Democratization and Electoral Violence in Sub-Saharan Africa, 1990-2007

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Abstract

This paper investigates two principal questions that are underexplored in the existing literature on democratization and African politics. First, how frequent has electoral violence been in Sub-Saharan Africa since the continent's grand democratic experiment began in 1990? Second, what explains the variation in why some elections are violent but others are not? To answer the first question, we disaggregate the concept of "electoral violence" into pre- or post-vote violence, incumbent or challenger violence, and level of violence, and we construct an original dataset measuring these characteristics. We generate several findings; most importantly, we find that significant electoral violence occurs in 19% of elections, that most violence occurs before the vote and is perpetrated by incumbents, and that if violence breaks out after an election it tends to be more severe and involve challengers. To answer the second question, we map out causal mechanisms linking elections to violence and derive some preliminary hypotheses. Our quantitative analysis using descriptive statistics does not yield any strong patterns, but suggests that margin of victory, regime type, cleavage structure, income level, and pathway to power matter. However, many of the hypothesized causal mechanisms are not captured in the quantitative cross-national data, and the data themselves are limited. To compensate, we turn to qualitative analysis, examining structurally similar cases that vary in whether or not significant electoral violence occurs. We find a consistent pattern that access to vital resources, especially land, is the central issue in cases of high electoral violence; that elections trigger claims of indigeneity in states with significant migration; and that high violence is more likely where local rival groups fighting over access to vital resources are essential to the winning electoral strategy of national political parties in close elections.

1. Introduction

In his classic text on democratization, Robert Dahl warned that political transitions are risky. To introduce competition and contestation into a previously closed political system invites the potential for conflict between incumbents and opponents as each weighs the costs of violence and toleration (Dahl 1971). Dahl's warning was not prominent among international and domestic advocates who launched sub-Saharan Africa's grand democratic experiment after the end of the Cold War. In a handful of years, the modal African political system switched from one-party hegemony to multi-party competitive elections, a remarkably swift and comprehensive set of regime changes across the continent.

In the first two decades of sub-Saharan Africa's grand experiment, many democratic gains are evident. On average, African political systems are more competitive, repeat multi-party elections are more common, media are more liberalized, civil society is stronger, public criticism is increasingly tolerated, and judiciaries are more independent. To take one common measure of the quality of democracies, the average Freedom House score for political rights and civil liberties for sub-Saharan states improved significantly between 1989 and 2007.[1] There is much, in short, to appreciate about Africa's democratic experiment.

However, the overall democratic gains mask an important empirical regularity, one that echoes Dahl's warning. The experience of many African democratic transitions, in particular during electoral periods, has been violent. In the last two years in particular,

[1] In 1989-1990, the average political rights and civil liberties scores for Sub-Saharan African countries was 5.91 and 5.32, respectively; in 2007, the same scores changed to 4.29 and 4.08.

the risk that elections may produce political violence has been all too apparent as two regionally important and moderately affluent states (in comparative Africa terms)—Kenya and Zimbabwe—experienced highly violent episodes of violence directly tied to electoral contests. Yet these are not the only countries where election-related violence has occurred—from Côte d'Ivoire to Equatorial Guinea to South Africa's transition to multi-racial democracy, violence has been a major feature of electoral periods.

Much of the early literature on Africa's democratic transition did not focus on the risk of violence. Rather, the foundational comparative works explained why the transitions occurred (Bratton and van de Walle 1997), the critical ingredients of democratic transitions, such as civil society (Harbeson et al 1994), what "democracy" means to voters (Schaffer 1998), and what might lock in democratic gains (Lindberg 2006), among other themes. However, the early optimism has been met with new skepticism, in particular of late. Robert Bates argues in a new book that political liberalization shortened the time horizons of African leaders during the past two decades, increasing the likelihood that state leaders would predate rather than develop institutions for the common good (Bates 2008).[2] In another recent influential book, Paul Collier argues that democratic elections increase the risk of political violence in poor countries (Collier 2009). These more Africa-oriented works dovetail with general arguments that Jack Snyder and others have made, namely that democratization in countries with weak institutions increases the risk of nationalism and armed conflict (Snyder 2000). The terms of a critical theoretical debate, one with clear policy implications, are now taking shape: does democratization increase the risk of political violence and, if so, how and why?

[2] In earlier work, Bates (1983) argued that electoral competition could lead to political actors to make ethnic claims to build winning coalitions.

In this paper, we address these questions in two principal ways. In the first major part of the paper, we describe the frequency of electoral violence. Despite the importance of knowing how common electoral violence is, there is a surprising lack of published empirical work on how frequent electoral violence is. To address this question, we specify and disaggregate an outcome of interest, namely "electoral violence." We do so in part because in the literature, especially the critical literature, there is fairly little explicit specification of a dependent variable describing violence related to elections. As part of the conceptual discussion, we argue that electoral violence should be disaggregated into pre and post-election violence, incumbent and challenger violence, and levels of violence. Having specified a dependent variable, we assemble a dataset covering African transitions from 1990 to 2007. Although Dorina Bekoe at the United States Institute of Peace has done similar research, we know of no other cross-national dataset that focuses specifically on electoral violence in Africa and that measures electoral violence across a nearly two-decade period.[3] The central finding from the study is that serious electoral violence is not nearly as frequent as critics suggest but frequent enough to warrant serious concern from advocates. Most elections of the 213 cases of elections in the dataset experience some violence, and 19% result in violence above a substantive threshold that we specify. There is, in short, considerable variation in whether or not elections trigger violence.

[3] Staffan Lindberg codes for violence in his dataset on African elections, which covers the 1990-2003 period. Lindberg codes for whether an election was peaceful, whether there were isolated incidents of violence, or whether there was a campaign of violence. While his research remains critical, and we build off his dataset, violence was not the focus of his research and we seek to improve on his conceptual categories and data collection.

The variation in frequency of electoral violence shapes the central theoretical puzzle of the paper, namely why does electoral violence occur in some African elections but not in others. To address the question, we spell out different plausible mechanisms for why political actors and non-candidates would resort to violence in an electoral context; we also isolate five sets of hypotheses that might explain variation in why violence is a feature of some African electoral contests but not others. The exercise is in part a mapping one, given that we find little in the literature that focuses on these theoretical questions. Much of the literature on violence focuses on violence in war, in particular civil war, which we hypothesize to have quite different dynamics from violence in electoral contexts.

