POPULAR PARTICIPATION AND POLITICAL VIOLENCE

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ABSTRACT

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The first essay "Protest: Onset and (De)Escalation" argues that the literature on Civil Conflict has reached a point of maturity in identifying the correlates of conflict; however, the risk factors for conflict are far more common than conflict itself. Even when underlying conditions appear similar, as for the countries impacted by the Arab Spring, diverse outcomes can arise. By modeling the escalation process and incorporating protest as a signal to the government and potential dissidents in society, this paper shows how similar starting conditions can lead to protest, government concessions, or even civil war. This paper also contributes to understanding the relationship between repression and dissent. We argue that repression may reduce overall dissent, but cause dissent that occurs to become more violent. Finally we examine some predictions of the model; this is done by complementing traditional conflict data from Uppsala Conflict Data Program (UCDP) with data on protest from the Social Conflict in Africa Database (SCAD). The analysis finds support for two key predictions in the model: the likelihood of a concession increases with protest size, and a non-monotonic relationship between protest size and the probability of escalation to conflict.

The second essay "Electoral Violence: An Empirical Examination of Existing Theories" argues that recent studies of election violence have found that violence mars as many as 80 percent of African elections. However, the ways in which violence is used to influence elections are still unclear. Two theoretical frameworks have been suggested. The first argues that violence is targeted directly at core opposition supporters in an attempt to prevent them from voting. The second is more nuanced and argues that it is more feasible to deter unaligned voters with untargeted violence because they are less committed to vote for any particular party. A party could increase their vote share by excluding unaligned voters if they have a stronger advantage in core supporters than unaligned voters, because excluding unaligned voters places more weight on each party's core sup-

porters. By combining survey data from the Round 4 Afrobarometer survey with event data from the Social Conflict in Africa Database, we compare the validity of these theories. First, we confirm that violence is associated with a reduced likelihood of voting. More importantly, we find important heterogeneity in this association. Supporting the first framework, voters with a strong political affiliation do cease voting if they personally fear violence. In support of the second framework, we find that unaligned voters are the only group significantly less likely to vote in the presence of violence, even without reporting a greater fear of violence. We conclude that both targeted and untargeted violence are potentially effective strategies, but untargeted violence appears to be more common.

Finally, the third essay "Profiling in Violent Elections" argues that recent theoretical and empirical research on election violence has presented several potential ways in which violence may be used to influence the electoral process. A key differentiation between emerging theories, as highlighted in Wallsworth (2016), is whether violence is targeted directly at opposition supporters or indirectly at unaligned voters more likely to vote for the opposition. Wallsworth (2016) demonstrated that reactions to violence are consistent with both strategies. Targeted violence is associated with a lower likelihood of voting, and unaligned voters were the only group to react to indirectly targeted violence. One way to distinguish which theory is more viable in a given country is to unravel how successfully a potential perpetrator of violence could profile the opposition. This paper examines the viability of profiling, which characteristics may be used to profile, who appears to be targeted by violence, and how characteristics which correlate with an individual's political affiliation also correlate with their fear of violence.

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Protest: Onset and (De)Escalation

1.1 Introduction

Civil war is a relatively rare phenomenon, though the risk factors for war are not. As stated in Walter (2009), "existing studies cannot explain variation in the outbreak of violence across countries that are at similar risk of civil war." She continues to argue that, "By viewing the decision to fight as part of a larger bargaining process and not as an isolated event...scholars can better explain why violence is more likely in some countries" (p. 244).

The events in the Arab Spring demonstrate the accuracy of Walter's statements and serve to motivate this paper. Protests occurred, to some degree, in more than a dozen countries across Northern Africa and the Middle East. Existing macroeconomic conditions were similar in many of these countries, as was the way in which the mass movements against their ruling regimes started. However, the sequence of events following the protests varied, as did the expectations these regimes had established for how they would respond to protest. In the end, some regimes stayed in place with little change, some toppled peacefully, others violently, and some still remain embroiled in civil war. This paper provides a potential explanation for how dramatically divergent results can arise from similar initial conditions, like those following the initial wave of protest in the Arab Spring.

By incorporating protest into a model of conflict we can address Walter's comments directly. With protest as an option, used to signal the strength of potential rebel groups to the government, conflict occurs only when the signal conveys inaccurate information. This set-up leads to novel conclusions. As with many models, dissent is driven by economic conditions. However, we find the form it takes is driven by expected strategic interactions, specifically expectations of repres-

sion, and non-economic determinants of the viability of conflict. This implies that similar macroeconomic conditions could lead to different outcomes. Additionally, modeling repression as a cost to protest allows us to examine when it is an effective tool to quell dissent. However, we argue that governments may not be capable of quickly lowering the expected level of repression, which can lead to conflict even when both parties would prefer to avoid it; because, too high an expected level of repression can cause violent dissent to occur. We also find a counterintuitive, non-monotonic relationship between protest and escalation to conflict, where the likelihood of conflict initially increases in protest size, then drops to zero once protest size surpasses a threshold.

The world depicted in the following model is one where two groups in society, the government and the rebels, must agree to split available resources. At the beginning of the interaction, policy determines the allocation of resources to each group. After seeing the allocation the rebels decide to use dissent, and whether or not that dissent will be violent. Protest is used by dissidents to learn about their own strength, while simultaneously sending an imperfect signal of that strength to the government. It is chosen when the cost, or the expected level of repression, is relatively low. On the other hand, if the rebels are fairly confident of their own strength and the expected level of repression is relatively high, they may choose to use violent dissent immediately.

After protest, the rebels learn their type precisely, whereas the government only observes a noisy signal, e.g. protest, which is likely to be larger if the rebels are strong. After observing the signal, the government is then able to adjust the policy in an attempt to appease the rebel group, or quell the rebellion.

Various conditions in the model can predict peace, immediate conflict, or protest that either escalates into conflict or results in a peaceful bargain. Immediate conflict occurs in response to governments that are expected to vigorously suppress protest, making the cost of protest too high for rebel groups to use. Protest occurs whenever the exogenously set policy is unfavorable to the rebels and the threat of repression is low. Following protest the interaction ends or escalation occurs. Escalation to conflict occurs after protest only if the government makes too small a concession to a rebel group that was in fact strong. Peace may follow protest in one of two ways: a

successful protest, where the protests themselves garner major concessions, or a failed protest by a weak rebel group, where the rebels rationally accept minor concessions.

Previous research has documented a common pattern between violent and non-violent dissent, periods of violent dissent are often preceded by periods of non-violent dissent (Gurr, 2000). To examine our model's predictions describing the relationship between violent and non-violent dissent, we combine two datasets with complementary measures of political dissent. The first is the Peace Research Institute at Oslo's (PRIO) Armed Conflict Dataset (ACD), which we use to measure civil conflict; the second is the Social Conflict in Africa Database (SCAD), from which we derive measures of protest. These data cover 42 African countries from 1990-2012.

These data provide a unique opportunity to examine the full spectrum of political dissent, and the determinants of escalation. At first glance the data show that non-violent dissent is a predictor of violent dissent. The main component of the analysis which follows is exploring the conditions that make protest more likely to de-escalate, escalate, or garner concessions. We find support for our main hypotheses. First, we observe a non-monotonic relationship between protest size and conflict escalation, where the largest protests observed in the data do not escalate. Second, we find the predicted linear relationship between protest size, and the probability of a concession, measured as an increases in civil liberties or political rights using Freedom House data.

Finally, we expand the model and allow the government to set the level of repression strategically. We argue that the baseline model, with repression taken as exogenous, may be appropriate if the government has a long history of being highly repressive, or cannot credibly commit to lower levels of repression. Endogenizing repression, under the assumption the government cannot commit, leads to a similar set of results, and we find that the cost associated with conflict becomes the main determinant of the type of dissent (protest or conflict) the rebel group will use in the first period.

1.2 Literature Review

There is a vast literature, both theoretical and empirical, examining the causes of civil conflict. Underlying much of this literature is the assumption that conflict is rational, a concept formalized by Fearon (1995). The binding principle among rational conflict models is that some agreement exists that all sides would prefer to avoid conflict; however, some inefficiency prevents an agreement from being reached. In his seminal paper, Fearon (1995) lays out three possible causes of rational conflict: information asymmetries with an incentive to misrepresent, commitment problems, and issue indivisibilities.

Several recent studies, including (Blattman and Miguel, 2010; Walter, 2009), ask why more implications of rational conflict models have not been tested. The basic premise of these arguments is summarized well by Walter (2009):

> Most studies of civil war have focused on the underlying structural conditions that encourage groups to go to war rather than on the bargaining problems that may stand in the way of settlement... By viewing the decision to fight as part of a larger bargaining process and not as a single isolated event, scholars can better explain why bargains are so rare in civil wars and why violence is more likely in some countries than others. (p. 244)

Using protest as a signal, of rebel strength, in a rational model of conflict allows us to derive and test hypotheses related to the aforementioned bargaining problems, as opposed to examining only the structural conditions which lead to conflict. We argue that the basic conditions that have been found to precipitate conflict, such as poor economic conditions or existing political instability do induce dissent Miguel et al. (2004), Hegre and Sambanis (2006). However, the form of dissent depends on the outcome of actions taken by potential dissidents and the reputation established by the government for reacting to non-violent dissent, specifically the repression of protest.

The theoretical literature on conflict and protest both have a diverse selection of models using asymmetric information as the primary friction driving conflict (Powell, 2002). Models like Chas-

sang and Padro-i Miquel (2009) use inaccurate information on the state of the world to generate conflict, others such as Wittman (1979) or more recently, Baliga and Sjöström (2004), use private information on the military capabilities of each side; our model makes a similar assumption. The focus in this paper, as opposed to much of the literature, is not in examining the conditions under which asymmetric information does or does not lead to conflict, but rather on examining if protest is potentially one way to convey this private information. As pointed out by Fearon (1995), asymmetric information alone is not enough to generate conflict; it requires an incentive to missrepresent that information or an inability to convey it accurately. We expand on this literature by investigating if protest way be one way to convey that assymetric information in an attempt to avoid conflict.

Many models also exist which use protest as a signal in a global games setting (Carlsson and Van Damme, 1993; Morris and Shin, 2001). This paper deviates from the traditional set-up of models like Melosh (2012) to focus on the escalation process and the government's response rather than trying to explain how movements overcome problems of collective action. The canonical framework for such a model uses a threshold value for the size of the protest, Granovetter (1978), and generally argues that if such a value is exceeded, the protest movement will grow massive and succeed in obtaining its demands. We agree with the basic intuition behind threshold models: large enough protests do succeed in convincing governments to grant major concessions. However, our model makes a major departure when examining what happens as the protest size increases towards this threshold. We predict that it is in these circumstances, when it is most likely that the government has misidentified a strong rebel group, that conflict is most likely to ensue.

Empirically, this paper contributes to a small but growing literature attempting to view conflict on a greater continuum. Early work such as Miguel et al. (2004) acknowledged measuring only the occurence as a weakness related to the available data. Other recent work such as Chaudoin et al. (2013) and Besley and Persson (2009) have leveraged improvements in available data. Our contribution runs closest to Besley and Persson (2009) in this regard: rather than trying to more accurately predict the breadth or intensity of conflict, we are most interested in examining the

determinants of different types of dissent. Specifically, we examine when non-violent dissent is most likely to escalate to violence.

An additional contribution of the model in this paper is an attempt to bring together literatures on protest, repression and dissent, and conflict. By doing this we are able to provide a possible explanation for Davenport's 'punishment puzzle' (Davenport, 2007): the idea that dissent is almost always positively correlated with repression; however, the impact of repression on dissent is highly inconsistent. In particular we provide an answer to one question he highlights: "Under what circumstances can authorities reduce dissent?" Our model argues that depending on the state of the world, repression can have no impact on dissent, eliminate dissent completely, or induce a change in the type of dissent rebel groups will use. We provide potentially testable implications that may help to explain otherwise inconsistent results in the empirical literature examining the impact of repression on dissent.

This paper makes some key departures from the theoretical literature on repression and dissent. By returning to an earlier viewpoint, where repression was modeled as increasing the cost of acts viewed as threatening to the power of the state, (Goldstein, 2001). However, we refine this by arguing that repression is effective only against non-violent dissent. If a group decides to use violence against the government, they have already acknowledged that it becomes acceptable for the government to respond in kind.

Similar to Pierskalla (2010), we argue repression is able to quell protest. However, we are able to generate any of the model's paths: conflict, peace, or protest, as equilibrium outcomes without needing to introduce a third party. The key departure from previous work for this paper and in Pierskalla (2010), is incorporating this cost into a strategic interaction where both actorsŠ, the rebels and the government, decision processes are explicitly modeled. Previous models such as Lichbach (1987) focused on the type of dissent used by rebels, whereas other models such as Moore (2000) focused on the governmentŠs decision on how to use repression. The decision process in both of those models was driven by a cost benefit analysis of the choices faced by the model's agents. Take for example Moore (2000), the model's dissidents use whichever type of dissent is

most cost-effective to get the government to alter their policies; however, the government's decision is not modeled. By incorporating both decisions, we can see when and why protest is preferred to conflict.

A recent trend in the empirical literature on dissent has been comparing violent and non-violent dissent (Chenoweth and Cunningham, 2013), this paper also contributes to this literature by distinguishing between the types of dissent used by potential rebel groups Cunningham (2013). Much of the recent work in the literature comparing violent and non-violent dissent has focused on the complementarity of different types of political dissent. We depart from this and examine an empirical observation made in this paper, and in earlier work by Gurr (2000): why does protest so often precede conflict, and what determines the path it takes.

By joining together intuition from related literatures on repression, protest, and conflict, this paper provides some potential explanations for unresolved questions found in each literature. We provide one potential explanation for inconsistent findings on the impact of repression on dissent, highlight the role of bargaining in explaining why some countries are able to avoid conflict, and move in the direction of testing implications of rational conflict models rather than analyzing what structural conditions encourage dissent. Though we do not provide definitive answers to any of these open questions, we provide direction and highlight the advantages of drawing from the diverse literature encompassing political dissent as a whole rather than focusing solely on one particular type of dissent.

1.3 The Model

1.3.1 Baseline Model

The Rebels (R) and Government (G) share a prize normalized to 1, and their respective shares are ω and $1-\omega$. This leads to the following payoff functions

$$U_R = \omega, \text{ and} \tag{1.1}$$

$$U_G = 1-\omega. \tag{1.2}$$

To start, $\omega = \omega_0$, and ω_0 is taken as exogenous. For G, strength is set and is public knowledge. However, R's strength is unknown to both R and G. We argue government resources are relatively well known, whereas the actual level of support for those wishing to oppose the government is not likely to be widely shared. Specifically, R is one of two types: strong (R_s) or weak (R_w). G and R share a common prior of α_0 on R's type being R_s[1]. After observing ω_0, R may either pay a cost ν to protest ($\mathcal{P}$), allowing G to shift ω from ω_0 to ω_1, or initiate conflict (C). G may only adjust ω following a protest[2].

If $\mathcal{P}$ was chosen, R pays a cost ν, learns their type, and a noisy signal, P, is observed by G. P can be thought of as the size of the protest. The signal $P \sim F_i(p)$ is such that for $P_1 > P_0$ $\frac{F(P_1|s)}{F(P_1|w)} > \frac{F(P_0|s)}{F(P_0|w)}$[3]. This implies that as protest size increases, the probabiltity it was generated by a strong rebel group increases. We argue that protest could be random for a number of reasons: weather and other natural phenomona may prevent some individuals from attending, issues of collective action may take longer to overcome than the initial wave of protest, or timing may

[1] This particular information structure is a simplification. What is necessary is R starting with some uncertainty over their own type and receiving a more precise signal than G.

[2] This assumption is justified by arguing that seizing more surplus or setting less favorable policies, if not done in response to civil unrest, would impose too high a cost in the form of potential international sanctions against the country. For example some preferential trade agreements tie human rights compliance to their existence, Davenport (2007) Hafner-Burton (2005). This is one example of a cost to repression for the government.

[3] $F_i(p)$ is any CDF that has the monotone likelihood ratio property and an unbounded likelihood ratio.

be poorly communicated as a movement begins. Regardless of the exact reason, we argue that those involved in the protest will gain more accurate information than the government, who only observes the protest's visible outcome.

The cost of protest, ν, could be thought of as representing many things but is meant to capture the expected level of repression in the government's response to protest[4].

Following protest, G makes an offer to R based on G's updated belief of R's type. Should conflict occur, by either R rejecting G's offer or R choosing conflict in the first period, each side incurs a cost that is paid regardless of the conflict's outcome. C_r is the cost of conflict for R, and C_g is the cost of conflict for G. Additionally, T_i is the probability R_i wins, with this probability being higher for strong rebel groups. The victor can set ω at any point of their choosing. Setting ω at their preferred point, of 1 or 0, implies the following expected payoffs from conflict[5]

$$E[U_R(C)] = T_i - C_r, \text{ and} \tag{1.3}$$

$$E[U_G(C)] = 1 - T_i - C_g. \tag{1.4}$$

Formally, the timing of the game is as follows.

1. The policy ω is exogenously set at ω_0, and nature draws rebel type, either R_s or R_w, with prior α_0 on the type being R_s. At this stage, all players have the same information, with rebel type being unknown.

2. R then has the choice to, initiate conflict (C), protest ($\mathcal{P}$), or stay home ($\varnothing$). Following C or $\varnothing$, the game ends, with each receiving the conflict payoff or payoff determined by ω_0, respectively.

3. If $\mathcal{P}$ was chosen, R pays a cost ν, learns their type and a noisy signal is observed by G.

[4]The government's strategic level of repression is examined in the following section.

[5]A payoff structure like this could be derived from a traditional conflict success function Skaperdas (1996).

4. Following the signal, G updates their beliefs of R's type, and is able to change ω_0 to ω_1 anywhere on the unit interval.

5. Finally, R can either accept ω_1 or choose to initiate conflict (C).

The layout of the game can be seen in Figure 1.1.

1.3.2 Solution

A perfect Bayesian equilibrium (PBE) of the extensive form game Γ is a strategy profile for R, G, and beliefs. These beliefs are consistent and updated using Bayes' rule. Actors maximize individual payoffs, and we assume in the event of a tie, conflict is avoided. For R, a strategy consists of an initial decision of $\{\mathcal{P}, C, \varnothing\}$, and a decision to $\{Accept, C\}$ as a function of G's offer following protest, ω_1, and their own type. For G, a strategy consists of an offer $\omega_1 \in (0,1)$ as a function of the signal, where the choice of ω_1 is consistent and rational.

In order to look at possible equilibria in the game, it is useful to start by deriving several conditions. Using backwards induction, the offers each rebel type will accept at the final node are

$$R_s : \omega_1 \geq T_s - C_r \equiv \omega'', \text{ and} \tag{1.5}$$

$$R_w : \omega_1 \geq T_w - C_r \equiv \omega', \tag{1.6}$$

where these reservation payoff levels come from the conflict option.

Backing up to the previous node, it is clear that G will offer either ω' or ω'' for ω_1. Any offer in the range $(\omega'', 1]$ is dominated by offering ω''. A higher offer would strictly lower G's payoff because either rebel type would accept ω''. For offers $\in (\omega', \omega'')$, G would be better offering ω'. Offers in this range are only accepted by R_w, implying G could raise their payoff by offering the lowest offer accepted by R_w: ω'. Finally, offers in $[0, \omega')$ can be eliminated by comparing G's payoff from guaranteed conflict to offering ω', because offering below ω' guarantees conflict. Using Equation (1.4) and ω' we see that offering ω' and avoiding conflict with R_w improves G's

payoff by $C_r + C_g$. This is because the government is able to save the cost of conflict, by offering the rebels their conflict-based reservation payoff.

Defining α_1 as G's updated belief of facing the strong type following the realization of the signal P, we can see G will offer ω'' if and only if

$$1 - \omega'' \geq (1 - \alpha_1)\left(1 - \omega'\right) + \alpha_1 (1 - T_s - C_g). \tag{1.7}$$

The LHS of the inequality is the guaranteed payoff of making a large concession: offering the strong rebel type's reservation payoff. This is balanced against the RHS, which corresponds to offering ω'. If the rebel type turns out to be weak (probability $1 - \alpha_1$), then the offer is accepted. Otherwise, conflict with the strong type ensues.

Rearranging Equation (1.7) and using Equations (1.5) and (1.6) to write it in terms of the likelihood ratio yields the following condition for G to make the higher offer:

$$\frac{\alpha_1}{1 - \alpha_1} \geq \frac{T_s - T_w}{C_r + C_g}. \tag{1.8}$$

Using Bayes' rule, the LHS of equation (1.8) can be rewritten as a function of the prior, α_0, and protest size, P. This leads to

$$\frac{\alpha_1}{1 - \alpha_1} = \frac{\alpha_0}{1 - \alpha_0} * \frac{f(P|R_s)}{f(P|R_w)} \geq \frac{T_s - T_w}{C_r + C_g}. \tag{1.9}$$

Defined implicitly in Equation (1.9) is a threshold value for P, call it P^*, which makes Equation (1.9) hold with equality. For values of $P \geq P^*$, G will offer ω'', while for $P < P^*$, G offers ω'. P^* is the smallest protest for which the government finds it optimal to make a large concession, rather than risking conflict with strong rebel types.

Using the government's decision rule, the payoffs to the rebels' strategy choices, C, P, and $\varnothing$ can be derived. The rebels' first-period decision can be characterized by comparing the payoff of the three strategy choices. First, the payoff for doing nothing, $\varnothing$, is fixed at ω_0.

Second, the expected payoff for immediate conflict, C, comes from a weighted average of the

conflict payoff for the two possible rebel types because the rebel group does not know its own type before choosing an initial strategy. Using the prior α_0 and each type's expected conflict payoff leads to

$$\alpha_0 * T_s + (1-\alpha_0) * T_w - C_r \equiv \omega_c. \tag{1.10}$$

Finally, the rebels' payoff to protest can be derived. After protest occurs, the payoff is dependent on G's decision rule only if the rebel group ends up being weak. This is because the strong rebel group will either accept an offer of ω'' or initiate conflict; both pay $T_s - C_r$. However, for the weak rebels their payoff could be $T_s - C_r$ or $T_w - C_r$, depending on the protest's outcome. Using this result and P^* as defined above, we can write the expected payoff for protest as

$$\begin{aligned} \alpha_0(T_s - C_r) + (1-\alpha_0)\left[F_w(P^*) * T_w + (1 - F_w(P^*))T_s - C_r\right] - \nu = \\ \omega_c + (1-\alpha_0)(1 - F_w(P^*))(T_s - T_w) - \nu. \end{aligned} \tag{1.11}$$

The first term is the probability the rebel group is strong, times their guaranteed payoff of $T_s - C_r$, the second term is the probability of a weak rebel group times the weighted average of their potential payoffs, which depend on the protest's outcome. The equation can then be rewritten as Equation (1.11), where we see the expected payoff to protest is the expected conflict payoff plus a premium related to the probability the rebels are weak, less the cost ν. Comparing Equations (1.10) and (1.11) leads us to the following condition for rebels to prefer protest to conflict:

$$\nu \leq (1-\alpha_0)(1 - F_w(P^*))(T_s - T_w) \equiv \gamma \tag{1.12}$$

Examining Equation (1.12) yields intuition as to when R prefers protest to conflict. The LHS ν is the cost one pays for choosing protest, and the RHS is the benefit, which can be broken into three pieces. First, $(1-\alpha_0)$ is the probability R is a weak type who can potentially benefit from protest. Second, $(1 - F_w(P^*))$ is the probability a weak rebel will produce a signal large enough

to convince G to offer ω''. Finally, $(T_s - T_w)$ is the actual difference between the strong and weak rebels' offers, $\omega'' - \omega'$.

