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

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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

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are reported for this campaign. This paper is devoted to the assessment of 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 control 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 and policies that seek to improve the welfare of the masses, it is crucial that citizens vote according to reason, not emotions. Yet it is only too easy for politicians to seek votes by stirring up emotions, whether 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 (2008) using Afrobarometer data across ten African countries. In this paper we focus our attention on the use of fear in elections.

But let us step back for a moment. The general question we face is: what can be done to reduce the role of emotions in the electoral process? 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 (2008) 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. The campaign also affected voting behavior, namely in terms of electoral participation.

If awareness campaigns can successfully reduce the role of emotions 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 diffuse 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 violence, undertaken nationwide in Nigeria before the 2007 elections. It worked primarily through town meetings and popular theatres, as a way to decrease collection 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 groups in control villages. To test for the presence of a reinforcement effect, we examine whether the effect of the message on perceptions and behavior of the targeted households is reinforced by proximity to other households in the same locations. To investigate diffusion to unexposed households, we test whether households not directly exposed to the campaign show effects similar to exposed households whenever they have close ties within the community.

Results provide some evidence of both reinforcement and diffusion 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 intimidation and community violence, 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 no effect 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 individuals. More recently, Angelucci and De Giorgi (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. Still in the context of a conditional cash transfer programme, Macours and Vakis (2008) extend the literature by considering explicit variables of interaction of households. However they only estimate reinforcement effects and do not have individual variation in networks. Finally our result that kinship proximity is more important than other measures of social interaction relates 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, while empirical results are presented in Section 5.

2. Context

Nigeria, the most populous country in Africa with estimated 146 million inhabitants[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 ranks 201 in 233 countries in terms of GDP per capita (1400 USD PPP in 2005[3]). Moreover, it has been

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

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 representatives, and senate; state governors, and assemblies). 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 protege 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. Violence was prominent in this process. Human Rights Watch, in a report released in May 2007[4], 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'. This violence was identified by Human Rights Watch to be originated from marginalized political groups, many of which dissidents formerly associated to PDP[5]. 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,

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

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

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. 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[6].

The main theoretical rationale of the campaign was to diminish collective action problems 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 action lowers its costs and so makes it easier to protest (i.e. resist intimidation). The campaign also worked through the provision of information about the candidates: by targeting the perpetrators of violence, voters were led to reconsider the value attributed to each candidate.

Crucially, AAIN agreed to randomize its campaign across locations at the state-level. This feature allowed us to design a field experiment in collaboration with the NGO where exogenous variation in violence and electoral behavior was targeted. The campaign was conducted in six states of Nigeria, in the three main socioeconomic large regions of the country: Lagos and Oyo in the Southwest, Kaduna and Plateau in the Middle-belt/North, and Delta and Rivers in the Niger Delta. Twelve target villages or urban neighborhoods were selected, two per state covered. The choice of states was not representative of Nigeria,

[6] For details of this campaign, including the full design of its media, and photos/films of its conduction, see http://www.iig.ox.ac.uk/research/08-political-violence-nigeria/default.htm.

as only states with a recent history of violence were chosen[7]. However, within all states, we chose four representative enumeration areas[8], from which we randomly chose the treated (subject to the campaign) locations. By design treatment and control enumeration areas are broadly comparable.

In both treatment and control locations a baseline survey was conducted among 50 randomly selected households immediately before the campaign (January 2007) - 1200 respondents in total. The same respondents (*panel*) were then resurveyed shortly after the elections (May 2007). By design, all respondents to the baseline survey were individually targeted by the campaigners (offered the materials of the campaign and invited to the town meetings and popular theatres) so as to facilitate the evaluation of the direct impact of the campaign. To study diffusion, an *oversample* of 25 households not directly exposed to the anti-violence campaign were added to the second round survey in treatment locations - 300 additional respondents. The first question in the oversample questionnaire asked whether the respondent had been approached by the campaigners, with the interview being pursued in case of negative response. All respondents – baseline and oversample – were asked about their social links to each of the 50 baseline households and their houses were represented in maps for the enumeration areas. The surveys were designed and supervised by the authors, using original instruments tested for the purpose of this experiment. Data collection was undertaken in direct collaboration with Afrobarometer and its Nigerian partner (Practical Sampling International).