We proceed with some preliminary quantitative hypothesis testing, using descriptive statistics, followed by qualitative case comparisons, which together yield two sets of conditional findings. First, while constrained by limited data to measure the hypothesized causal relationships, our quantitative analysis in general does not lend strong support to any of the conventional explanations that we test. That said, there are some weak indications to suggest that competitive contests in authoritarian or anocratic states with comparatively moderate wealth but low growth rates, and states with non-dominant ethnic groups but groups still large enough to form the core of a winning coalition appear more likely to experience electoral violence. We recognize, however, the limits of the analysis, and we plan to employ other statistical models in the future. In this paper, however, we turn to qualitative comparison for more leverage. Here we find a consistent and surprising pattern whereby significant electoral violence repeatedly occurs in states with internal migration and around local conflicts over critical resources, in

particular land. We hypothesize that elections are triggering two sets of dynamics. First, in the context of weak, corruptible institutions, where the exercise of power outflanks property rights, electoral contests create a window of opportunity to change access to vital resources, notably land but also secondarily marketplace rights and trade routes. Elections thereby politicize preexisting resource rivalries, and local actors in turn mobilize to gain or protect their access to such resources. Second, democratic contests ignite organic claims to indigeneity, to claims of "sons of the soil," which doubly serve as a rhetoric of legitimate access to land. Finally, where these local resource grievances dovetail with national electoral incentives—where national political parties need the votes of rival local groups fighting for access to vital resources—we find electoral violence to be more likely. The analysis does not apply to all cases of electoral violence in Africa, notably in mineral-wealthy countries such as Equatorial Guinea. But the hypothesis explains a good deal of variation, suggesting why significant electoral violence has occurred in places such as Kenya, Zimbabwe, Zanzibar, Côte d'Ivoire, Nigeria, and South Africa but while it tends to be absent in places such as Ghana, Mali, Chad, Zambia, Mozambique, Benin, and other locations. Moreover, the finding is at least theoretically consistent with studies of electoral fraud in other locations, where landholding inequality is seen as a generator of electoral fraud (Ziblatt 2009).

2. Scope Conditions, Concepts, and Data Collection

Why limit the study of electoral violence to Africa? There are three principal reasons. First, the research is a mid-range cross-national study, focused on one world region of nearly 50 countries during an 18-year period, rather than on a global sample across different historical periods. The regional and temporal focus affords a degree of

control over the sample. Second, sub-Saharan African countries have experienced a historically similar transition period. The end of the Cold War triggered a remarkable change from single-party rule to multi-party competition. Yet there is considerable variation in that experience, in particular in how many and which African states experienced electoral violence. Sub-Saharan Africa is thus a region that underwent a historical shift in regime type but with significant variation in the dependent variable of interest—electoral violence. Third, in recent years, African elections have produced notable cases of electoral violence, in particular in Kenya and Zimbabwe. There are thus practical and policy implications to the analysis—policymakers and others have interest in anticipating and understanding how and when electoral violence is likely to occur.

A study of electoral violence should acknowledge that neither "violence" nor "electoral violence" is a concept with a consistent social scientific definition, and so we start by defining our terms. Indeed, one limitation of existing studies is a wide and inconsistent (across studies) definition of the dependent variable. In Collier's work, for example, "violence" ranges from civil war to riots, to political strikes, and assassinations (Collier 2009). In Bates work, the dependent variable for state failure (including violence) is a dummy variable for the formation of militias (Bates 2008, 147). In Snyder's work, the dependent variable is armed conflict (Snyder 2000). While impressive, we find the specifications not specific enough for evaluating whether and how multiparty electoral processes trigger violence and not specific enough to capture the modal pattern of electoral violence (versus wars or coups) in sub-Saharan Africa. Hence we seek a more specific conceptualization of violence related to elections, namely "electoral violence."

At the broadest level, by violence or political violence we mean the deliberate use of physical harm or the threat of physical harm for a political purpose. Overt physical violence can take the form of beatings, torture, and murder, but violence is also evident by its threat—by coercive intimidation. “Electoral violence” refers to physical violence and coercive intimidation directly tied to an impending electoral contest or to an announced electoral result. One problem with this seemingly straightforward definition is that in some instances it can be difficult to know whether violence is, or is not, directly related to an election. For these ambiguous cases, when coding in the dataset we consider any politically related violence that occurred six months prior to an election or three months after an election to be "electoral violence”.[4]

Even if these conceptual parameters help to clarify “electoral violence,” the concept requires further disaggregation—and we propose to do so along three dimensions. First, the concept of electoral violence encompasses at least two distinct logics of violence. Elections are always formal contests between incumbents and challengers, and thus we propose that incumbents and challengers will generally use violence for different reasons and in different circumstances. Incumbents more often than not employ violence to maintain power using the coercive means of the state, while challengers use violence to protest outcomes using non-state means. These two logics link up to distinct threads in the literature on violence. On the one hand, the former relates to studies of state violence and repression and on the other hand the latter relates to studies of rebellion and protest. A cross-national empirical study of electoral violence, we contend, thus should at least record the origins of violence, something which most

[3] Violence that was clearly unrelated to an impending or past election was not coded as electoral violence.

existing studies of electoral violence do not do. In our dataset, we create two categories—incumbent (referring to any state agent, militia, political party member, or hooligan who acts on behalf of the party that controls the executive) and challenger (which refers to any party member, militia, or hooligan acting on behalf of the political party that does not control the executive). The categories are not mutually exclusive—in some cases, incumbents and challengers can both commit violence in an election.

A second source of disaggregation concerns whether violence occurred before or after elections are held. Again, we observe two distinct logics of violence, or at least the possibility for the presence of two distinct logics of violence. On the one hand, pre-vote violence would occur as political actors seek to shape voting preferences and patterns. That is, pre-vote electoral violence occurs in periods with uncertainty about voting outcomes when political actors seek to shape those who vote and how they vote. By contrast, post-vote violence generally expresses a logic of response to an outcome, either to an actual voting result or to an announced rigged result. For the latter, violence is, at least ideal-typically, less an effort to shape uncertainty and more an effort to change a political direction. Both points highlight a general truism about elections, which is that they represent periods of fluid authority when who governs is in question—a point to which we will return.

The disaggregation between incumbent and challenger and pre-vote and post-vote violence suggests some initial hypotheses. We might hypothesize, for example, that because incumbents have access to state coercive forces they would be more likely to engage in electoral violence. They simply have greater capacity to engage in violence, as well as to respond to challenger violence. By contrast, even though challengers are less

likely to engage in election violence overall, if they do resort to violence they are probably more likely to do so after an election. Before an election, given coercive power asymmetries between incumbents and challengers, challengers would theoretically prefer persuasion (including patronage promises) because of the risk that violence would trigger a repressive response from a state, thereby removing challengers from a contest. To be sure, challengers could trigger incumbent violence to diminish the legitimacy of an election they fear they would lose, but that seems an unlikely scenario. By contrast, after an election, challengers have less to lose if an incumbent has declared victory.