1.3.3 Equilibria

We define the possible equilibria with respect to the rebels' initial strategy choice. This is done because the decision rule for the government is not dependent on the strategy choice of the rebels but rather on the outcome of the signal. This leads us to the following potential equilibria:

1. Protest Equilbrium

   Rebels: $\{\mathcal{P}; (R_s : accept\ if\ \omega_1 \geq \omega''; R_w : accept\ if\ \omega_1 \geq \omega')\}$

2. Immediate Conflict Equilibrium

   Rebels: $\{C; (R_s : accept\ if\ \omega_1 \geq \omega''; R_w : accept\ if\ \omega_1 \geq \omega')\}$

3. Peace Equilibrium

   Rebels: $\{\varnothing; (R_s : accept\ if\ \omega_1 \geq \omega''; R_w : accept\ if\ \omega_1 \geq \omega')\}$

- For all equilibria

  Government: offer ω'' if $P \geq P^*$, ω' o.w.

Proposition (1) defines when each of the potential equilibria occur.

Proposition 1. *Equilibrium with exogenous repression: There is a unique perfect Bayesian equilibrium determined as follows:*

1. *For $\omega_0 < \omega_c$ and $\nu < \gamma$, Protest*
2. *For $\omega_0 < \omega_c$ and $\nu > \gamma$, Immediate Conflict*
3. *For $\omega_0 > \omega_c$ and $\nu < \omega_c + \gamma - \omega_0$, Protest*
4. *For $\omega_0 > \omega_c$ and $\nu > \omega_c + \gamma - \omega_0$, Peace*

Figure 1 divides the (ω_0, ν) parameter space according to the rebels' equilibrium strategy choice and can be used to prove Proposition 1 graphically. The three lines in the graph represent the indifference conditions between each pair of strategies and are equivalent to the conditions presented in Proposition 1. The vertical line at ω_c is the expected value of conflict, or the peace-conflict indifference curve. The horizontal line at γ corresponds to the conflict-protest indifference curve. Finally, the downward sloping line is the protest-peace indifference curve.

The figure depicts clearly when each of the potential equilibria occur. For $\omega_0 < \omega_c$, dissent always occurs because conflict dominates peace in this region. Finding that poor conditions generate conflict is not surprising; however, the type of dissent depends on the level of repression expected by the rebels, ν, compared to γ. For too high a level of repression, protest becomes costly to the rebels relative to conflict. This is a key result, that a low state of the world encourages dissent but does not necessarily cause conflict. Furthermore, we generate a counterintuitive result with respect to repression; if conditions are very poor, repression is unlikely to be an effective tool at quelling dissent, defined as protest or conflict. This is because the rebels always have conflict as an option, and although repression may prevent protest, by raising its cost, it leaves conflict as the only viable alternative.

Finally, we see for values of ω_0 greater than ω_c, the rebels choose either peace ($\varnothing$) or protest. Protest is chosen for relatively low combinations of ω_0 and ν. When peace weakly dominates conflict, protest can still achieves higher payoffs than conflict, gross of repression. This results from the premium protest earns over conflict. Here we see another counterintuitve result with repression: only when economic conditions are better than some minimum threshold, ω_c, can repression be used to quell dissent.

Perhaps the most interesting result from this set-up is seeing explicitly how having conflict as a reservation payoff drives when political dissent occurs. If the state of the world is ever below this value, some form of dissent is going to happen. However, the form political dissent takes depends on the government's actions. This is interesting because we generate a few counterintuitive results related to the impact of repression, and provide a possible way to explain the variation in conflict

outbreak across ostensibly similar countries. Low levels of repression can lead to more political dissent, in the form of protest, but high levels of repression switch the type of dissent to conflict. Additionally, repression is only effective when the state of the world is better than the conflict option. This implies that countries with especially poor conditions may actually want to encourage protest and take actions to prevent conflict from occurring. Ironically, this also implies that countries where conditions are slightly better may want to use repression to prevent any chance of conflict, which could result from failed protests.

These results raise the question as to what the optimal level repression should be, which is analyzed in the following section. However, modeling ν as fixed is appropriate if we are interested in short run changes. Considering that the rebels must make their decision to protest before repression actually occurs, they are likely to place a great deal of weight on past experience interacting with the government. It would be very easy for a government to make the response to a very small incident dramatic and public, raising the expected level of repression, but it could take much longer for a government to convincingly commit to a lower level of repression.

1.3.4 Strategic Repression

In the literature on repression, two important questions are: what purpose it serves and what is its relationship with dissent? Some theoretical models have argued that very high levels of repression should quell all dissent, for example Pierskalla (2010). For the purpose of this section, we will examine what the level of repression would be if G could costlessly set ν at the beginning of the game. When a range of values is possible, we assume G chooses the minimum possible level of repression[6].

Examining the use of repression highlights the issue caused by the opposing nature of the preferences for the government and rebels in the game. The government can use repression to impact the type of dissent used by the rebel. Figure 1 highlighted this tradeoff; repression can influence the choice between protest and conflict for values of $\omega_0 < \omega_c$, and the choice between peace and

[6]This could be done by imposing a small increasing cost for repression, or through the use of lexicographic preferences.

protest for values of $\omega_0 \in [\omega_c, \omega_c + \gamma)$. Using our understanding of how the level of repression impacts the rebels' equilibrium strategy choice, and continuing with the use of backwards induction, we only need to figure out when the government prefers which rebel strategy choice.

We start by deriving the expected payoffs to the government for each rebel strategy. The total payoff available in the game ex-ante is $1 - (C_r + C_g) * P(\text{Conflict Occurs})$.

With peace, conflict does not occur, and the rebels receive a payoff of ω_0. Since R and G share a total payoff of 1, this implies the Government's payoff is

$$Peace : 1 - \omega_0 \tag{1.13}$$

With protest, conflict occurs if: (1) the rebel group is in fact strong, probability α_0; and (2) the strong rebel group fails to accurately convey they are strong to the government. That occurs when the protest is relatively small, specifically less P^*, probability $F_s(P^*)$. This leaves an aggregate payoff, gross of repression, of $1 - \alpha_0 F_s(P^*)(C_r + C_g)$. This is the total available surplus, less the deadweight loss of conflict times the probability conflict will occur; after subtracting off the rebels' payoff we get

$$Protest : 1 - \omega_c - \gamma - \alpha_0 F_s(P^*)(C_r + C_g). \tag{1.14}$$

Finally, for conflict, the aggregate payoff is $1 - (C_r + C_g)$. Subtracting off the rebels' expected payoff from conflict leaves the government with

$$\text{Immediate Conflict} : 1 - \omega_c - (C_r + C_g). \tag{1.15}$$

From here, we can compare the payoffs from each to determine when the government prefers each strategy.

We focus first on examining the case when Protest dominates Conflict. Substituting the respective values into the payoffs for each, we see that protest dominates conflict if

$$\frac{C_r + C_g}{T_s - T_w} \geq \frac{(1-\alpha_0)(1-F_w(P^*))}{(1-\alpha_0 F_s(P^*))} \equiv A. \tag{1.16}$$

It is straightforward to show the RHS of the inequality is always less than one. This gives Lemma 1.

Lemma 1. *For $\frac{C_r+C_g}{T_s-T_w}$ large enough (≥ 1 is sufficient), the government prefers protest to immediate conflict.*

This condition says that when the cost of conflict, $C_r + C_g$, is large relative to the potential gain from risking conflict, $T_s - T_w$, then the government would prefer to allows protest in order to gain more information on the rebel's strength.

Next we examine when peace dominates protest, this is done by a direct comparison of the government's payoffs for each, and results in the following

$$\omega_0 \leq \omega_c + \gamma + \alpha_0 F_s(P^*)(C_r + C_g). \tag{1.17}$$

To find the governments chosen level of repression, it is useful to understand when and how repression impacts the rebels' strategy choice. This is made clear in Proposition 1 and Figure 1.2. For $\omega_0 < \omega_c$, the rebels will only choose between protest and immediate conflict, and repression directly impacts that choice by acting as a cost to protest. For values of $\omega_0 \in (\omega_c, \omega_c + \gamma)$, the rebels will choose either protest or peace, and again this choice depends on the level of repression. Finally, for $\omega_0 > \omega_c + \gamma$, the rebels always choose peace, and the level of repression has no impact on that choice.

This implies there are two cases to examine: $\omega_0 < \omega_c$, and $\omega_0 \in (\omega_c, \omega_c + \gamma)$. If $\omega_0 < \omega_c$, the rebels will only use protest or immediate conflict. So, Equation (1.16) determines the government's preferred strategy choice. Examining Equation (1.16) shows this is independent of ω_0, and that for high enough cost of conflict it is optimal to allow protest, while for low enough conflict costs G will set repression high enough to induce conflict[7]. Because γ is the level of repression which

[7]This is most clearly seen using lemma 1 and seeing that this is always true for $C_r + C_g > (T_s - T_w)$, as $(T_s - T_w)$ is actually the upper bound because $A \leq 1$.

equates the payoffs of protest and immediate conflict for the rebels, it can be used to calculate the optimal level of repression for $\omega_0 < \omega_c$. With $C_r + C_g \geq (T_s - T_w) * A$, the optimal level is any value of $\nu \leq \gamma$, or by assumption $\nu = 0$. For $C_r + C_g < (T_s - T_w) * A$, G wishes to induce conflict and needs to set $\nu > \gamma$, implying $\nu = \gamma + \varepsilon$, for some ε close to zero.

The second case is for values of $\omega_0 \in (\omega_c, \omega_c + \gamma)$. Here if repression is low enough, the rebels will use protest, otherwise they will choose peace. Examining Equation (1.17), we see that in this range, the government always prefers peace. This implies setting $\nu \geq \omega_c + \gamma - \omega_0$, or $\nu = \omega_c + \gamma - \omega_0$ by assumption. Because ν must be positive, beyond $(\omega_c + \gamma)$ G sets $\nu = 0$. This range of ω_0 is exactly when G would like dissent to occur, so he could lower the rebels' share of resources. However, the rebels choose $\varnothing$ regardless of the level of ν.

Combining these results leads us to the following proposition determining the government's optimal level of repression.

Proposition 2. *Strategic Repression: The minimal level of repression required to induce the rebels to take the government's preferred strategy is determined as follows:*

1. *For* $\omega_0 < \omega_c$

   (a) *If* $C_r + C_g \geq (T_s - T_w) * A$, $\nu = 0$

   (b) *If* $C_r + C_g < (T_s - T_w) * A$, $\nu = \gamma + \varepsilon$

2. *For* $\omega_0 \geq \omega_c$, $\nu = max\{\omega_c + \gamma - \omega_0, 0\}$

This section assumed that it was costless for the government to repress; what if it was not? Repression generates a discrete change to the expected payoff for the government by potentially inducing the rebels to change their strategy. For low values of ω_0 this can change protest to conflict, and for higher values it can prevent protest. Including a cost would not change the levels of repression, unless the level needed to either induce conflict, or prevent protest cost more than the change in expected payoff for the government. For example, it could induce the government to switch to setting $\nu = 0$ in Case 1 or 2 of Proposition 2.

Even with repression endogenous, protest and its associated dynamics are still potential outcomes. The interesting consideration from this discussion would be understanding the impact of policies intended to raise the cost of repression for a government. Although this could potentially reduce the likelihood of immediate conflict occurring, such policies raise the level of non-violent dissent, which can escalate. No one wants to encourage repression, so perhaps the real solution to preventing protest escalation is looking again at the model's argument for why it escalates. Escalation of protest is the result of protest being a noisy signal; however reducing the noise in the signal could lead to immediate conflict. What this implies is that in order to reduce the chance of conflict, policies which both, reduce the level of repression and the amount of noise in protest must be enacted together. For example policies which increase civil liberties and freedom of the press may allow information to travel more freely, reducing the noise of protest, while simultaneously reducing the expected level of repression. In short, we must consider the costs of encouraging political dissent, and ensure that doing so does not simply lead towards violence.

1.4 Model Analysis and Implications

Focusing on the baseline model, which takes repression as exogenous, we can derive potentially testable implications which highlight the main contribution of the model: distinguishing not only when dissent occurs, but also what form it takes. For each parameter in the model, we discuss how each potential strategy's payoff is impacted and then how it moves each of the indifference curves. Then the model is examined starting in a state of protest, and the determinants of the potential paths following protest are analyzed. Finally, the determinants of the optimal level of repression, from the model's extension, are explored.

1.4.1 The use and type of dissent

Consistent with many models of political dissent, as the state of the world improves, overall dissent decreases. In the model, this corresponds with increases in ω_0. Unlike some others, for example Dal Bó and Dal Bó (2011), ω_0 does not influence the payoff of political dissent directly. However, as ω_0 increases, the payoff of either form of dissent, conflict or protest, will fall relative to peace. How the rebels' equilibrium strategies change with ω_0 can be seen in Figure (1.2) by moving along the horizontal axis.

Similarily, the impact of varying ν, the expected level of repression, can be seen in Figure (1.2) by moving along the vertical axis. As ν increases, protest becomes less likely; whether that leads to conflict or peace depends on ω_0. For other parameters in the model, it is easiest to examine how they impact γ and ω_c and then move the respective curves in Figure (1.2).

Starting with the impact of the cost of conflict, the government's portion C_g only impacts the rebels' payoff for protest and does so through its impact on P^*. As C_g increases the likelihood of protest increases, because G lowers the threshold protest size, P^*, at which they will make a large concession. This impact can be seen in Figure (1.3) by raising γ and shifting the protest-peace indifference curve outward. The figure highlights an interesting implication: increasing C_g raises the overall occurence of dissent. Although the use of conflict in the first period decreases, the overall

chance of conflict occurring may go up or down. This is due to the chance of conflict following protest, and the increased use of protest. Figure (1.3) shows these changes; the highlighted area to the right of ω_c was previously peace, whereas the area to the left was previously conflict.

The rebels' cost of conflict, C_r, impacts the payoffs for both conflict and protest. For conflict, the prediction is simple, as it directly reduces the payoff. However, for protest the impact has two parts. The first part is a direct reduction equal to C_r. The second part offsets this reduction because the government lowers P^*. In most cases, the direct negative effect dominates. The end result is a much more intuitive one than with C_g: as C_r increases, there is a reduction in the occurence of dissent and a switch from immediate conflict to protest. All of this can be seen in Figure (1.4).

As the prior probability of being strong, α_0, increases, the rebels' payoff for any type of dissent also increases. The intuition for this is straightforward, whether using conflict or protest, the more likely the rebels are a strong type, the more likely it is they end up with the strong type's reservation payoff. This results in a clear rise in the overall occurence of dissent, as can be seen in Figure (1.5). However, the impact of α_0 on γ is ambiguous, implying we can not make any predictions on how the composition of dissent changes with α_0[8].

The last piece to consider is the impact of the rebels' strength parameters, T_s and T_w. We consider two possible changes here: holding $T_s - T_w$ fixed while raising $T_s + T_w$, or holding $T_s + T_w$ fixed while raising $T_s - T_w$. The first case could be thought of as increasing the strength of the rebels relative to the government. This raises the mean value of ω_c, which clearly leads to more dissent overall. However, because the governmentŠs decision only takes into account which group he is likely to face, not the level of the average payoff, the mix of dissent will remain unchanged. In other words, the value for P^* is not impacted if we change only the mean value of the rebelsŠ strength. Figure (1.5) is the exact picture of this scenario.

For the second case, increasing the spread of $T_s - T_w$ rather than the level, the impact on dissent depends on α_0, because the direction in which ω_c moves is also dependent on α_0. It is more interesting to investigate the case where we fix ω_c, while raising the variance in conflicts outcome,

[8]Figure (1.5) fixes γ to show only the increase in overall dissent.

the spread of $T_s - T_w$. A change like this raises P^*, switching some dissent from non-violent to violent. This occurs because the government becomes more willing to risk conflict, or less willing to give a major concession, as the difference between the reservation payoffs increases.

1.4.2 Protest: Escalation or De-Escalation

It is of value to examine when each of the three potential paths following a protest are predicted to occur: peace without concession, escalation, or peace with concession. Here concession refers to the government offering ω'' without conflict occuring. The key determinants of which path occurs are the size of the protest and the threshold protest value, P^*, which is the protest size above which G makes a concession.

This leads to one of the main relationships we test. Above P^*, G should make an offer either type will accept and conflict should not occur. However as protest size increases towards P^* the probability of conflict increases. This is because the larger the protest the higher the likelihood it came from a strong rebel type, but it is not until G sees a protest larger than P^* that they are willing to make a large concession. This implies the probability of escalation first increases, then decreases in protest size. To be precise Figure 1.6, tracks the probability of conflict occuring as a function of protest size, the increasing portion of the curve is exactly equal to α_1, after P^* the probability of escalation drops to zero as the government will then make a large concession.

Understanding the determinants of P^* is instrumental in understanding the path following protest. P^* decreases in the cost of conflict and α_0, increases in $T_s - T_w$, and is unimpacted by ω_0[9]. This implies, conditional on protest having occurred, higher α_0, C_r, or C_g all increase the likelihood of a major concession by lowering P^*. Additionally, as $T_s - T_w$ increases, the probability of a concession decreases. The intuition for this result is that G stands to gain more by risking conflict as $T_s - T_w$ increases.

[9]These can be seen clearly by examining Equation (1.12).

1.4.3 Model Assumptions

In this model, protest occurs because the rebels gain an informational advantage when protesting, and weak rebels can do no worse than their reservation payoff by protesting. However, this gain is in part offset by protest's cost: repression. First, it is worth noting that as long as the rebels expect to gain more information, even if the information is not perfect as the model set-up implies, there will still be a premium from protesting. Second, the key condition for the protest to be informative requires that the signal follows the MLRP: larger protests must be more likely when rebel groups are strong, and the government and rebels must receive different signals. This raises a few questions about when the model's assumptions would hold and why the rebels may gain more information than the government when protesting. Specifically, this raises concerns about what would happen if: 1) not all rebels would be willing to engage in violence; 2) whether or not protest size is correlated with rebel strength, and 3) why the rebels may gain more information in the protest process than the government.

The first two concerns are closely related. One way to explain why protest size is correlated with rebel strength would be to create a model where individual participation in protest was explained explicitly. If individual preferences for government policies determined participation in protest and a fraction of individuals would be willing to use violence, then anytime the government saw a protest they would know some fraction of those protesting would be rebels. A related issue is whether or not protest movements are actually linked to groups willing to commit violence. However, the same basic argument holds, as long as some fraction of the other anti-government movement supporters may join with the rebellion's cause, the signal would still be correlated with the strength of the rebel movement. Furthermore, if the government has less information than the rebels do about the relationship between the rebels and the protestors, that may provide one explanation for the difference in the signals the government and rebels receive.

There are a number of potential explanations for why the rebels may receive a more accurate signal than the government. For one, if they were in part responsible for organizing the protest,

they may have a much more accurate sense of the degree of conviction among those who showed up, and those who did not show up, than the government. In the case where the protests were not actually organized by potential rebels, they may still gain more information than the government by participating and recruiting other groups who oppose the government. Additionally, the protest may provide an opportunity for learning by doing. The act of organizing protestors may test some of the rebels' capabilities that would also be useful when committing more violent acts of defiance, such as the ability to organize and communicate. However, a caveat of this work is that the model is a much better fit in situations where there is a direct link between protest and rebel movements. As the disconnect grows stronger, the model's assumptions become less realistic and the model's predictions are likely to be less accurate.

1.5 Data Sources

To investigate the implications of this model, we turn to data from the Social Conflict in Africa Database (SCAD) and Peace Research Institute at Oslo's (PRIO) Armed Conflict Database (ACD). Combining this political dissent data with Freedom House ratings, World Bank World Development Indicators (WDI), and data from the Cingranelli-Richards (CIRI) human rights data project leads us to a sample of 48 countries covering the time period 1990-2012.[10] Combining the non-violent events data from SCAD with more traditional conflict data in the ACD provides a unique way to examine the full continuum of political dissent, and allows several of the model's key predictions to be examined.

To measure conflict, PRIO's ACD database was chosen for two main reasons. First, the SCAD data were meant to be non-overlapping with the PRIO data, so this should prevent the double counting of events between datasets. Second, the PRIO data covers the entire sample in the SCAD data and has available all events at a highly dissagregated level. For protest, there are fewer sources available; SCAD was chosen due to its exhaustive coverage and the ease of identifying non-violent events which involved the government as a target. This paper uses the minor conflict threshold in the ACD to consider a country in conflict; however, years coded as inactive in the ACD are not counted as in conflict.[11] All periods not considered as in conflict are included in the following analysis, regardless of the level of protest that occured. No distinction is made between periods of peace and protest, we only make a distinction for coding periods as in conflict. This paper uses multiple continuous measures of protest intensity and measures protest on a continuum rather than an indicator for occurrence.

Table (1.1) below displays summary statistics for the political dissent data. The data contains 51 onsets of conflict within the 1101 country years of data, with 10 conflicts that were ongoing in 1990. Overall, 22.1 percent of all periods were coded as being in conflict. For protest several dif-

[10] From 1990-1992 there are 47 countries, Eritrea became independent in 1993.

[11] The definition of conflict used in the ACD is: "a contested incompatibility that concerns government and/or territory where the use of armed force between two parties, of which at least one is the government of a state, results in at least 25 battle-related deaths." Strand et al. (2003) (pg. 1)

ferent measures were derived from the event data. The first, *Protest 1*, is the number of events and only captures the occurence of protest; regardless of the size or duration of an event, it adds only one to the count. The second, *Protest 2*, measure counts the number of protest-days in a country-year. The third, *Protest 3*, counts the number of distinct location-days in a country-year. SCAD codes distinctly seperate locations, for the same event, as additional data points. For example a protest movement with protests in two major cities would likely end up adding two to the count in this measure. This is the preferred measure, as it captures both the occurence and extent of protest. Longer protest movements covering more area are captured better with this measure. For analysis we focus on results using the natural log of the protest measures; this is done to smooth out some of the over-dispersion present in the data. An ideal measure would contain a strong indication of the number of participants, but a reliable estimate of this is unavailable.

To measure Concessions, changes in the Freedom House (FH) ratings for a country were used. An ideal set-up would allow us to trace exactly who demanded what and what concessions were made; however, no such data exist. The FH ratings do capture many fundamental rights, for which groups may be willing to fight. Each year they publish two indices for every country, one for Political Rights and another for Civil Liberties. Their index is well suited to measuring concessions within a given country because they use the prior year's score as a benchmark. This could complicate cross-country comparisons, but we are interested in within-country changes. From FH's description of the rating process, "A score is typically changed only if there has been a real-world development during the year that warrants a decline or improvement" House (2014). From the perspective of capturng major concessions, this is ideal. Table (1.2), which follows, summarizes the levels and frequency of changes to the FH ratings. Each of the two indices is rated on a 1 to 7 scale, with 1 being the most free. A concession is measured as an increase towards greater freedom, on either index, between the current year and the next.[12]

[12]The FH data includes 2013, so we are able to create this lag for the entire time period.

1.6 Results

The empirics in this paper focus on protest; specifically, whether or not it occurs and subsequently, if it escalates. Examining this leads to two main conclusions. First, although we do not reject other theories of protest, such as those in the global games literature highlighting its ability to overcome issues of collective action, we find evidence suggesting it is a potential signaling mechanism used to avoid more violent types of dissent. This implies that policies which allow protest to accurately convey public support may be effective in reducing the occurrence of violent dissent. Second, the data on protest have not reached the same state of maturity as that on conflict. Specifically the ability to match protest movements to specific groups and demands is not available. If data like this becomes available, additional implications of the model could be examined. However, the patterns found are consistent with the predictions of the model: larger protests generate concessions more often, and the largest protests are the least likely to escalate to conflict.

1.6.1 The Impact of Protest

Starting from a period of protest, the model predicts that one of three things could occur: escalation to conflict, a concession, or peace without concession. Equation (1.9), which defines the government's offer decision, determines when the government would make a concession, defined as offering ω'' in the model. The probability of escalation in the model is closely related to the same equation, however it is also dependent on the rebels' true type. We start by analyzing concession and then move towards predicting escalation to conflict.