[7]This selection was systematically conducted by looking at reports for earlier elections. See for instance: Human Rights Watch, 'Testing Democracy: Political Violence in Nigeria', April 2003.

[8]Enumeration areas were chosen whithin a large and representative sample composed for the 2007 Afrobarometer survey in Nigeria. Their sample was drawn using census data.

Note that the design of the experiment depended mainly upon two stages, i.e. that the campaign would influence perceptions (namely reduce the perceived threat of violence), and that this would in turn affect voting. Hence, the surveys were designed to elicit evidence on each stage. The questions on violence were asked both prior to the campaign, focusing on a reference period ('the last year'), and after the campaign/elections, focusing on what had happened just before and during the elections (i.e. 'from January', when the baseline survey was in the field). The questions on voting were based on intentions (before) and self-reported actual decisions (after) regarding the elections of April 2007.

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 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 interested in the average treatment effect, which is discussed in detail in Collier and Vicente (2008). 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[9]. Formally, let $\widetilde{n}_i = \frac{1}{M} \sum_{j=1}^{M} g_{ij}$, where M is the number of treated respondents in the same location. 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.

[9] Given the way the data are generated – all respondents are asked about the same 50 treated households in their village – 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:

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

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:

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

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

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

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 villagers 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 in lieu of 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 a message that is communicated to individuals who are socially close to each other benefits from a kind of social multiplier effect.

We also test whether the campaign message communicated directly to panel respondents also 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 should 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 panel 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[10]. 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. We see that there is no difference across panel households between control and treated villages. There is only one variable that is significantly different at the 10% level – a normal finding given the number of variables we 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. We see that most characteristics are not significantly different between oversample and control households. This is, however, not true for a small subset of variables which we therefore need to control for in the subsequent analysis: namely, schooling, religious intensity, and ownership of radios (all higher in the oversample). There is therefore some evidence that there was some selection into the oversample (respondents stating up-front that they were not approached by AAIN campaigners). A possibility is

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

that ‘more average’ respondents over-reported campaign reach and were then left out of the oversample.

In this paper, we focus our attention on four outcome variables - this is a concise set, meant to be representative of the wide range of outcomes we have available, as reported in Collier and Vicente (2008). The first two capture the respondent’s perceptions regarding violence. The first of these two, which we call *violence*, is an 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 an 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 the respondent. In both cases, variables violence and intimidation are scaled so that higher values correspond to worse outcomes.

The other two outcome variables of interest capture behavior. The first, which we name *postcard*, is an experimentally generated measure of 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. 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 would imply an effort cost. This is the reason we may regard this measure as incentivecompatible. If the respondent mailed the postcard, the variable *postcard* takes value 1. The second behavior variable, which we call *voting*, takes value 1 if the respondent voted for Atiku Abubakar, AC's presidential candidate, the candidate that was generally associated with political instability.

In Table 2, we display averages for control and treatment groups for the baseline values of these variables (with the exception of postcard, which has no baseline). A breakdown between panel respondents in control and treatment villages reveals no significant difference in the baseline. However, a lower value in the oversample for the violence variable stands out as significant. This may be due to the aforementioned possible self-selection into the oversample, and/or, given that baseline values for the oversample are retrospective, it may be due to a recall bias[11]. We will deal with both potential problems ahead: by running regressions with second-round data only, and by instrumenting oversample selection.

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 (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'). We call this variable *chatting*. We also construct another measure of social proximity, whereby a link from i to j is assumed to exist if i can identify j by name and claimed to be related to j (the exact question

[11] Note that for both violence and voting variables, only the oversample baseline measure is retrospective (i.e. those questions were asked to the panel in the pre-election survey), while for intimidation, all groups' baseline measures are retrospective. Although there is a clear difference between violence and intimidation at the baseline, when looking at a wider range of related outcomes (available upon request), for which we have the same kind of design variation (retrospective and non-retrospective baseline questions for the panel), it should be noted that we cannot find a clear pattern of retrospective bias.

used was 'Are the following individuals relatives of yours, i.e. members of your family? Yes-No'). We call this variable *kinship*.