A third source of variation concerns level. Not all electoral violence should be lumped into the same category—that is, a case where incumbent security forces beat a few opposition supporters is categorically different from a case where incumbent security forces orchestrate the killing of 100 opposition supporters and the forced removal of many thousand more. In reflecting on this question and in consulting cases, we have isolated four distinct levels of violence, for which we code in the dataset, but in substantive terms we focus on two categories—minimal electoral violence and high electoral violence. The four levels are as follows: the first level is simply that of no reported electoral violence before or after a vote (coded as 0). A second level of violence is violent harassment, indicated by police breaking up rallies, party supporters fighting, street brawls, opposition newspapers being confiscated, and limited short-term arrests of political opponents (coded as 1). A third level of violence is violent repression, as indicated by long-term high-level arrests of party leaders, the consistent use of violent intimidation, limited use of murders and assassinations, and torture (coded as 2). And a

fourth level is a highly violent campaign, in which there are repeated, coordinated physical attacks leading to 20 or more deaths (coded as 3).

As noted, these four levels of violence can be conceptualized in turn as belonging to two distinct categories—elections in which violence plays a minimal role and elections in which violence is a central feature. It is these two categories that are the substantive focus in the paper. Corresponding to our categories above, peaceful elections or ones coded has having had harassment (levels 0 and 1) we consider to be elections with minimal violence. These are cases with some intimidation, fights, newspaper confiscation, short-term detentions, even electoral disqualifications, but where murder, assassination, or mass detentions for long periods of time do not feature. By contrast, we consider elections with repression or a campaign of violence (levels 2 and 3) to be highly violent; these would be elections with a centrally organized campaign of repression, killing, and torture with over-time consistency and multiple units of perpetrators.

Available detailed data on electoral violence across Africa is not abundant and constitutes a limitation to any dataset on the topic. To be sure, some cases are well-documented and well-reported, but many are not. In choosing a data source to code cases cross-nationally, we sought a single source that was reliable and that had comprehensive coverage for African states. One limitation of non-governmental human rights organization reporting, for example, is that documentation is uneven across African countries. The same is largely true for international newspaper coverage. In choosing a data source, we thus wanted to avoid a systematic reporting bias that stemmed from the data source's reporting patterns.

The main source on which we rely is therefore the U.S. State Department annual Human Rights reports. The State Department began issuing annual reports on all countries in 1993, and we consulted every report for a country with an election between 1993 and 2007. For the years 1990-1992, we combined three major sources, in particular the Amnesty International and Human Rights Watch annual human rights reports as well as journalism coverage in *Africa Report*. The data are not flawless, and in many cases the State Department reports are not highly detailed. However, the State Department reports, in particular, as well as the combined reports for the years in which the State Department did not issue reports provide a level of detail sufficient to code elections into the categories outlined above.

Other data that correspond to independent variable data come from a mix of sources, as discussed below. Two additional quick points: we build the dataset around that of Staffan Lindberg (2006), which records all elections and other variables of interest between 1990 and 2003 (and we thank the author for providing us with that data). We fill in by hand the years not covered in his dataset. Finally, many countries hold parliamentary and presidential elections at different points in time, and one important question is whether violence is more or less frequent in one type of election or another. However, in practice, many elections are held in close temporal proximity and distinguishing whether violence, if it exists, relates to the presidential and parliamentary elections can be difficult given the level of detail in the existing data sources. Thus, if parliamentary and presidential elections are held within three months of each other, even if in different years, we code the case as a single electoral case. This leaves us with 213 cases in the dataset.

3. General Patterns of Electoral Violence in sub-Saharan Africa, 1990-2007

How common is electoral violence in African elections? Does serious electoral violence tend to happen before or after elections and who most typically commits electoral violence—incumbents or perpetrators, or both?

At the most general level, we find that serious incidents of electoral violence (as measured by repression and campaigns of violence) occur in about 19% of all elections held in sub-Saharan African between 1990 and 2007. By contrast, less severe forms of violence in which violence is not a central feature of an electoral period—what we label harassment—occur in about 39% of cases. There is no reported electoral violence in about 42% of all cases. These figures stand in contrast to the impression that Snyder, Bates, and Collier give of democratization instigating violence in poorer, weakly institutionalized countries; the results suggest instead that there is considerable variation among African electoral campaigns.

In terms of the cases themselves, and in the interests of transparency, we list the cases of serious election violence by country and year that the election was held in Table 1. As the list indicates, there are a number of African countries where high levels of electoral violence tend to recur more than once: Kenya, Zimbabwe, South Africa (in the 1990s), Equatorial Guinea, Guinea, Togo, Nigeria, Côte d'Ivoire, and Ethiopia. One interesting aspect of this list is that many of the countries are the states that have greater comparative wealth and development, in particular Kenya, Zimbabwe, South Africa, Côte d'Ivoire, and at least arguably Nigeria. On the other hand, several of the much poorer African states such as Togo, Guinea, and Ethiopia also fall into the same category. We

examine the relationship between income levels and electoral violence in greater detail below.

Table 1: Cases of High Electoral Violence, 1990-2007

MAURITANIA	1992	NIGERIA	1992
COMOROS	1992	ANGOLA	1992
SENEGAL	1993	CAR	1992
TOGO	1993	KENYA	1992
EQLGUINEA	1993	NIGERIA	1993
TOGO	1994	REPUBLICOFCONGO	1993
IVORYCOAST	1995	GUINEA	1993
EQLGUINEA	1996	SOUTHAFRICA	1994
SUDAN	1996	KENYA	1997
NIGER	1996	LESOTHO	1998
TOGO	1998	ZIMBABWE	2000
GUINEA	1998	IVORYCOAST	2000
NIGERIA	1999	MADAGASCAR	2001
EQLGUINEA	1999	ZIMBABWE	2002
SOUTHAFRICA	1999	NIGERIA	2003
ETHIOPIA	2000	TOGO	2005
TANZANIA	2000	ETHIOPIA	2005
SUDAN	2000	NIGERIA	2007
TOGO	2003	KENYA	2007
EQLGUINEA	2004		
ZIMBABWE	2005		
BURUNDI	2005		

In terms of when the violence occurs in an electoral period, the violence is overwhelmingly taking place in the pre-vote periods. Of the 124 cases of some form of electoral violence, including harassment, either pre or post vote, 117 of them recorded pre-voting violence during the electoral campaign. In some cases, too, as we shall see, those cases also had post-vote violence, but the preponderance of violence occurred before election day. There are 41 cases of high electoral violence in the sample. Of these, 31 occurred before voting took place. By contrast, of the 124 cases of any recorded violence, 37 occurred after elections were held. Of the 41 cases of high electoral violence, 15 occurred after elections were held. As those numbers suggest, there are in

our dataset 30 cases where there was electoral violence both before and after voting day, 5 of which had high levels of violence. These results suggest two important findings that were not obvious to us before constructing the dataset: first, where electoral violence occurs in Africa, it is most often part of an electoral campaign before voting takes place; violence is thus most often an effort to shape voting patterns and preferences. Second, if electoral violence occurs after an election, such violence is more likely to have a high level than violence before an election, which suggests that the stakes and the terms of the contest change after the election is held.