The model's set-up can be translated into a latent utility framework to analyze concession. The government is comparing the expected utility from two potential choices: offering ω' and risking conflict with strong types, or offering ω'' and "paying for peace." Equation (1.9) compares this decision. Embedding this condition into a latent utility model requires comparing the expected utility from each possible choice, and the addition of an error term meant to capture unobserved factors influencing this decision. Doing this, and moving all items to the LHS leads to the following,

which describes the probability a concession occurs.

$$P(Concession) = (\frac{\alpha_0}{1-\alpha_0} * \frac{f(P|R_s)}{f(P|R_w)} - \frac{T_s - T_w}{C_r + C_g} + \varepsilon \geq 0). \tag{1.18}$$

Equation (1.18) demonstrates when the utility from making a concession, offering ω'', is large enough relative to risking conflict, offering ω'. Using comparative statics derived in section 4, we know that this probability will increase with increases in α_0, protest size, C_r, or C_g, and will decrease with increases in $T_s - T_w$. The theory does not predict a relationship between concession and ω_0, conditional on protest occurring. This leads to our first form hypotheses:

Hypothesis 1. *The probability of concession increases in protest size.*

To examine Hypothesis 1, we use fixed effects regressions of the following form:

$$Concess_{it} = \beta_1 Protest_{it} + \beta_2 X_{it} + \gamma_i + \delta_t + \varepsilon_{it} \tag{1.19}$$

Where i indicates country, t indicates the year, γ and δ are country and time fixed effects, X_{it} is a vector of control variables, and ε_{it} is the error term. For the dependent variable we generally look at the net change of the FH ratings from one year to the next, and we use logged values of all three measures of protest size discussed in the data section. Examining these specifications in Table (1.3) we see a consistent positive relationship between protest size and the probability of concession. To quantify this impact, it is easiest to examine the final column, where we use an indicator that is equal to one if either scale improved from the current year to the next. We see a coefficient of .08, which means that each doubling of protest size corresponds to an increase of about eight percentage points in the likelihood of a concession. To put this in terms relative to the data, a protest one standard deviation above the norm raises the likelihood of observing an increase in the Freedom House ratings by nearly fifty-percent.

We also address a number of possible concerns about the robustness of this impact. First, most specifications include both country and year fixed-effects, this removes heterogeneity at the respective levels. Alternatively, we examine the relationship without each of these and see a similar

pattern. We also examine alternative functional forms, including the square term of protest size and use a fixed-effects probit estimation instead of a linear probability model and find similar results. Finally, we investigate the possibility that periods of protest are themselves tumultuous, and result in a temporary reduction in the FH ratings for a country, so what we observe would actually be a recovery to normal levels. To address this we change the dependent variable to be a lagged version of the net change in the FH ratings, and find an insignificant negative impact with a much smaller magnitude.

The second relationship we analyze is which protests escalate. The relationship between protest size and the probability of escalation is more nuanced. Escalation occurs only with the strong rebel group and only when they fail to produce a large enough signal for the government to offer ω''. Below P^*, this probability matches exactly α_1, and should be increasing in protest size. However once the protest size passes P^*, the government offers ω'' and conflict should be avoided. Taken literally, this implies a probability which increases to a point and then sharply drops off to zero. This leads to our second hypotheses:

Hypothesis 2. *The relationship between protest size and conflict escalation is non-monotonic, first increasing then decreasing.*

We examine Hypothesis 2 using non-parametric specifications and look for a pattern matching Figure (1.6); we also look for an inverted-u shaped relationship with regression specifications. This sudden drop in the probability of escalating to conflict predicted by the theory is not far from what we see in the data. Table (1.4) shows the results from a fixed effects regression, when examining protest days and location-days, we see significant results on the protest size and its square, after controlling for country and year fixed effects. To put the impact in perspective, we calculate and graph the margins for the range of values protest takes on in the data. In both specifications, we see the largest protests in the data predict a negative likelihood of escalation. Overall, the observed pattern is exactly what is predicted by theory, an initially increasing then decreasing probability of escalation.

Given the non-monotonic nature of our prediction, we have no a priori reason to believe a

quadratic term is the correct way to capture the true relationship between protest size and the likelihood of escalating to conflict. One way to address this is to use locally linear polynomial estimation, which essentially estimates and smooths a series of linear regressions. To do this we employ STATA's lpoly command and trace the relationship between protest size and the probability of conflict occurring in the next period. We do this in two ways, first a simple univariate relationship, regressing protest size on the indicator for escalating to conflict. Then because we would like to eliminate time and country invariant unobservables, we first partial out the fixed effects, and then conduct the same regression using the residuals left after partialling out the fixed effects. We again focus on *protest 2* and *protest 3*, because *protest 1* lacks sufficient variation to identify the relationship.

Examining Figures (1.9) – (1.12) we see that both measures of protest, with and without the inclusion of fixed effects, come to the same conclusion as the regression specifications. When protests are relatively small the relationship between size and escalation to conflict is stable or slightly increasing; however, the largest protests in the data do not escalate. The results here may not be causal; however, the relationship we observe and predict is a rather specific one and few if any alternative explanations come to mind quickly for the resulting inverted-u shaped relationship between protest size and the likelihood of escalation to conflict. Furthermore, the use of time and country fixed effects go a long way to eliminate sources of heterogeneity.

In short, the correlations we find may not be causal, but they are strikingly similar to the model's predictions. Furthermore, common intuition would often suggest that the largest protests are the ones that escalate into full scale conflict, an intuition rejected here in theory and with data. Perhaps most importantly, we also find the predicted non-monotonic relationship between protest size and escalation, a relationship specific to this model.

1.7 Conclusion

Examining the escalation to conflict in a rational setting may help us to better understand why conflict occurs in lieu of negotiated settlements. The model presented here is clearly a simplification, and focuses on one possible explanation for the roles of protest and repression. Nonetheless, the insight provided by the model, allows us to examine if asymmetric information is a driving force causing conflict. The results in this paper, both theoretical and empirical point towards such a relationship.

If asymmetric information is a driving force for conflict, this should influence the types of policies to encourage in countries moving along the political spectrum towards democracy. Transitions such as those which occurred throughout the sample period covered by the data used in this paper are known to be conflict-prone, (Hegre and Sambanis, 2006; Hegre et al., 2001). If this is the case, helping to create transparent mechanisms for expressing political viewpoints may be a key factor in avoiding conflict. Reducing the cost of using non-violent mechanisms of dissent does lead to more use as shown in Cunningham (2013). However, simply encouraging protest could lead to conflict if the information conveyed through protest is not accurate.

Further research will be needed if we are to truly understand the relationship between violent and non-violent political dissent. The analysis here suffers from several shortfalls, most prominently an inability to break out who the participants in a given event are beyond classifying them as civilians and governments within a country. As better data becomes available we may be able to refine the hypotheses we wish to test, and begin to compare them to alternatives. For example, it would be interesting to compare a model of asymmetric information to a model examining commitment problems. Would this result in similar predictions as to the use of protest and repression? If not, can we compare the two possibilities? In such a case simply advocating transparent mechanisms for expressing political viewpoints would not likely be enough; and creating international mechanisms to enforce agreements may be a preferable path to avoid future conflict. Nonetheless, while common intuition suggests the largest protests are the most likely to escalate to conflict, this

intuition is rejected by both the theory and empirical results of this paper.

The literature on civil war has a strong tradition of uncovering relationships, though rarely are the actual causal mechanisms in these relationships pinned down. This paper provides a step in that direction, by developing and testing the predictions of a model of asymmetric information. Additionally, highlighting the idea that both violent and non-violent political dissent have the same underlying causes, this research suggests that a lot can be learned by examining what differences provoke one over the other.

