Kinship		Distance	
		OLS															
Main Explanatory Variables	constant	1.321***		1.304***		1.321***		1.256***		1.507***		1.511***		1.522***		1.395***	
		(9.430)		(8.876)		(9.757)		(13.435)		(8.984)		(8.689)		(9.427)		(11.164)	
	time	-0.024	-0.042	-0.030	-0.046	-0.017	-0.030	-0.096*	-0.113*	-0.024	-0.042	-0.030	-0.046	-0.017	-0.030	-0.096*	-0.113*
		(-0.550)	(-0.895)	(-0.676)	(-0.970)	(-0.409)	(-0.713)	(-1.767)	(-1.838)	(-0.548)	(-0.895)	(-0.674)	(-0.970)	(-0.408)	(-0.713)	(-1.762)	(-1.838)
	treated village	-0.064*		-0.072**		-0.064**		-0.076**		-0.040		-0.045		-0.039		-0.054	
		(-1.931)		(-2.260)		(-2.243)		(-2.105)		(-1.151)		(-1.395)		(-1.332)		(-1.526)	
	time*treat	0.037	0.042	0.045	0.047	0.033	0.032	0.100	0.107	-0.024	-0.009	-0.018	-0.004	-0.030	-0.021	0.044	0.064
		(0.679)	(0.772)	(0.832)	(0.870)	(0.669)	(0.682)	(1.570)	(1.581)	(-0.463)	(-0.167)	(-0.336)	(-0.084)	(-0.590)	(-0.436)	(0.706)	(0.974)
	network			0.614		-0.296**		0.069*				0.500		-0.222*		0.065	
				(1.241)		(-2.356)		(1.672)				(1.050)		(-1.660)		(1.540)	
	network*time			-0.718	-0.504	0.675***	0.768***	-0.209*	-0.212*			-0.718	-0.504	0.675***	0.768***	-0.209*	-0.212*
				(-0.882)	(-0.720)	(2.653)	(2.916)	(-1.867)	(-1.758)			(-0.880)	(-0.720)	(2.645)	(2.915)	(-1.862)	(-1.757)
	network*treat			-0.443		0.476***		-0.091**				0.333		0.715***		-0.083**	
				(-0.890)		(3.233)		(-2.247)				(0.710)		(5.186)		(-1.991)	
	network*time*treat			**0.545**	**0.397**	**-0.867*****	**-0.928***	**0.242****	**0.249****			**0.615**	**0.546**	**-0.910***	**-0.904***	**0.219***	**0.208***
				(0.668)	**(0.566)**	**(-3.081)**	**(-3.372)**	**(2.155)**	**(2.054)**			**(0.747)**	**(0.775)**	**(-3.479)**	**(-3.367)**	**(1.953)**	**(1.719)**
Demographic/Political 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
Adjusted R-squared		0.004	0.000	0.004	0.000	0.007	0.008	0.007	0.007	0.018	-0.001	0.026	-0.002	0.032	0.008	0.023	0.004
Number of Observations		1,948	1,141	1,948	1,141	1,948	1,141	1,806	1,061	1,490	877	1,490	877	1,490	877	1,418	838

Note: t-stats reported; these are corrected by clustering at the location (enumeration area) level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 10: Regressions of 'Voting for AC (Opposition) in Presidential Elections' (with two time periods)