In terms of who commits the violence, our results indicate that incumbents (or groups affiliated with incumbents) are the primary perpetrators. Of the 124 cases of any violence, incumbents were the primary perpetrators in 105 of the cases; by contrast, challengers were the primary perpetrators in only 18 of those cases. The ratio is similar for cases of high electoral violence: of the 41 cases, incumbents were perpetrators in 35 of them; by contrast, challengers were perpetrators in nine of the cases.

If divided by when the violence occurs, incumbents are clearly the most common perpetrators in pre-voting violence: of the 117 cases of any pre-election violence, 98 involved incumbents as perpetrators; of the 31 cases of high pre-election violence, 27 involved incumbents. For challengers, of the 117 cases of any pre-election violence, only 13 involved the opposition; of the 31 cases of high pre-election violence, challengers were perpetrators in only six. For post-election violence, the patterns are different, especially for high- level violence: of the 15 cases of high-level violence after elections, incumbents were perpetrators in 12 while challengers were perpetrators in six cases. In other words, if high-level violence occurs before the election, challengers are likely to be

perpetrators only 19% of the time, whereas if such violence occurs after the election, challengers are likely to be perpetrators 40% of the time.

The results are interesting on their face. First, incumbents are the primary perpetrators of electoral violence; they use coercive means to win contests and protect their power, and this runs contrary to some of Collier's work in which he claims challengers are the primary wielders of violence (Collier and Vicente 2008). Second, with the occurrence of high post-election violence, the chances that challengers will be involved increase when compared to the pre-election period.

To summarize, the main finding is that significant electoral violence is not as frequent as critics suggest, but frequent enough to cause serious concern for advocates. That significant violence occurs in nearly one fifth of all African electoral contests means that advocates should pay attention to the risk that democratic elections can trigger. A secondary finding, which is perhaps unsurprising but still quite important, is that incumbent forces overall commit more violence than challengers do. However, conforming to expectation, where challengers engage in violence, especially significant violence, they are more likely to do so after an election occurs and, furthermore, if electoral violence occurs after the election it is likely to be at a higher level.

4. Mechanisms and Hypotheses

The descriptive statistics in the previous section demonstrate important variation among African elections in terms of occurrence and levels of violence. A critical question thus concerns which elections are likely to experience significant violence and why. In the next section of the paper, we address the question at an aggregate level, though we remain cautious about statistical analysis where countries are the units of observation for

at least two reasons. One is that we suspect there will not be one pathway to electoral violence; two is that we do not have enough quality measures at the national aggregate level to tap into the most relevant hypothesized mechanisms. We proceed in three ways. First, we map out plausible causal mechanisms and hypotheses, given that there is fairly little explicit review and discussion of such in the relevant literature. Second, we test our observable implications with some available data using basic descriptive statistics (with the above caveat in mind). Third, we pursue a qualitative analysis of structurally similar cases that do and do not experience significant electoral violence.

What are some plausible hypotheses about why electoral violence would occur in some elections but not in others? There are surprisingly few studies of the question. On the one hand, while the literature on democratization is vast, the question has not captured significant scholarly attention. The questions that have dominated the literature concern defining and conceptualizing democracy (e.g. Collier and Levitsky 1997; Munck and Verkulien 2002); explaining divergent paths of successful and unsuccessful democratic transitions (e.g. Przeworski et al. 2000; Bunce 2003); examining and explaining democratic procedures and procedural thresholds, from elections, to voting, to constitutions, to liberal rights (e.g. Diamond 1999); managing multi-ethnic democratic contests (Reilly 2002); and, most recently, documenting a backlash against democracy promotion (Carothers 2007).

To be sure, there are exceptions. As noted earlier, Snyder, Collier, and Bates all have warned that democratization risks accelerating conflict and political disorder. Fabrice Lehoucq places violence within a broader study of clandestine electoral fraud (Fahoucq 2003). Within Africa, there are a few studies of the relationship between

violence and democratization (Basedau et al 2007; Klopp and Zuern 2007; Scarritt et al. 2001). From these, some hypotheses can be derived, as discussed below.

Another potential source of hypotheses is the study of violence, which has gained substantial traction in political science and related disciplines during the past decade. However, the focus of most research concerns the causes and dynamics of violence in civil war or, to a lesser degree, the onset and dynamics of genocide and mass killing (Fearon and Laitin 2003; Kalyvas 2007; Blattman and Miguel 2009; Straus 2007). There are a number of studies of violent riots and ethnic conflict, but democratic competition is not generally a part of the story (Horowitz 2001; Varshney 2002). Indeed, overall scholars of violence have paid comparatively little attention to the prospect and presence of violence as the product of democratic contestation. Again, there are exceptions, notably the work of Steven Wilkinson who argues that in close electoral contests local elites will use electoral violence to raise the salience of ethnicity in order to secure victory (Wilkinson 2004).

We hypothesize that there are differences between the logic of violence in wartime and violence not in war. Stathis Kalyvas argues that civil wars are defined by bifurcated sovereignty (Kalyvas 2006). Moreover, the use of force and violence by definition defines the contest between government and insurgent forces, and both government and insurgent forces have access to institutions of violence in the form of armies, weapons, and irregulars. By contrast, electoral contests do not take place generally in the context of bifurcated sovereignty, nor do they legitimately entail the deployment of force to win contests. The net implication is that we should expect that the

logic of violence to be different in electoral contests in comparison to the logic of violence in armed conflicts.

In the existing, albeit small literature on electoral violence, the principal incentives that observers highlight are strategic ones from the perspective of candidates and political party elites (Collier 2009; Klopp and Zuern 2007; Snyder 2000; Wilkinson 2004). The main mechanisms hypothesized in the literature are: 1) using violence to shape voting preferences and turnout, including appealing to exclusionary ethnic communities to win support, increasing fears of retribution for electoral support of another party, decreasing turnout through intimidation, or raising the salience of identity in order to gain electoral support (Collier 2009, Snyder 2000, Wilkinson 2004); 2) using violence to disrupt an electoral process, thereby preventing voting (Klopp and Zuern 2007); or 3) using violence to strengthen a bargaining position, vis-à-vis either inter-party or intra-party coalitions and rivalries (Klopp and Zuern 2007). To these we would add two mechanisms that do not seem to receive much attention in the literature: 4) using violence to change the demography of a constituency by driving out supporters from an opposing political party and 5) using violence to discipline would-be defectors from a winning coalition. These mechanisms are not mutually exclusive, but analysts should identify which is most operative. We hypothesize that instrumental ethnic appeals, changing demography, and derailing an electoral process are most likely to produce the greatest magnitude of violence, as each requires sustained and widespread violence in order to produce the desired result.