APPENDICES

APPENDIX A

FIGURES FOR CHAPTER 1

Figure 1.1 Formal Game Tree

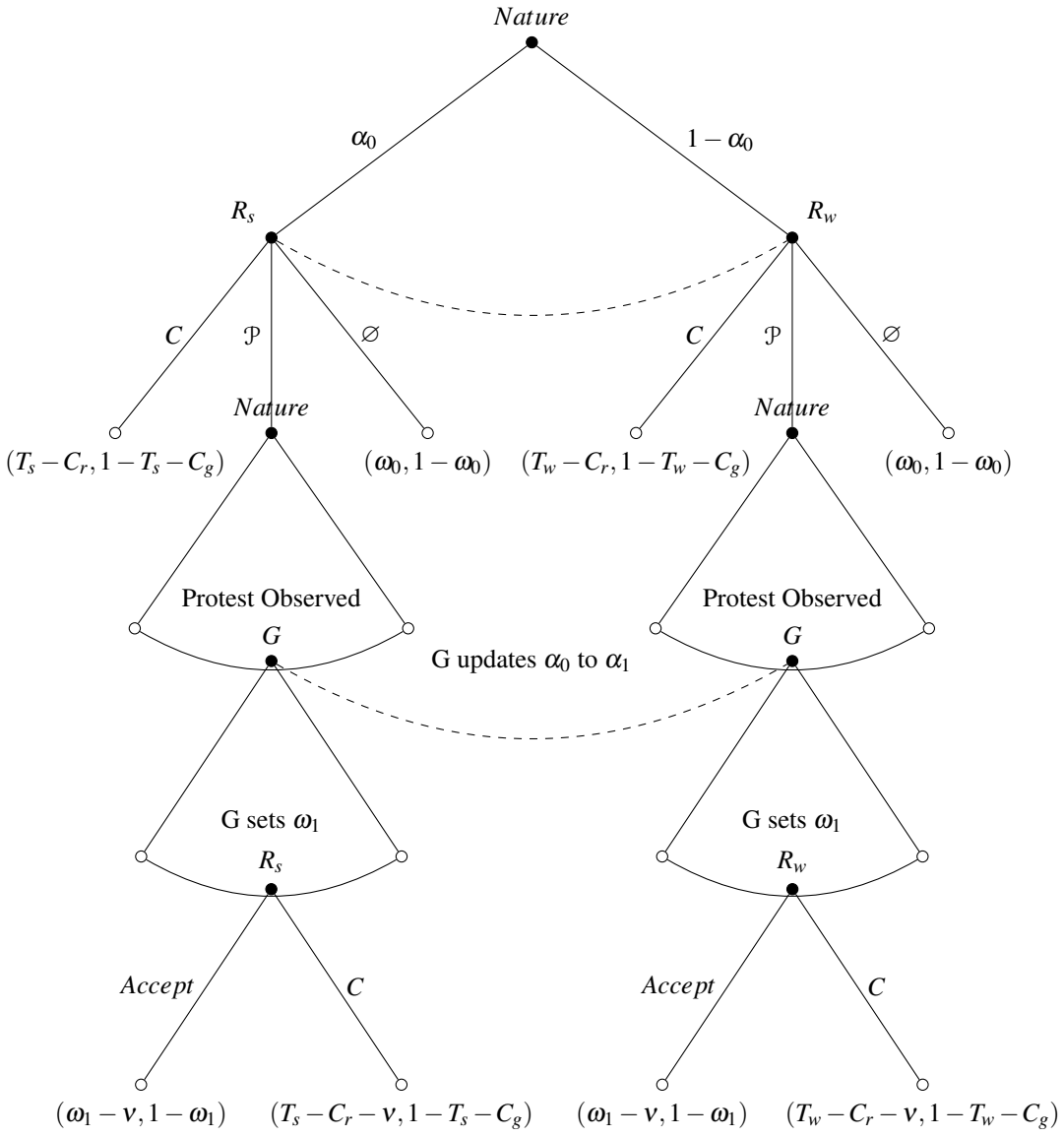


Figure 1.2 Impact of C_g on Equilibrium Strategies

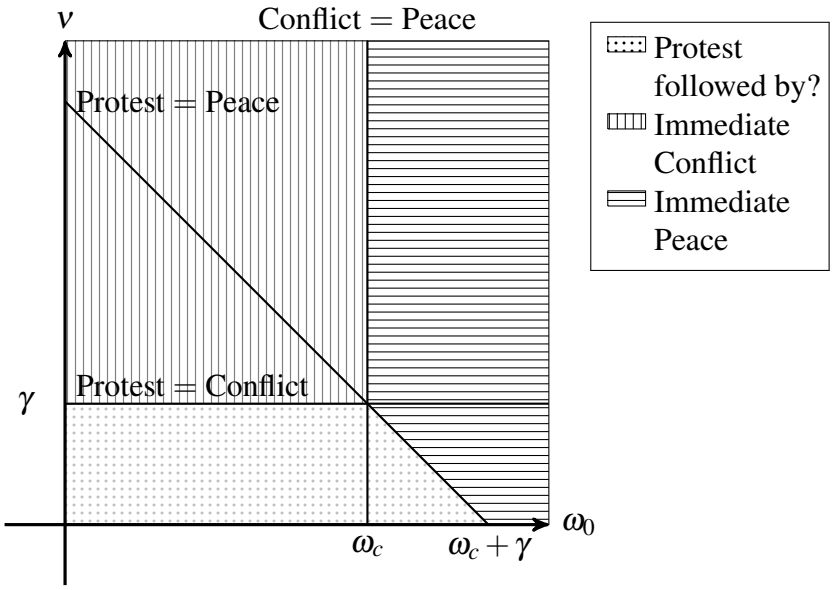


Figure 1.3 Equilibrium Paths for ν and ω_0

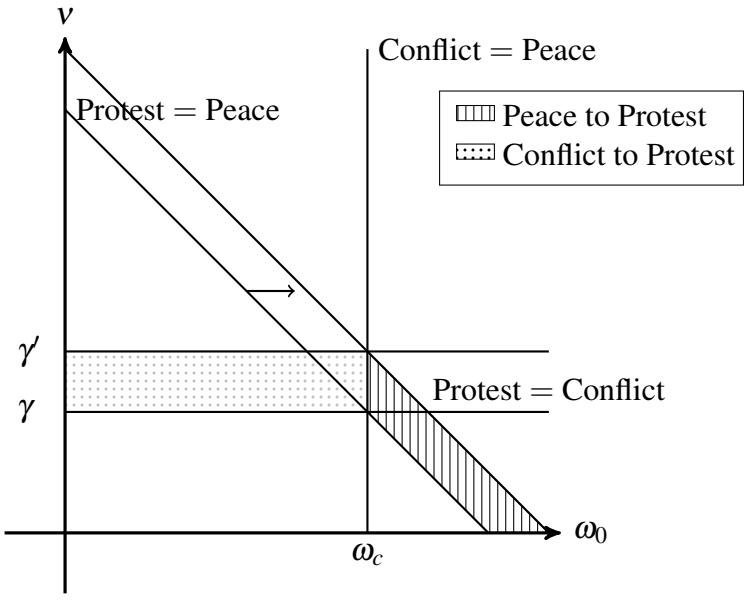


Figure 1.4 Impact of C_r on Equilibrium Strategies

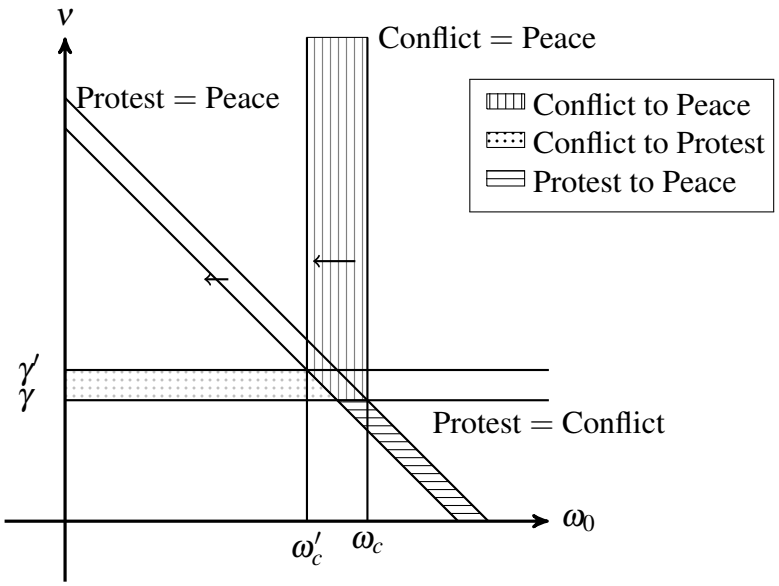


Figure 1.5 Impact of α_0 on Equilibrium Strategies

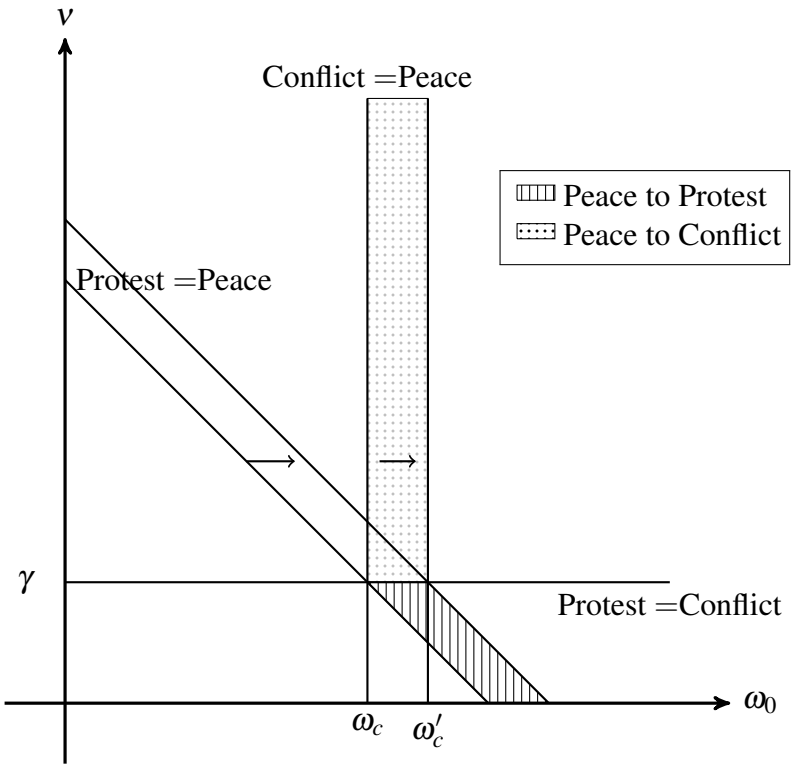


Figure 1.6 Protest Size and Escalation

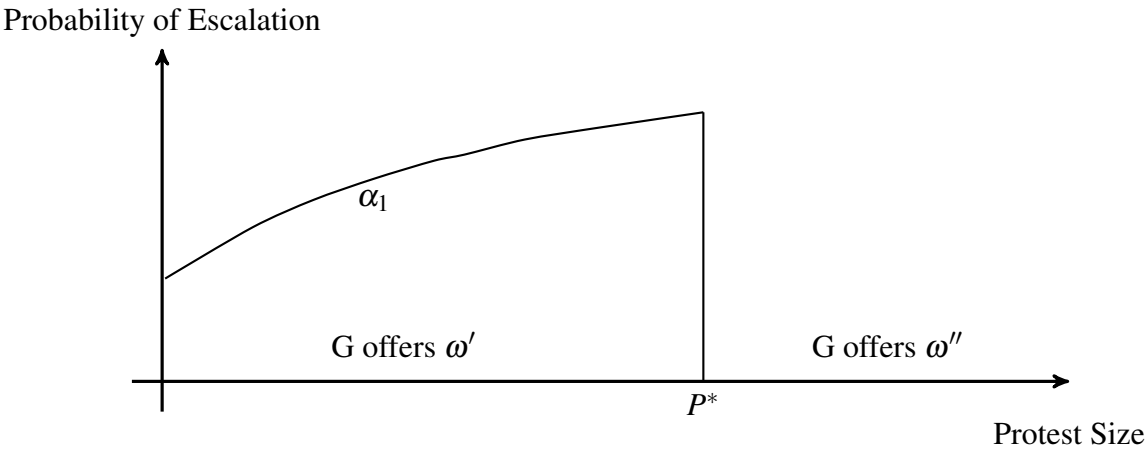


Figure 1.7 Escalation versus Protest size

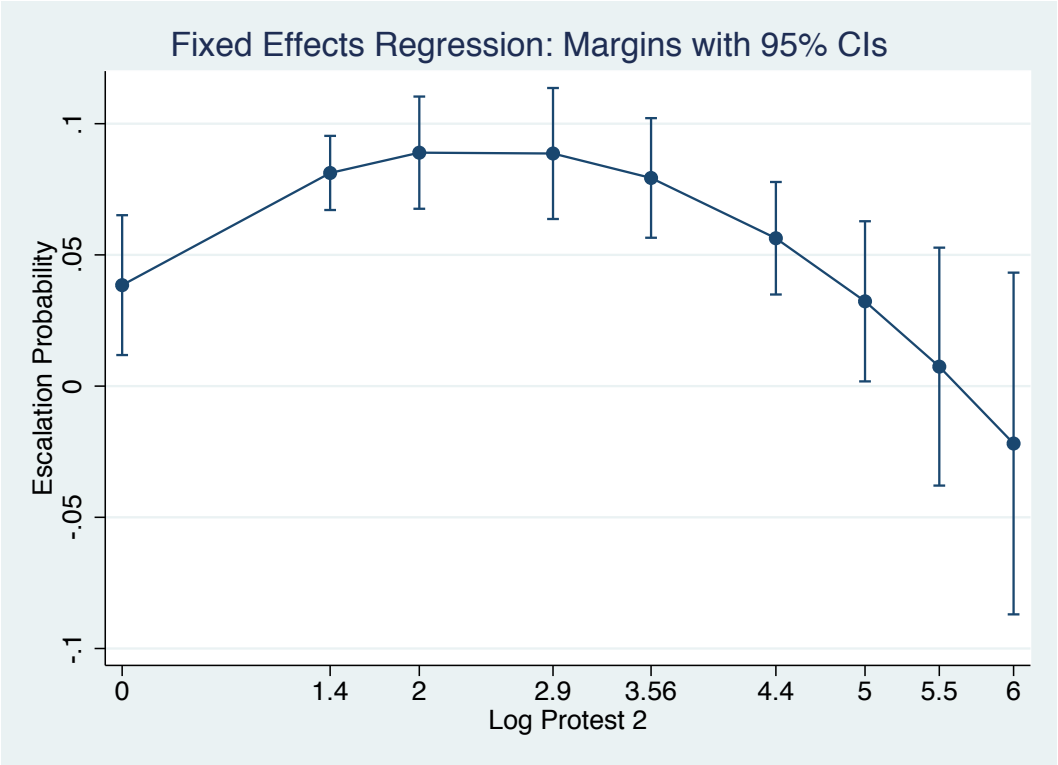


Figure 1.8 Escalation versus Protest size

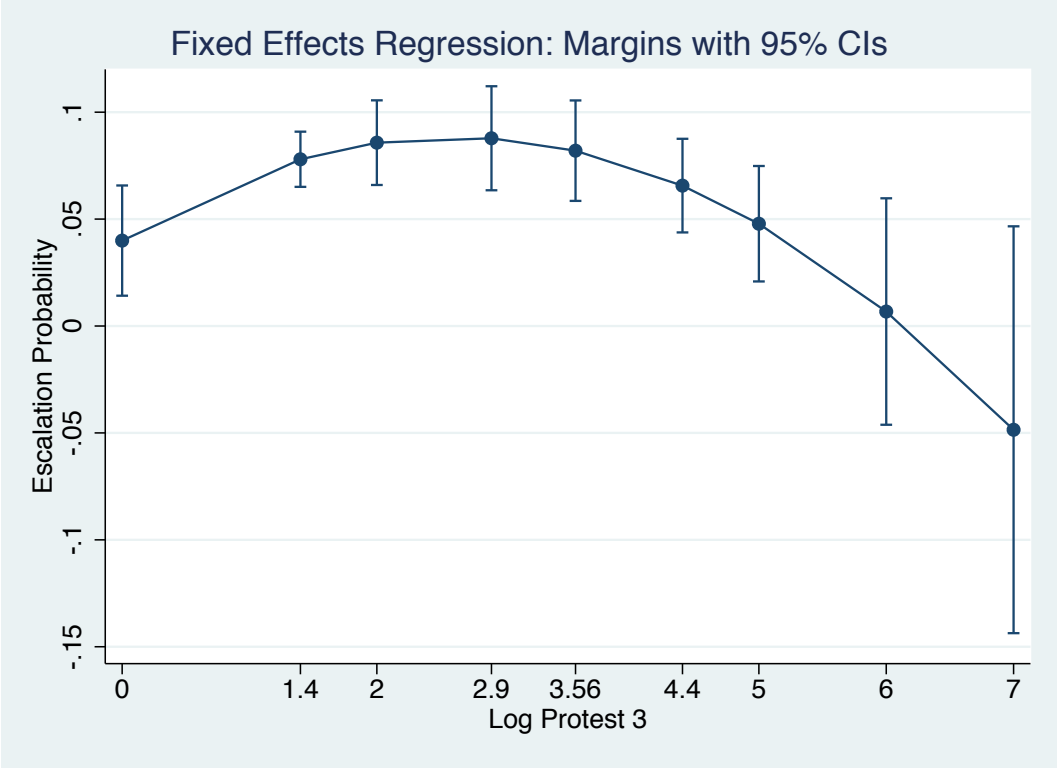


Figure 1.9 Escalation versus Protest size

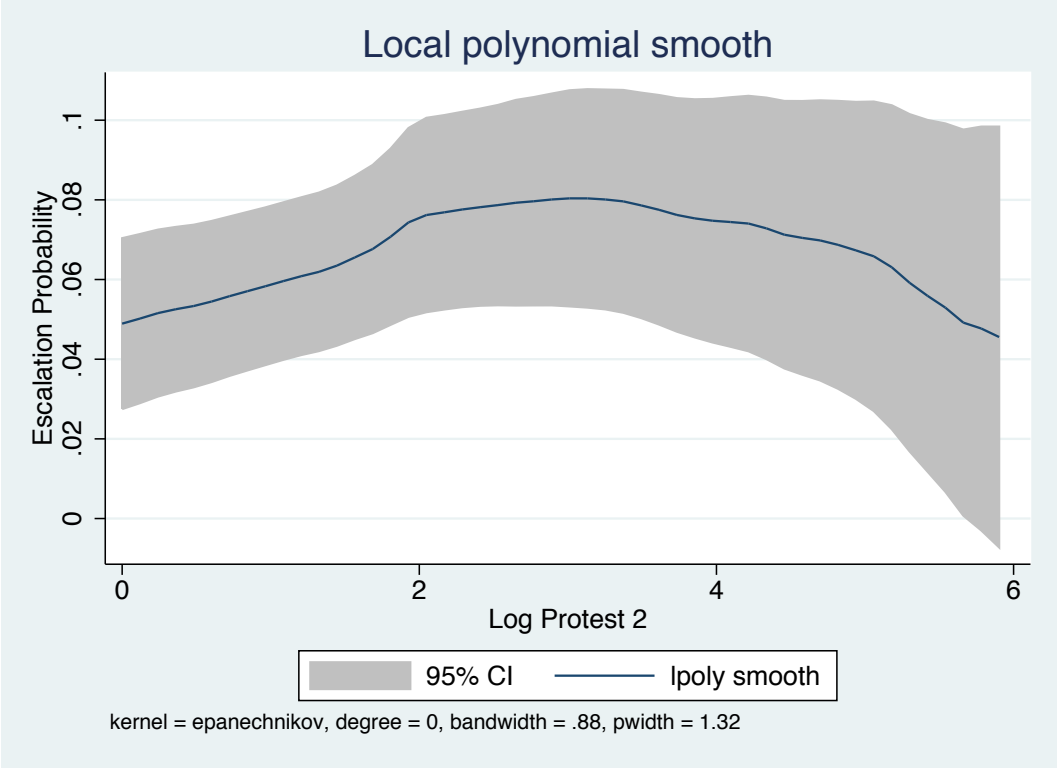


Figure 1.10 Escalation versus Protest size

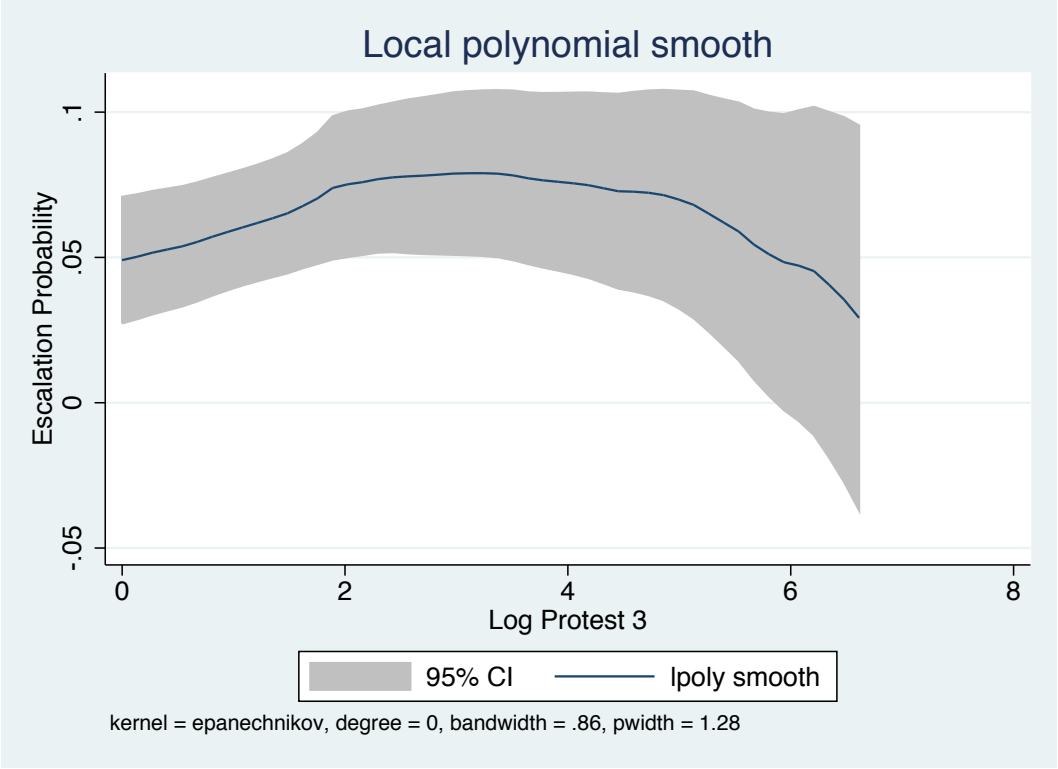


Figure 1.11 Escalation versus Protest size

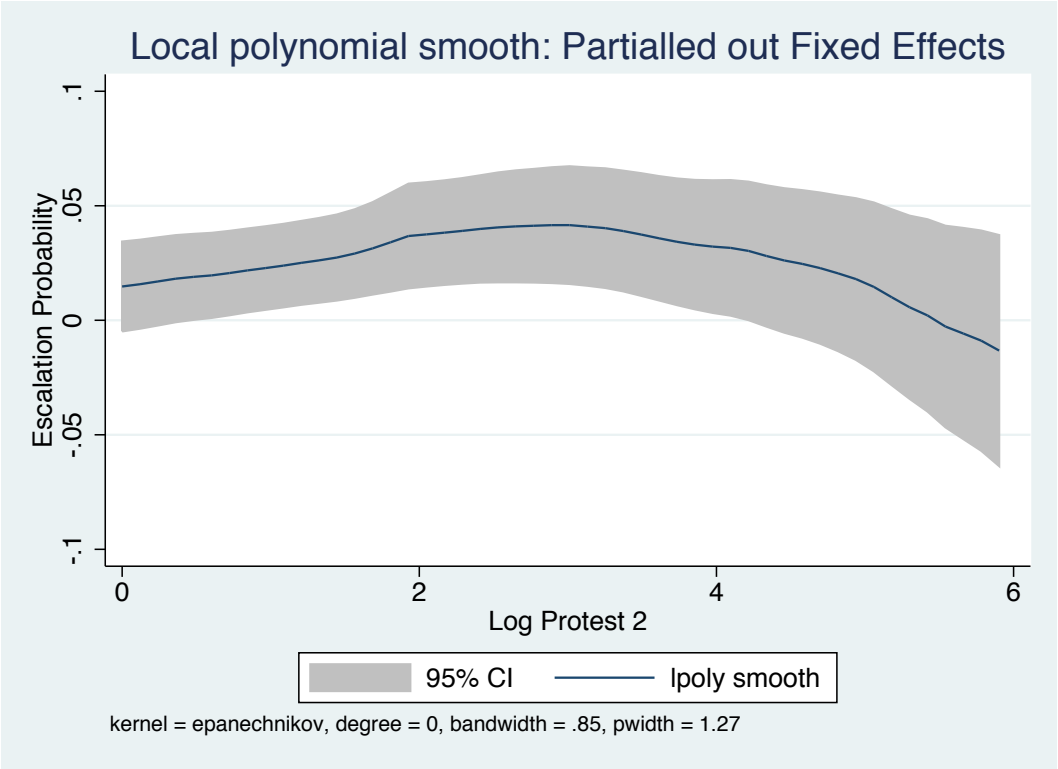


Figure 1.12 Escalation versus Protest size

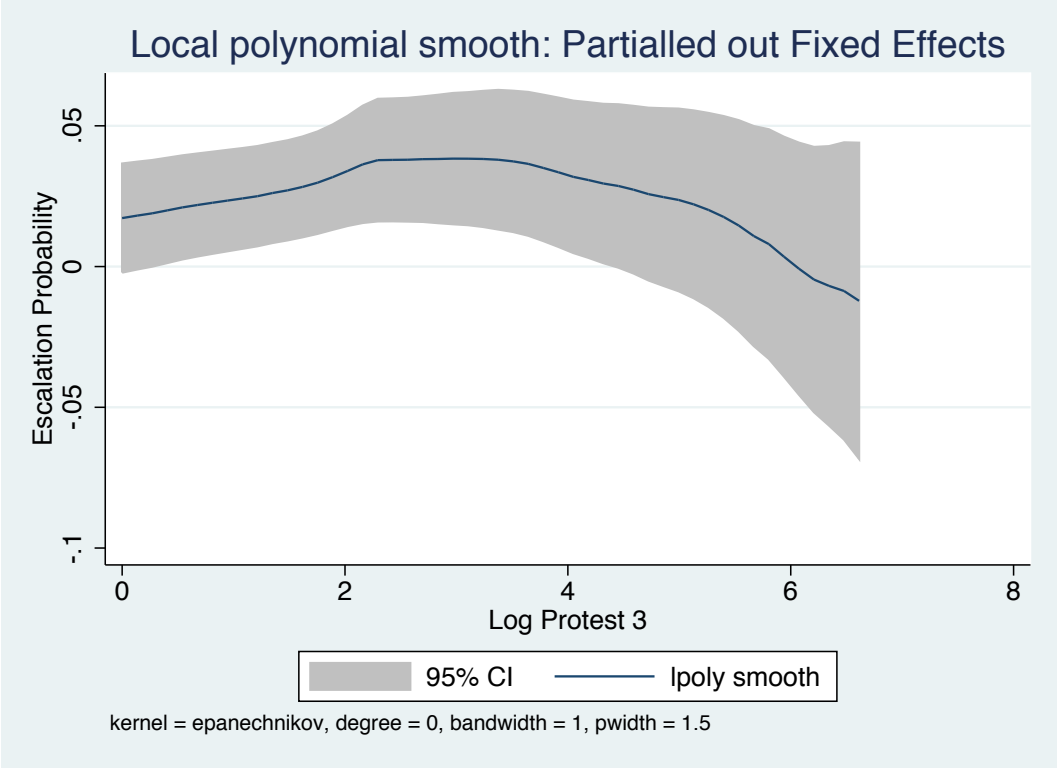
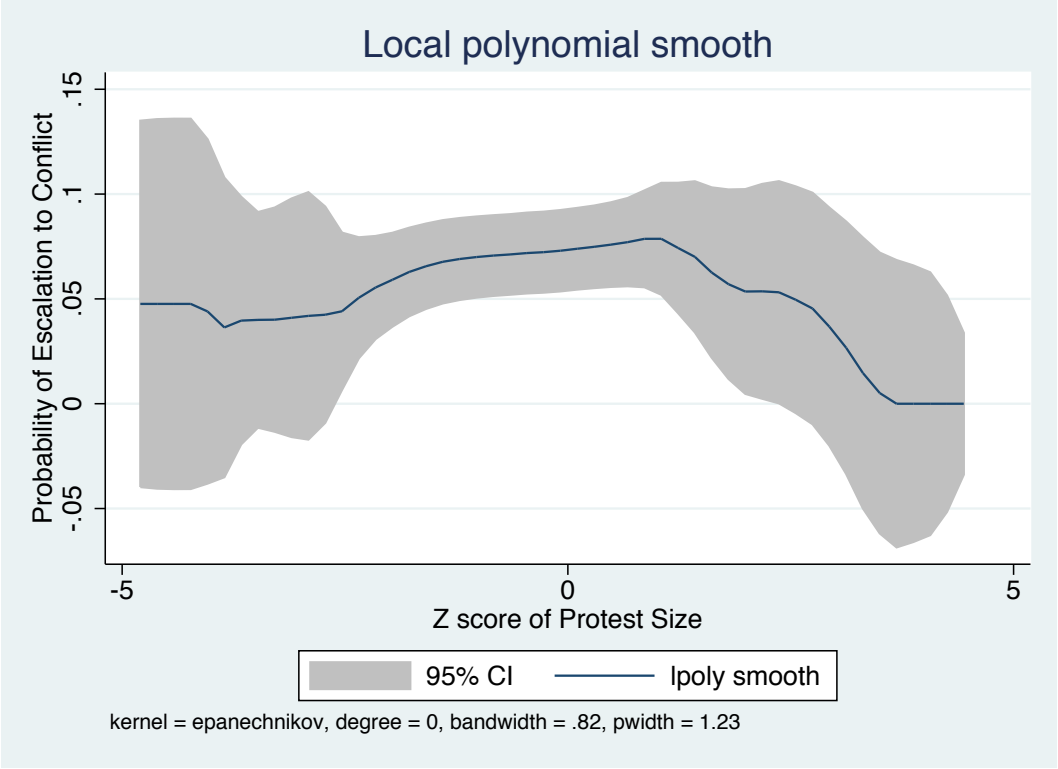


Figure 1.13 Protest Size and Conflict Escalation



APPENDIX B

TABLES FOR CHAPTER 1

Table 1.1 Political Dissent Data

Variable	Obs	Mean	Std. Dev.	Min	Max
Protest 1	1101	4.74	11.50	0	282
Protest 2	1101	22.72	46.59	0	366
Protest 3	1101	30.70	77.26	0	884
P3 Per capita	1101	0.37	1.17	0	17.33
log of P3	1101	1.77	1.80	0	6.78
Protest Indicator	1101	0.53	0.50	0	1
Conflict Indicator	1101	0.22	0.41	0	1
Means Excluding Conflict Periods					
Variable	Obs	Mean	Std. Dev.	Min	Max
Protest 1	858	4.62	12.10	0	282
Protest 2	858	22.58	47.51	0	365
Protest 3	858	31.77	83.19	0	884
Protest Indicator	858	0.68	0.47	0	1

Table 1.2 Freedom House Ratings

Variable	Obs	Mean	Std. Dev.	Min	Max
FH Political Rights	1101	4.67	1.81	1	7
FH Civil Liberties	1101	4.40	1.37	1	7
Concession Political Rights	1101	0.10	0.30	0	1
Concession Civil Liberties	1101	0.11	0.31	0	1
Concession FH	1101	0.17	0.38	0	1
Net Change Both Ratings	1101	-.08	1.02	-6	9

FH Political Rights, and FH Civil Liberties are the mean values of the actual Political rights and civil liberties ratings, 1 represents the greatest degree of freedom. Concession, is an indicator for concession, an improvement in the respective rating, with Concession FH implying an improvement in either rating.

Table 1.3 Protest Size and Concessions

Concession Measure	Net Change in FH Rating										Lagged Net Change	Increase Indicator
Log Protest 1	0.13***				0.16**		0.09*	0.128*	0.0976**	0.0728	-0.0412	0.0813***
	(0.05)				(0.07)		(0.05)	(0.07)	(0.04)	(0.10)	(0.07)	(0.03)
Log Population	-1.09	-0.97	-0.97		-2.74**	0.01	-0.0296	-2.772**	-0.825	-1.089	1.027	-0.605*
	(0.76)	(0.75)	(0.75)		(1.11)	(0.03)	(0.03)	(1.08)	(0.79)	(0.76)	(0.65)	(0.31)
Log Protest 2		0.07**										
		(0.03)										
Log Protest 3			0.07**									
			(0.03)									
Protest 3 PerCapita				0.07*								
				(0.04)								
Log Protest 1-Country Demeaned						0.19***						
						(0.07)						
Time Trend								0.04				
								(0.03)				
Economic Shock									-0.04			
									(0.23)			
Log Protest 1-Square term										0.02		
										(0.04)		
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes	Yes
Inclusion	1	1	1	1	2	2	2	2	1	1	1	1
Observations	818	818	818	818	550	550	550	550	768	818	820	583
R-squared	0.044	0.043	0.044	0.039	0.072	0.058	0.051	0.036	0.034	0.044	0.04	0.06
Number of Countries	47	47	47	47	44			44	46	47	46	45

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 1.4 Protest Size and Escalation to Conflict

Outcome:	Conflict Onset	Conflict Onset	Conflict Onset
Protest Measure	1: Events	2: Event Days	3: Event Location-Days
Log Protest	0.02	0.043**	0.037**
	(0.03)	(0.02)	(0.02)
Log Protest Squared	0.00	-0.009**	-0.007**
	(0.01)	(0.00)	(0.00)
Log Population	-0.24	-0.26	-0.26
	(0.24)	(0.24)	(0.24)
Observations	818	818	818
R-squared	0.025	0.032	0.031
Number of Countries	47	47	47
Robust standard errors in parentheses			
*** p<0.01, ** p<0.05, * p<0.1			

Table 1.5 Transition Rates

	Peace t+1	Protest t+1	Conflict t+1
Peace t	0.57	0.38	0.05
Protest t	0.15	0.78	0.08
Conflict t	0.06	0.16	0.78

A period is classified by the most extreme political dissent to have occured, any protest event in SCAD qualifies as protest, and PRIO's minor conflict threshold is used for conflict.

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CHAPTER 2
Electoral Violence: An Empirical Examination of Existing Theories

2.1 Introduction

Elections are intended to provide a peaceful method for groups to compete for political power, sometimes directly replacing violent alternatives. However, significant debate exists over how elections can be used in post-conflict, or conflict-prone, societies as a solution to violence (Chandra, 2001: Reilly, 2002). In situations where political parties have not yet accepted the rules of the game set forth by elections, violence is theorized to be effective at manipulating election results (Collier and Vicente, 2012; Machado et al., 2011; Robinson and Torvik, 2009). Although many theories of election violence exist, they and the effectiveness of electoral violence remain largely untested (Hickman, 2009). This paper critically examines two existing theoretical frameworks for how violence is used to influence election outcomes.

This paper, like others, such as Goldsmith (2015) and Höglund (2009), focuses on election violence as separate from other types of political violence and starts with the following broad definition of election violence from Fischer (2002):

> Any random or organized act or threat to intimidate, physically harm, blackmail or abuse a political stakeholder in seeking to determine, delay or to otherwise influence an electoral process.

With such a broad definition of electoral violence and so many possible reasons for its occurrence, it is necessary to refine the focus of electoral violence for this paper. We choose to focus

on violence as a tactic of voter manipulation and examine in what way patterns of electoral violence are consistent with the strategic use of violence to influence the outcome of an election by the purposeful targeting and attempted exclusion of particular groups of voters. This type of electoral violence has been the subject of significant and varied academic work, including case studies, empirical work like in Hickman (2009), and the two theoretical papers we focus on: Chaturvedi (2005), hereafter CH, and Collier and Vicente (2014), hereafter CV. These papers outline two different frameworks regarding how violence may be used to influence election outcomes.

In CH, violence is targeted at opposition supporters with the intention of preventing them from voting. In CV, a more nuanced possibility is examined; they argue violence is not targeted directly at the opposition but instead that the use of untargeted violence disenfranchises unaligned voters, who have less at stake in the election. When unaligned voters cease voting due to violence, more weight is placed on each party's core supporters. Thus, if one party starts with an advantage in core supporters, relative to their support among unaligned voters, they will increase their vote share by using untargeted violence.

There is limited empirical evidence on how violence impacts individual voting behavior, with the exception of a few studies like (Bratton, 2008; Collier and Vicente, 2014; and Hickman, 2009), focusing on single elections. For this reason, we focus on examining how individuals respond to election violence. We do this using survey data from Afrobarometer and the conflict event data from the Social Conflict in Africa Database (SCAD). This allows us to examine a broader sample of countries and heterogeneity in response to, and fears of, violence. Understanding how electoral violence influences behavior in elections is a fundamental part of unraveling the mechanisms which drive electoral violence and will allow us to refine existing theories.

The Afrobarometer round 4 survey of 20 countries was intended to measure political attitudes across Africa (Afrobarometer Data, 2008). We use it to measure perceptions of violence, political participation, and political affiliation. This is among the first papers to use survey measures of violence, and we argue this provides several potential advantages over traditional event count measures, especially when we are interested in understanding how individual characteristics are

associated with different perceptions of violence. We also compare measures of violence from the survey to more traditional event count data. Although there is a strong correlation between these measures, we do not believe they measure the exact same phenomena. We argue that asking individuals about their fear of being victimized is well suited for analyzing who is impacted, or targeted, by violence. This measure shows significant variation between respondents, even within small geographical areas where all individuals are likely to observe the same occurrences of violence. In other words, the occurrence of events does not necessarily result in all individuals in the area fearing violence. Therefore, the survey responses may be capturing individual differences which are most likely the result of some individuals having experienced targeted violence.

Leveraging these unique measures of violence, we are able to examine how well reactions to violence align with the frameworks presented in CH and CV. Consistent with the framework provided in CH, which argues that individuals targeted with violence will cease voting, any individual who fears violence is less likely to vote. Additionally, consistent with the story of untargeted violence in CV, we find that swing or unaligned voters are the only group more likely to abstain from voting in the presence of violence, even when they do not report a greater fear of violence. This leads us to conclude, consistent with both frameworks, that targeted violence could cause abstention among voters of any political affiliation, whereas untargeted violence is only likely to reduce turnout among unaligned voters.

When examining who fears violence, rather than how individuals react to violence, we find an ordering of those fears based on political affiliation. Supporters of the majority or ruling party fear violence the least, followed by swing or unaligned voters, while supporters of other political parties report the highest fear of violence in elections. A number of theories of election violence could support these findings; however one explanation consistent with this finding would be the use of the state security apparatus to repress competition. We argue that more detailed data on the actual events would be the best way to determine why this ordering occurs in the data.

Finally, we turn to examining where violence occurs and how that relates to the division of the electorate. We find that districts with a higher fraction of unaligned voters also have more

occurrences of violence. This implies that although both frameworks are potentially viable, the one laid out by CV appears to better explain the patterns observed in the data. Further work is needed to investigate what conditions determine when each framework is more likely and to rule out other potential explanations.

This paper makes considerable progress in explaining who reacts to election violence and how. Given the prevalence of violence in elections and the debate over how "electoral engineering" can be used to help mitigate violence in elections, having a thorough understanding of how violence influences electoral behavior is important if we are to try to mitigate its impact.[1] Furthermore, suggestive evidence supports the idea that even in societies where there may be elections, the use of election violence may be preventing serious opposition from forming. This implies international responses should be strong when obvious infractions are observed. Otherwise, this potentially effective strategy of violence is likely to persist.

[1]For a thorough comparison of the two leading theories of "electoral engineering," and accompanying examples of their success and failure see Reilly (2002).

2.2 Literature Review

Elections play a fundamental role in democratic governance, "They [elections] provide legitimacy through direct popular participation, and, in turn, legitimacy creates capacity for effective governance" (Brown, 2003). Violence in this process, then, has the capacity to undermine not only the elections, but also the legitimacy of the government itself. A large body of work has examined the use of elections in post-conflict societies; however, as the Afrobarometer data, and previous research show, some violence is quite common in African elections, Goldsmith (2015), Lindberg (2006), and Straus and Taylor (2009). Given the importance of elections and the high degree of suffering violence can cause, it is not difficult to motivate the need to understand electoral violence.

