



All quiet on election day? International election observation and incentives for pre-election violence in African elections



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ABSTRACT

This article argues that the increasing international interest in elections as exemplified by the rise of international election monitoring induces temporal shifts in the use of violent intimidation by political actors. The presence of international electoral missions lowers the potential for election-day violence relative to the pre-election period because domestic actors likely refrain from intimidating opposition candidates or voters before the eyes of international observers, but creates incentives for political actors to engage in violent manipulation in parts of the electoral process receiving considerably less international attention, such as the pre-election period. The article expects that international election observation increases the incidence of violent manipulation during electoral campaigns. An empirical analysis of election-related violence for African elections in the 1990–2009 period shows that the presence of election observers increases the incidence of pre-election violence, but has no effect on election-day violence.

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1. Introduction

In the 2011 parliamentary elections in Egypt, the first elections after the ouster of Hosni Mubarak, commentators observed that election day was surprisingly peaceful after a campaign period marred by substantial violence.¹ Research on election violence also notes that election day, even when preceded by substantial violence, is often surprisingly peaceful (Laakso, 2007: 228, Höglund, 2009: 416). Conceptualizing violence before and during the voting as a form of manipulation, this article suggests an explanation for the seemingly higher incidence of violence before elections relative to violence on election day. I argue that temporal shifts in the incidence of violence are a response to increased attention to electoral processes, particularly

election day. International organizations engaged in election monitoring have successfully documented and criticized fraudulent elections, and such fraud has resulted in domestic and international punishments (Tucker, 2007). To avoid negative publicity and punishment, domestic elites strategically shift violent intimidation to the pre-election period in internationally monitored elections, resulting in increased violent manipulation before elections but relatively lower levels of violence on election day.

The article makes important contributions to the literature on electoral violence and strategic manipulation. First, it develops an argument that emphasizes the effect of international actors on violence before and during elections and systematically evaluates these expectations in a set of African elections. The study of elections and violence, particularly violence that occurs before or during the actual polling, lacks systematic analyses and existing small-N studies have mostly emphasized the importance of domestic factors such as the competitiveness of elections (Wilkinson, 2004; Chaturvedi, 2005; Collier and Vicente,

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¹ "In a Surprise, Calm Prevails in Egypt's Elections." *New York Times*, November 28, 2011, p. A1.

2012). The central proposition developed and evaluated here is that in elections monitored by international observers, incumbents shift violent intimidation to the pre-election period to avoid international criticism and possible punishments. An empirical analysis of African elections in the 1990–2009 period confirms that monitored elections have a greater risk of violence in the pre-election period, thus demonstrating the importance of international factors in the occurrence of pre-election violence. The analysis also finds that elites in control of the media are less likely to strategically displace the use of violence, thus showing that domestic actors consider the potential costs of manipulation when adapting to the presence of international monitors.

Second, by showing that international monitors can displace more direct forms of fraud such as violent intimidation, the article adds to existing research on strategic manipulation that has focused primarily on less verifiable forms of manipulation (Hyde and O'Mahoney, 2010; Simpson and Donno, 2012). Unlike other forms of manipulation, violent intimidation can occur before and during the polling, allowing me to assess whether temporal dynamics in the use of violent manipulation support the theoretical arguments. While the incidence of pre-election violence in observed elections alters the situation on the ground, thus making it difficult to examine whether the presence of observers directly deters violence on election day, results show that international monitors have no statistically discernible effect on election-day violence. Difference of means tests for violent events before, during, and after elections with and without observers present additional illustrations of the theoretical dynamics outlined in the article.

The article proceeds as follows. I briefly review the literature on electoral violence and strategic manipulation. The next section develops expectations on the effect of international monitoring on pre-election violence. Empirical findings for African elections held from 1990 to 2009 support the main hypotheses.

2. Violence as manipulation

The relationship between elections and violence has only recently started to be examined systematically. The fact that electoral violence can occur before, during, or after elections creates difficulties for developing appropriate research designs. In addition, the causes of election violence may differ depending on when it occurs. Violence that takes place before elections or on election day is frequently an attempt to influence election outcomes and could thus be conceptualized as a form of election fraud, whereas violence that occurs after elections might be a response to outcomes, particularly if fraud occurred (Sisk, 2008; Höglund, 2009). Likely for this reason, most research focuses on either pre- or post-election violence, with the majority of systematic research investigating the effect of elections on violence in the post-election period. Recent work has examined the effect of post-conflict elections on civil war recurrence (Brancati and Snyder, 2013; Flores and Nooruddin, 2012), whether the incidence of election fraud increases the likelihood of protests and

violence after elections (Tucker, 2007; Daxecker, 2012), the impact of institutional factors on post-election civil conflict (Cheibub et al., 2012), and the relationship between elections, ethnic group exclusion, and ethnic civil war (Cederman et al., 2013).²

Compared to post-election violence, the causes and consequences of pre-election violence are poorly understood. Much of the research on this subject is either descriptive in nature or limited to a small number of countries, but generally supports the notion that electoral violence occurring before or during elections is usually designed to influence election outcomes by intimidating voters (Wilkinson, 2004; Chaturvedi, 2005; Laakso, 2007; Bratton, 2008; Höglund, 2009; Collier and Vicente, 2012).³ Practitioners involved in managing electoral violence confirm the use of violence as a manipulative strategy, arguing that “violence during the campaign and election day almost always has political motives” (Joint Task Force on Electoral Assistance Report, 2011: 55; United Nations General Assembly, 2010).⁴ It thus seems reasonable to conceptualize violence before or during the actual polling as a form of electoral manipulation, but when and why do elites resort to violent intimidation? Most case study and small-N assessments on the topic emphasize the importance of electoral competition for elites' decision to use violence. Collier and Vicente (2012), for example, argue that electoral violence is employed when unpopular incumbents are concerned about losing or fringe challengers want to increase their vote share and discuss these dynamics in several illustrative cases. In a subnational analysis of ethnic violence in India, Wilkinson (2004) argues that elites suppress or incite ethnic violence as a function of whether minority support is crucial for electoral success. This line of research thus suggests that the competitiveness of elections plays a major role in the use of violent intimidation before or during elections, but much less is known on what other factors, in particular international determinants, could produce violence. For example, does the presence of international organizations that could punish actors for using intimidation have an effect on whether and when violence is used as a strategy?