However, another perspective, which is not well-represented in the literature, is to focus on non-candidate, from-below incentives to commit violence. Here we hypothesize

that electoral periods provide opportunities to change existing power and resource allocations. Social actors perceive electoral periods as short windows of political indeterminacy in which to act on local grievances and to claim (or protect) resources. This is particularly true in states with weak property rights enforcement—i.e. in states with weak, corruptible institutions where control of political power is highly determinative for resources are determined. In short, local elites and their constituents would use violence in electoral periods in order to change resource distributions because such periods are short windows of indeterminacy. When electoral periods close, so too do opportunities to change resource allocations. We would hypothesize that where local grievances are particularly acute we would expect electoral periods to bring those grievances to the fore and to raise tensions between those who control and those who do not control local scarce resources.

The reflection on causal mechanisms indicates a theoretical puzzle that is consistent with the empirical variation demonstrated in the previous section: if electoral violence makes sense, why is it not more common? Collier's analysis is that electoral periods engender races to the bottom, in effect, and that violence is the strategy of choice when bribery fails (Collier 2009). However, we hypothesize that electoral violence is more likely to occur when there are joint incentives, both from the candidate's electoral strategic perspective and from the perspective of influential local actors who would mobilize to change resource distributions in electoral periods.

From these general observations, and from the broader existing literature, we derive some testable hypotheses.

First, all other things being equal, the predicted margin of victory should influence the use of violence and choice of strategy. Close or competitive elections should induce political actors to use violence to shape voting preferences or to change constituencies (redistricting by other means), either locally or nationally. By contrast, if there is a predicted large margin of victory, the predicted winner would have no need to employ violence; that said, the predicted loser would have an incentive to disrupt the electoral process using violence (or after if a defeat is evident).

Second, from a non-candidate perspective, the greater the fluidity in a regime's "rules of the game," the greater is the uncertainty about future elections and therefore the shorter the window of opportunity to use coercive means to change resource allocations. How settled are the "rules of the game" is difficult to measure, but two ideas come to mind. One is that in consolidated democracies uncertainty declines; there is predictability about the nature of a regime's rules. Thus, we would predict that electoral violence would be less prevalent in highly democratic regimes. Two is that, following Lindberg, repeat elections at the democratic end of a spectrum would also have a low probability of electoral violence. In particular, violence would be less likely if candidates and party leaders share an expectation of future free elections—and hence another opportunity to change resource and power allocations—judged by evaluating past electoral quality.

Third, cleavage structures are likely to play an important role in strategies for violence. We would expect that greater internal or international migration increases local grievances and creates opportunities for candidates or non-candidates to make indigenous claims. Indeed, electoral appeals frequently induce "sons of the soil" arguments (Bates 2008), as politicians appeal to voters in their home districts. In addition, we see another

pathway for how cleavage structure might trigger violence. Following the work of Dan Posner (2005), we would expect that violence would be more likely when competing ethnic groups have a chance of forming the core of a winning coalition, i.e. where two or more ethnic groups have large enough constituencies to form the core of a winning coalition. In mainland Tanzania, the Democratic Republic of Congo, or Chad, with many small groups, we would expect the cleavage structure to discourage ethnic appeals and thus ethnic-based electoral violence to be less likely. In places with overwhelming ethnic majorities, such as Rwanda, Burundi, and Botswana, we would similarly expect that ethnic appeals would be unnecessary—unless the party representing the ethnic majority was out of power or highly fractionalized (as in Burundi in the 2000s and Rwanda before 1994).

Fourth, we hypothesize that electoral violence will be tied to income level. One hypothesis is, following Collier, that greater poverty leads to greater violence. Another hypothesis is that violence may also be likely in comparatively wealthier countries—in these cases, political elites may not wish to take countries to war, thereby risking economic revenue streams, but still may wish to use violence to win elections or to change resource distributions—in contrast to poor countries where the constraints against taking a country to war are fewer. A third political economy hypothesis is that economic decline would increase the incentives for violence, as resources are scarcer and the economic future appears more uncertain.

Fifth, we hypothesize that violence may be a political "repertoire"—that is, political elites will use violence in elections if violence has been one way that they have obtained or protected power in the past. Violence is a legacy or extension of a pathway to

power, and where incumbents in particular took power through a coup or military victory, electoral violence would be more likely.

5. Hypothesis Testing

Not all of these hypotheses are testable with existing data, in particular at the cross-national level, but in this section we do some preliminary hypothesis testing with available data.

For predicted margin of victory, we look first at the margin of victory between the first and second largest vote-winners in elections in the first round (if there are two rounds). Margins of victory are not foolproof measures of anticipated electoral uncertainty, as violence, intimidation, disqualifications, boycotts, and rigging all shape voting patterns and will skew a margin in what political elites otherwise would have expected to be a close election. Nonetheless, we examine the relationship, and we find a somewhat weak result in the expected direction, as Table 2 shows.

Table 2: Electoral Violence and Margin of Victory

	20-Point Spread	20-40 Point Spread	40-60 Point Spread	60-80 Point Spread	80+ Spread
No/Low Violence	52 (80%)	37 (71%)	31 (86%)	22 (92%)	28 (85%)
High Violence	13 (20%)	15 (29%)	5 (14%)	2 (8%)	5 (15%)

These results do not clearly indicate that vote closeness per se is correlated to high levels of electoral violence, but still 70% of high electoral violence cases happen if there was a 40% or less margin of victory. More work on the relationship is needed, in particular we would like to control for cases where the opposition boycotts the election and where

election returns are fraudulent, and to disaggregate by whether winners or losers initiated violence with large or small spreads.

For testing the "settledness" of the rules of the game, we examine the relationship between regime type and electoral violence, and again the results are weakly confirmatory. We use the terminology as well as the data measuring regime type from Polity, where a regime is classified as authoritarian if its Polity IV score is -6 or lower, anocratic if its score is between -5 and +5, and democratic if its score is +6 or greater. If an election is held in an authoritian or anocratic state, high levels of electoral violence are roughly equally common in percentage terms, and 70% of high violence cases occur in anocratic states, as Table 3 shows. There is a drop-off in violence among cases coded as democratic. Yet even in democracies we find examples of high violence—South Africa in 1994 and 1999, as well as Kenya in 2007 being the principal cases.