2.2.1 Can Elections Replace Violence?

In post-conflict societies, elections are often intended to trade 'bullets for ballots'; however, as pointed out in Rapoport and Weinberg (2000), succession is the most turbulent time for any type of government. A great deal of research has considered the consequences of "electoral engineering", or how an electoral system can mitigate or encourage the use of violence based on election results. Although, violence is not the only dimension affected by the choice of electoral system, as pointed out in Norris (2007): "Electoral rules are not neutral." The combined impact of violence and the electoral system needs to be evaluated for how it influences representation, especially for contentious groups in conflict-prone societies. In such situations, the violence may not be limited to elections; in fact, a key condition for recurrent civil conflict discussed by Walter (2004) is the lack of a non-violent method to influence governance.

There are two major theories on how elections can be used to mitigate conflict; both of these theories are discussed at length in Reilly (2002). The first, consociationalism, argues all groups should be given a fair voice in governance. The theory suggests a proportional representation system may alleviate conflict by giving groups adequate representation. The alternative theory argues that doing so encourages division along existing ethno-religious divides and does nothing

to address underlying issues. Instead they suggest a reciprocal-vote approach, which is intended to foster inter-group cooperation[2].

2.2.2 The Impact of Violence

The existing research examining the direct link between election outcomes, electoral participation, and violence is limited to a few studies analyzing particular countries in detail. The first, Hickman (2009), examines the impact of violence on voter turnout and election results in Sri Lanka. He finds violence perpetrated by individuals associated with one political party, leads to a reduction in turnout for the opposition in that district. Hickman argues that the impact is small, did not change the election results, and that the use of violence by both sides cancels out in the aggregate. Another paper using the Afrobarometer data from Nigeria, shows that vote buying is far more effective than violence (Bratton 2009). Bratton also demonstrates that the most common response to any form of "illegitimate campaigning", e.g. violence or vote-buying, is abstention. We extend this literature by examining multiple countries and looking at heterogeneity in the impact of violence.

Empirically, Blattman (2009) and Collier and Vicente (2014) provide the most convincing causal evidence of the impact of violence on political participation. The first, Blattman (2009), uses variation in exposure to violence in Uganda, which he argues to be exogenous, to estimate the impact of that exposure on political participation. He finds that having been exposed to violence, nearly two decades earlier, makes an individual more likely to be involved in the community and more likely to vote. Collier and Vicente (2014), use random placement of anti-violence campaigns during the 2007 Nigerian election to create exogenous variation. The campaign successfully reduced election violence in the regions of implementation, and they found, like we do, that violence reduces voter turnout.

The timing of violence in each study likely explains their contradictory conclusions. In Blattman (2009), the exposure to violence is almost two decades earlier, and increases political participation. In Collier and Vicente (2014), the violence is current, and they find it decreases voter turnout. The

[2]For a thorough examination of these issues as they relate specifically to African countries, see Lindberg (2005).

difference could be explained by differential long and short run effects. In the long run observing violence may make an individual wish to be more involved in politics because of past experiences. However, in the short run, it may make the act of voting too dangerous or costly.

For such a prevalent topic, relatively little empirical literature exists. Data limitations are likely the primary reason for this. Furthermore the irregular nature of elections in many countries, especially where election violence is a common problem, yields serious estimation issues. Some of these issues are discussed in Goldsmith (2014) and Cheibub et. all (2012). We address some of the issues related to data limitations, by exploring the use of more general political violence data to measure election violence and demonstrating that survey data can be used to measure election violence.

2.2.3 Causes and Types of Electoral Violence

Understanding the causes of electoral violence requires a solid understanding of what we mean by electoral violence. Broadly speaking, this paper uses the definition from Fischer (2002). Another paper Kehailia (2014), published as part of a series of case studies on election violence from practioner experience at International Foundation for Electoral Systems (IFES), (Cyllah et. all 2014), lays out a useful typeology of election violence. This typology classifies election violence into eight types, based on who is involved in the incident. The examples he provides for each type, demonstrate how different types of electoral violence can have fundamentally different causes, and be used for different purposes.

Such a typology is useful when studying election violence, especially when considering the underlying causes or intent of election violence. However, even within a single type the causes, goals, and purpose may differ. This paper focuses on election violence where the intent is vote deterrence, specifically strategically manipulating who votes with the intent of influencing the election. Even in such a narrow range as vote deterrence, the purpose of doing so may not always be manipulating the outcome on polling day; some groups may simply have the intent of undermining the electoral process as a whole. Take one of the examples from (Kehailia 2014), where unidentified

gunmen opened fire on a polling station in the Democratic Republic of the Congo (DRC), with supporters of both parties present. As we will show, this could be consistent with the models that follow assuming they could infer that the voters in attendance were far more likely to support one party over the other, but actions like this could also be intended to undermine the electoral process. Nonetheless, focusing on who perpetrates acts of electoral violence and their intended target is an illuminating thought process, and a crucial missing piece in currently available data.

We are unaware of a complete survey of the causes of election violence exists at this time, though an excellent summary can be found in Goldsmith (2015). We also discuss several potential causes of electoral violence that are beyond the scope of this paper. One commonly recurring message throughout the literature written by election practioners is this so-called culture of impunity, mentioned in Bekoe (2011) and Cyllah et, all (2014). Basically, time and time again countries in Africa experience some level of election violence, form committees to investigate the occurrences, and make few prosecutions or fail to create independent bodies with the power to enforce punishments. These actions set the stage for violence to occur again.

Group affiliations and ethnicity are often used by parties to encourage violence. For example, politicians in the 2011 election campaign in the DRC refferred to members of a political group aligned with an ethnic minority as "mosquitoes" and advocated to "spray some insecticide." In Nigeria former president Olusegun Obasanjo said the election was a "Do-or-die" affair. So often violence is encouraged publicly from leading political figure heads, who use existing ethnic tensions to divide and conquer. A sizeable academic literature has also suggested that election violence often mirrors existing ethnic, religious, and political tensions in society. As stated in Bates (1983), "electoral competition arouses ethnic conflict."

A final use of electoral violence we will discuss briefly, one which this paper's results suggest is plausible, is government repression of potential competition. This relates to a classical example of rational conflict, the first-strike advantage. This can be summarized by arguing that even if the use of violence is costly now, if it permanently eliminates competition it may be strategically viable. There are countless examples of candidate assassinations and unjust prosecution. One recent

example of such behavior includes the arrest of two opposition candidates before their political rallys in Uganda's 2015 elections Bariyo (2015), though this particular abuse of state resources was non-violent it is a good example of how state backed political parties can abuse the system in their favor. Nigeria's 2007 elections represent another, more violent, example of the abuse of states resources during an election campaign. The elections resulted in about seventy deaths, which according to Suberu (2007), included assassinations. This sort of manipulation is the subject of several theoretical papers, which address abuse of the state security apparatus to repress competition in elections. It is easy to see how preventing competition from forming may be one way in which semi-autocratic governments can stay in power via the use of elections.

Even though electoral violence is distinct from political violence in general, it is clear that it can take many forms, and occur for many reasons. This paper focuses on two theoretical frameworks that clarify how violence may be used to manipulate the outcome of an election. Before turning to a detailed discussion of these papers, we discuss related theoretical papers on political competition, which incorporate violence.

2.2.4 Theories Political Competition and Violence

The theories we focus on are not the only possible explanations for electoral violence, and our findings potentially lend support to a variety of theories. This investigation suggests that further research will be needed to determine the conditions that make a particular theory fit a country or situation better than another. One paper of particular interest Ellman and Wantchekon (2000) introduces the threat of violence by a third party into a traditional Hoteling model of political competition. In this model, the threat of violence by a third party can influence the party's policies and how individual voters vote, without actually occurring. We think this paper complements our own finding, that the fear of violence alone is sufficient to influence voting behavior. Another paper Robinson and Torvik (2009), focuses on the related question of which group of voters a party benefits by intimidating, undecided voters or opposition supporters. They derive a series of propositions that analyze how competitive the elections are with and without particular groups of

voters participating in the election and conclude that swing voters are usually the optimal target for violence.

Incorporating violence into traditional models of political competition, as is done by many of the models discussed in this paper, can be useful to understand the mechanisms driving its occurrence. The papers highlighted in this section, and examined more thoroughly in the following section are important steps in that direction. We feel that much of the existing theoretical literature takes too narrow a view of election violence, by examining only a single country. We perhaps step too far in the opposite direction, by examining many countries; however, we feel this is an important step towards understanding how generalizable existing models are for studying election violence.

2.3 Models and Hypotheses

This section contrasts how violence influences electoral behavior in two leading models of election violence; Chaturvedi (2005), hereafter CH and Collier and Vicente (2012), hereafter CV. With few papers aside from Bratton (2008) and Goldsmith (2015) which examine how violence influences voting behavior and frameworks which produce differing predictions about the use of violence in elections we argue this is the natural starting point for investigating election violence.

The first model, CH, sets up a two-party winner-takes-all election where each side attempts to maximize the difference between their expected vote shares. In the second model, CV, each side seeks to maximize their vote share. In both models, violence is used to influence the election results by changing the composition of the electorate which votes on Election Day. The models differ in who abstains from voting following the use of violence.

In CV the assumption made is that if violence occurs, all swing voters would abstain from voting. In contrast to this assumption, CH assumes that violence is directly targeted; each party targets the opponent's core supporters and swing voters are not directly impacted by violence. Each model describes and sets up swing voters in a slightly different fashion. We take liberty in altering and simplifying these models to better facilitate comparison.

The core assumption in both models is that violence can reduce voter turnout, we turn this into our first hypothesis.

Hypothesis 3. *Violence, perceived, threatened, or actually occurring, reduces the likelihood of an individual voting.*

As discussed, these two frameworks provide two different ways in which violence may be used to influence voting behavior. The first framework assumes that anyone who is targeted by violence will be less likely to vote as a result of fear and intimidation. This framework is used in Chaturvedi (2005), where they assume that violence is targeted directly at opposition supporters. The second framework from Collier and Vicente (2012) assumes that instead of targeting individuals with violence it may be used to create havoc in an area and certain groups of people, specifically unaligned

voters, would be more likely to abstain from voting in the presence of violence than other groups, such as voters with a strong political affiliation. The Afrobarometer data, complemented by the Social Conflict in Africa (SCAD) data, allows us to examine both possibilities, if the presence of violence generally or fears of targeted violence influence voting behavior.

If violence is used as a scare tactic, intended to disenfranchise large groups of swing voters[3], as in the CV model, we would expect to see differential reactions to violence. CV assumes that swing voters, since they care least about the outcome, will have stronger reactions to violence. This leads to our second hypothesis.

Hypothesis 4. *Individuals without a strong political affiliation should be less likely to vote, if they expect or fear violence, than those with.*

The framework in CH argues that core party supporters are targeted by violence, and therefore cease voting. If this is the case, we would expect to see core party supporters reporting a greater fear of violence in elections, this leads to our next hypothesis.

Hypothesis 5. *Individuals with a strong political affiliation should be more likely to fear violence during an election.*

Starting with a solid understanding of how people react to the threat of violence during elections is essential if we wish to design empirical models capable of predicting its occurrence.

[3]The rational for this is explained in the following section, the intuition for who wishes to do this relates to who has more core supporters in an election who will continue to vote even when violence occurs

2.4 Comparing Simplified Models

The preceding section discussed the assumptions made with regards to how individuals react to violence in the two models. This section examines simplified versions of these models focusing on how violence impacts each party's vote share, while removing the alternatives present in the original models. This simplification facilitates a more direct comparison of the models.

2.4.1 Collier and Vicente Model (CV)

Each political party, $\mathcal{C}$, the challenger and $\mathcal{I}$, the incumbent, wish to maximize its share of the votes. A unit mass of voters are divided into three groups, C, I, & S. C, is the fraction of voters supporting the challenger; I, is the fraction of voters supporting the incumbent; and S, is the fraction of unaligned or swing voters. If neither candidate were to take any actions, a fraction of swing voters, α, would vote for the incumbent, and $(1-\alpha)$ for the challenger. Voters in group C and I always vote for their respective candidate.

Each party has the ability to use violence in an election to intimidate swing voters. Core supporters, C and I, are not affected[4]. We assume that violence has a linear impact on voter participation, after violence, of level v, the fraction of voters who continue to vote is $(1-v)$. This implies that if swing voters are targeted, with violence of level v, then $(1-v)S$ of them vote.

Without loss of generality (WLOG), we focus on results for the incumbent, we can do this because each party faces an opposite but symmetric set of choices. Absent violence, the incumbent's vote share is,

$$VoteShare = \frac{I+\alpha S}{C+I+S}. \tag{1.1}$$

Including violence, $\mathcal{I}$'s vote share is

[4]One could interpret this as coming from a cost-benefit calculation: with the amount of violence needed to deter core supporters being too costly.

$$VoteShare = \frac{I + (1-v)\alpha S}{C + I + (1-v)S} \tag{1.2}$$

Note that violence is not indexed by party, rather the total amount of violence between both parties is what matters. However, only one side will use violence in equilibrium under these assumptions.

The derivative, or gain in vote share from violence, after some simplification is:

$$\frac{\partial VoteShare}{\partial v} = \frac{((1-\alpha)I - \alpha C)S}{(C + I + (1-v)S)^2} = MB \tag{1.3}$$

Rearranging shows that the incumbent will raise their vote share using violence iff,

$$\frac{1-\alpha}{\alpha} > \frac{C}{I}. \tag{1.4}$$

Assuming a constant marginal cost of violence, γ, the following proposition determines the equilibrium level of violence,

Proposition 3. *Party I will set $v = 1$ iff: their vote share increases by excluding the average swing voter, and the marginal benefit to excluding all swing voters exceeds the marginal cost,*

$$\frac{((1-\alpha)I - \alpha C)S}{(C + I + S)(C + I)} > \gamma \tag{1.5}$$

The proof is as follows, if $\frac{1-\alpha}{\alpha} > \frac{C}{I}$, then $\mathcal{I}$ gains vote share by disenfranchising swing voters. This implies the opposition party would not use violence because of the modelŠs symmetry. The second condition, $\frac{((1-\alpha)I-\alpha C)S}{(C+I+S)(C+I)} > \gamma$ determines if the total benefit exceeds the total cost. If both conditions hold, $\mathcal{I}$ gains enough vote share for violence to appear to be a viable strategy. The intuition for this is that more weight is placed on core supporters as swing voters stop voting. So a party would only wish to use violence, and disenfranchise unaligned voters, if they have a greater advantage among core supporters then they have among swing voters.

A number of interesting predictions can be derived from this model. The impact of violence

on a party's vote share increases in S, the fraction of swing voters. This is the result of more voters being discouraged when violence occurs. The impact of violence also increases in own party support. This is because as swing voters leave the election, due to violence, more weight is placed on the ratio of core supporters. Finally, the impact of violence decreases in opposition support, C as set-up above, and in the fraction of swing or unaligned voters supporting the perpetrating party, α as set-up.

2.4.2 Chaturvedi Model (CH)

Two main differences occur when examining the CH model, first the goal of each party is to maximize their plurality rather than their share of the votes; CH defines plurality as the difference in expected votes. Second, violence is targeted at the oppositions support base. In order to preserve the predictions from the original model, we continue to maximize the plurality.

The composition of the voters remains the same, a unit mass of voters are divided into three groups, C, I, & S. C, is the fraction of voters supporting the challenger; I, is the fraction of voters supporting the incumbent; and S, is the fraction of undecided or swing voters. If neither candidate were to take any actions, a fraction of swing voters, α, would vote for the incumbent[5].

In this model violence is indexed by the party using it, because it is targeted, again WLOG we focus on the incumbent's decision to use violence. The incumbent seeks to maximize their plurality, difference in vote share. This can be derived, for the incumbent, as follows:

$$\textit{Incumbent Votes} = (1 - v_C)I + \alpha S \tag{1.6}$$

$$\textit{Challenger Votes} = (1 - v_I)C + (1 - \alpha)S \tag{1.7}$$

$$\textit{Plurality} = (1 - v_C)I - (1 - v_I)C + (2\alpha - 1)S. \tag{1.8}$$

[5]This last assumption is the biggest divergence from the original model, however it leaves the direction of the predictions unchanged.

We see immediately that the return on violence is proportional to the opponents support base,

$$\frac{\partial Plurality}{\partial v_I} = C. \tag{1.9}$$

This simplified model does lose some of the nuanced predictions of the original model; however, the core predictions remain. The impact of violence increases with the oppositions vote share, and the use of violence increases as the fraction of swing voters decreases. Both of these predictions are opposite those in the CV inspired set-up.

Contrasting this with the CV model, we see that instead of only one side using violence, both sides always use some violence[6]. This is because as long as the opponent has some supporters in the election, eliminating some of those supporters would raise the perpetrating party's plurality. In other words, in this model both sides always have a positive marginal benefit from violence, whereas only one side has a positive marginal benefit in CV. Even with these simplifications, we are able to see how different assumptions about who is targeted by, and reacts to, violence can lead to drastically different predictions for its use. We turn to discussing these differences in the next section.

2.4.3 Hypotheses from Baseline Models

Swing voters receive a great deal of attention in models of electoral competition, and are a primary focus of both models of election violence highlighted in this paper. They are also one of the primary distinctions between the two models. The intuition for this difference is also fairly straightforward. In the CV model as the fraction of swing voters increases, eliminating them from the election places greater weight on each party's core supporters, so whichever side has a stronger advantage in core supporters then in unaligned supporters stands to gain more by excluding undecided voters[7]. The CH model makes the opposite prediction, as the fraction of swing voters increases the return to

[6] Adding a cost of violence to this simplified model could change that, for example with a constant marginal cost we could see no violence being used if that cost was too high.

[7] This prediction assumes that the dominant parties predicted fraction of voters among swing voters is less than their fraction of core supporters, if this does not hold the weaker party would use violence. The resulting prediction that violence increases with the fraction of swing voters would remain unchanged.

violence decreases. In the simplified model, the intuition for this is that a reduction in swing voters implies an increase in core supporters. This leads to the following hypotheses comparing the predictions of the two models:

Hypothesis 6. *Observed violence should increase (CV), or decrease (CH) as the fraction of swing voters in the electorate increases.*

One key difference between these models, aside from who the models assume should be targeted with violence, is the difference in objective functions: maximizing the difference in votes versus the maximizing vote share. In part, testing between these models would also be testing the functional form differences in them. However, the main research question in this paper focuses on examining whether reactions to violence are consistent with the assumptions in either framework, not specifically on which model or objective function's predictions better fit the data. So, although the differing objective functions do drive some of the predictions in each model, we focus on examining whether reactions to violence are consistent with either of these theories. Specifically, do unaligned voters show stronger reactions to violence, and does directly targeted violence dissuade aligned voters from voting?

2.5 Data Sources

The primary data sources used in this paper are the round 4 Afrobarometer (AB) surveys and the Social Conflict in Africa Database (SCAD). The AB surveys provide a unique look at political sentiment across 20 African countries. They included a variety of questions related to elections, political participation, and election violence. The SCAD data allows survey measures of violence to be compared to more traditional event data. Combined, these data sources provide a unique perspective to examine election violence.

2.5.1 Political Affiliation

In the theories discussed in this paper, political affiliation is a key determinant of how an individual reacts to electoral violence. Furthermore, the theories use division of the electorate as an important predictor of electoral violence. The AB survey provides two potential measurements of political affiliation, which can also be used to estimate division of the electorate. The preferred measure is created from the following questions. The survey first asks, "Do you feel close to any particular political party?". If the respondent answered yes, they were then asked, "Which party is that?" Respondents who said "No", to the first question are classified as unaligned voters. Respondents who provided a specific party identification are then placed in one of three groups: 1) those who support the party with the highest number of responses in the country, 2) those who support the party with the second highest number of responses, and 3) those who support any other party.

The survey asks another related question, "If a presidential election were held tomorrow, which party's candidate would you vote for?" Respondents are divided into an equivalent set of groups using this measure. We focus on the first set of questions for two reasons. First, the theories care about party supporters, and the first set of questions asks directly about party affiliation. Second, we show that there is a high degree of overlap between the measures. Finally, using information gathered from the Inter-Parliamentary Union, we coded the question "Which party is that?" as 1 if the ruling party or coalition was indicated or as 0 if it was not.

Table (2.1) displays summary statistics on political affiliation. Slightly over one-third of respondents support the strongest political party, *Majority Party*, almost a three to one ratio when compared to the next largest party, *Second Party*. When examining which party individuals claim they will vote for, this ratio rises. Table 2.2, demonstrates considerable overlap between the two measures of political affiliation (i.e., support and voting). The exception to this is that many individuals may not have a strong political affiliation with a party but still know which party they would vote for.

Both measures share one particularly problematic weakness: political intimidation may have swayed an individual's reporting behavior in one of two possible ways. Those who are fearful of intimidation may be more likely to report an affiliation with the intimidating party or refuse to report one at all. To examine whether the reported political affiliation seems to be biased by intimidation, we leveraged questions available in the AB survey.

The AB data contains a wide variety of questions related to the political atmosphere in each country. We are able to use this to investigate whether individuals may be misreporting their political affiliation because they fear repercussion from admitting an affiliation with minority political parties. In particular, we focus on one question from the survey, "how often: do people have to be careful of what they say about politics?", and examine its correlation with an individual's political affiliation. If individuals are afraid to accurately report their political affiliation, we may find that people who feel they must be careful what they say about politics are more likely to report an affiliation with the ruling party or to report no affiliation at all. The OLS regressions in Table (2.3) examine this possibility.

We find that the more careful an individual feels they must be about what they say, the less likely they are to report being a member of the ruling party. This provides evidence that despite their need to monitor how they talk about politics, people are not misreporting their political affiliation in favor of the ruling party. When examining the second possibility, we do see that individuals who feel they must "always" be careful what they say are slightly more likely to be categorized as an unaligned voter by our primary measure; however, we find no such relationship with the alter-

native measure of political affiliation which asks which party they would vote for. This evidence suggests that the efforts by the survey enumerators to instill trust when discussing such contentious topics must have alleviated at least some concerns among a majority of respondents.

2.5.2 Political Participation

To measure political participation we derive two variables from the AB data. The first is retrospective and is the primary variable of interest. The second is hypothetical and is derived from the same question as our second set of political affiliation questions. The two variables are as follows:

- *Voted*: Reported that they voted in the previous National Election in the country. 72% of respondents report they voted in the last election.
- *WontVote*: When asked, "If a presidential election were held tomorrow, which party's candidate would you vote for?"; respondent reported they would not vote. 8% of respondents report they would not vote in a hypothetical election.

2.5.3 Violence Measure

Of particular importance to this paper is how the AB data were used to measure violence, especially how we capture the difference between targeted and untargeted violence. Most papers trying to predict violence or understand its impact use event data, which counts the number of events reported in newspapers meeting some criteria in a given time and space. We use event data but complement it with two survey measures of violence. One of our measures of violence is fundamentally different from even data. It is a survey response to the question, "During election campaigns in this country, how much do you personally fear becoming a victim of political intimidation or violence?" Respondents could choose one of four categories; "A lot", "Somewhat", "A little bit", or "Not at all". This is then turned into an ordered categorical variable on a zero to three scale, with three corresponding to fearing violence, "A lot". We call this variable, "fear violence". We choose this as our measure of targeted violence because of the way the question was worded,

referring specifically to the individual, but retained it as a measure of targeted violence because it differs significantly from other measures of violence even within small geographic areas, implying that the remaining variation must be attributable individual differences among the respondents. It is possible these individual differences are not the result of targeting, but patterns we see such as political affiliation being significant even after controlling for district fixed effects suggest this is a reasonable measure for targeted violence.