While the role of international actors has not been evaluated with regard to violent intimidation, research on other forms of electoral manipulation has documented that the presence of international election observers creates

² This literature extends earlier work on democratization and conflict (see Daxecker, 2007 for an overview). Yet with some exceptions, this research evaluates the relationship between elections and large-scale violence such as civil war, which likely omits incidents of election violence that did not pass the necessary severity threshold.

³ Some of this research presents empirical evidence for the presence and effectiveness of violent manipulation. Bratton (2008), for example, shows survey results documenting how voter intimidation is widespread in the African context and succeeds in depressing turnout.

⁴ For example, violence during the campaign period – whether committed by incumbents or opposition groups – aims to increase the chance of victory by clearing the playing field of opponents, influencing partisan choice, or deterring opponents' supporters from voting. On election day, violence can similarly be used to prevent supporters of rival parties from voting, influence the composition of the electorate, or disrupt elections.

incentives for elites to engage in fraudulent behavior in the pre-election period (Simpser and Donno, 2012; Hyde and O'Mahoney, 2010). Hyde and O'Mahoney, for example, argue that incumbents will attempt to manipulate parts of the electoral process that draw less international scrutiny and thus reduce the likelihood of punishment. Their empirical results show that the presence of international observers increases the likelihood of pre-electoral fiscal manipulation (Hyde and O'Mahoney, 2010). Similarly, Simpser and Donno (2012) demonstrate that the quality of governance, including the rule of law, bureaucratic quality, and media freedom, decline when countries hold elections monitored by high-quality international observer missions. Furthermore, Beaulieu and Hyde (2009) document that opposition movements observe incumbents' incentives for strategic adaptation and are subsequently more likely to boycott elections observed by international monitors.

Existing work on strategic manipulation has focused primarily on how the presence of international election observers induces changes from more to less-verifiable forms of fraud. Yet the possibility that international observers could displace more direct types of election fraud such as violent intimidation remains unexplored. While I do not dispute that monitoring could produce changes in the types of fraud used, the key issue is likely whether fraud will be noticed and documented by international monitors. Since international actors focus most of their attention on election-day, domestic actors could simply shift fraudulent methods to periods when they may go undetected or receive little attention, rather than invest in costly and less certain methods.⁵ Evaluating the effect of international monitors on violent intimidation has additional advantages. First, since violent intimidation can occur throughout the electoral process, I can examine whether the presence of international observers induces temporal shifts in manipulation in line with theoretical expectations. Second, data availability concerns should be less problematic for violent intimidation than for other forms of manipulation such as tampering with voter lists or vote buying. Research on political violence has produced a number of micro-level data sources on various forms of violence, including electoral violence.⁶ Finally, violent intimidation is arguably one of the more deplorable forms of electoral manipulation in that it can result in physical harm and death, thus making it helpful to know more about its causes.

3. International observers and incentives for violent manipulation

The main proposition put forward in this article is that the presence of international observers increases the likelihood of violent intimidation in the pre-election period. I

expect that in elections under intense international scrutiny, such as those observed by credible international observers, violent manipulation is displaced to the campaign period.⁷ The presence of international monitors constrains the use of violent intimidation (and others forms of verifiable fraud) on election day. During the polling, dozens or hundreds of short-term observer teams visit multiple polling stations to document instances of fraud, and incumbents and opposition actors will avoid being caught in front of international observers. Empirical research on international election monitoring has demonstrated observers' ability to document fraud (Kelley, 2008, 2012; Hyde, 2011). Moreover, the detection of election fraud, especially blatant forms of fraud such as violence, can result in punishment by international and domestic actors. Research has shown that fraudulent elections, particularly when such fraud is documented by international observers, increase the likelihood of post-election protests and violence (Tucker, 2007; Hyde and Marinov, 2008; Daxecker, 2012). In addition to domestic punishments, research shows that international actors have reduced international benefits or imposed other punishments such as shaming, mediation, or sanctions in response to highly fraudulent elections (Hyde and O'Mahoney, 2010; Donno, 2010). Consequently, domestic actors will refrain from using violent manipulation when international observers are present, but the negative consequences of electoral manipulation are less likely to occur when intimidation is used before international actors shift their attention to the election, such as the campaign period.

Violence in the pre-election period is less likely to be detected or, at a minimum, less likely to be criticized as strongly by international actors supervising the electoral process. Practitioners and scholars frequently lament the international community's overwhelming emphasis on election day (Carothers, 1997; Hyde and Kelley, 2011). As Carothers (1997) points out, monitors typically observe only a small portion of the electoral process, but pay much less attention to the pre-election period. More importantly, even missions observing the entire electoral process "tend to base their postelection statements primarily on election-day events" (Carothers, 1997: 21). Similarly, Hyde and Kelley (2011:3) argue that many groups continue to "arrive too late or are too understaffed to evaluate the full pre-election period and document whether there were problems." Several credible observer organizations now deploy long-term observers who observe the electoral process months before the elections, but given the small number of these observers, it is more difficult for them to document all instances of violent intimidation. Moreover, even when long-term monitors observe violent incidents in the pre-election period, it is easier for domestic actors to disguise the responsibility for such incidents in the run-up

⁵ Kelley (2012: 77), for example, points out that less verifiable forms of cheating are more costly and difficult to pursue than more easily detectable forms of manipulation. Arguments in Hyde and O'Mahoney (2010) and Simpser and Donno (2012) assume that incumbents are willing and able to engage in costlier forms of fraud.

⁶ Recent years have seen an explosion of data collection efforts and studies at the micro-level, several of which are described in more detail in the empirical section.

⁷ Since this argument assumes that fraud could be detected and punished by international observer organizations, I focus on international organizations that are capable and willing to expose irregularities. Because international observers usually require several months to prepare for monitoring missions and are thus invited many months before elections, it seems reasonable to assume that domestic actors have sufficient time to adapt their manipulation strategies (Hyde, 2011).

to elections than on election day. What this overwhelming attention on election-day fraud suggests is that international monitors should increase the likelihood of more direct forms of fraud – not only less verifiable forms – that are available in the pre-election period.⁸

H1. The presence of credible international observer organizations increases the likelihood of election-related violence in the pre-election period.