Table 3: Electoral Violence and Regime Type

	Authoritarian	Anocratic	Democratic
No/Low Violence	18 (72%)	81 (74%)	54 (92%)
High Violence	7 (28%)	29 (26%)	5 (8%)

The results are again weak support for the hypothesis presented earlier. Going forward we will examine change in regime type over time to measure the "settledness" of a regime. We also recognize that the presence of electoral violence will shape how Polity codes a country's level of democratic quality.

For cleavage structure, we do not have data on migration, but to measure cleave structure we examine the Politically Relevant Ethnic Groups (PREG) index developed by Posner (2004). The advantage of the PREG index over other fractionalization indexes is

the explicit focus on "politically-relevant" groups as opposed to all cultural or linguistic groups. As Table 4 indicates, there is not a clear correlation between PREG and electoral violence. That said, the category that has the greatest probability of high levels of electoral violence are those between 0.41 and 0.6.

Table 4: PREG Score and Electoral Violence

	0.00-0.20	0.21-0.40	0.41-0.60	0.61-0.80
No/Low Violence	48 (86%)	28 (80%)	34 (65%)	24 (80%)
High Violence	8 (14%)	7 (20%)	18 (35%)	6 (20%)

Interpreting the result is a little problematic given how the index calculates ratios using the Herfindahl concentration formulation, and attempting to capture the dynamic processes involved in ethnic competition with a single value is problematic on a number of counts. Nonetheless, the result offers some weak support to the claim that situations where there is neither an overwhelmingly dominant group or many small groups, but rather where more than one group can plausibly claim to form the core of a winning coalition, are more likely to produce electoral violence.

With regard to income, we divided countries into three categories: where per capita GDP translates into a dollar a day, where per capita GDP translates into one to two dollars a day, and where per capita GDP translates into more than two dollars a day. We recognize that there are other ways to divide comparative income levels, but such is our first-cut, and it reveals that middle-category states are most susceptible to electoral violence:

Table 5: Income and Electoral Violence

	<$1 per day GDP/Cap	$1-$2 per day GDP/Cap	>$2 per day GDP/Cap

No/Low Violence	85 (85%)	36 (64%)	42 (87.5%)
High Violence	15 (15%)	20 (36%)	6 (12.5%)

This is a somewhat stronger finding that we will test in other ways and explore theoretically. It challenges the theory that holding elections in poorer countries causes greater violence and suggests that electoral violence (but not war) is more likely where there is a greater amount of wealth.

With regard to economic growth and decline, we look at growth rates in the year before the election took place. Here the data again do not produce a very strong result. On average, lower growth rates have a greater likelihood of high electoral violence, but the relationship is not a strong one.

Table 6: Electoral Violence and Economic Growth of Previous Year (% Annual GDP per capita)

	<-10%	-5%--10%	0%--4%	1%-5%	6%-10%	>10%
No/Low Violence	1 (50%)	10 (77%)	44 (71%)	84 (88%)	18 (82%)	3 (50%)
High Violence	1 (50%)	3 (23%)	18 (29%)	12 (13%)	4 (18%)	3 (50%)

Average growth in the year preceding the election for High Violence cases: 0.83%
Average growth in the year preceding the election for No/Low Violence cases: 1.71%

With regard to pathway to power, we test this in two ways. First, we generally find that countries holding their first elections after a civil war are least likely to experience high levels of electoral violence. Of the 163 cases where states were neither in civil war nor held a first election immediately after a civil war termination (which we determine using the Uppsala Armed Conflict Database), 33 (20%) experienced high levels of violence; of the 28 cases of first-elections after civil war, three (10%) experienced high levels of electoral violence; and of the 22 that held elections during

civil war, five (23%) experienced high levels of electoral violence. Thus, the probability of high electoral violence is similar for states in or outside civil war, but declines for states that hold their first elections after a civil war termination. This is a finding worth exploring. Second, we find a similar difference, between those countries where the incumbent candidate or regime came to power via a coup or military victory and those countries in which the incumbent did not. Of the 100 cases where incumbents came to power through military victory or coup, 24 experienced high levels of electoral violence (24%). By contrast, of the 113 cases where incumbents did not come to power in that way, 17 experienced high levels of electoral violence (15%). This suggests that countries in which the incumbent came to power via a coup or military victory may be somewhat more prone to electoral violence.

One final statistical point, which is that we examine whether more political violence is reported during years in which elections are held as compared to years in which elections are not held. Such a comparison is necessary to parse out how much reported political violence is in fact due to impending or past elections. Using data from the Political Instability Task Force (PITF) Worldwide Atrocities Event database, which records instances of political violence resulting in five or more non-combatant deaths, we examine the log of the total number of deaths from recorded events for non-election and election years for African countries between 1995 and 2005. The time frame for the data is somewhat curtailed and the coding scheme used by the PITF excludes a number of low-intensity violent events, but the analysis suggests that elections do not have a dramatic effect on the intensity of political violence in Africa. Indeed, the mean of the log of annual deaths for non-election years is 4.93, while the mean for election years is 3.93;

non-election years seem to be somewhat more violent than election years. Thus, although countries such as Zimbabwe and Kenya have experienced a dramatic surge of political violence related to elections in recent years, our data show that when looking at the universe of violence in Africa, elections are not the main source of violence; violence is likely to be as prevalence and of greater magnitude in non-election years.

To conclude this section, the cross-national descriptive statistics do not suggest any robust causal pattern, but they suggest that, in general, closer elections in countries that authoritarian or anocratic, that are poor but not extremely poor with low growth rates, with mid-levels of fractionalization, that are not holding the first elections after a civil war termination, and where the incumbent came to power by violence are the most likely to experience high levels of electoral violence. However, as noted, we recognize that the measures remain at a very macro level to gain greater analytical leverage, we turn to a qualitative discussion and comparison of different cases.

6. Qualitative Comparisons

One reason that the cross-national statistical results do not reveal clear causal patterns is equifinality: there may be different pathways to the same outcome of high electoral violence. Thus, rather than seeking to explain all the instances of electoral violence, we sought, through qualitative case examination, consistent patterns in cases of high electoral violence. In our examination, we yielded one surprising result, which is that in many cases where electoral violence occurred there is a consistent pattern of conflict over local resources, in particular indigenous claims to land or access to markets, connected to patterns of migration. In comparing cases where electoral violence did and did not occur, the key difference appears to be whether and how local conflicts over

resources and indigenous claims to those resources map onto national electoral contests. Where local resource grievances play out in constituencies or speak to ethnic audiences that are essential for a party to put together a winning coalition, there we find high electoral violence to be most likely.