In order to measure untargeted violence, we use the event data from the SCAD and the survey question which asks, "In your opinion, how often, in this country: Does competition between political parties lead to violent conflict?" This, like the previous survey question, is also placed on a zero to three scale. This survey question has slightly more variance explained by geographic heterogeneity with an R-squared of about four percentage points higher, than the other question. Also, as demonstrated in Table 2.5 Column (2), it has a stronger correlation with the event data. For these reasons we argue that the first question measures targeted violence, like in the CH framework, and the second question, along with the event count data, better measures untargeted violence. The term untargeted violence was chosen to highlight the key difference between the models we are focusing on, but neither the data nor the theory requires this violence be truly untargeted. Take this example from a Human Rights Watch report,

> Witnesses reported numerous incidents to Human Rights Watch in which armed thugs, usually though not exclusively from the PDP, shot into the air or otherwise threatened voters with violence, created chaos, and then ran away with the ballot boxes. In some instances, these groups shot directly at individuals from opposing parties. In other cases, their threatening behavior and public display of weapons ranging from knives to firearms was sufficient to scare off their opponents, as well as ordinary voters. (pg. 6)

The untargeted violence in this example is the public display, which impacted all voters. Another way to think of untargeted violence is general or pervasive violence. To measure this we use measures of violence that we can't identify who was targeted, even when the events which caused

the measure to increase may have been targeted. However, for the hypothesis we wish to focus on with these measures of untargeted violence that is fine, because we are interested in seeing if unaligned voters react more strongly to violence, even if it is not directed specifically at them and these measures of violence do not suggest this violence was directed primarily at unaligned voters.

Each approach to measuring violence has its own strengths and weaknesses. The main advantage of using a survey response is that the survey provides measures of each person's perceived level of the threat, individual-level demographic characteristics, as well as each respondent's political affiliation and behavior. Furthermore, not all violence can be captured in event data. For example, threats of harm are one particular category included in the definition of electoral violence that event count data would be incapable of capturing. The ability to control for heterogeneity in small geographic regions and not wipe out our measures of violence with those controls is key to our ability to examine targeted and untargeted violence.

The primary disadvantages of the survey measures relate to the inability to ascertain what experience led a respondent to report a higher level of perceived violence and its possible correlation with individual attitudes. To overcome some of the issues this poses, we use complementary questions on the AB survey and control for a variety of alternative explanations for the observed correlations when we move to our results section.

The following tables summarize and compare the violence measures used in this paper. Of particular interest is how the survey measures of violence and event count data correlate. Following Goldsmith (2015), we use all events in a window around elections from the SCAD data to capture election violence. We use the dates of all elections, which overlap with both SCAD's 1990 - 2012 timeframe and The National Elections across Democracy and Autocracy (NELDA) data's timeframe, which ends in 2010. Table (2.4) shows the means of these measures. Because of the over–dispersion present in the event data, the logged values and indicators are used in analyses[8].

Table (2.5) examines how well the survey measures of violence predict violent events using regional level averages of violence in OLS regressions with country-level fixed effects[9]. The mea-

[8]The actual conversion takes the natural log of the number of events plus one.

[9]The region is usually a countryŠs first-level administrative division

sure, *Fear Violence*, shows a much weaker correlation than *Violent Competition*. This again supports our idea that these measures capture different phenomena and the idea that the "fear violence" measure may be well suited to capturing targeted violence.

2.6 Empirical Investigation

2.6.1 Reactions to Violence: Targeted and Untargeted

Fundamental to any theory of election violence is an understanding of how violence influences behavior in elections. Both of the models assume that violence reduces voter turnout, an assumption which has found validity in other empirical work such as Collier and Vicente (2014) and Bratton (2008). To examine this and the two potential frameworks for how violence influences voting behavior, we explore how self-reported measures of voting in the AB data are related to violence using a variety of violence measures.

Given the unique structure of the available data, careful consideration needs to be given to the method of analysis used. The AB survey was conducted between 2008 and 2009 across 20 different African countries, all taken at times that do not directly correspond with the country's elections, which poses unique data analysis issues. However, cross-country survey data is becoming more commonly used in political science research. Two major examples are the AB surveys which we use and the Latin American Public Opinion Project, Seligson et. all, which examines similar questions in Latin-American countries, e.g., Machado et. all. With the introduction of these data sets, some debate has arisen surrounding their use. Several issues raised include: the validity of pooling results across countries, whether or not fixed effects at the survey or country level are sufficient to deal with heterogeneity, and the proper level at which to cluster standard errors.

To address issues related to country level heterogeneity and the validity of pooling results across countries, we include varying levels of fixed effects: country, region, and district. Country level fixed effects are used primarily to control for major differences across countries which will affect the survey responses, such as the overall atmosphere of violence, the type of electoral system, and amount of time since the last election occurred.

Region and district effects are used to examine variation within smaller geographic areas. Regions correspond to the countries' first or second level administrative division. Regional fixed

effects are used primarily because some variables show little variation at the district level and to contrast the results across levels of fixed effects. The district is related to the Primary Sampling Unit (PSU) and is a cluster of about eight respondents living within relatively close proximity to each other, usually the same town or village. Primarily we focus on specifications with district level fixed effects because all individuals within these relatively small areas are subject to a similar set of experiences and we can then attribute differences in survey responses to variation in individual level characteristics as well as targeting as a result of these characteristics.

Lastly, we address the issue of standard errors by clustering at the regional level; this avoids the common issue of many observations with too few clusters. This level of clustering is also appropriate given that many independent variables are measured at this level.

Finally, to explore the sources of heterogeneity and how they impact the results, we include interaction terms of country level variables with individual characteristics. Doing this allows us to explore some sources of cross-country heterogeneity while controlling for other unobserved sources of country-level heterogeneity. This technique is used instead of comparing the range of coefficients country-by-country, largely because we have too few observations in some relevant sub-populations within specific countries. This leads us to baseline specifications to examine the impact of violence on voting as follows:

$$Y_{ij} = \beta V_{ij} + \gamma_j + \varepsilon_{ij}. \tag{1.10}$$

Where i denotes the individual and j denotes the location (country, region, or district). Y_{ij} is an indicator that equals one if the respondent i reported they voted, V_{ij} is the measure of violence, for individual i, γ_j is a fixed effect, the level of which varies across specifications. Finally, ε_{ij} is an individual error term. Additionally, we examine specifications like the following to investigate the sources of cross-country heterogeneity.

$$Y_{ij} = \beta V_{ij} + \delta V_{ij} * V_j + \gamma_j + \varepsilon_i. \tag{1.11}$$

Where notation remains the same as the previous specification, with the addition of V_j, the country level mean of violence. This appears only as an interaction because the base term is subsumed by γ_j.

Table (2.7) presents the primary results for H1: Violence, perceived, threatened, or actually occurring, reduces the likelihood of an individual voting. These results include no control variables and differing levels of fixed effects across three measures of violence. Control variables, such as demographic factors, are excluded from these baseline specifications because we believe, and will demonstrate, that groups with a lower propensity to vote are often targeted with violence, i.e. unaligned voters. Normally, one would attempt to remove that bias; however, if those demographic factors, like ethnicity, are used to identify an individual's political affiliation, removing the observed correlation would result in a downward bias on our coefficient of interest. For example, if ethnicity is used to identify political affiliation and target an individual with violence, controlling for ethnicity may reduce the impact of violence on voting, by attributing a reduction in the likelihood of voting to ethnicity, when it is due to those individuals being targeted with violence because of their ethnicity.

Different measures of violence used are meant to capture exposure in different ways. As discussed in the previous section, *Fear Violence* is meant to measure targeted violence, whereas the other two capture untargeted violence. Finally, the varying levels of fixed effects are included in order to eliminate idiosyncratic differences across areas and countries that may affect voting behavior. They are especially useful at the smallest level, the PSU, to understand how differing fears of violence within even a small town or village can have differential impacts on voting behavior. It is reasonable to assume that individuals within one of these districts are exposed to a nearly identical set of violent events and that observed differences in responses and behavior are largely attributable to the idiosyncrasies in perceived risk of violence.

Interpreting these results, we find that individuals who report fearing violence "A lot" or "Somewhat" are between three and four percentage points less likely to vote than those who fear violence "A little bit" or "Not at all". This is seen in Table (2.7). When looking at the event data,

the bottom row of Table (2.7), we see that individuals in a region with at least one event reported are a little more than four percentage points less likely to vote. Column (2) in Table (2.7) interacts the individual violence measure with the country level average of the respective measure of violence. For our measure of targeted violence, the mean is insignificant and the results are unchanged. For the measures of untargeted violence, we see an increase in the primary coefficient, resulting in a greater reduction of voting in areas where violence occurs. However, this increase is mitigated by the significant and positive coefficient on the interaction term, in countries where the mean level of violence is greater the effect of an individual event is lessened. We argue this is actually the result of single events causing a greater reduction in voter turnout when violence is less common.

Before addressing concerns about the validity of these estimates, we turn to our second hypothesis: individuals without a strong political affiliation should be more likely to be deterred from the polls by violence. This hypothesis comes from the assumption in the CV model that unaligned voters are the most responsive to violence. To investigate this we create an indicator dividing voters into two groups. The survey asked respondents if they felt close to a particular political party. Respondents who answered "No" were classified as unaligned voters in our variable *Unaligned Voter*. The follow-up question if they answered "Yes", "Which party is that?", was used to determine the actual political affiliation among decided voters. The following equation shows the baseline specification to investigate heterogeneous reactions to violence,

$$Y_{ij} = \beta V_{ij} + \nu PA_{ij} + \delta PA_{ij} * V_{ij} + \gamma_j + \varepsilon_{ij}. \tag{1.12}$$

The addition in this specification is PA_i, an individual political affiliation indicator, which equals 1 if the respondent does not have a strong affiliation with any political party.

The specifications in Table (2.8) show an interesting pattern of how violence influences voting behavior. The coefficient on *Fear Violence* is negative and significant in all specifications, with no heterogeneous relationship between political affiliation and fear of violence. In other words, anyone who fears violence is less likely to vote. With the other measures of untargeted violence, *Violent Competition* and the event count measure, we find the opposite pattern: there is no primary

effect but there is a heterogeneous effect, by political affiliation. Only respondents who are classified as unaligned voters are less likely to vote in the presence of untargeted violence. Although the level of significance on the interaction varies, the point estimates are consistent. Furthermore, as we discussed previously, the impact on the event measures are likely to be attenuated toward zero. We see a fairly consistent estimate of a one category increase in reported violence corresponding to about a 1.5 percentage point reduction in the likelihood of voting, regardless of political affiliation, for personal fear of violence *Fear Violence*. For the event measure of violence, the results indicate that in regions where an event occurred, unaligned voters are between two and four percent less likely to vote, Columns (4) - (5) Table (2.8). Finally, the survey measure of occurrence, *Violent Competition*, shows results similar to the event measure.

This dichotomy of results is important and supports the key assumptions in both models of how violence influences voting behavior. It implies that targeted violence may be effective at preventing all voters, including core party supporters, from voting, as in the CH model. It also implies that untargeted violence may be enough to disenfranchise unaligned voters, as in the CV model.

It is reasonable to assume that the fear of violence in elections may in fact cause an individual to abstain from voting; however, the nature of this data does not allow direct claims of causality. Furthermore, there are several alternative, non-causal, explanations for the observed negative correlation between violence and voting. We investigate and rule out several of these explanations, including: omitted variables, response or recall bias, non-linear impacts, and correlations with alternative forms of manipulation.

First, to address omitted variables, we re-run some of the above specifications with a set of demographic control variables and subsequently address the possibility that some individuals may systematically report higher levels of perceived violence for reasons that are correlated with voting behavior. For these we focus on *Fear Violence* as the measure of violence. After the inclusion of demographic controls for age, gender, education, and socio-economic status, there is a modest reduction on the coefficient of about half a percentage point, but no change in sign or level of significance, Table (2.9) Columns (1) and (2). The difference between these columns is that Col-

umn (2) includes indicators for each of the possible responses. Demographic factors are certainly important determinants of voting behavior, though they do not alter the observed relationship.

Another concern is that individuals may be reporting a fear of violence for reasons not connected specifically to election violence. To address this we include *fear crime* with the assumption that this may capture the correlation between other types of violence and a higher propensity to report greater fear of election violence. Including *fear crime* in Table (8) Column (5) results in a coefficient near zero on *fear crime* and no change to our coefficient of interest. This provides some evidence that our results are not driven by other, more general fears of violence.

Additionally, we may worry that individuals who are disillusioned with the democratic process and thus less likely to vote may also be more likely to report that violence occurs throughout the election process. This could lead to a biased estimate of the impact of violence on voting. To examine this alternative explanation, an AB item which asks individuals about their satisfaction with democracy was included in an additional specification. This showed no change on the violence coefficient, but as one would expect, it did have a strong positive correlation with voting, Table (2.9) Column (6). These additional specifications cannot eliminate all possible alternative explanations, but do eliminate some potential non-causal explanations for the observed negative correlation between violence and voting.

A second potential concern is that structural conditions which encourage violence may also discourage voting. For example, remote areas may be more violence-prone and entail higher voting costs. Our baseline specifications make significant headway in eliminating this as an alternative explanation through the use of fixed effects. The smallest level of fixed effects we used, the district, is the intended primary sampling unit (PSU) in the AB surveys. In such a small geographic area in which an enumerator is able to walk from house-to-house collecting survey responses, all individuals would likely be exposed to identical structural conditions. Given that we see that fearing violence retains a significant correlation with voting when controlling for district fixed effects (i.e., wiping out district-level variance in structural conditions), this seems to be an unlikely explanation. This highly disaggregated level of fixed effects also addresses a number of other concerns

with regards to the endogeneity of violence. For example, one may be concerned with the endogeneity of the timing of violence. However, being that we see reactions to violence vary even within a small town or village and across different political affiliations such concerns with factors common across national, or even sub-national levels, are mitigated. Additionally, we examined another measure violence which should not be correlated with voting but would be correlated with structural conditions: post-election violence. A high number of post-election violent events would indicate a generally violent district and thus more fear of violence. However, when including the number of post-election violent events in an additional specification, we do not see a significant reduction in voting behavior, again demonstrating that structural conditions are not driving the observed correlation, these specifications were ommitted.

Another possible estimation issue is that the impact of violence is non-linear. Furthermore, given that our dependent variable is dichotomous, linear probability models have an obvious limitation: they do not restrict the estimate to the zero-one range. Given that the dependent variable voting has a mean of about .7, this is an unlikely concenr. To examine if our effects are non-linear, we ran a variety of alternative specifications and find results to be similar to our baseline specifications. Figure (**??**) graphs the estimated probabilities of voting for the average respondent in the sample using a fixed effects probit model. This figure also displays a linear trend. However, Column (2) in Table (9) does show that those reporting fearing violence "Somewhat" or "A lot", the two highest categories, drive our results. However, the estimated magnitudes do not change very much.

The last possibility we examine is that past voting behavior strongly influences an individual's view of violence. To address this concern, we examine the impact of violence on the hypothetical *WontVote* measure while controlling for an individual's past voting behavior. Once again the predicted influence of violence on voting, this time measured by *WontVote*, remains unchanged. Given that the negative correlation of violence with voting remains after considering a number of non-causal explanations, it is reasonable to conclude that there is a causal impact.

Even addressing all of these potential alternative explanations, it is possible that other factors

may be driving the observed correlation. Take for example the endogeneity of elections themselves, elections may be pushed quickly during violent times as a hope to resolve a conflict, however even in such cases there would be significant variation with regards to where violence occurs within a country, and we see that voting rates are lower in more violent areas. The fact that these results hold across such a diverse set of countries in Africa, suggests that violence does reduce the likelihood an individual votes. The ability to control for other sources of violence, and the heterogeneity in reactions to violence are all results that would be difficult to attribute to a reversed relationship. In summary, fearing violence seems to have a consistently negative impact on voting behavior. We also find that general or untargeted violence appears to reduce voting among unaligned voters. For this reason, we now turn to analyzing how reported fears of election violence are related to political affiliation.

2.6.2 Who Fears Violence in Elections?

Both theories make implicit assumptions with regards to who should fear violence in an election. Using these assumptions, we derived H3: core party supporters are more likely to fear violence during an election. This was based on the CV model's assumption that violence is directed at core party supporters. To test this, we ran a series of OLS regressions, this time focusing on political affiliation. Because demographic characteristics may predict political affiliation, we again started by excluding controls and relied heavily on the use of differing levels of fixed effects to see if different groups of people reported systematically different fears of violence within the same election.

Focusing on the results in Table (2.10), we do not see direct support for the simple hypotheses laid out in H3. However, political affiliation is a strong predictor of electoral violence. Focusing first on the results in Column (1), which includes no fixed effects, the basic pattern we see throughout this analysis emerges: supporters of the ruling party fear violence the least, followed by unaligned voters and supporters of the second largest party. However, aside from those supporting the majority or ruling party, differences are small and insignificant. The ability to control

for heterogeneity at such a small geographic unit is important; there is considerable geographic variation in the concentration of political parties in the data and we would like to ensure we are controlling for that when we compare fears of violence across different political groups. Examining Columns (2) – (5), which include district fixed effects, we see the pattern remains largely unchanged. Column (2) replicates Column (1), but introduces fixed effects to control for the possibility of individuals of the same affiliation living in particularly violent or non-violent areas. Column (3) uses a dichotomous measure of violence, dividing respondents into high and low categories. Column (5) does the same, but uses the alternative survey measure of violence. Column (4) uses the alternative classification of political affiliation which categorizes individuals as supporters of the ruling party, or another party. Regardless of the specification, the results are similar. This finding, that small political parties report the highest and majority party members the lowest fear of violence, is consistent with an alternative explanation of the use of election violence: repression and control of the state security apparatus. This is most evident in Column (4) in Table (2.10), where we see that those who support the ruling party fear violence significantly less than all other groups.

Overall, the results from examining the core assumptions made in these models are mixed. We find strong evidence that violence reduces the likelihood of voting, although there is important heterogeneity in those results. The observed behavior is consistent with both models. As in the CV model, we see that unaligned voters are more reactive to violence: simply the occurrence of violence is enough to deter them. We can also support the assumption of the CH model that violence targeted at the opposition's supporters will reduce turnout: regardless of political affiliation, anyone who personally fears becoming victimized by violence is less likely to vote. We do not see a pattern consistent with either model when examining who is targeted based solely on political affiliation. Taken as a whole, this evidence suggests that people react to electoral violence in a method consistent with theories of voter manipulation, although this does not tell us anything directly about how it is actually used in elections. To examine that, we turn to aggregate results and analyze how division of the electorate predicts the level of violence in an area within a country.

2.6.3 Where Does Violence Occur?

We test one hypothesis derived from the models' predictions related to division of the electorate, H4: Observed violence should increase (CV) or decrease (CH) as the fraction of unaligned voters in the electorate increases. This allows for a direct test between the two models, which has useful implications for trying to predict the occurrence of election violence.

To test this hypothesis, we examine how the fraction of unaligned voters, measured using *Unaligned Voter*, is related to three measures of violence. Looking at the results in Table (2.11), we see no relationship between the fraction of unaligned voters and respondents' reported fear of violence. However, the association between violence and the fraction of unaligned voters differs depending on the measure of violence. Using the event count measure, we see a significant positive correlation. However, using the survey measure of untargeted violence, *Violent Competition*, we see that the association, although positive, is insignificant[10]. For the event count measure, a one percentage point increase in the fraction of unaligned voters leads to an increase of approximately one-half a percentage point in the likelihood an event occurs. Results are similar using the log number of events, but the interpretation is far clearer with the original indicator. The insignificant relationship between fearing violence, which is our measure of targeted violence, was expected. If violence is untargeted as in the CV model, we would not expect an increase in fear of targeted violence.

A more thorough test between these models would require a more accurate picture of whom the violence in these areas was aimed at and who perpetrated it. Taking the evidence for the predictions and assumptions together, the CV model is more supported, such that violence is used primarily to deter unaligned voters from voting. Furthermore, the evidence also points toward the majority party being the perpetrators as well as an alternative explanation: violence may be intended to keep everyone except majority parties from participating in elections. This is not surprising given that the vast majority of elections in Africa are won by incumbent political parties.

[10]Removing the weighting raises the value of the coefficient by about one-third and results in a significanct relationship, this is likely due to the CV model being a better fit in countries with more observations, which recieve less weight in the regressions after including survey weights.

2.7 Conclusion

Democracies are, unfortunately, not immune to violence. The hope is that elections can be used as a means of peaceful competition; however, this is not always the case. The models examined in this paper provide one potential explanation for the use of violence in elections: strategic manipulation of who votes on polling day. There is validity for each framework's assumption of how violence influences behavior in elections: both targeted and untargeted violence seem to be effective at reducing turnout. We found important sources of heterogeneity in reactions to, and in fears of, election violence. All voters who fear being victimized by violence react in the same way: they are more likely to abstain from voting. However, not all voters cease voting simply because violence has occurred in general. We find that unaligned voters are the only group who react to the occurrence of violent events even when their level of fear is unchanged. We argue core party supporters must be targeted by violence to cease voting, but unaligned voters need not be targeted.

Given the importance of fearing violence in predicting voting behavior, we also explore the correlates of that fear. We find political affiliation predicts an individual's fear of electoral violence. We show an ordering exists among voter groups: supporters of opposition groups and small political parties report the highest levels of fear, followed by unaligned voters and voters aligned with the majority or ruling party.

Examining the implications of the models discussed in this paper leads to weaker conclusions. We find some evidence for the predictions made by the CV model: greater violence is observed in regions with more unaligned voters, when using event count measures. Their model, though, is not the only story consistent with such a pattern; it is possible that more unaligned voters could correspond to greater competition in an election, which may itself lead to more violence. Nonetheless, these models have highlighted an important direction for future research: uncovering the relationship between election violence and the division of the electorate. With better data on the perpetrators of election violence, we could more thoroughly test between these theories rather than only examine the validity of their assumptions.

However, this paper has made strides in circumventing data limitations. We demonstrate the validity of using survey data to measure election violence. The use of survey data provides some unique advantages; we can see how fear of violence varies even when individuals are exposed to the same set of events. Doing so, we are able to conclude that some electoral violence is likely being targeted at particular groups. We found, even when controlling for heterogeneity at relatively small geographic scales, towns or villages, that differential fears of violence are related to political affiliation. Second, we found that even when there is relatively little electoral violence, as measured by event data, some individuals report a fear of electoral violence and react to that fear. These findings are important because recent papers such as Goldsmith (2015) and Lindberg (2005) have argued violence has had a minimal impact on most elections. We argue that caution must be taken with these conclusions because we find people react to low levels of violence and that the estimated impacts, when using more general measures of political violence, are underestimated. Furthermore, if threats of violence are sufficient to generate a fear of violence, then the implications of models like that in Ellman and Wantchekon (2000), where off equilibrium violence can alter the election results, need to be seriously considered.

Understanding why a political party uses violence in an election is essential to designing policies to reduce its occurrence. If, as the theories we focused on suggest, violence is used to strategically manipulate voter turnout in an election, the key to preventing violence may lie in the electoral system itself. By increasing the need for political parties to appeal to voters other than their core supporters, the viability of violence as an electoral strategy could be significantly reduced. Regardless of the underlying mechanisms, our results suggest that prevention strategies should focus on areas with a high fraction of unaligned voters and on members of relatively small political parties. However, without identifying the underlying causes of election violence, a permanent solution is unlikely to emerge. More research is needed to test between theories of election violence. Although this paper takes important steps in that direction, its contribution identifies reactions to, and not the sources of, election violence.

APPENDIX

Table 2.1 Political Affiliation Summary

		Individual		Country		
Variable	Obs	Mean	Std. Dev.	Mean	Min	Max
Trust Ruling	26173	1.60	1.13			
Trust Opposition	25453	1.22	1.06			
Careful What Say	26445	1.75	1.12			
Supports						
Ruling Party	27713	0.34	0.47	0.34	0.00	0.71
Majoirty Party	27713	0.36	0.48	0.37	0.19	0.71
Second Party	27713	0.11	0.31	0.10	0.02	0.27
Other Party	27713	0.09	0.29	0.09	0.01	0.32
No Party\Unaligned	27713	0.41	0.49	0.41	0.18	0.63
Votes						
Majority Party	27713	0.44	0.50	0.45	0.22	0.78
Second Party	27713	0.13	0.34	0.13	0.02	0.31
Other Party	27713	0.11	0.31	0.11	0.01	0.30
No Party\Unaligned	27713	0.32	0.47	0.32	0.09	0.64

Table 2.2 Agreement Between Measures

	Would Vote For				
Support	Unaligned	Majority	Second	Other Party	Total
Unaligned	0.62	0.24	0.08	0.07	11,407
Majority	0.07	0.85	0.07	0.02	9,985
Second	0.07	0.22	0.69	0.03	2,944
Other Party	0.14	0.07	0.04	0.75	2,549
Total	8,290	11,995	3,670	2,930	26,885

Each cell is the fraction of individuals who reported that combination, divided by the number of individuals who reported supporting that group. The denominator for each cell is the final column.