The argument has thus far assumed that all actors are equally likely to adopt manipulative strategies in response to the presence of international monitors. Yet the salience of international or domestic legitimacy concerns would arguably affect the extent to which domestic actors engage in strategic adaptation. In particular, incumbents who heavily restrict the media should be less concerned about the costs of manipulation since they can limit media coverage of violent intimidation even if it is documented by international observers.⁹ The ability to suppress or only selectively publicize the reports of international monitors would suggest that elites face a lower risk of domestic punishment for being caught cheating, and there should therefore be less displacement of violence to the pre-election period. The effect of international monitors on pre-election violence is thus expected to be conditional on the extent of media freedom.¹⁰

H2. The presence of credible international observer organizations increases the likelihood of election-related violence in the pre-election period, but this increase is more pronounced in states with a partly free or free press.

The above discussion implies that the presence of international observers should lower the incidence of blatant forms of fraud on election day since such fraud would likely be documented and punished by observers. Yet an examination of the direct effects of monitors on election-day violence is complicated for several reasons. First, and most importantly, once domestic actors perpetrate violence in the run-up to the election to avoid international scrutiny, bargaining dynamics among the actors involved change. Violence committed by incumbents to intimidate the opposition during the campaign, for example, may be followed by retribution from nonstate actors, making it

problematic to examine violence on election day without considering how preceding violence has altered the bargaining situation. Second, elites may shift manipulative strategies geographically in addition to adjusting them temporally. Hyde's analysis of Armenia (2007), for example, shows that the incumbent's vote share (and thus likely fraud) in observed polling stations was lower than in unobserved ones, suggesting that the use of manipulation strategies continues at unmonitored polling stations, albeit at a reduced rate. In a randomized field experiment, Ichino and Schündeln (2012) present evidence for a geographic displacement effect, showing that the presence of election monitors displaces irregularities to unobserved registration centers.¹¹ Elites may thus use violent intimidation in polling stations without international observers since such manipulation may be more difficult to detect. While such incidents may still be reported to international monitors, they are more difficult to classify as outright incidents of manipulation because of not having been observed directly. Finally, one would anticipate that violence is highest on or close to election day because of the high-stake nature of electoral processes in unconsolidated regimes, complicating empirical efforts to compare the incidence of violence before and during elections. These concerns suggest that assessing the direct effect of observers on election-day violence is difficult, which is why I do not specify formal hypotheses for election-day violence and present election-day models primarily to increase confidence in results for pre-election violence.

4. Empirical analysis

4.1. Data and variables

I test the empirical implications of the theoretical arguments on a set of legislative and executive elections in Africa for the 1990–2009 period. African elections offer a useful sample for testing the argument for several reasons. First, African countries started to adopt competitive elections in the early 1990s, thus representing a set of elections in unconsolidated regimes that went through a similar transition period at roughly the same time. Practitioners engaged in mitigating electoral violence often emphasize the importance of contextual factors, and focusing on a set of elections in the same region can thus be preferable to a global analysis (Joint Task Force on Electoral Assistance Report: 28). Second, limiting the sample to unconsolidated regimes seems warranted since the option to engage in fraud, in particular violent manipulation, is usually not available in consolidated democracies.¹² The quality of elections in Africa is often contested, making these elections a good test case for the theoretical argument. Lindberg's (2006) analysis of African elections, for example,

⁸ It is of course important to note that not all forms of fraud can be used throughout the entire electoral process. For example, stuffing ballot boxes, falsifying vote counts, or tampering with the tabulation process are forms of fraud available only on election-day. Similarly, altering the composition of election administration bodies or barring opposition candidates from running are forms of fraud limited to the pre-election period. Yet some items on the "menu of manipulation," such as vote buying or violent intimidation, can occur both in the pre-election period and on election day (Schedler, 2002).

⁹ In addition to domestic costs, it would be interesting to examine whether adaptation in the incidence of fraud is conditional on international legitimacy concerns, such as dependence on foreign aid or natural resource exports. Yet since international election monitoring is a form of leverage that may be adopted precisely because of such international dependence, it would be difficult to disentangle the effect of monitoring vis-à-vis foreign aid or natural resource dependence, which is why I focus on conditional effects stemming from domestic legitimacy concerns here.

¹⁰ Note that this expectation does not contradict evidence suggesting that press freedom on its own is associated with lower levels of electoral manipulation (Birch, 2011).

¹¹ While the authors focus on domestic observers and the voter registration period rather than the actual voting, the findings should extend to international observers and election-day fraud.

¹² Although it should be noted that the elections in many consolidated regimes were marred by substantial manipulation, including violent unrest, when democracy was first established.

shows that 44 percent of elections in the 1990–2003 period were considered flawed. Moreover, the incidence of violence as a form of manipulation is common in African elections, with estimates showing that violence affects 19 to 25 percent of elections (Bekoe, 2010). Finally, disaggregated data for the incidence of political violence are available for the African context yet no comparable data exist for a global examination of the argument.

I create a dataset that includes all elections held in African countries (including North Africa) for the 1990–2009 period. Data for elections come from the National Elections Across Democracy and Autocracy (NELDA) collected by Hyde and Marinov (2012). The unit of analysis is the election-round, meaning that runoff elections and multiple-round legislative elections are coded as separate cases.¹³ The data include 330 election rounds for the 1990–2009 time period.

Data for election violence in the pre-election period and on election day come from the Social Conflict in Africa Database (SCAD) described in Salehyan et al. (2012).¹⁴ SCAD codes information on social conflict events based on Lexis-Nexis searches of news wires. The data include a variety of event types, including demonstrations, violent riots, strikes, pro-government violence, violence by nonstate actors, and intergovernmental violence. In creating the dependent variables, I exclude demonstrations and strikes because they are nonviolent events. Of the violent events remaining, only those related to elections are included in the creation of dependent variables. SCAD codes the issues at stake in each event, which include a category for conflict events pertaining to elections.¹⁵ The dependent variables thus include information on election-related violent events committed by governmental and nonstate actors. Since violent intimidation is a form of fraud available to both government and opposition actors (Birch, 2011), it seems reasonable to include incidents committed by state and nonstate actors. To provide an example of the kinds of events subsequently included as instances of violent manipulation, SCAD contains an entry for January 29, 2009 that involved nonstate violence pertaining to elections and occurred in the run up to the 2009 South African

parliamentary elections held on April 22. A brief description of the event explains that “gunmen shoot dead an ANC official in the run up to an election.” Arguably, this event seems to capture violence intended to disrupt the ANC’s campaign and distort the electoral process, which fits well with the conceptualization of pre-election violence as manipulation adopted in this article.