We proceed with some fairly short comparative illustrations. Our comparative parameters are that we choose among cases that generally fit our profile of close elections, in countries with mid-level fractionalization and with GDP per capita of roughly between $1-$2. The central cases that fall into this profile and that we examine in greater detail are Kenya (generally high-violence), Côte d'Ivoire (high violence), Zimbabwe (high violence), and Ghana (low violence). We also treat Kenya in 2002 as a case of low violence. We oversample on the high-violence cases because we are developing a hypothesis, though in future iterations we plan to explore low-violence cases in greater detail.

Since Kenya returned to multi-party politics in 1992, the country has experienced four separate elections (1992, 1997, 2002, 2007).[5] Of these, only the 2002 polls did not experience significant electoral violence. In 1992, there was significant violence between Kalenjins and Kikuyu in the Rift Valley Province both before and after the election. In 1997, in addition to state repression against pro-democracy protesters in the capital Nairobi, there was significant violence in the Coast Province by coastal populations against "uplanders," who were generally Kikuyu and Luo. And in 2007, the ruling party was widely perceived to rig results, leading to opposition protest, followed by state repression, followed by attacks against Kikuyus in the Rift Valley and attacks by Kikuyus

[5] The Kenya case is based upon the author's (Straus) fieldwork in the country, CIPEV 2008, ICG 2008, Klopp and Zuern 2007, Mueller 2008.

in various provinces. The key difference in 2002 is that the then-incumbent, Daniel Arap Moi, was not running for office, and the two leading opposition candidates were both from the same ethnic group (the Kikuyu).

In Kenya, the most consistent electoral violence in Kenya is against the Kikuyu, who are the largest ethnic group in the country at roughly 30% of the Kenyan population. In general terms, non-Kikuyu perceive the Kikuyu to have benefited considerably from favoritism under Kenya's first President, a Kikuyu (Jomo Kenyatta). In particular, well-connected Kikuyu were awarded large tracts of land that had belonged to British settlers or had been state territory. This was particularly true in the Rift Valley, where Kalenjin groups dominated. Kenya's primary source of hard currency is cash crops, followed by tourism—in other words land (both from agriculture but also from access to grazing rights) is a critical resource in the country. Relatedly, starting under Kenyatta Kikuyus migrated to other economically-productive areas as merchants and businessmen, including the Coastal Province where the tourism industry is very active. In both 1992 and 1997, the then-ruling KANU party needed to win both the Coastal and Rift Valleys in order to win the presidency, which it did.

How do we make sense of the Kenyan electoral violence? In the three episodes where electoral violence took place, the electoral periods seem to have opened opportunities for local grievances to come to the political forefront. In each case where violence was most intense, indigenous groups that perceived themselves to be disenfranchised by a migrant group (Coastal groups and Kalenjin vs. Kikuyus) used to the electoral periods to make indigenous claims to key resources, principally land in the Rift Valley. In all cases, the local indigenous grievances mapped onto the national

electoral contest—the ruling party in each case needed the electoral support from the Coastal and Kalenjin groups, respectively, to carry the general election. By contrast in 2002, where both candidates were Kikuyu, the electoral period did not provide an opportunity for indigenous non-Kikuyu groups to raise the prospect of redistributing vital resources.

In Côte d'Ivoire, the basic dynamic is similar, although how the electoral violence manifested is.[6] In 1995, the principal violence was in Western areas where indigenous, mainly Beté and Guere groups, attacked internal migrants, who were mostly Baoulé. In addition, indigenous groups attacked internal Muslim migrants from Northern Côte d'Ivoire and immigrants from Burkina Faso and Mali, principally. In 2000, the situation was different. There an election was held amidst the disqualification of the main Northern candidate, Alassane Ouatarra. An election was held between a coup leader, General Guei, and a longtime opposition candidate, Laurent Gbagbo. The standard account is that Guei rigged the elections, leading to street protests by Ouattara and Gbagbo supporters. Guei later fled, Gbagbo was declared the winner, leading in turn to protests from Ouattara supporters and then violence repression by state security forces against his supporters. No election has been held since then, as civil war broke out in Côte d'Ivoire in 2002.

The 1995 election is the more typical story of electoral violence. The main product in the country is cacao and coffee exports. During the first presidency, encouraged by President Felix Houphouet-Boigny (a Baoule), large numbers of migrants moved into the Western part of the country, which was rich but uncultivated forestland. There they obtained large tracts of land from indigenous groups, and in turn created very

[6] The Ivoirian case discussion is based upon the author's (Straus) fieldwork, as well as Akindes 2004; Chauveau 2006; Dozon 1997.

large and lucrative plantations. Later immigrants from West Africa came as well. In the late 1990s, Côte d'Ivoire was fresh from a period of economic decline and stagnation, and the election period created an opportunity for indigenous groups (in particular unemployed adult children of parents who had given away land) to make claims on their land. They sought to recapture the land that they felt was rightfully theirs. The violence came as those actors were backed by local politicians in the opposition (who were from that area) and as the state sided with the migrants, given that the ruling party's coalition was based on support from Baoule and Northerners.

A similar dynamic is apparent in Zimbabwe.[7] Significant violence has marked the last three elections held in Zimbabwe—in 2000, 2005, and 2008. The general dynamic is that militia groups, party members, and security force personnel who support the ruling ZANU-PF political party attack supporters of the opposition, MDC supporters. In several instances, especially in the most recent elections, the violence has been especially brutal, with agents of the state murdering, torturing, and displacing large numbers of opposition supporters. In the Zimbabwe elections, in particular the most recent ones, the ruling party faced stiff competition from the MDC. Most recently, the MDC won the first round of presidential voting, as well as a majority in parliament. Before the second round, state agents attacked MDC supporters in districts that ZANU-PF could win and where the MDC had made electoral gains before the MDC candidate pulled out of the contest.

The political parties in Zimbabwe are not principally ethnic parties, with each party drawing support from the two largest ethnic groups (the Shona and Ndebele). However, a key underlying dynamic of the violence in Zimbabwe concerns land, in

[7] The Zimbabwe case is based upon HRW 2008, Kriger 2009, ZHRNF 2008, and African Confidential Reports 49/10; 49/12; 49/14.

particular land that "white farmers" controlled even after the end of white rule in Zimbabwe. In each of the elections, President Robert Mugabe has instrumentalized local land grievances, promising to and then seizing white farmer land to redistribute to ruling party supporters and powerful elites in his coalition. Thus, again we have a situation where the incumbent faced a close election, where there were high local land grievances, where the elections ignited claims to indigeneity, and where the local resource conflict was important to the national political game. In other words, those who made local land claims represented a constituency from which the ruling ZANU-PF needed electoral support to carry the elections.