Table 2.3 Is Political Affiliation Accurately Reported?

VARIABLES	Supports Ruling	Supports Ruling	Unaligned	Undecided
Careful what you say				
Rarely	0.00	-0.01	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)
Often	-0.02	-0.04***	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)
Always	-0.04***	-0.05***	0.03**	0.02
	(0.01)	(0.01)	(0.01)	(0.01)
Fixed Efffects	Country	District	District	District
Observations	26,445	26,445	26,445	26,445
R-squared	0.15	0.30	0.18	0.18
Robust standard errors in parentheses				
*** p<0.01, ** p<0.05, * p<0.1				

Unaligned corresponds to our primary measures of political affiliation, asking if respondents feel "close to" a political party. The second measure, undecided, asks what party they would vote for in a hypothetical election.

Table 2.4 Violence Summary, Regional Level

		Mean	S.D.	Min	Max
Fear Violence	372	1.11	0.55	0.03	2.75
Voted	372	0.72	0.13	0.18	1.00
Violent Competition	372	1.43	0.52	0.04	2.89
Log Events Before	350	0.23	0.53	0.00	3.18
Log Events After	350	0.37	0.69	0.00	3.64

Table 2.5 Simple Correlations Survey and Event Violence Measures

VARIABLES	Fear Violence	Violent Competition	Violent Competition
Log Events Before	0.0251	0.0838**	0.0641
	(0.04)	(0.03)	(0.06)
Log Events After			0.0219
			(0.05)
Fixed Effects	Country	Country	Country
Observations	350	350	350
R-squared	0.566	0.681	0.682
	Standard errors in parentheses		
	*** p<0.01, ** p<0.05, * p<0.1		

Table 2.6 Additional Comparison of Violence Measures

VARIABLES	Log Events	Log Events
Violent Competition	0.198**	
	(0.09)	
Fear Violence		0.021
		(0.07)
Fixed Effects	Country	Country
Observations	350	350
R-squared	0.31	0.30
	Standard errors in parentheses	
	*** p<0.01, ** p<0.05, * p<0.1	

Table 2.7 Baseline Impact of Violence on Voting

	Voted	Voted	Voted
Fear Violence	-0.038***	-0.066**	-0.0366***
	(0.01)	(0.02)	(0.01)
Fear Violence*		0.0254	
Country Mean		(0.02)	
N = 26984			
R-squared	0.043	0.043	0.14
Violent Competition	-0.013	-0.0875***	-0.003
	(0.01)	(0.03)	(0.01)
Violent Comp*		0.0515**	
Country Mean		(0.02)	
N = 26120			
R-squared	0.041	0.042	0.14
Event Indicator	-0.0435***	-0.0635**	Not
	(0.01)	(0.02)	applicable
Log Events*		0.047	
Country Mean		(0.05)	
N = 25249			
R-squared	0.042	0.043	0.14
Fixed Effects	Country	Country	District
Standard Errors Clustered by Region			
*** p<0.01, ** p<0.05, * p<0.1			

Column (1) and (2) differ by an interaction with the country level mean of the violence measure. Column (3) includes fixed effects for the 1820 districts, Events are measured at a higher level, and therefore drops out. † Violence here refers to the same measure of violence listed directly above. All measures are indicators, events indicate at least one event in the region, the survey measures refer to the top two categories.

Table 2.8 Baseline Results, Heterogenous Relationship Between Voting and Violence

	(1)	(2)	(3)	(4)	(5)
Dependent Variable:	Voted	Voted	Voted	Voted	Voted
Violence Measure:	Fear Violence	High Fear	Violent Competition	High Violence	Event Indicator
Violence	-0.016***	-0.029***	-0.005	0.001	-0.022
	(0.00)	(0.01)	(0.00)	(0.01)	(0.01)
Unaligned Voter	-0.134***	-0.130***	-0.123***	-0.127***	-0.122***
	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)
Violence*Unaligned Voter	-0.002	-0.006	-0.012*	-0.025*	-0.034*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Supports Majority	0.030***	0.030***	0.030***	0.030***	0.0287***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Fixed Effects	Country	Country	Country	Country	Country
Observations	26984	26120	26513	25763	26120
Dependent Variable:	Voted	Voted	Voted	Voted	Voted
Violence Measure:	Fear Violence	High Fear	Violent Competition	High Violence	Event Indicator
Violence	-0.016***	-0.033***	-0.001	0.007	Not
	(0.00)	(0.01)	(0.00)	(0.01)	Applicable
Unaligned Voter	-0.134***	-0.127***	-0.118***	-0.114***	-0.121***
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
Violence*Unaligned Voter	0.001	-0.000	-0.011†	-0.026*	-0.027
	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)
Supports Majority	0.020*	0.022**	0.023**	0.023**	0.0230**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Fixed Effects	District	District	District	District	District
Observations	26984	26120	26513	25763	26120

Standard Errors Clustered by Region

*** p<0.01, ** p<0.05, * p<0.1

† P-value of .112. High Fear and High Violence variables are indicator variables for the two highest categories

Table 2.9 Expanded Specifcations of the impact of Violence on Voting

VARIABLES	(1) Voted	(2) Voted	(3) Voted	(4) Voted	(5) Voted	(6) Voted	(7) Voted	(8) WontVote	(9) Voted	(10) Voted
Fear Violence	-0.012***		-0.013***	-0.012***	-0.012***	-0.011***	-0.012***	0.005*	-0.015**	-0.009
	(0.003)		(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.007)	(0.006)
A little		-0.009								
		(0.009)								
Somewhat		-0.034***								
		(0.011)								
A lot		-0.033***								
		(0.010)								
age	0.009***	0.009***	0.009***	0.009***	0.009***	0.009***	0.009***	0.0004**		0.009***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)		(0.000)
Female	-0.021***	-0.021***	-0.017**	-0.026***	-0.020***	-0.018**	-0.018**	0.010**		-0.023***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.004)		(0.007)
Wealth	-0.001	-0.001	-0.001	-0.002	-0.001	-0.003	-0.000	-0.010***		-0.001
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)		(0.004)
Education	0.005**	0.005**	0.004**	0.001	0.005**	0.004*	0.003	0.005***		0.005**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)		(0.002)
ViolentComp			-0.001							
			(0.004)							
Log Events				-0.028**						
				(0.011)						
Fears Crime					-0.003					
					(0.003)					
Democratic Satisfaction						0.011***				
						(0.004)				
Vote not Secret							-0.010**			
							(0.004)			
Voted								-0.073***		
								(0.008)		
Violence*Time since election									-0.000	-0.000
									(0.000)	(0.000)
Fixed Effects	Region	Region	Region	Country	Region	Region	Region	Region	Region	Region
Observations	25,711	25,711	24,524	24,457	25,643	23,823	24,150	25,711	25,749	24,629
R-squared	0.161	0.162	0.163	0.128	0.161	0.168	0.162	0.1	0.044	0.161

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.10 Perceptions of Violence

VARIABLES	Fear Violence	Fear Violence	HighFear	HighFear	HighViolence
	(1)	(2)	(3)	(4)	(5)
Supports Majority	-0.071***	-0.119***	-0.042***		-0.049***
	(0.03)	(0.03)	(0.01)		(0.01)
Supports Second	0.037	-0.004	-0.012		-0.019
	(0.03)	(0.04)	(0.02)		(0.02)
Unaligned Voter	0.0201	-0.040	-0.009		-0.032**
	(0.03)	(0.03)	(0.01)		(0.01)
Supports Ruling				-0.039***	
				(0.01)	
Supports Other				0.006	
				(0.01)	
Fixed Effects	None	District	District	District	District
Observations	26,984	26,984	26,984	26,984	26,120
R-squared	0.001	0.271	0.232	0.232	0.259
	Robust standard errors in parentheses				
	*** p<0.01, ** p<0.05, * p<0.1				

The ommited category in all specifications, except Column 4, is supports other minority party. In column 4 it is unaligned voters, as all other parties are put into the Supports Other category.

Table 2.11 Division of the Electorate and Violence: Unaligned Voters

VARIABLES	Log Events	Log Events	ViolentComp	Fear Violence
Fraction Unaligned†	0.531** (0.23)	0.528** (0.23)	0.145 (0.13)	-0.063 (0.14)
Supports Majority‡		-0.001 (0.02)	-0.09*** (0.02)	-0.10*** (0.02)
Unaligned‡		-0.00 (0.01)	-0.06** (0.02)	-0.02 (0.03)
Fixed Effects	Country	Country	Country	Country
Observations	26,273	26,273	26,120	26,984
R-squared	0.24	0.24	0.183	0.165
Robust standard errors in parentheses				
*** p<0.01, ** p<0.05, * p<0.1				

† This is the regional fraction of unaligned voters. ‡ These are individual political affiliation indicators.

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CHAPTER 3
Profiling in Violent Elections

3.1 Introduction

Recent theoretical and empirical research on election violence has presented several potential ways in which violence may be used to influence the electoral process. A key differentiation between emerging theories, as highlighted in Wallsworth (2016), is whether violence is targeted directly at opposition supporters or indirectly at unaligned voters who are more likely to vote for the opposition. Wallsworth (2016) found that reactions to violence are consistent with both strategies. Targeted violence is associated with a lower likelihood of voting, and unaligned voters were the only group to react to indirectly targeted violence. Using data from a broad sample of twenty African countries, neither framework emerged as a clearly better fit in the data. One way to distinguish which theory is more viable in a given country is to unravel how successfully a potential perpetrator of violence could profile supporters of the opposition. This paper examines the viability of profiling by identifying the characteristics that are associated with fearing violence.

Before we delve into understanding if and how profiling could be occurring in these elections it is useful and interesting to examine who fears violence the most in each country. In fact, the optimal target of violence is the topic of related theoretical work on election violence in Robinson and Torvik (2009). To do this we use data from Afrobarometer's Round 4 surveys, which ask respondents how much they fear becoming a victim of election violence. Most often, we find that opposition supporters report the highest fear of violence. Then, using this data we examine easily identifiable characteristics which can be used to infer someone's political affiliation and investigate whether or not these characteristics are also correlated with a respondent's fear of violence. Unsurprisingly, we find ethnicity to be the strongest characteristic which predicts both an individual's

political affiliation and their fear of violence.

When we turn to examining how and if profiling occurs, based on other identifiable characteristics, the results varied. Socio-economic characteristics and where an individual lives predict fears of violence, an individual's political affiliation, or both in some but not all countries. We argue characteristics which are correlated with both an individual's fear of violence and their political affiliation may be particularly useful in matching models of election violence to various countries.

Finally, we seek to identify which characteristics explain the observed variation in fears of violence, focusing on whether self-reported or predicted measures of political affiliation can explain more of the variation. The results from this analysis show that both predicted and reported measures of political affiliation are significant predictors of an individual's fear of violence, although neither explain much of the variance. We find that ethnicity consistently explains more of the variation in fear across individuals than location or political affiliation.

Understanding how profiling occurs in elections is useful beyond simply allowing us to build more accurate models of election violence. Having a better understanding of who the most likely victims of election violence are in a given country can help authorities tasked with preventing election violence identify the most at-risk groups and better allocate what may be limited resources to prevent violence.

3.2 How can the viability of profiling help distinguish between models of election violence?

Existing theoretical models of election violence vary on a few key dimensions, including the research question, who perpetrates violence, and how or at whom violence is targeted. Understanding the viability of profiling helps the most when distinguishing on the last dimension: how or at whom election violence is targeted.

Some models like those in Ellman and Wantchekon (2000) or Collier and Vicente (2012) do not assume the need to identify the victims of violence for it to be effective. In Collier and Vicente (2012), they assume that different types of voters, specifically strong and weak party supporters, react to violence differently. They argue that weak party supporters, which one can think of as unaligned voters who lean towards supporting a given party, will stop voting if any violence occurs. If this is the case, a party need not target opposition supporters directly. Instead, they can gain vote share by using untargeted violence in areas where they have an initial advantage among strong party supporters relative to weak supporters.

Models like the one presented in Ellman and Wantchekon (2000) also do not need the ability to target violence for it to be an effective strategy to manipulate elections. In their model, all individuals are impacted by violence. They do assume that opposition supporters bear a greater cost when violence occurs than those who control the threat of violence. Although this model is more applicable to violence that may be used to forcibly overturn elections results rather than manipulate an election, understanding whether or not profiling is a viable option may still help determine if this model of violence could be applied to a particular country.

On the other hand, some models explicitly examine who should be targeted by violence in elections and under what conditions. That is the primary question investigated in Robinson and Torvik (2009). They divide the electorate into three groups. Two of the groups lean ideologically towards the two parties, and the third group consists of the swing voters. Their analysis focuses on when it is most viable to use violence and which group that violence should be directed at.

Their interesting conclusion is that it can be "more attractive for an incumbent to disenfranchise the swing voters than the core supporters of the opposition." However, for such a model to be viable, it must be possible to identify the group of voters you plan to target.

The last model we discuss is Chaturvedi (2005). In his model, resources can be split among traditional ideological campaigning and violent campaigning. Violence is targeted directly at opposition supporters to prevent them from voting, whereas ideological campaigning is meant to persuade unaligned voters. This model, like the last one, assumes no collateral damage occurs when violence is used as a campaign tactic.

The papers presenting these models succesfully motivate them with examples. However, we may be able to improve on that by empirically examining whether profiling seems to be possible in a particular country. If there is no way to identify opposition supporters, perhaps violence like that proposed in Ellman and Wantchekon (2000) may be the best fit because the violence in their model is untargeted but results in a benefit for the perpetrating party. If location is the strongest predictor, a model which bases the effectiveness of violence on the relative distribution of voter types in an election like Collier and Vicente (2012) becomes a strong candidate. Finally, if violence can be targeted directly at opposition supporters on the basis of some observable characteristics like ethnicity, then models like Robinson and Torvik (2009) and Chaturvedi (2005) become more viable.

3.3 Data

The data used in this paper comes from the Round 4 Afrobarometer surveys. This data is uniquely suited to analyzing who is targeted by violence in elections as it asks questions about an individual's demographic characteristics, political affiliation, and fear of violence.

In order to measure violence, we use a question which asks, "During election campaigns in this country, how much do you personally fear becoming a victim of political intimidation or violence?" Respondents could choose one of four response options: "A lot", "Somewhat", "A little bit", or "Not at all". This is then treated as an ordered categorical variable on a zero to three scale, with

three corresponding to fearing violence “A lot”. Throughout, we assume that an individual’s fear of violence is directly correlated with the probability that anyone with similar characteristics would be targeted or victimized by violence during an election in their country.

In order to measure political affiliation, we use a series of questions which ask first, “Do you feel close to any particular political party?”. Then, if the respondent answered yes, they were asked “Which party is that?” Using this question we divide voters into four groups: those who support the largest and second largest parties in a country, those who support any other party, and those who do not support any political party. The fraction of respondents falling into each of these four categories and their respective mean fears of violence are presented in Table (3.1).

Finally, we also use some questions on socio-economic and demographic characteristics of the respondents. This includes questions about gender, age, living conditions, wealth, and education. For gender we used a dichotomous variable, called female, which equals one if the respondent is female. Age is the respondent’s age truncated at 80 years old by Afrobarometer to protect the respondent’s identity. Living conditions, wealth, and education are all categorical variables with higher categories corresponding to more wealth, better living conditions, or having completed a higher level of education. The means for these variables, overall and by country, are presented in Table (3.2).

3.4 Results

3.4.1 Who fears violence and where?

In order to understand if profiling is occurring or how someone may be targeted with violence during elections, we first need to know who is being targeted. To measure this we use the question, "How much do you personally fear becoming a victim of violence during elections in your country" We examine how responses vary across self-reported political affiliation broken down into four categories: supports majority party, supports the second largest party, supports any other party, and supports no party. Table (3.1) presents, by country, the fraction of respondents falling into each category, the mean response for their fear of violence, and whether or not their mean response is statistically different from supporters of the majority party. Table (3.8) in the appendix examines the same variables, demeaned at the district level. Both tables report the means as differences from the majority party's mean fear of violence, to highlight which groups fear violence the most in each country.

In most countries, opposition supporters, either of the second largest party or those supporting other small parties, report the highest fears of violence. The most common exception to this is a few countries in which individuals who do not report supporting a political party have the highest reported level of fear. Only in one country, Zimbabwe, do the majority party supporters report the greatest fear of violence. This suggests that the most likely perpetrator of election violence is the majority party. We conclude this because in most countries the majority party's most relevant opposition reports the highest fear of violence. One explanation for this is that they may be able to perpetrate violence without fear of reprisal and could be abusing state controlled resources to do so.

There are several exceptions to this pattern. The first exception of note is in the countries where violence does not change based on political affiliation. Some countries in this category include: Malawi, Mali, Lesotho, and Kenya. For Kenya, this may be because by the survey was

taken not long after the extreme violence following their turbulent 2008 election cycle, which may have raised fears of violence dramaticallly for everyone. The next three exceptions, South Africa, Liberia, and Botswana, share another pattern. All three of these countries have unaligned individuals reporting the greatest fear of violence.

Taking a closer look at the countries where unaligned voters report the greatest fear of violence illuminates some other interesting patterns. In three of these countries, the majority party has a much higher level of support than any other party. In South Africa and Botswana the majority party has more than four times the level of support than the next largest party, in Liberia it is much closer but the majority party still has fifty percent more supporters in the data than the second largest party. This suggests an alternative way in which the majority party may be using violence to keep power through the election cycle: by oppressing potential competition to prevent them from forming a viable opposition party. With such a dominant plurality of the vote, no one party is strong enough to take down the majority party, so creating an atmosphere of fear throughout the election process and deterring participation may be the most effective strategy to manipulate the election.

In most countries, it appears that the majority party is the most likely perpetrator of violence, and that this violence is directed at their opposition, whether that happens to be another large party, a number of small parties, or oppressing the population in general. However, this does not tell us anything about how they identify and target the opposition with violence. Comparing Table (3.1) to Table (3.8) in the appendix begins to illuminate one potential way in which this may be occurring: using violence in areas that are more likely to contain opposition supporters. In nearly half of the countries, statistical significance on the predictive power of political affiliation reduces or dissapears after adjusting for mean differences of violence by location. To investigate this further we now turn to examining what predicts political affiliation, specifically looking at demographic characteristics that may be easy to infer based on an individual's appearance.

3.4.2 What predicts an individual's political affiliation?

As pointed out in Collier and Vicente (2012), the easier it is to infer one's political affiliation, the easier it is to use violence to manipulate an election. However, which characteristics predict that affiliation and the accuracy of those predictions varies dramatically by country. We now turn to analyzing which visible characteristics predict an individual's political affiliation. To do this we ran a series of linear probability models by country for the two primary categories of political affiliation: supports majority, and supports the largest opposition party.[1] This analysis has two purposes: first, it illuminates which factors are strong predictors of political affiliation in each country; second, we can use the knowledge of what identifiable characteristics predict one's political affiliation in order to examine how parties may be profiling their opposition's supporters with violence.

The Afrobarometer survey contains a large number of variables which could potentially be used to infer someone's political affiliation, including how well they trust particular political parties, what issues they feel are most salient, and questions related to socio-economic status. We focus solely on those questions which could reasonably be associated with outwardly visible characteristics: gender, age, education, wealth, living conditions, ethnicity, and where the respondent lives. We omitted two other variables which fall into this category, religion and language, because they were often almost perfectly collinear with ethnicity or location. This led us to run a series of linear probability models with the following form:

$$Pr[PA_i = J] = \beta_1 Female_i + \beta_2 Age_i + \beta_3 LivingConditions + \beta_4 Wealth + \beta_5 EDucation + \beta_6 Ethnicity + \beta_7 Location, \quad (2.1)$$

where J is either being a member of the majority, or second largest political party. Specifications are run seperately by affiliation and country. Female is a dummy variable which equals one if

[1] Other parties were omitted for two main reasons, first aside from the top two categories there were two few observations in any given party to accurately predict the probability of one being affiliated with that party. Second, it seemed unlikely that unaligned individuals would appear to be a coherent group.

the respondent was a female, and ethnicity and location are a series of dummy variables for all potential ethnicities and districts in the country.

A non-linear model, like probit or logit, would ordinarily be preferred to a linear probability model in cases when you wish to use the predicted values. However, the large number of observations, in some countries as high as seventy-five percent, which have the outcome variable perfectly predicted by either location or ethnicity eliminates this as the optimal estimation technique.

Tables (3.3) and (3.4) report the raw coefficients for the demographic characteristics, along with their significance levels in columns (4)-(8). Columns (9) and (10) present the p-values for a joint F-test of the significance of location and ethnicity dummies. What predicts an individual's political affiliation varies dramatically by country. Female, age and education are significant predictors in about fifty percent of countries in the sample. Ethnicity is a statistically significant predictor of affiliation in all twenty countries, and location matters, independently of ethnicity, in about half of the sample. Although not surprising, this lends support to the idea that political affiliation can be inferred from visible characteristics. Ethnicity, location, gender, and age are all characteristics one can glean without significant effort or the use of informants. Socio-economic characteristics are also likely to be relatively easy to identify.

The next question then is two-fold: does an individual's predicted political affiliation correlate with violence better than their reported affiliation, and are significant predictors of an individual's political affiliation more likely to be correlated with their fear of violence? These questions address a common underlying idea: if violence is directed at particular groups of people, looking like you belong to that group may be more detrimental than actually belonging to that group. If this hypothesis is supported by the data, we may gain a better understanding of what characteristics are being used to target people with violence.

3.4.3 How are individuals targeted with violence?

To get a better idea of how profiling may occur, we examine how the predicted values of political affiliation, specifically the probability one supports the majority party or the primary opposition

party, correlate with fears of violence. Figure (3.1) presents the difference in mean reported fears of violence by country between majority and opposition supporters for both self-reported and predicted political affiliation. For each country, the leftmost bar uses the self-reported measure of political affiliation and the bar on the right uses predicted affiliation from the models described in the previous section. The numbers above or below each bar are the p-values for the difference, while the difference itself is represented by the height of the bar. We report the difference, rather than a series of absolute means, because the difference helps illuminate which group fears violence the most in each country. A positive value indicates that majority members report a greater fear of violence, whereas a negative value indicates opposition supporters report a greater level of fear. This figure demonstrates that the predicted values of political affiliation produce results which mirror the self-reported values, with the exception of the larger magnitudes in the difference. These larger magnitudes are at least in part explained by the fact that these values come from a regression framework where a value of 1 actually indicates someone reported that affiliation for the reported measure, whereas the average predicted value would range from .3 to .5 depending on their affiliation, resulting in a larger magnitude when comparing the coefficients.

Predicted affiliation is essentially a weighted average of demographic characteristics. We would like to be able to identify which characteristics are used in which country to profile individuals. To do this, we run an additional set of regressions and compare the results to the models which were used to predict political affiliation. Variables which are significant predictors of both an individual's fear of violence and their political affiliation are of particular interest because they are the most likely characteristics to be used to target individuals with violence. Table (3.5) presents the results of the regressions analyzing how these characteristics are affiliated with fears of violence and Table (3.6) compares which predictors are significant for predicting political affiliation, fearing violence, or both.

Perhaps most interesting, and least surprising, is the significant degree of overlapping predicting power for ethnicity. Ethnicity is significantly associated with violence in all but three countries and is a significant predictor of political affiliation in all countries. This implies that ethnicity is

one particularly strong characteristic which could be used to infer political affiliation and target an individual with violence. Another finding worth noting is that location significantly predicts an individual's political affiliation far more often than it does their fear of violence. Taken together, this suggests that direct targeting of violence may be the more oft used strategy, even if indirect targeting is a potentially viable tactic.