We also investigate the effect of geographical distance between i and j. In each enumeration area, we had each respondent represented on single map. Moreover, each enumerator was asked to represent his itinerary and to calculate the distance he or she went through between interviews. See Figure 1 for an example of these maps. From the position of each respondent on these maps, we constructed a set of up-down and left-right coordinates for each of them. From these coordinates distances between each ij pair were calculated. Because maps differ in scale, these distances were rescaled to make them comparable across all locations (using the distances reported by the enumerators). 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.

5. Empirical results

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

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

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.

Two sets of results are presented. The first set, shown in columns 1 to 4, compares panel respondents in the control and treated villages. Here the interpretation of θ is that of a reinforcement effect. The second set of results, shown in columns 5 to 8, compare 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[12]. 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. Demographic and political controls are included in all regressions[13].

Results show that the perception of community violence is significantly less 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

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

[13] These controls are selected from the variables displayed in Table 1. The exact same control variables are used in all regressions where individual controls are reported to be included.

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.

Coefficient τ captures the way in which perceptions of community violence vary systematically with distance from panel respondents. Since these 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, perceptions of violence nevertheless fall on average, as shown in columns 1 and 5[14].

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 ben-

[14]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.

eficial 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 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* variable. 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 Table 6. The dependent variable takes value 1 if the respondent declared voting for the presidential candidate from the opposition party AC, and 0 otherwise. In general the campaign had a negative effect on this variable; however in the Table shown 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.

Taken together, these results offer some evidence in favor of both reinforcement and diffusion effects. What is perhaps 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 together. 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 sometimes even 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. As explained in the data section, where this information comes from varies across regressions. 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. As referred, 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 4 to 6. Of course, using the baseline data has the *a priori* advantage of controlling for differential time trends across treatment and control groups.

Results for dependent variable *violence* are reported in Table 7. The coefficient of $T_{ivt}w_{iv}$ (time-treat) gives α, the average treatment effect. The coefficient of $n_i T_{ivt} w_{iv}$

(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[15]. 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 stay on positive, they completely lose any statistical significance. These findings reinforce and broaden our earlier findings from Table 3.

Next we look at perceptions of *intimidation*. Estimation results, reported in Table 8, are very similar to those shown in Table 7: 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* are reported in Table 9. Here 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 because the dependent variable is dichotomous and hence

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

contains little information: indeed, when estimating (3.7) all observations with identical values of y_{ivt} over time are dropped, dramatically reducing 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 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 10. For the *postcard* regression, we report results for a one-period version of (5.1) instead. For *violence* and *intimidation*, the model is estimated in first difference. For *voting*, 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 in the *violence* regression where it remains significant.

Before concluding, we present an additional set of robustness checks for the diffusion effect. As discussed earlier, oversample respondents were selected *after* the campaign among households that had not been directly exposed to it. Descriptive data on oversample vs. control groups introduced the possibility of selection bias for the diffusion results. We have dealt with it primarily by including additional controls – i.e., individual characteristics or fixed effects. But there may remain potential 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)[16]. This non-parametric approach bypasses the difficulties associated with propensity score matching – especially issues regarding balancedness. Results, shown in Table 11, confirms 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 treatment. The campaign also has reduced voting for the opposition, a point discussed in detail in Collier and Vicente (2008). These findings lend clear credibility to the homogeneous effects we estimated before.

Our last set of robustness checks seeks to the instrument treatment for oversample households. Our main concern here 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. We use two instruments: an average of questions about membership of institutions at the village level (a social capital measure)[17], and an alternative measure of distance (distance to the mean coordinates of the panel respondents). These instruments are jointly significant in the instrumenting regression, albeit weakly. 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

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

[17] 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.'.

$\widehat{w}_{iv}n_i$ 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 12. We find significant interaction effects for distance in the *violence* and *intimidation* regressions - lost for kinship -, which confirm the existence of diffusion effects (even if the relevant network measure is not robust). We also confirm a significant kinship interaction effect in the *postcard* regression.

6. Conclusion

In this paper we have reported results from a field experiment designed to evaluate the reinforcement and diffusion network 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 in treated households, we examined whether the impact of the campaign on perceptions and behavior is reinforced by proximity to other 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 some evidence of both diffusion and reinforcement effects. Findings suggests 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 no effect on voting behavior per se.