Dependent Variable ----->		Voting for AC (Opposition) in Presidential Elections															
		Homogeneous Effect (Panel vs. Control)		Reinforcement Effect (Panel vs. Control)						Homogeneous Effect (Oversample vs. Control)		Diffusion Effect (Oversample vs. Control)					
				Chatting		Kinship		Distance				Chatting		Kinship		Distance	
		Logit															
Main Explanatory Variables	constant	-2.796***		-2.721***		-2.656***		-3.560***		-3.239***		-3.173***		-3.099***		-3.407***	
		(-3.512)		(-3.563)		(-3.426)		(-4.305)		(-4.784)		(-4.718)		(-4.623)		(-4.032)	
	time	0.701**	1.145***	0.685**	1.245***	0.672**	1.124***	0.769	1.402***	0.682**	1.145***	0.674**	1.245***	0.661**	1.124***	0.794	1.402***
		(2.199)	(3.732)	(2.151)	(2.888)	(2.096)	(3.068)	(1.490)	(2.648)	(2.243)	(3.732)	(2.194)	(2.888)	(2.144)	(3.068)	(1.575)	(2.648)
	treated village	0.746**		0.768**		0.688*		0.719		0.639		0.719		0.614		0.104	
		(2.073)		(2.094)		(1.874)		(1.516)		(1.059)		(1.294)		(1.001)		(0.294)	
	time*treat	-0.746*	-1.361***	-0.732*	-1.460***	-0.737*	-1.348***	-0.925	-1.913***	-0.829**	-1.551	-0.821**	1.639	-0.806**	1.105	-1.028*	-1.422
		(-1.854)	(-3.442)	(-1.798)	(-2.928)	(-1.770)	(-3.037)	(-1.619)	(-3.180)	(-2.523)	(-1.610)	(-2.496)	(0.184)	(-2.432)	(0.154)	(-1.924)	(-1.172)
	network			-0.463		2.633*		-0.363				0.272		2.607		-0.400	
				(-0.352)		(1.800)		(-0.443)				(0.200)		(1.559)		(-0.657)	
	network*time			-0.659	2.029	-1.463	-0.595	0.184	0.828			-0.691	2.029	-1.389	-0.595	0.167	0.828
				(-0.728)	(0.342)	(-0.879)	(-0.105)	(0.232)	(0.681)			(-0.720)	(0.342)	(-0.913)	(-0.105)	(0.223)	(0.681)
	network*treat			0.770		-2.577		0.589				2.178		-1.669		1.183*	
				(0.533)		(-1.645)		(0.709)				(1.278)		(-1.078)		(1.906)	
	network*time*treat			**0.554**	**-1.920**	**0.579**	**-0.205**	**0.071**	**-0.411**			**1.000**	**32.628**	**1.624**	**35.252**	**-0.122**	**1.082**
				(0.349)	**(-0.316)**	**(0.251)**	**(-0.035)**	**(0.089)**	**(-0.334)**			**(1.009)**	**(0.348)**	**(1.059)**	**(0.376)**	**(-0.162)**	**(0.361)**
Demographic/Political 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
Adjusted R-squared		0.207	0.100	0.208	0.101	0.209	0.102	0.227	0.141	0.196	0.189	0.204	0.192	0.199	0.191	0.287	0.194
Number of Observations		1,960	246	1,960	246	1,960	246	1,816	240	1,496	126	1,496	126	1,496	126	1,424	124

Note: t-stats reported; these are corrected by clustering at the location (enumeration area) level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 11: Regressions of 'Turnout in Gubernatorial Elections' (with two time periods)