The socio-economic characteristics of gender, age, living conditions, wealth, and education, have much more varied predictive power. Gender and living conditions significantly predict both in five countries, with the others often predicting either political affiliation or fear of violence but not both. In some respects this makes sense there are likely to be people of all genders, ages, and varied socio-economic status in most political parties so these characteristics are less likely to be useful in differentiating between supporters of the various political parties. On the other hand, when these are significant predictors of both, it may provide useful information as to what exactly divides these parties. Take for example Nigeria, where wealth is a strong predictor of which party an individual supports and their fear of violence. This may be because control of oil revenues is a major political issue. Furthermore, it is unsurprising to see location is a significant predictor of political affiliation in Nigeria given their country's tradition of alternating rule between someone from the north and someone from the south.

Finally, in Table (3.7) we analyze what best explains the variation in individual fears of violence. This table reports the R-squared values from country level regressions including a full set of dummies for location in Column (1), ethnicity in Column (2), and the reported or predicted values of support for majority or largest opposition party in Columns (3) and (4). All Columns use the exact same set of observations in each country, and the R-squared values are directly comparable in Columns (3) and (4) because each is a regression with two variables and an intercept. However, Columns (1) and (2) are not as easily compared, the number of ethnicities and districts in each of the countries has substantial variation. Again, we find that ethnicity does the best job in explaining the variation in fearing violence. We unfortunately then cannot determine if this has to do with existing ethnic tensions, or the fact that ethnicity is a strong predictor of political affiliation.

Overall, these results support the idea that the easier one's political affiliation is to identify, the more viable a strategy violence becomes. Furthermore, it is clear that no single model is going to successfully predict the when, where, and why of election violence in all countries. However, the intuition of existing models does provide a lot of useful correlates to investigate and raises the idea that profiling may be one way in which individuals are targeted with violence throughout the electoral process.

3.5 Conclusion

Analysis suggests that in the majority of countries examined, when violence is a concern in the electorate, it seems to be primarily perpetrated by the majority party. Furthermore, the violence seems to be most often directly targeted at opposition supporters. This is an interesting addition to the finding in Wallsworth (2016) that both types of violence appear to be effective at manipulating elections. The findings in this paper suggest that the type of violence used could depend on what characteristics can be used to infer an individual's political affiliation.

When we turn to analyzing an individual's political affiliation, we find that ethnicity is one of its strongest and most consistent predictors. Ethnicity also predicts an individual's fear of violence. However, we do not have the ability to disentangle if the reason ethnicity predicts an individual's fear of violence is actually the result of profiling.

Finally, we use these predictors of political affiliation, along with the predicted value itself, and examine how they correlate with an individual's fear of violence. The predicted values of political affiliation tend to mimic the results using the actual values. We argue that this suggests that visible predictors may be useful for identifying an individual's political affiliation.

This evidence leads us to conclude three things. First, the ruling party generally appears to be the perpetrator of violence. There are plenty of interesting and important exceptions to this, but this leads us to suggest that international authorities, and independent agencies within governments, are where reform to prevent violence must originate from. It is too easy for those who control the state security apparatus and judicial systems to commit violence without fear of repercussion and this trend needs to stop if we hope to prevent violence in elections. Second, violence tends to be targeted at the majority party's most relevant opposition. At least observationally, the countries in which unaligned voters fear violence the most are all countries where there is not an opposition party strong enough to challenge the majority party. Further investigation would be needed to validate this claim, but the pattern is certainly interesting. The final conclusion we draw is that the way in which violence is targeted is strongly correlated with the easiest ways to infer political

affiliation, be that location, ethnicity, or other demographic characteristics. Richer data on socio-demographic traits of individuals in these countries along with the make-up of the political parties within them could strengthen this claim significantly.

These findings are also useful to validate assumptions in existing theory, and help to determine which theories are most applicable to which situations. All four of the theories discussed in this paper could be applied to some, but not all, countries in this sample. In fifteen of the twenty countries, ethnicity may enable violence to be directly targeted. In a small sample of countries, indirect targeting of violence may be occurring on the basis of location. Understanding how it is possible to use violence in a given country can help researchers to develop theories catered to a particular situation, and to choose which theory to base their decisions upon.

Overall, it is clear violence has become an unfortunately integral part of elections in Africa. With those who perpetrate violence most often being immune to punishment, significant reforms need to be made from the outside and independent enforcement agencies should be created within countries where election violence is problematic. Furthermore, encouraging parties to form along less contentious divides, like ethnicity, could also make the use of violence less effective by making it harder to infer an individual's political affiliation. Looking at violence through the lens of economic theory provides useful tools to attempt to predict and prevent its occurence. The analysis in this paper suggests that the theories presented in Chaturvedi (2005), Collier and Vicente (2012), and Robinson and Torvik (2009), and other papers provide a solid structure to build on when attempting to understand electoral violence.

APPENDIX

Figure 3.1 Comparing Political affiliation and Fear of Violence

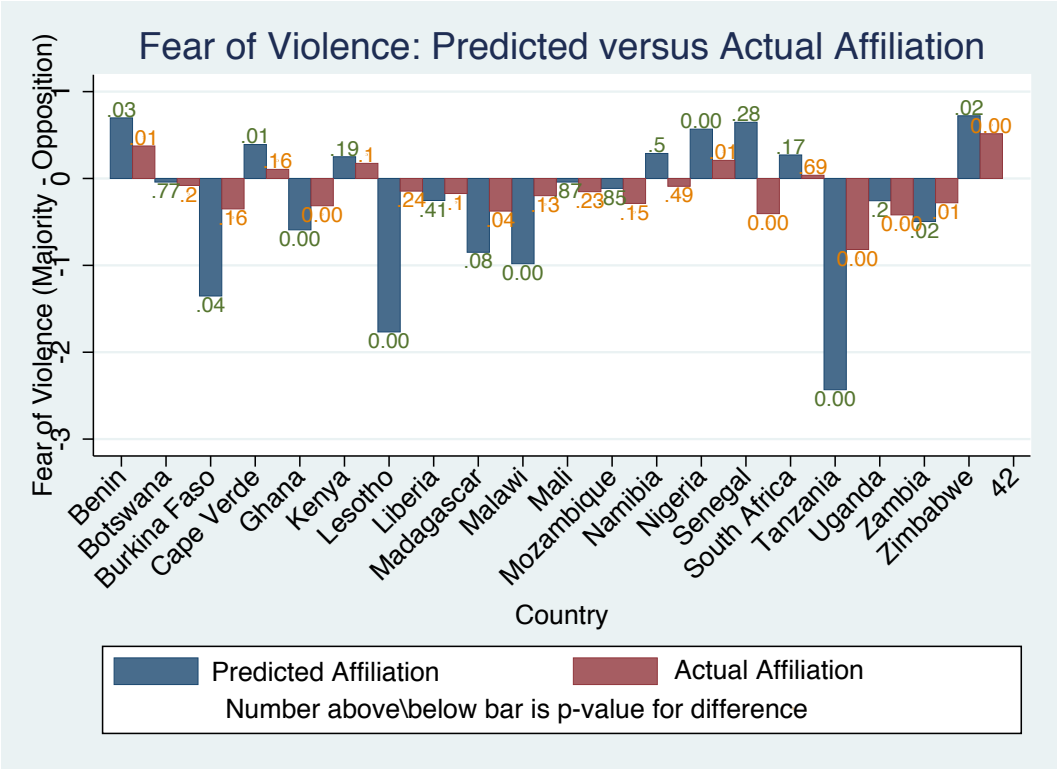

Table 3.1 Mean Violence and Political Affiliation Breakdown by Country

Party Affiliation:		Majority		Second		Other		None	
Country	N	Mean Violence	Fraction Supporting	Mean Violence	Fraction Supporting	Mean Violence	Fraction Other	Mean Violence	Fraction Supporting	Mean Violence
Benin	1200	0.58	0.19	0.75	0.04	-0.35**	0.14	0.25***	0.64	-0.25***
Botswana	1200	0.27	0.55	0.23	0.13	0.09	0.09	-0.06	0.23	0.1**
Burkina_Faso	1200	1.01	0.38	1.03	0.02	0.37	0.08	0.38***	0.52	-0.10
Cape_Verde	1264	0.50	0.28	0.61	0.27	-0.11	0.02	-0.07	0.43	-0.2***
Ghana	1200	0.91	0.36	0.78	0.19	0.33***	0.03	-0.17	0.43	0.15**
Kenya	1104	1.82	0.41	1.83	0.14	-0.15	0.11	-0.08	0.33	0.05
Lesotho	1200	0.98	0.33	0.93	0.11	0.15	0.06	-0.04	0.50	0.08
Liberia	1200	0.95	0.20	0.68	0.13	0.18*	0.12	-0.01	0.56	0.37***
Madagascar	1350	0.85	0.24	0.81	0.03	0.36**	0.06	0.18	0.67	0.03
Malawi	1200	1.01	0.48	0.98	0.09	0.21*	0.08	-0.05	0.35	0.04
Mali	1232	0.98	0.24	0.93	0.10	0.16	0.32	-0.02	0.34	0.05
Mozambique	1200	0.88	0.63	0.87	0.03	0.24	0.01	-0.15	0.33	0.00
Namibia	1200	1.13	0.44	1.05	0.06	0.08	0.12	0.23**	0.38	0.09
Nigeria	2324	1.43	0.22	1.32	0.13	-0.2**	0.12	-0.14*	0.52	0.25***
Senegal	1200	0.83	0.31	0.74	0.07	0.39***	0.15	0.21**	0.48	0.06
South_Africa	2400	0.97	0.39	0.93	0.06	-0.06	0.08	-0.04	0.48	0.08*
Tanzania	1208	0.93	0.71	0.85	0.05	0.81***	0.04	-0.27	0.20	0.25***
Uganda	2431	1.59	0.38	1.47	0.17	0.43***	0.08	-0.17*	0.37	0.11**
Zambia	1200	1.09	0.22	0.95	0.19	0.33***	0.14	-0.32***	0.46	0.21**
Zimbabwe	1200	2.41	0.37	2.51	0.08	-0.52***	0.01	-0.54	0.55	-0.12*

*** p<0.01, ** p<0.05, * p<0.1

The p-values indicate whether the mean is statistically different from the mean fear of violence reported by respondents affiliated with the majority party. Mean violence for Second, Other, and None reported as difference from mean of Majority Party.

Table 3.2 Means of Demograhic Data: Overall and by Country

	Female	Age	LivingCond	Wealth	Education
Overall Mean	0.50	36.30	2.63	2.78	3.15
Overall Min	0	18	1	1	0
Overall Max	1	80	5	5	9
Benin	0.50	35.38	2.37	2.74	2.08
Botswana	0.50	40.15	2.50	2.66	3.38
Burkina Faso	0.50	36.52	2.75	2.97	1.49
Cape Verde	0.50	37.78	2.83	2.89	3.20
Ghana	0.50	38.89	2.75	3.02	2.86
Kenya	0.50	35.19	2.13	2.50	3.77
Lesotho	0.50	41.30	2.06	2.41	2.85
Liberia	0.50	35.78	2.70	3.09	3.02
Madagascar	0.50	39.50	2.70	2.75	2.88
Malawi	0.50	35.45	2.69	2.84	2.44
Mali	0.50	39.10	2.46	2.74	1.20
Mozambique	0.50	30.64	2.89	2.83	3.07
Namibia	0.50	34.73	2.93	2.91	4.04
Nigeria	0.50	31.30	3.17	3.10	4.40
Senegal	0.51	38.87	2.06	2.57	2.00
South Africa	0.50	37.77	2.77	2.95	4.31
Tanzania	0.50	37.51	2.33	2.39	3.06
Uganda	0.50	33.71	2.56	2.51	3.37
Zambia	0.50	35.08	2.51	2.76	3.40
Zimbabwe	0.50	36.53	2.74	2.85	3.83

Table 3.3 Linear Probability Models Predicting Probability of Supporting Majority Party

(1) Country	(2) Obs	(3) Fraction	(4) Female	(5) Age	(6) LivingCond	(7) Wealth	(8) Education	(9) Ethnicity†	(10) Location†
Benin	1126	0.19	-0.06**	-0.01	0.01	0.01	0.01	0***	0.33
Botswana	1170	0.55	0.06**	0.02**	0.04**	0.00	-0.02**	0***	0.48
Burkina_Faso	1062	0.38	-0.07**	0.00	0.02	0.04**	-0.03***	0.00***	0.52
Cape_Verde	1092	0.28	0.00	0.03**	0.05***	0.00	0.02**	0.00***	0.14
Ghana	1100	0.36	-0.03	-0.01	0.04***	0.00	-0.01	0.00***	0.06*
Kenya	1060	0.41	-0.05*	-0.01	0.02	-0.02	0.00	0.00***	0.00***
Lesotho	1149	0.33	0.05*	0.01	0.03**	0.00	-0.03***	0.00***	0.31
Liberia	1178	0.20	0.03	0.04***	0.00	0.02*	0.01**	0.23	0.11
Madagascar	1295	0.24	-0.06**	0.00	0.01	0.01	0.01	0***	0***
Malawi	1113	0.48	0.03	-0.01	0.05***	0.01	0.00	0.00***	0.08*
Mali	1197	0.24	-0.09***	0.00	0.01	0.04**	0.00	0.00***	0.25
Mozambique	1056	0.63	0.01	0.03**	0.02	0.02	0.03**	0.01***	0.42
Namibia	1185	0.44	-0.01	0.02**	0.00	0.02	-0.03***	0.00***	0.00***
Nigeria	2219	0.22	-0.07***	0.01	-0.01	0.02*	0.01	0.00***	0.00***
Senegal	1106	0.31	0.00	0.04***	0.00	-0.04*	-0.01*	0.01***	0.5
South_Africa	2290	0.39	-0.02	0.00	0.00	0.01	-0.02**	0.00***	0.00***
Tanzania	1180	0.71	0.1***	0.05***	0.01	0.02	-0.02*	0.00***	0.31
Uganda	2364	0.38	0.01	0.03***	0.03***	0.01	-0.01	0.00***	0.00***
Zambia	1133	0.22	-0.04	0.02*	0.02*	0.00	-0.01	0.00***	0.13
Zimbabwe	1160	0.37	-0.04	0.01	0.00	-0.02	0.03***	0.00***	0.54

*** p<0.01, ** p<0.05, * p<0.1

† These columns report the p-values from a F-test of joint significance for all the relevant dummies, for either ethnicity or location. Observations is the number of observations for the country, and used observations is the number of observations which were kept for estimation. Age is in decades.

Table 3.4 Linear Probability Models Predicting Probability of Supporting Largest Opposition Party

(1) Country	(2) Obs	(3) Fraction	(4) Female	(5) Age	(6) LivingCond	(7) Wealth	(8) Education	(9) Ethnicity†	(10) Location†
Benin	1126	0.04	0.00	0.00	-0.02**	0.01	0.01*	0.00***	1
Botswana	1170	0.13	0.00	0.00	-0.01	0.01	0.00	0.00***	0.83
Burkina_Faso	1062	0.02	-0.01	0.00	0.01	0.00	0.01**	0.00***	0.28
Cape_Verde	1092	0.27	0.00	0.00	-0.04**	0.00	-0.02**	0.00***	0.9
Ghana	1100	0.19	-0.07***	0.00	-0.03***	-0.01	0.00	0.16*	0.02**
Kenya	1060	0.14	-0.01	0.01	-0.01	0.02	0.00	0.02**	0***
Lesotho	1149	0.11	-0.04**	-0.01*	-0.01	0.01	0.01*	0.05**	0.77
Liberia	1178	0.13	-0.05**	-0.04***	0.01	0.00	0.01	0.04**	0***
Madagascar	1295	0.03	-0.01	0.00	0.01	0.00	0.00	0.00***	0.98
Malawi	1113	0.09	-0.02	-0.02**	-0.02**	-0.01	0.00	0.00***	0***
Mali	1197	0.10	0.01	-0.01	-0.02**	-0.02*	0.00	0.00***	0.03**
Mozambique	1056	0.03	-0.01	0.00	-0.01	0.00	0.00	0.27	0.97
Namibia	1185	0.06	-0.01	0.00	0.01	-0.01	0.02***	0.76	1
Nigeria	2219	0.13	-0.03**	0.01	0	0.02***	0.00	0.00***	0***
Senegal	1106	0.07	0.02	0.00	-0.01	-0.01	0.01	0.26	0.08*
South_Africa	2290	0.06	-0.01	0.00	0.00	0.00	0.00	0.00***	0.00***
Tanzania	1180	0.05	-0.04***	-0.01***	-0.01**	-0.01**	0.00	0.00***	0.31
Uganda	2364	0.17	-0.09***	-0.03***	-0.02**	0.00	0.02***	0.00***	0.00***
Zambia	1133	0.19	-0.08***	-0.03***	-0.01	-0.02**	0.01	0.00***	0.00***
Zimbabwe	1160	0.08	0.00	0.00	0.00	-0.01	-0.01*	0.00***	0.06*

*** p<0.01, ** p<0.05, * p<0.1

† These columns report the p-values from a F-test of joint significance for all the relevant dummies, for either ethnicity or location. Observations is the number of observations for the country, and used observations is the number of observations which were kept for estimation. Age is in decades.

Table 3.5 Demographic Traits and Fear of Violence

Country	Female	Age	LivingCond	Wealth	Education	Ethnicity†	Location†
Benin	0.04	0	-0.02	0.01	0.05***	0.02**	0.09*
Botswana	0.08*	0	-0.04*	-0.03	0	0.03**	0.15
Burkina Faso	0.18**	0.01*	-0.04	-0.03	0.04	0.01**	0.48
Cape Verde	0.04	0	-0.02	0.01	0.01	0***	0.03**
Ghana	0.09	0	0.02	-0.1***	-0.03	0***	0.07*
Kenya	0.05	-0.01**	-0.04	-0.01	-0.04*	0***	0.2
Lesotho	0.13*	0	-0.05	0.08**	-0.04*	0***	0.27
Liberia	0.02	0	-0.03	-0.03	0.02	0***	0.86
Madagascar	0.08	0	-0.08**	-0.03	0.01	0***	0.58
Malawi	0.02	-0.01***	-0.1***	-0.03	-0.03	0***	0.74
Mali	0.08	0	0.06*	-0.11**	-0.04*	0.11	0.37
Mozambique	0.05	-0.01**	-0.03	-0.02	0.02	0.24	0.52
Namibia	0.01	0	0	-0.03	-0.02	0***	0.02**
Nigeria	0.16***	0	0.03	-0.11***	-0.03**	0***	0.16
Senegal	0.02	-0.01**	-0.03	-0.04	0.03	0***	0.68
South Africa	0.08*	0	0.04*	-0.06***	-0.02	0***	0.41
Tanzania	0.05	0	-0.12***	0.02	-0.01	0***	0***
Uganda	0.06	0	-0.06**	0.01	0.04***	0***	0.07*
Zambia	0.18***	0	-0.06**	0.04	0.02	0.16	0.32
Zimbabwe	0.06	-0.01**	-0.01	-0.05	0.01	0.06*	0.18

*** p<0.01, ** p<0.05, * p<0.1

† These columns report the p-values from a F-test of joint significance.

Table 3.6 Comparing Predictors of Violence and Political Affiliation

Country	Female	Age	LivingCond	Wealth	EDucation	Ethnicity	Location
Benin	Affiliation	x	Affiliation	x	Both	Both	Violence
Botswana	Both	Affiliation	Both	x	Affiliation	Both	x
Burkina Faso	Both	Violence	x	Affiliation	Affiliation	Both	x
Cape Verde	x	Affiliation	Affiliation	x	Affiliation	Both	Violence
Ghana	Affiliation	x	Affiliation	Violence	x	Both	Both
Kenya	Affiliation	Violence	x	x	Violence	Both	Affiliation
Lesotho	Both	Affiliation	Affiliation	Violence	Both	Both	x
Liberia	Affiliation	Affiliation	x	Affiliation	Affiliation	Both	Affiliation
Madagascar	Affiliation	x	Violence	x	x	Both	Affiliation
Malawi	x	Both	Both	x	x	Both	Affiliation
Mali	Affiliation	x	Both	Both	Violence	Affiliation	Affiliation
Mozambique	x	Both	x	x	Affiliation	Affiliation	x
Namibia	x	Affiliation	x	x	Affiliation	Both	Both
Nigeria	Both	x	x	Both	Violence	Both	Affiliation
Senegal	x	Both	x	Affiliation	Affiliation	Both	Affiliation
South Africa	Violence	x	Violence	Violence	Affiliation	Both	Affiliation
Tanzania	Affiliation	Affiliation	Both	Affiliation	Affiliation	Both	Violence
Uganda	Affiliation	Affiliation	Both	x	Both	Both	Both
Zambia	Both	Affiliation	Both	Affiliation	x	Affiliation	Affiliation
Zimbabwe	x	Violence	x	x	Affiliation	Both	Affiliation
Totals							
Both	5	3	5	2	3	15	4
Affiliation	8	8	5	3	7	3	8
Violence	1	3	2	3	3	2	2
Neither	6	6	8	12	7	0	6

Both indicates the variable was a significant predictor of being a member of either political party, and of violence. Violence and Affiliation indicate it was a significant predictor in the respective category, x for none. A ten percent significance level was assumed throughout the table.

Table 3.7 Variance in Fear of Violence Explained

			Political Affiliation	
Country	Distance	Ethnicity	Predicted	Reported
Benin	0.03	0.09	0.00	0.01
Botswana	0.04	0.14	0.01	0.00
Burkina_Faso	0.03	0.10	0.01	0.00
Cape_Verde	0.02	0.23	0.03	0.01
Ghana	0.05	0.16	0.01	0.01
Kenya	0.08	0.14	0.01	0.00
Lesotho	0.04	0.05	0.01	0.00
Liberia	0.03	0.12	0.00	0.02
Madagascar	0.02	0.13	0.00	0.00
Malawi	0.01	0.07	0.01	0.00
Mali	0.02	0.13	0.00	0.00
Mozambique	0.06	0.11	0.00	0.00
Namibia	0.03	0.14	0.01	0.01
Nigeria	0.03	0.25	0.02	0.02
Senegal	0.03	0.08	0.00	0.01
South_Africa	0.01	0.18	0.00	0.00
Tanzania	0.16	0.28	0.06	0.03
Uganda	0.07	0.13	0.00	0.02
Zambia	0.05	0.08	0.01	0.01
Zimbabwe	0.05	0.10	0.01	0.02

Each cell reports the R-squared value of a regression with the respective set of dummy variables. Predicted and Reported affiliation report the R-squared value from a linear regression with either the reported or predicted likelihood of being affiliated with the majority or largest opposition party.

Table 3.8 Mean violence by Affiliation, Demeaned by District

Country	N	Overall Mean	Differences From Majority		
			Second	Other	None
Benin	1200	0.58	-0.17	0.15*	-0.2***
Botswana	1200	0.27	0.08	-0.11	0.06
Burkina_Faso	1200	1.01	0.26	0.27**	-0.03
Cape_Verde	1264	0.50	-0.03	0.13	-0.05
Ghana	1200	0.91	0.18**	-0.37**	0.18***
Kenya	1104	1.82	-0.04	0.18*	0.10
Lesotho	1200	0.98	0.02	0.03	0.01
Liberia	1200	0.95	0.18*	0.01	0.35***
Madagascar	1350	0.85	0.24	0.16	0.02
Malawi	1200	1.01	0.14	0.05	-0.02
Mali	1232	0.98	0.16	0.03	-0.03
Mozambique	1200	0.88	0.25	-0.15	0.01
Namibia	1200	1.13	0.08	0.15*	0.04
Nigeria	2324	1.43	-0.03	-0.10	0.18***
Senegal	1200	0.83	0.41***	0.19**	0.05
South_Africa	2400	0.97	0.02	-0.01	0.07*
Tanzania	1208	0.93	0.08	0.01	0.17**
Uganda	2431	1.59	0.45***	0.10	0.13**
Zambia	1200	1.09	0.18*	-0.23**	0.16*
Zimbabwe	1200	2.41	-0.4***	-0.30	-0.08

The p-values indicate whether the mean is statistically different from the mean fear of violence reported by respondents affiliated with the majority party. All values are reported as average deviations from the district mean for the relevant party.

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