These findings presented in this paper together with Collier and Vicente (2008) 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, and in affecting respondents' willingness to express their disapproval of electoral violence and voting. 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. It remains to be seen in future research whether a campaign directed at them may reduce the risk of electoral violence directly.

In the results reported here, social and geographical proximity between households are taken as given and remain 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 – churches, civil society – for the campaign message that could magnify its effect by canvassing their neighborhood. This deserves further investigation.

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Figure 1: A Map for an Enumeration Area, with Enumerator Itineraries

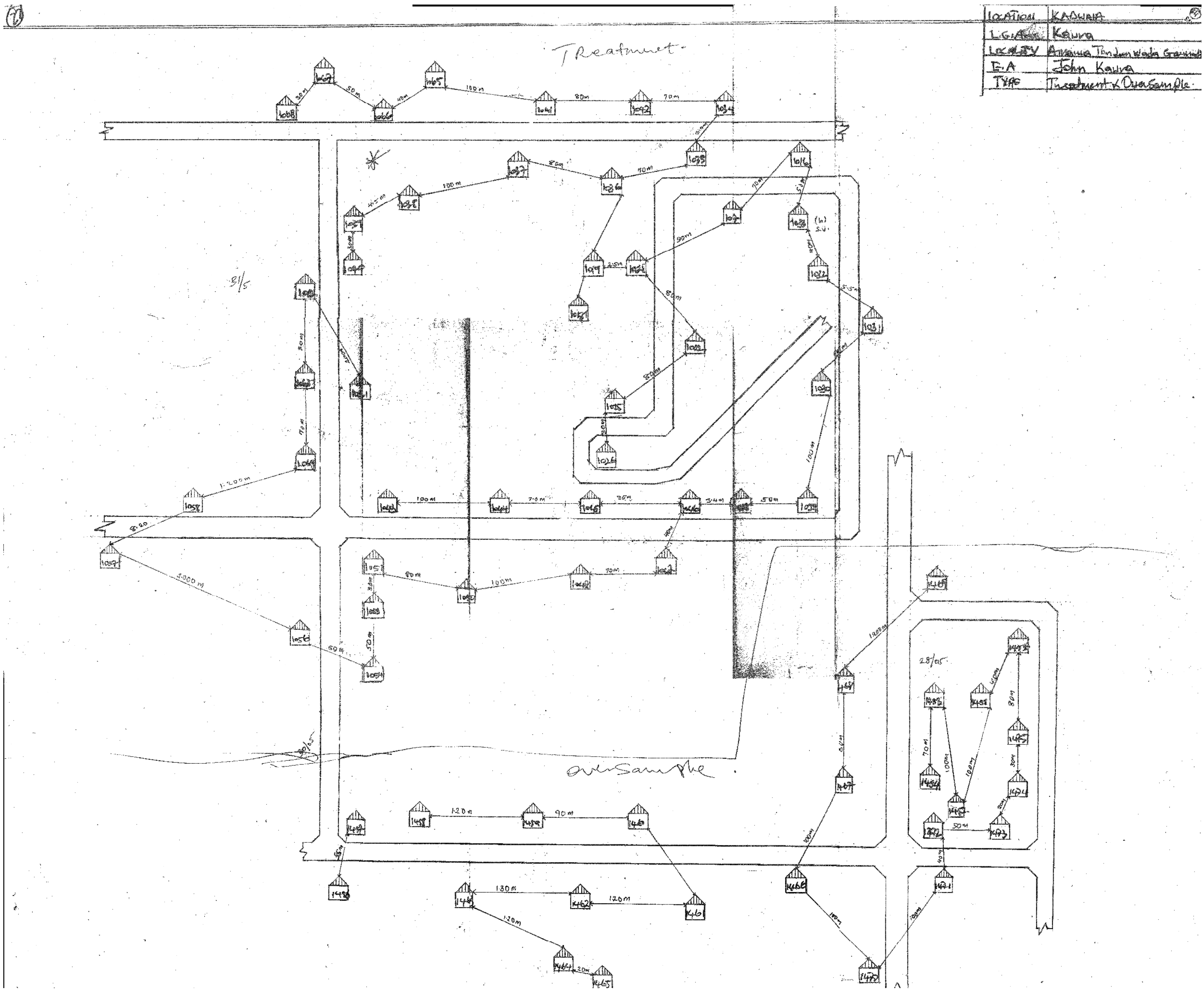

Table 1: Differences across treatment and control areas - demographics, attrition