Although the national-level characteristics vary, one can see similar dynamics in other major high-electoral violence episodes, notably in Zanzibar, South Africa, and Nigeria. In Zanzibar, the ruling CCM party faces strong competition from the CUF. The latter represents—or is said to represent—Arab interests, in particular of those whose land was confiscated in the Afro-Shirazi revolution in 1964. Electoral periods raise the prospect of land redistribution, and CCM officials and their supporters use violence to ensure that CUF does not win and land is not redistributed. In South Africa, there is equally a history of land inequality and the prospect of indigenous claims to redistribute land. That is one way to interpret the 1994 violence, in which those associated with the apartheid regime funded a "third force" to ignite inter-ethnic violence and to derail the elections. In 1999, the violence was principally between Zulus and labor migrants to the predominantly Zulu region of Kwa-Zulu Natal. Finally, in Nigeria, when electoral violence has taken place, it has often taken place in Jos and other northern areas, where again the conflict is between indigenous groups and settlers (Ostien 2009).

Ghana is the main case of low electoral violence examined in the paper. To be sure, electoral violence is not absent from Ghanaian politics; there have been episodes of violent local disputes in electoral contests since the democratic reforms in the 1990s, in particular in Northeast areas between the Kusasis and Mamprusis in Bawku district. Moreover, the nature of the disputes resembles the local resource grievances seen in the other cases, whereby the two groups make rival claims about who is native and therefore who has indigenous rights to land and chieftaincy succession (Lund 2003). In several recent elections the rivalries turned violent, in particular in 2000 after the incumbent National Democratic Congress (NDC) reportedly claimed that a victory for the opposition National Patriotic Party (NPP) would lead to recognition of a different chief (Kelly 2007). Thus, the structure of elections creating windows of opportunity to act on local land grievances is similar to other countries that have experienced significant electoral violence.

However, electoral violence in Ghana has remained low-intensity relative to the other cases. It has tended to be limited to the poorer and more resource-scarce northern regions and has not destabilized the electoral process at the national level. This outcome is surprising in part because Ghana exhibits some of the characteristics of cases of high violence—namely, close elections, mid-fractionalization levels, and above average wealth, even if average growth tends to be higher than in other locations. Hence the question: why has Ghana not experienced the kind of electoral violence witnessed in Kenya, Côte d'Ivoire, Zimbabwe, Zanzibar, Nigeria, and South Africa?

Our hypothesis is the following, subject to further research: in Ghana, the local land and chieftaincy grievances do not have national electoral implications. The groups in

question are small and local enough (combine roughly 2.7% of the total Ghanaian population) that neither of the major political parties, whose ethnic cores lie elsewhere, has an electoral interest in stoking or encouraging significant violence. The violence would not play to the larger key constituencies and would open the parties up to criticism from the other party. Moreover, the groups are small and balanced enough that overt backing of violence would not create a substantial electoral advantage for one party over another. This is not to say that national political parties do not need to attract the votes of northern ethnic groups during elections; they do, as neither party's core ethnic following is large enough to win an election on its own. But there is a limit to how far the parties will go in backing the local groups, and the two main parties generally seek to contain, rather than support, violence.

There are two other differences between Ghana and the other cases. First, the two dominant ethnic groups that the two major political parties are perceived to represent (Ewe:NDC and Akan:NPP) do not have a history of conflict over local resources such as land between them. Ewe are not migrating in large numbers into Akan regions and controlling key economic resources, and vice-versa. This is significantly different from Kenya and Côte d'Ivoire, where migration entwines the electorally most important ethnic groups. Second, the local claims to indigeneity in Ghana pertain to pre-colonial population movements; moreover, neither group has a clear homeland outside the region where they are voting.

To summarize, there is a consistent pattern in the high electoral violence cases of violence between groups for control over access to vital local resources, with at least one group claiming indigeneity. Moreover, where the local grievances map onto, and are key

to the winning electoral strategy of national political parties, there is a greater prospect for electoral violence. In the cases without high levels of electoral violence, elections still trigger bitter claims to redistribute local resources, but such claims do not have major national electoral implications—they involve small groups who are not major players on the national political stage. In short, we find evidence to support the hypothesis that electoral violence is more likely to occur when there are joint incentives, both from the candidate's electoral strategic perspective and from the perspective of influential local actors who would mobilize to change resource distributions in electoral periods.

7. Conclusions

In this paper, we have focused on two issues related to an important theoretical and policy problem. The first concern was to conceptualize, identify, and document the frequency of electoral violence in Africa using an original dataset. The findings presented here represented a first look at an original dataset; several findings were not obvious *a priori*. First, significant electoral violence in Africa since 1990 is not nearly as common as critics suggest but common enough to be a real cause for concern. Second, most significant electoral violence tends to take place prior to elections, with incumbents tending to be the primary perpetrators of the violence. Third, when violence occurs after elections, the violence tends to be at a higher level than in the pre-election period, and is more likely to involve the opposition as a primary perpetrator. These findings suggest that multiple logics of electoral violence are present across the continent.

The second concern in the paper is to focus on the variation and to ask why and how electoral violence occurs in some locations but not in others in Africa. In the paper, we explore a number of causal mechanisms that would link electoral contests to violence,

and we put forward several plausible hypotheses. While the available quantitative data with which to test the hypotheses are limited and general, we nonetheless proceed to examine correlations between country characteristics and electoral violence. While not an overwhelming pattern, the data indicate that high electoral violence is most likely to occur in countries with some combination of close elections, authoritarian or anocratic regime characteristics, mid-levels of fractionalization, low growth rates, and per capita income of $1-$2 a day.

We recognize the limits of the quantitative analysis and turn to qualitative comparisons to identify causal patterns and to explain at least a chunk of the electoral violence. The main conclusion is that electoral violence typically involves grievances over vital local resources, often land but also access to marketplace and trading rights, in places where there is strong internal migration and where the local conflict maps onto national political contests. That is, there need to be joint incentives—between local resources grievance and national electoral strategies—for significant violence to occur.

More generally, we conclude that elections create windows of opportunities to change the distribution of power and resources at both the national and local levels. All other things being equal, when the stakes are high at the local level—such as access to land or marketplace rights in places where land and marketplace are key sources of wealth—the prospect of using violence is greater. When these local incentives can be matched to electoral strategic incentives to win elections, the probability of using violence is even greater. We further hypothesize that the ability to change a pre-existing distribution is greater in highly patrimonial states with weak property rights enforcement, where state control strongly influences the capture of resources.

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