I create two dependent variables. To measure pre-election violence, I count the number of election-related violent events in the three months preceding each election-round, but exclude violence on election day. Three months is of course an arbitrary time frame, but seems reasonable to plausibly establish the causal processes. While campaign periods can last longer than three months, a much longer time frame could be problematic for election rounds that are scheduled in shorter time frames, such as repeat or runoff elections. Even with a three-month time frame, 75 of 330 election-rounds were held less than three months after an initial vote. For these election rounds, I only include violent events that occurred between the earlier and subsequent election round to ensure that violent events are not counted more than once. Empirically, this measure ranges from 0 to 25, with 29.7 percent of cases experiencing one or more violent events. The second dependent variable codes information on the same events (election-related violence by state and nonstate actors) for the day of each election.¹⁶ This measure ranges from 0 to 3 empirically. Approximately 16.5 percent of election-rounds experience election-related violence on election day.

The key independent variable used to examine hypothesis 1 measures whether a credible international election observer mission was present at an election-round. Data were collected by the author and elections are coded as hosting a credible mission if one or more of the following organizations were present: The Carter Center, Commonwealth, Electoral Institute of Southern Africa (EISA), European Parliament (EP), European Union (EU), International Republican Institute (IRI), National Democratic Institute (NDI), and the United Nations (UN).¹⁷ The theoretical argument expects that the threat of negative publicity and punishments for blatant manipulation on election day creates incentives for strategic manipulation, which is why it is important to note that all of the organizations included have criticized fraudulent elections in the past. Organizations such as the African Union (AU) or the Economic Organization of West African States (ECOWAS), which are considered less willing or able to criticize elections, are therefore

¹³ Concurrent elections (legislative and executive elections held on the same day) are coded as a single event.

¹⁴ Other disaggregated datasets for political violence in Africa are available, including the Armed Conflict and Location Events Data (Raleigh et al., 2010) or the UCDP Geo-referenced Event Dataset (Sundberg et al., 2010). SCAD has three advantages over these alternative sources. First, SCAD codes information on the issues involved in each event, allowing me to include only violent events relating to elections. Second, unlike the UCDP data, SCAD includes information on violent events that did not result in fatalities. Since violent intimidation implies physical harm or injury, but not necessarily death, it is preferable to use SCAD to operationalize violent intimidation. Third, SCAD codes the initiator for each conflict event, allowing me to distinguish between violence perpetrated by the government and violence committed by nonstate actors.

¹⁵ SCAD identifies up to three issues for each event. In addition to including all events in which elections were listed as an issue at stake, I also reviewed the event descriptions for events related to democracy, pro-government, or unknown issues to make sure that no election-related violent events were left out. This process produced several additional events that were clearly election-related and they were therefore included in the creation of the dependent variables.

¹⁶ One problem with creating separate measures for pre-election and election-day violence is that the unit of analysis in SCAD is the event, not the event-day, meaning that some events last several days or even weeks. While the vast majority of events start and end on the same day, a few violent events start before election-day but continue throughout it. I code such cases as separate events for the pre-election and election-day variables. An alternative approach would be to convert SCAD events into event-days (although this procedure might exaggerate the influence of some conflict events).

¹⁷ The focus on credible missions is similar to Kelley (2012) and Simpser and Donno (2012). Reports by the listed organizations were consulted for each election round to verify whether they deployed a monitoring mission.

excluded. Empirically, 29 percent of election rounds in the data were monitored by one or more credible mission.

To test hypothesis 2, I add a variable measuring the level of press freedom using data from Freedom House, lagged by one year.¹⁸ The variable is categorical indicator of press freedom in the year before the election, coded 0 for not free, 1 for partly free, and 2 for countries with a free press. I interact the election observation measure with the press freedom variable to examine the hypothesized conditional relationship. While research has shown that press freedom is a consistent and significant factor in reducing the incidence of electoral manipulation (Birch, 2011), hypothesis 2 expects that the interaction between observers and press freedom is positive and significant, meaning that a displacement effect is more pronounced in states where the media can report more freely.

I include several variables to control for additional factors that could influence the occurrence of election monitoring and electoral violence. The first control variable accounts for the competitiveness of elections, an explanation for pre-election violence frequently emphasized in the literature (Chaturvedi, 2005; Collier and Vicente, 2012; Wilkinson, 2004). I operationalize the competitiveness of elections by calculating the margin of victory for each previous election round in the data.¹⁹ I code information for preceding election rounds since it would be problematic to include information on election outcomes to explain violence that occurs before these results are known. Second, I create a variable that indicates whether fraud occurred in the election prior to each election-round in the data. While this article focuses on explaining violent manipulation, it is likely that other forms of fraud (i.e. fraud not limited to violent intimidation) occurring in previous elections might affect whether leaders are likely to consider violent manipulation. I create a measure of fraud based on judgments in the U.S. State Department's Human Rights reports.²⁰ These reports provide assessments of the quality of elections and were used to collect data on the presence of fraud for each preceding election.²¹ For each election, the variable is coded 1 if the report includes statements such as that elections did not represent the will of the people, or that elections were highly fraudulent, or that they were judged not free and fair, were considered as falling short of international standards, or were reported to

be marred by grave or blatant violations or manipulation. The variable is coded 0 if the U.S. State Department report characterized elections as generally free and fair, if elections were seen as reflecting the general will of the people, or if the overall election assessment noted minor problems but stated that the elections were generally adequate.

Fourth, I control for countries' reliance on development assistance with data on official development assistance from the Aiddata project. Foreign aid provision is a form of international leverage that could influence both the probability of monitoring and the incidence of violent intimidation. The variable measures net ODA flows in constant U.S. dollars for each country in the data.²² Ethnic differences are frequently argued to influence electoral violence and I thus control for the effect of greater ethnic diversity by including the ethnolinguistic fractionalization index.²³ Economic development is widely seen as a key contributor to the occurrence of political violence, and I include a variable measuring GDP per capita for each election-year.²⁴ Population size is the final control variable and simply indicates the size of a country's population.²⁵ The press freedom, ODA, and GDP per capita variables are lagged by one year to reduce endogeneity concerns. The ODA, GDP per capita, and population size variables are log-transformed because of high skewness.