		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** 0.006	900
	age	**32.955**	**33.027**	**0.072** 0.960	1,198	**32.030**	**-0.925** 1.373	897
	household size	**6.430**	**6.332**	**-0.098** 0.708	1,200	**6.727**	**0.297** 0.843	900
	single	**0.383**	**0.392**	**0.009** 0.049	1,149	**0.473**	**0.091** 0.064	880
	married	**0.581**	**0.585**	**0.004** 0.046	1,149	**0.490**	**-0.091** 0.061	880
	schooling (0-9)	**4.308**	**4.673**	**0.365** 0.374	1,200	**5.140**	**0.832***** 0.288	900
Ethnicity and Religion	yoruba	**0.318**	**0.273**	**-0.045** 0.166	1,200	**0.283**	**-0.035** 0.170	900
	hausa	**0.072**	**0.160**	**0.088** 0.089	1,200	**0.157**	**0.085** 0.086	900
	igbo	**0.157**	**0.102**	**-0.055** 0.113	1,200	**0.097**	**-0.060** 0.117	900
	christian	**0.621**	**0.762**	**0.141** 0.124	1,199	**0.687**	**0.066** 0.135	899
	muslim	**0.344**	**0.233**	**-0.111** 0.129	1,199	**0.293**	**-0.051** 0.141	899
	religious intensity (1-6)	**4.764**	**5.022**	**0.258** 0.206	1,185	**5.190**	**0.426*** 0.236	889
Occupation	job stability (0-3)	**1.363**	**1.378**	**0.015** 0.157	1,200	**1.483**	**0.120** 0.198	900
	agriculture	**0.158**	**0.117**	**-0.042** 0.064	1,200	**0.117**	**-0.042** 0.072	900
	industry/services: trader	**0.125**	**0.118**	**-0.007** 0.032	1,200	**0.170**	**0.045** 0.039	900
	housework	**0.120**	**0.098**	**-0.022** 0.033	1,200	**0.083**	**-0.037** 0.044	900
Property and Expenditure	house	**0.606**	**0.605**	**-0.001** 0.107	1,199	**0.512**	**-0.094** 0.118	896
	land	**0.526**	**0.573**	**0.047** 0.114	1,199	**0.515**	**-0.011** 0.122	894
	cattle	**0.329**	**0.327**	**-0.002** 0.100	1,198	**0.441**	**0.112** 0.102	896
	radio	**0.888**	**0.928**	**0.040** 0.033	1,199	**0.940**	**0.052*** 0.031	899
	cell phone	**0.512**	**0.608**	**0.096** 0.116	1,197	**0.542**	**0.030** 0.130	897
	household expenditure (naira/month)	**19,001**	**22,188**	**3,186.514** 4,655.297	1,003	**24,162**	**5,161** 5,119	770
	pdp 2003 presidential	**0.453**	**0.483**	**0.030** 0.085	1,149	**0.460**	**0.007** 0.095	880
	anpp 2003 presidential	**0.159**	**0.079**	**-0.080** 0.075	1,149	**0.100**	**-0.059** 0.085	880
	ac 2003 presidential	**0.026**	**0.046**	**0.020** 0.023	1,149	**0.033**	**0.007** 0.022	880
	pdp 2003 governor	**0.450**	**0.459**	**0.009** 0.080	1,149	**0.383**	**-0.067** 0.093	880
	anpp 2003 governor	**0.128**	**0.083**	**-0.045** 0.062	1,149	**0.157**	**0.029** 0.078	880
	ac 2003 governor	**0.033**	**0.030**	**-0.003** 0.022	1,149	**0.020**	**-0.013** 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
Panel Attrition	panel re-surveying	**0.97**	**0.95**	**-0.02** 0.01	1200			

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 (baseline violence and political preferences, attrition)

		Control	Treatment	Difference	Number of	Treatment	Difference	Number of
Selected Outcome Variables	violence	**1.175**	**1.283**	**0.108** 0.176	1,184	**0.380**	**-0.795***** 0.141	884
	intimidation	**1.142**	**1.089**	**-0.053** 0.040	1,195	**1.100**	**-0.042** 0.041	898
	voting	**0.077**	**0.145**	**0.068** 0.047	1,200	**0.117**	**0.040** 0.054	900