Dependent Variable ------>		Turnout in Gubernatorial Elections															
		Homogeneous Effect (Panel vs. Control)		Reinforcement Effect (Panel vs. Control)						Homogeneous Effect (Oversample vs. Control)		Diffusion Effect (Oversample vs. Control)					
				Chatting		Kinship		Distance				Chatting		Kinship		Distance	
		Logit															
Main Explanatory Variables	constant	1.892		1.731		1.920		1.823		-0.039		-0.131		0.007		-0.145	
		(1.231)		(1.153)		(1.187)		(1.181)		(-0.034)		(-0.115)		(0.006)		(-0.108)	
	time	-1.671***	-2.639***	-1.581***	-2.439***	-1.630***	-2.550***	-1.395***	-2.629***	-1.734***	-2.639***	-1.643***	-2.439***	-1.694***	-2.550***	-1.444***	-2.629***
		(-4.926)	(-5.701)	(-4.431)	(-5.136)	(-4.688)	(-5.394)	(-6.544)	(-2.737)	(-4.900)	(-5.701)	(-4.284)	(-5.136)	(-4.650)	(-5.394)	(-6.547)	(-2.737)
	treated village	-0.773**		-0.708*		-0.570		-0.986***		-2.176***		-2.174***		-2.179***		-1.992***	
		(-2.070)		(-1.873)		(-1.259)		(-2.713)		(-5.637)		(-5.590)		(-5.504)		(-4.785)	
	time*treat	0.778*	1.184**	0.636	0.946*	0.555	0.890	0.532	1.505	1.535***	-15.106	1.449***	-14.052	1.500***	-15.195	1.130***	-14.882
		(1.774)	(2.153)	(1.421)	(1.659)	(1.140)	(1.392)	(1.645)	(1.471)	(4.173)	(-0.006)	(3.676)	(-0.002)	(3.979)	(-0.001)	(4.061)	(-0.006)
	network			-1.114		-0.612		0.377				0.610		0.361		-0.180	
				(-0.423)		(-0.240)		(0.540)				(0.196)		(0.127)		(-0.273)	
	network*time			6.575	16.310	1.465	3.667	0.728	-0.285			6.778	16.313	1.541	3.668	0.801	-0.286
				(0.967)	(1.354)	(0.557)	(0.650)	(0.934)	(-0.134)			(0.889)	(1.354)	(0.493)	(0.651)	(0.915)	(-0.134)
	network*treat			4.501		4.857		0.042				-0.101		-0.042		2.285	
				(1.197)		(1.156)		(0.056)				(-0.032)		(-0.014)		(1.556)	
	network*time*treat			**-10.788**	**-19.204**	**-6.418**	**-8.537**	**-0.230**	**3.511**			**-6.007**	**-16.313**	**-1.158**	**-3.668**	**-1.433**	**0.286**
				(-1.441)	**(-1.449)**	**(-1.441)**	**(-1.109)**	**(-0.278)**	**(1.418)**			**(-0.786)**	**(-0.000)**	**(-0.369)**	**(-0.000)**	**(-1.481)**	**(0.000)**
Demographic/Political 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
Adjusted R-squared		0.167	0.475	0.170	0.484	0.171	0.487	0.187	0.510	0.265	0.677	0.268	0.690	0.266	0.680	0.305	0.653
Number of Observations		1,534	298	1,534	298	1,534	298	1,416	272	1,288	164	1,288	164	1,288	164	1,222	148

Note: t-stats reported; these are corrected by clustering at the location (enumeration area) level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 12: Regressions using Kinship and Distance