I use negative binomial regression to analyze pre-election and election-day violence since the dependent variables are event counts. The standard deviation of the number of pre-election violent events is almost three times larger than the mean, suggesting substantial overdispersion which is why a poisson count model is not appropriate.²⁶ All models include standard errors clustered by country to account for within-country correlation.

There are several concerns with regard to causal inference. First, a potential source of bias stems from the coding of the dependent variable. Information on violent events is based on media sources and since unobserved elections may generate less international attention, they might appear less violent than they really are. Data on actual events rather than media reports would be necessary to eliminate concerns over representativeness and completeness, yet are not usually available for a large number of cases. However, a recent comparison of violent events in a dataset based on media sources (Armed Conflict and Location Event Data) to data on actual events culled from secret U.S. government war logs (Wikileaks) showed that ACLED closely parallels Wikileaks, which should help alleviate concerns over reporting bias (O'Loughlin et al., 2010). Second, a relationship between

¹⁸ Data come from Freedom House and are available at <http://www.freedomhouse.org/report-types/freedom-press>. In results not presented, I included a variable measuring the level of democracy (with data from the Polity IV project) instead of press freedom. Democracy levels did not significantly affect the incidence of election violence, and because of multicollinearity concerns, I do not include the democracy variable in the reported results.

¹⁹ Data on election results come from the African Elections Database available at <http://africanelections.tripod.com/>. For concurrent elections, I calculate the margin of victory for the election with the smaller margin of victory. The margin of victory is calculated by subtracting the percentage of votes for the second-place finisher from the winning candidate or party's percentage in the election round preceding the current election.

²⁰ Available online at www.state.gov/g/drl/rls/hrrpt/.

²¹ For some elections before 1990, coding the presence of fraud in the preceding election was not possible because the country either did not hold elections or because previous elections were held in a year for which U.S. State Department reports were not available.

²² Available at <http://www.aiddata.org/content/index>.

²³ Data for ELF come from Roeder (2001). Large numbers in ELF indicate greater ethnic diversity.

²⁴ GDP data come from the Penn World Tables, version 6.3, available online at <http://pwt.econ.upenn.edu/>.

²⁵ Data come from the Penn World Tables, version 6.3.

²⁶ The negative binomial model is an extension of the Poisson model for over-dispersed count data, meaning that the conditional variance exceeds the conditional mean. Diagnostic tests using Stata's `countfit` command confirmed that the negative binomial model is strongly preferred to the poisson, zero-inflated poisson, and zero-inflated negative binomial model.

observers and violence could be the result of observer organizations anticipating violence in elections rather than causing violent manipulation. Observers may be more likely to monitor elections that have a high risk of being violent and an empirical relationship could thus be driven by observers' anticipation of problems rather than the presence of observers. For example, election violence is common in countries like Ethiopia, Guyana, Kenya, or Nigeria, and international organizations could be especially inclined to observe the voting in those states. Finally, and related to the previous point, elections monitored by international observers might differ substantially from those that are not monitored, and these unobserved differences between observed and unobserved elections rather than the presence of monitors might be driving an empirical relationship between observers and violence.

These three types of bias would apply to all portions of the electoral process and suggest that observed elections experience higher rates of violence before, during, and after elections rather than just the pre-election period as hypothesized in the theoretical section. I nevertheless use coarsened exact matching to address the second and third concern mentioned above. I match on variables that are likely to influence observer anticipation of election violence and decisions to invite observers (Iacus et al., 2012). To account for observers' anticipation of violence, I create a dichotomous variable indicating whether previous election rounds experienced election-related violence or not. Data for election dates and violence in previous elections come from NELDA and SCAD. I draw on research on the determinants of election observation to account for differences between observed and unobserved elections (Hyde, 2011; Kelley, 2012). I expect that observers more frequently observe elections that experienced fraud in the preceding election-round, that were observed by international monitors in the past, that occur in countries with low levels of democracy, and that take place in states receiving official development assistance. The data are then pre-processed using the previous violence variable described above, the previous fraud variable, a democracy variable, the ODA variable, and a new variable coded 1 if elections were observed by a credible mission in the past.²⁷ Matching preprocesses the data and excludes observations from the treatment group (i.e. observed elections) and control group (i.e. unobserved elections) that differ fundamentally on covariate values and thus might be driven by the absence or presence of observers. In sum, matching creates a sample in which observations that could be correlated with the presence of monitors are pruned from the data.

5. Results

The first model presented in Table 1 provides support for hypothesis 1. The coefficient for the international

²⁷ Data for democracy come from Polity IV. I use three strata for continuous variables (democracy and official development assistance). Matching on these variables prunes 90 observations from the sample. Slightly changing the number of strata did not alter the main results presented.

Table 1
Negative binomial models of election violence, 1990–2009.

| Variables | Pre-Election Violence | | Election-Day Violence | |
|---------------------------------------|-----------------------|---------------------|-----------------------|--------------------|
| | Model 1 | Model 2 | Model 3 | Model 4 |
| Observers | 0.932** (0.287) | 0.364 (0.279) | -0.183 (0.373) | -0.060 (0.493) |
| Press freedom ^{t-1} | -0.384† (0.229) | -1.014** (0.284) | -0.099 (0.208) | -0.029 (0.244) |
| Press*observers | - | 1.261** (0.370) | - | -0.247 (0.443) |
| Margin of victory | -0.006 (0.005) | -0.006 (0.006) | -0.003 (0.007) | -0.003 (0.007) |
| Previous fraud | 0.469* (0.221) | 0.553* (0.221) | 0.558 (0.453) | 0.546 (0.451) |
| ODA, logged ^{t-1} | -0.172 (0.591) | -0.158 (0.565) | 0.063 (0.232) | 0.076 (0.229) |
| GDP per capita, logged ^{t-1} | -0.151 (0.245) | -0.231 (0.245) | 0.419+ (0.252) | 0.435+ (0.255) |
| Population, logged | 0.859† (0.447) | 0.757† (0.421) | 0.218 (0.159) | 0.224 (0.162) |
| Ethnic fractionalization | 1.173* (0.594) | 0.992 (0.604) | -0.632 (0.564) | -0.595 (0.565) |
| Constant | -7.244** (2.163) | -5.534** (2.101) | -6.545* (2.977) | -6.842* (3.030) |
| N | 218 | 218 | 218 | 218 |

Notes: Estimates are coefficients with clustered standard errors in parentheses.