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 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	-0.001***		-0.602	0.591	-0.001***
			(-1.015)	(0.571)	(-2.747)		(-0.798)	(0.635)	(-3.235)
	network*treat		**1.080**	**-0.356**	**0.001*** **		**2.069** **	**-0.136**	**0.001*** **
			(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**	-0.000*		-0.162	0.531***	-0.000*
			(0.073)	(2.236)	(-1.807)		(-0.403)	(2.918)	(-1.682)
	network*treat		**-0.005**	**-0.435** **	**0.000***		**0.929** **	**-0.245**	**0.000**
			(-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 ------>		Homogeneous Effect (Panel vs. Control)	Reinforcement Effect (Panel vs. Control) Chatting	Kinship	Distance	Homogeneous Effect (Oversample vs. Control)	Diffusion Effect (Oversample vs. Control) Chatting	Kinship	Distance
		Postcard							
		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	0.000		2.074	-0.696	0.000
			(0.453)	(-0.680)	(0.295)		(1.419)	(-0.264)	(0.054)
	network*treat		**2.080**	**3.939**	**-0.000**		**4.365***	**4.461***	**-0.000**
			(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'

Dependent Variable ------>		Homogeneous Effect (Panel vs. Control)	Reinforcement Effect (Panel vs. Control) Chatting	Kinship	Distance	Homogeneous Effect (Oversample vs. Control)	Diffusion Effect (Oversample vs. Control) Chatting	Kinship	Distance
		Voting for AC (Opposition) in Presidential Elections							
		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	-0.000		-0.542	1.642	-0.000
			(-0.808)	(1.080)	(-1.005)		(-0.458)	(1.053)	(-1.104)
	network*treat		**1.242**	**-2.258**	**0.001****		**3.633****	**-0.259**	**0.001*****
			(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 'Conflict within Community' (with two periods)

		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)**					
				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***		-0.000				0.506		-1.814***		0.000	
				(0.279)		(-2.726)		(-0.080)				(0.437)		(-2.870)		(0.033)	
	network*time			-1.137	-1.637	2.442***	3.087***	-0.001**	-0.001*			-1.137	-1.637	2.442***	3.087***	-0.001**	-0.001*
				(-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**		-0.000				-0.412		1.763***		0.000	
				(-0.586)		(2.105)		(-0.221)				(-0.329)		(2.880)		(0.059)	
	network*time*treat			**2.083**	**2.636**	**-1.847****	**-2.405****	**0.001****	**0.001****			**2.494**	**3.022**	**-2.000****	**-2.625*****	**0.001****	**0.001***
				(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 8: Regressions of 'Physical Intimidation' (with two time periods)

		Physical Intimidation															
Dependent Variable ------>		**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**		0.000*				0.500		-0.222*		0.000	
				(1.241)		(-2.356)		(1.672)				(1.050)		(-1.660)		(1.540)	
	network*time			-0.718	-0.504	0.675***	0.768***	-0.000*	-0.000*			-0.718	-0.504	0.675***	0.768***	-0.000*	-0.000*
				(-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***		-0.000**				0.333		0.715***		-0.000**	
				(-0.890)		(3.233)		(-2.247)				(0.710)		(5.186)		(-1.991)	
	network*time*treat			**0.545**	**0.397**	**-0.867*** **	**-0.928*** **	**0.000** **	**0.000** **			**0.615**	**0.546**	**-0.910*** **	**-0.904*** **	**0.000* **	**0.000* **
				(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 9: Regressions of 'Voting in Presidential Elections' (with two time periods)

		Voting for AC (Opposition) in Presidential Elections															
Dependent Variable ------>		**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*		-0.000				0.272		2.607		-0.000	
				(-0.352)		(1.800)		(-0.443)				(0.200)		(1.559)		(-0.657)	
	network*time			-0.659	2.029	-1.463	-0.595	0.000	0.001			-0.691	2.029	-1.389	-0.595	0.000	0.001
				(-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		0.001				2.178		-1.669		0.001*	
				(0.533)		(-1.645)		(0.709)				(1.278)		(-1.078)		(1.906)	
	network*time*treat			**0.554**	**-1.920**	**0.579**	**-0.205**	**0.000**	**-0.000**			**1.000**	**32.628**	**1.624**	**35.252**	**-0.000**	**0.001**
				(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 10: Regressions using kinship and distance