Dependent Variable ------>		Violence		Intimidation		Postcard		Voting (for AC)		Turnout	
		Reinforcement	Diffusion	Reinforcement	Diffusion	Reinforcement	Diffusion	Reinforcement	Diffusion	Reinforcement	Diffusion
		OLS		OLS		Logit		Logit		Logit	
Main Explanatory Variables	constant	-0.711***	-0.711***	-0.087	-0.087	-1.753**	-2.736**	1.405**	1.405**	-2.454**	-2.454**
		(-2.848)	(-2.846)	(-1.535)	(-1.534)	(-2.136)	(-2.461)	(2.447)	(2.447)	(-2.510)	(-2.510)
	treated village	-0.190	0.657***	0.085	0.038	0.366	0.202	-2.045***	0.166	1.204	-15.416
		(-0.670)	(2.586)	(1.381)	(0.631)	(0.851)	(0.457)	(-3.068)	(0.022)	(1.139)	(-0.001)
	kinship	2.432***	2.432***	0.613**	0.613**	-2.362	-1.141	0.069	0.069	4.481	4.480
		(2.642)	(2.640)	(2.040)	(2.039)	(-0.681)	(-0.385)	(0.012)	(0.012)	(0.787)	(0.787)
	distance	-0.848	-0.848	-0.161	-0.161	0.068	-0.057	0.829	0.829	-0.146	-0.146
		(-1.482)	(-1.481)	(-1.210)	(-1.209)	(0.089)	(-0.077)	(0.681)	(0.681)	(-0.068)	(-0.068)
	kinship*treat	**-1.760***	**-1.964****	**-0.776****	**-0.751****	**4.037**	**4.848***	**-3.158**	**21.127**	**-8.203**	**-4.480**
		(-1.831)	**(-2.123)**	**(-2.518)**	**(-2.459)**	**(1.161)**	**(1.684)**	**(-0.483)**	**(0.211)**	**(-1.231)**	**(-0.000)**
	distance*treat	**0.994***	**0.861**	**0.197**	**0.155**	**-0.139**	**-0.106**	**-0.320**	**0.953**	**3.667**	**0.146**
		(1.735)	**(1.503)**	**(1.479)**	**(1.168)**	**(-0.180)**	**(-0.141)**	**(-0.259)**	**(0.312)**	**(1.443)**	**(0.000)**
Demographic/Political Controls		No	No	No	No	Yes	Yes	No	No	No	No
Individual Fixed Effects		Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Adjusted R-squared		0.077	0.087	0.011	0.009	0.053	0.101	0.151	0.195	0.524	0.659
Number of Observations		1,036	819	1,061	838	908	712	240	124	272	148

Note: t-stats reported; these are corrected by clustering at the location (enumeration area) level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 13: Nearest-neighbor Matching

Dependent Variable ----->	Violence	Intimidation	Postcard	Voting (for AC)	Turnout
treated village	0.372***	0.044	0.119**	-0.087**	0.111***
	0.126	0.061	0.057	0.035	0.035
Number of Observations	801	820	823	823	709

Note: st. errors reported. * significant at 10%; ** significant at 5%; *** significant at 1%.

Matching was performed on statistically significant different demographics (across oversample and control), together with the instruments used in this paper.

Table 14: IV Estimates

	Dependent Variable ----->	Violence	Intimidation	Postcard	Voting (for AC)	Turnout
		Heterogeneous Effect	Heterogeneous Effect	Heterogeneous Effect	Heterogeneous Effect	Heterogeneous Effect
		IV	IV	IV	IV	IV
Main Explanatory Variables	constant	-0.844***	-0.177*		0.051	-0.074**
		(-2.802)	(-1.652)		(0.984)	(-2.453)
	treated village	0.838**	0.141	-0.127	-0.047	0.024
		(2.518)	(1.148)	(-0.761)	(-0.823)	(0.554)
	kinship	1.955*	0.308	-0.298	-0.090	-0.357*
		(1.868)	(0.685)	(-0.610)	(-0.685)	(-1.693)
	distance	-1.149*	-0.414	0.172	0.007	-0.102
		(-1.701)	(-1.634)	(0.562)	(0.083)	(-1.264)
	kinship*treat	**-1.400**	**-0.395**	**1.047****	**0.081**	**0.432****
		(-1.268)	**(-0.823)**	**(2.322)**	**(0.562)**	**(2.148)**
	distance*treat	**1.164***	**0.417***	**-0.195**	**-0.007**	**0.110**
		(1.725)	**(1.654)**	**(-0.641)**	**(-0.078)**	**(1.365)**
Demographic/Political Controls		No	No	Yes	No	No
Individual Fixed Effects		Yes	Yes	No	Yes	Yes
F-stat on Excluded Instruments		28.288	27.249	23.651	27.206	31.954
Adjusted R-squared		0.082	-0.000	-0.031	0.005	0.032
Number of Observations		812	831	709	834	716

Note: t-stats reported (corrected by clustering at the location level). * significant at 10%; ** significant at 5%; *** significant at 1%.

Instruments are distance to mean panel coordinates and an average of membership of local organizations (social capital).