** $p < 0.01$ * $p < 0.5$ † $p < 0.1$ (two-tailed tests).

Table 2
Predicted number of events for significant independent variables, Table 1 model 1.

| Variables | Model 1 ($\Delta\%$) |
|--------------------------------|------------------------|
| Observers = 0 | 0.33 |
| Observers = 1 | 0.99 (+200) |
| Previous fraud = 0 | 0.45 |
| Previous fraud = 1 | 0.74 (+64.4) |
| Press freedom = 0 | 0.67 |
| Press freedom = 2 | 0.35 (-47.8) |
| Population - 1SD | 0.15 |
| Population + 1SD | 1.18 (+686.7) |
| Ethnic fractionalization - 1SD | 0.37 |
| Ethnic fractionalization + 1SD | 0.65 (+75.7) |

observer variable is positive and significant at the 95 percent confidence level. In substantive terms, the probability of violence increases by 200 percent when the international observer variable is varied from 0 to 1. As expected, elections in which international monitors observed the electoral process have an increased risk of violence in the three months prior to elections. The evidence thus supports the notion that domestic actors strategically adapt their use of electoral manipulation. Results for control variables are mostly in line with expectations. Elections with a previous history of fraud are at a greater

Table 3
Predicted number of events for observer and press freedom interaction, Table 1 model 2.

| | Not free | Partly free | Free |
|--------------|----------|-------------|------|
| No observers | 1.10 | 0.40 | 0.14 |
| Observers | 1.59 | 2.03 | 2.60 |

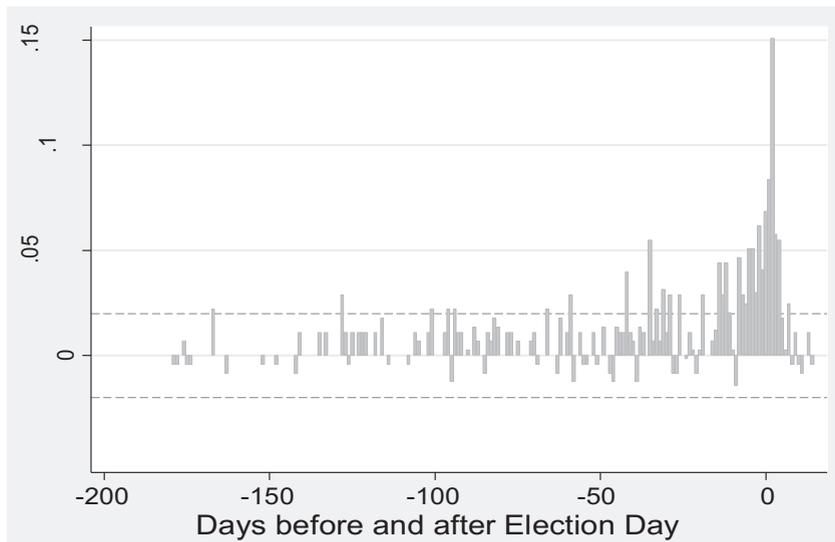


Fig. 1. Differences in means of violent events for observed and unobserved elections in Africa, 1990–2009 (SCAD).

risk of pre-election violence and greater levels of press freedom reduce the number of pre-election events, although the coefficient for press freedom is significant only at the 90 percent confidence interval. In addition, increases in population size and ethnic fractionalization result in more pre-election violence. No significant effect is found for the margin of victory, development assistance, and GDP variables. Substantive effects for significant variables in model 1 are presented in Table 2.

The second model in Table 1 shows support for the conditional relationship between observers, press freedom, and pre-election violence proposed in hypothesis 2. The coefficient for the interaction term is significant and positive, indicating that more displacement occurs in elections where the press is free or partly free. The coefficient for the observer variable is insignificant in this model, indicating the absence of a significant relationship between observers and pre-election violence when press freedom is zero. To facilitate interpretation of the interaction, I calculated the predicted number of expected pre-election events for various levels of press freedom in observed and unobserved elections. As Table 3 shows, the predicted number of events in countries without press freedom is comparable regardless of whether observers were present, indicating that little displacement occurs when incumbents can control what the media reports. Yet in states where the media is free or partly free, the difference in the expected number of violent events becomes much more pronounced.

The third and fourth model evaluate the effect of international monitors on election day violence. The coefficient for the observer variable is negative but not statistically significant. Model 4 also finds no significant relationship between observers, press freedom, and election-day violence. As discussed earlier, violent intimidation that precedes elections by weeks or months likely changes the bargaining dynamics among domestic actors, which makes it difficult to assess the direct effect of observers on election-day violence. In addition, geographic

displacement of fraud may occur in addition to temporal displacement, counteracting potential deterrent effects of international monitoring on election-day manipulation. Finally, *ex ante* expectations suggest that bargaining should be most competitive (and thus potentially violent) on election day, which complicates efforts to compare levels of violence before and during elections.

An additional problem with the above research design is that the time periods used to create dependent variables in the pre-election and election-day models differ greatly, with the pre-election period covering three months compared to a single day for election day. This setup makes it difficult to evaluate more disaggregated patterns in the incidence of violence before and during elections. To examine more closely whether temporal trends correspond to the theoretical expectations, I used data from SCAD to calculate the number of daily violent election-related events for each election round, starting at 180 days before the election and continuing throughout election day until 15 days after elections.²⁸ I then computed two-sample difference of means tests for the number of violent events on each of these days to examine whether observed elections have a higher incidence of violent events before elections. The difference of means test compares two groups of observations and tests the hypothesis that the means of both groups are identical. Fig. 1 shows the differences in means for observed and unobserved elections from 180 days before elections to 15 days after election-day. I observe that the differences in means are almost always positive in the run-up to election-day, indicating that the mean number of violent events is larger in observed elections than unobserved elections. These positive differences are statistically significant at the 95 percent confidence interval (indicated by the dashed line) on several occasions before elections, with

²⁸ Event onsets were used to create the figure because some events in SCAD last several days.

especially pronounced spikes approximately 45–25 days before elections, 15–10 days before elections, and surrounding election day. The positive and often significant differences in means before elections support the theoretical arguments made here. The positive and statistically significant differences in means surrounding election day are unsurprising given that violence preceding elections likely spurs subsequent violent events. In addition, violence is expected to be more frequent when the stakes are highest.