Dependent Variable ------>		Violence		Intimidation		Postcard		Voting	
		Reinforcement	Diffusion	Reinforcement	Diffusion	Reinforcement	Diffusion	Reinforcement	Diffusion
		OLS		OLS		Logit		Logit	
Main Explanatory Variables	constant	-0.711***	-0.711***	-0.087	-0.087	-1.753**	-2.736**	1.405**	1.405**
		(-2.848)	(-2.846)	(-1.535)	(-1.534)	(-2.136)	(-2.461)	(2.447)	(2.447)
	treated village	-0.190	0.657***	0.085	0.038	0.366	0.202	-2.045***	0.166
		(-0.670)	(2.586)	(1.381)	(0.631)	(0.851)	(0.457)	(-3.068)	(0.022)
	kinship	2.432***	2.432***	0.613**	0.613**	-2.362	-1.141	0.069	0.069
		(2.642)	(2.640)	(2.040)	(2.039)	(-0.681)	(-0.385)	(0.012)	(0.012)
	distance	-0.001	-0.001	-0.000	-0.000	0.000	-0.000	0.001	0.001
		(-1.482)	(-1.481)	(-1.210)	(-1.209)	(0.089)	(-0.077)	(0.681)	(0.681)
	kinship*treat	**-1.760***	**-1.964****	**-0.776****	**-0.751****	**4.037**	**4.848***	**-3.158**	**21.127**
		(-1.831)	**(-2.123)**	**(-2.518)**	**(-2.459)**	**(1.161)**	**(1.684)**	**(-0.483)**	**(0.211)**
	distance*treat	**0.001***	**0.001**	**0.000**	**0.000**	**-0.000**	**-0.000**	**-0.000**	**0.001**
		(1.735)	**(1.503)**	**(1.479)**	**(1.168)**	**(-0.180)**	**(-0.141)**	**(-0.259)**	**(0.312)**
Demographic/Political Controls		No	No	No	No	Yes	Yes	No	No
Individual Fixed Effects		Yes	Yes	Yes	Yes	No	No	Yes	Yes
Adjusted R-squared		0.077	0.087	0.011	0.009	0.053	0.101	0.151	0.195
Number of Observations		1,036	819	1,061	838	908	712	240	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: Nearest neighbor matching

Dependent Variable ------>	Violence	Intimidation	Postcard	Voting
treated village	0.372***	0.044	0.119**	-0.087**
	0.126	0.061	0.057	0.035
Number of Observations	801	820	823	823

Note: st. errors reported for matching and t-stats reported for IV (latter corrected by clustering at the location level). * 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 12: IV estimates

Dependent Variable ------>		Violence Heterogeneous Effect IV	Intimidation Heterogeneous Effect IV	Postcard Heterogeneous Effect IV	Voting Heterogeneous Effect IV
Main Explanatory Variables	constant	-0.844***	-0.177*		0.051
		(-2.802)	(-1.652)		(0.984)
	treated village	0.838**	0.141	-0.127	-0.047
		(2.518)	(1.148)	(-0.761)	(-0.823)
	kinship	1.955*	0.308	-0.298	-0.090
		(1.868)	(0.685)	(-0.610)	(-0.685)
	distance	-0.001*	-0.000	0.000	0.000
		(-1.701)	(-1.634)	(0.562)	(0.083)
	kinship*treat	**-1.400**	**-0.395**	**1.047****	**0.081**
		(-1.268)	**(-0.823)**	**(2.322)**	**(0.562)**
	distance*treat	**0.001***	**0.000***	**-0.000**	**-0.000**
		(1.725)	**(1.654)**	**(-0.641)**	**(-0.078)**
Demographic/Political Controls		No	No	Yes	No
Individual Fixed Effects		Yes	Yes	No	Yes
F-stat on Excluded Instruments		28.288	27.249	23.651	27.206
Adjusted R-squared		0.082	-0.000	-0.031	0.005
Number of Observations		812	831	709	834

Note: st. errors reported for matching and t-stats reported for IV (latter 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).