5.1. Robustness tests

I conduct a series of additional robustness tests to ensure that the above results are not influenced by the measurement of variables and the estimation methods employed. Robustness tests focus on pre-election violence to keep with the central proposition of the article and conserve space.²⁹ The first two robustness tests distinguish between the perpetrators of election violence. While the theoretical argument does not distinguish between state and nonstate actors, incumbents may be more concerned about international punishment for using violence in front of international observers than nonstate actors, who are not usually the targets of international sanctions imposed on governments in response to electoral manipulation.³⁰ Since SCAD provides information on the perpetrators involved, I create separate dependent variables for election-related violent incidents initiated by state and nonstate actors, respectively. Models 1 and 2 show that the main theoretical contention is confirmed for state actors as indicated in the positive and significant coefficient for the observer variable in model 2, but not for violent events committed by nonstate actors. The coefficient for the observer variable is positive but misses conventional significance levels, showing that the displacement of violent intimidation takes place only for incumbents (Table 4).

Model 3 evaluates whether large observer organizations have a different effect on elites' incentives to displace violence. When coding data for the presence of a credible observer mission, I also collected information on the number of observers deployed by each mission. From this information, I created a variable coded 1 for each observation mission with more than 15 deployed observers, 0 otherwise. The positive and significant coefficient suggests that the effect of election monitoring on pre-election violence holds for large monitoring missions. Model 4 examines the effect of low-quality observers (i.e. organizations that never or almost never criticize elections) on pre-election violence. The theoretical argument suggests that a displacement effect should occur only for elections monitored by organizations that actually impose costs for using violent intimidation on election day. Using the Data on International Election Monitoring (DIEM) collected by Kelley and Kolev (2010), I

create a variable coded 1 for elections observed by the African Union and the South African Development Community, two organizations that frequently observe African elections but have a reputation for failing to criticize even highly problematic ones. Some observations are dropped from the analysis because DIEM ends in 2004, but the insignificant coefficient for the low-quality observer variable indicates that temporal shifts in violent manipulation do not occur in elections observed by friendly organizations.

The fifth model evaluates whether the violent events included in the creation of dependent variables adequately capture the concept of violence as a form of electoral manipulation. SCAD includes a large number of violent conflict events unrelated to elections that were excluded from the dependent variables, such as disputes over resources, religious discrimination, education, or foreign affairs (among others). I create an additional dependent variable that includes information on violent events that were not considered election-related.³¹ This variable includes events such as a dispute between residents over village boundaries in Cameroon that occurred in May 7, 2005, less than three months before legislative elections were to be held. Since I anticipate that monitors induce shifts in election-related violent events but not unrelated violence, I expect there to be no empirical relationship between international observers and violent events that could not be conceptualized as a form of electoral manipulation. Model 5 supports this expectation since the coefficient for the observer variable is not statistically significant.³²

Models 6–8 explore whether the effect of international monitoring and violent manipulation holds for international observer missions that pay more attention to the pre-election phase by deploying long-term observers (LTOs). In such missions, observers arrive weeks or months prior to the election and should thus be more capable of detecting and criticizing violence in the pre-election period. Yet as discussed earlier, even missions that deploy LTOs cannot dedicate the same amount of attention to the pre-election period because the number of LTOs is much smaller than the contingent of observers deployed on election day. Moreover,

²⁹ I conducted an identical set of robustness tests for election-day violence.

³⁰ Alternatively, research suggests that incumbents have an advantage in employing manipulative strategies (Hyde, 2007). Yet since the data show that nonstate actors are responsible for a larger percentage of violent election-related events than state actors, this explanation seems unlikely to pertain to violent intimidation.

³¹ As noted earlier, a few events in which the issue at stake was coded as democracy, pro-government, and unknown actually revealed a relation to elections in the brief issue description and these events were included in the variable on election-related violence.

³² This finding also addresses an alternative explanation for a relationship between international election monitoring and pre-election violence. Kelley (2009, 2012) shows that international observers are more likely to endorse elections preceded by violence because of concerns over escalating levels of violence and further destabilizing the situation. While her research does not explore the determinants of violence, her findings imply that domestic actors could strategically use pre-election violence to benefit from observers' willingness to assess such elections more leniently rather than the theoretical explanation put forward here. Yet Kelley (2012: 187) argues that all instances of violence – regardless of whether related to elections or not – result in more positive assessments by international observers and includes all violence before elections in her empirical test. Hence, her argument would suggest a positive and significant relationship between international observers and all types of pre-election violent events rather than only election-related violence. Since empirical results show that monitors affect the occurrence of election-related violence but not other violent events, they seem more supportive of the theoretical explanation developed in this article.

Table 4
Robustness tests for pre-election violence.

| Variables | State violence | Nonstate violence | Large missions | Low-quality IOs | Non-election violence | Long-term observers | LTOs, state violence | LTOs, nonstate violence | No matching |
|---------------------------------------|--------------------|---------------------|---------------------|---------------------|-----------------------|---------------------|----------------------|-------------------------|---------------------|
| Observers | 0.657* (0.289) | 0.398 (0.272) | – | – | 0.857 (0.659) | – | – | – | 0.997** (0.266) |
| Large observer mission | – | – | 0.878** (0.309) | – | – | – | – | – | – |
| Low-quality IOs | – | – | – | –0.260 (0.466) | – | – | – | – | – |
| Long-term observers | – | – | – | – | – | 0.657* (0.277) | 0.568* (0.248) | 0.138 (0.287) | – |
| Press freedom | 0.068 (0.327) | –0.228 (0.213) | –0.373+ (0.224) | –0.595* (0.249) | –0.619+ (0.369) | –0.347 (0.240) | 0.068 (0.331) | –0.220 (0.214) | –0.214 (0.164) |
| Margin of victory | –0.009 (0.005) | –0.008 (0.005) | –0.006 (0.005) | –0.002 (0.006) | 0.001 (0.010) | –0.004 (0.005) | –0.007 (0.006) | –0.008 (0.005) | –0.002 (0.003) |
| Previous fraud | 0.529 (0.324) | 0.406 (0.292) | 0.499* (0.216) | 0.486 (0.309) | –0.141 (0.605) | 0.441* (0.191) | 0.488 (0.341) | 0.401 (0.287) | 0.472* (0.200) |
| ODA, logged ^{t-1} | 0.605 (0.498) | 0.206 (0.397) | –0.181 (0.574) | –0.300 (0.594) | –0.814** (0.279) | –0.167 (0.561) | 0.609 (0.501) | 0.194 (0.385) | –0.301 (0.343) |
| GDP per capita, logged ^{t-1} | –0.132 (0.449) | –0.062 (0.199) | –0.138 (0.234) | 0.183 (0.376) | 0.401 (0.281) | –0.086 (0.249) | –0.072 (0.484) | –0.023 (0.209) | –0.065 (0.206) |
| Population, logged | 0.382 (0.289) | 0.564+ (0.306) | 0.850+ (0.441) | 0.900 (0.571) | 1.323** (0.211) | 0.831+ (0.433) | 0.383 (0.293) | 0.555+ (0.303) | 0.748** (0.289) |
| Ethnic fractionalization | 1.628 (1.173) | 0.859 (0.676) | 1.004+ (0.582) | 1.316+ (0.789) | –0.100 (0.820) | 1.197+ (0.615) | 1.608 (1.223) | 0.879 (0.686) | 0.963+ (0.504) |
| Constant | –9.222+ (5.434) | –6.839** (2.066) | –7.009** (2.981) | –8.974** (2.981) | –11.945** (3.310) | –7.346** (2.347) | –9.601 (5.873) | –6.863** (2.261) | –6.205** (1.829) |
| N | 218 | 218 | 218 | 152 | 218 | 218 | 218 | 218 | 279 |

Notes: Estimates are coefficients with clustered standard errors in parentheses.

** $p < 0.01$ * $p < 0.5$ $ip < 0.1$ (two-tailed tests).

having separate groups of observers for different parts of the electoral process can create difficulties in integrating the observations of long- and short-term observers, which could prevent sufficient attention to pre-election problems in organizations' final reports or post-election statements. Election-day observers may feel primarily responsible for commenting on what happens on election day and thus "consciously or subconsciously discount earlier problems" (Bjornlund, 2004: 146). It is thus not clear whether the deployment of pre-election missions is sufficient to deter the use of manipulation, including violent intimidation, in the pre-election period. To investigate this issue empirically, I create a measure coded 1 for credible election observer organizations that deployed long-term observers, 0 otherwise. The coefficient for this variable remains positive and significant, suggesting that the presence of pre-election missions is not sufficient in deterring violent manipulation before elections. Yet the effect of long-term observers is again conditional on the actors engaged in violence. Distinguishing between state and nonstate actors in models 6 and 7, I find that the presence of LTOs successfully alters the behavior of nonstate actors but not incumbents.

The last model (model 9) specifies the same model as the first model in Table 1, but without matching. Results are very similar to the matched sample. The coefficient for the observer variable is positive and significant in the models for pre-election violence, again confirming hypothesis 1.

6. Conclusion

This article emphasizes how international election observers influence the strategic calculation of domestic elites participating in electoral processes. I have argued that the

threat of punishment by international organizations engaged in election monitoring creates incentives for incumbents and nonstate actors to shift the use of violent intimidation to the pre-election period. A systematic analysis of pre-election violence in African elections for the 1990–2009 period supports the theoretical argument. The article confirms and extends earlier research on international election monitoring and strategic manipulation by examining how the presence of international election monitors can induce shifts in direct forms of manipulation (Hyde and O'Mahoney, 2010; Simpser and Donno, 2012). Empirical findings present support for strategic manipulation with regard to violent intimidation by showing that the presence of international monitors increases the likelihood of pre-election violence. Matching methods are used to address the possibility that observers monitor elections anticipated to be violent and to alleviate concerns that unobserved differences between monitored and unmonitored elections drive empirical relationships between election observation and violence. In addition, the analysis evaluates whether strategic manipulation deters the use of violent intimidation on election day. While no direct relationship exists between monitoring and election-day violence, I argue that geographic displacement of violence, changes in bargaining dynamics as a result of pre-election violence, and the higher *ex ante* probability of violence on election day make it difficult to assess the direct effect of monitors on election-day violence.

There are several avenues for future research. First, results for pre-election violence indicate that the displacement effect induced by international monitors is limited to violence committed by incumbents. I suspect that incumbents are more sensitive to reputational and other costs imposed by

international organizations, but more research is necessary to examine the differing incentives of state and nonstate actors in more detail. Second, it would be important to examine whether strategic adaptation occurs outside of the African context. While research has documented shifts in non-violent forms of manipulation at the global level (Hyde and O'Mahoney, 2010; Simpser and Donno, 2012), disaggregated data on the incidence and timing of election-related violence are available only for Africa. However, the fact that recent elections in Afghanistan, Cambodia, Colombia, Pakistan, and the Philippines experienced substantial violence but were also monitored by international organizations suggests that the argument could apply to other regions, and extending the geographical domain of existing data should therefore be a priority.

The policy implications of this research may appear controversial, but it is impossible to establish whether the presence of monitors induces temporal shifts in the use of violence that would have occurred in their absence, or whether their presence could result in a net increase of violence. While I do not find that the displacement effect disappears for observer missions that pay greater attention to the pre-election period, the findings nevertheless support the frequent call for greater supervision of all parts of the electoral process. It is possible that improvements in the quality and numbers of long-term observers will succeed in deterring the use of pre-election intimidation by state actors (Hyde and Kelley, 2011). Another promising avenue for international observer organizations is to work more closely with domestic observer organizations, which could better monitor the pre-election environment in cases where international organizations lack a strong presence.

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Appendix

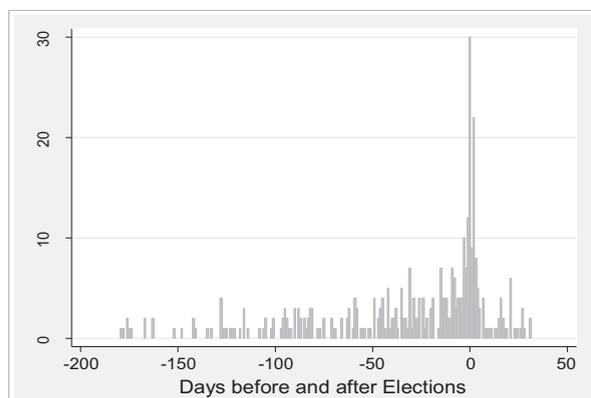


Fig. A1. Number of violent events in African Elections, 1990–2009 (SCAD)

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