

Preying on the poor

The impact of repressive violence on citizen behavior

Lauren E. Young*

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Abstract

State repression is used in many countries by unpopular regimes to force citizens to speak or act against their preferences. It is often assumed that repression is effective in shaping citizen behavior; however, there is enormous heterogeneity in how citizens respond. I argue that repression is most effective against the poor, and that this effect may be driven by both psychological and physical vulnerability. I test my theory using data from the case of Zimbabwe and two empirical strategies at the constituency and individual level that draw on exogenous variation in economic scarcity and exposure to repressive violence. The results presented here show that repression is more effective in shaping the behavior of citizens living in a state of economic scarcity. These results are robust across three analyses using independent variation in repression and scarcity. I also rule out alternative explanations by showing that repression and scarcity are not accompanied by changes in preferences, differences in the type of repression, or differences in the effectiveness of clientelism. These results help explain why underdevelopment is associated with authoritarian, non-responsive institutions, and why we see less redistribution to the poor than we would expect in many poor democracies.

*PhD Candidate, Columbia University Department of Political Science (ley2106@columbia.edu, www.laurenyssayoung.com). Thanks to Sarah Berens, Daniel Berger, Graeme Blair, Christopher Blattman, Alexander Coppock, Macartan Humphreys, Albert Fang, Grant Gordon, Kimuli Kasara, Adrienne LeBas, Isabela Mares, Suresh Naidu, and Gabriella Sacramone-Lutz for comments on the design and analysis of this project, as well as seminar audiences in the Columbia Political Economy Breakfast, Columbia Graduate Seminar, Midwest Political Science Association, and the American Political Science Association. I am indebted to many anonymous political analysts and scholars in Zimbabwe who shared the data and insights that made this project possible. The author thanks the U.S. Institute for Peace, the Columbia University Graduate School of Arts and Sciences and the Department of Political Science, the International Peace Research Association Foundation, and the National Science Foundation for support for her travel and time.

1 Introduction

In March 2008, Zimbabweans went to the polls. Economic mismanagement had given many reason to vote against the incumbent regime despite a large police presence at the polling stations and a history of electoral intimidation and violence during elections. In the days after the poll, the results were not announced and police were deployed in the streets, leading opposition voters to fear that the government was rigging the results. During this period, voters sent messages to the BBC ([BBC News, 2008](#)):

“Police have already been deployed on the streets in Harare and are telling people not to assemble, to keep quiet. I have never been this afraid before.”

“People talked freely - even in the voting queues - of their discontent at Mugabe rule. They openly said they would vote for change...”

“...people will burst with anger and probably demonstrate or become violent.”

The reactions of voters to the same series of political events are highly diverse. Fear, indifference, and anger are all common reactions by citizens to threats of punishment by the state for political action or speech. What explains this heterogeneity in voter reactions to state intimidation?

Intimidation and the threat of violence are frequently used by ruling parties to deter resistance to the regime. [Straus and Taylor \(2012\)](#) report that between 1990 and 2007, around one in five elections in sub-Saharan Africa was affected by significant levels of electoral violence, usually perpetrated by the incumbent. [Hafner-Burton et al. \(2014\)](#), using data assembled by [Hyde and Marinov \(2012\)](#), find that in 30% of elections worldwide between 1981 and 2004, governments used violence or harassment against their opponents during the pre-election period. I define repression as “the actual or threatened use of physical sanctions against an individual or organization, within the territorial jurisdiction of the state, for the purpose of imposing a cost on the target as well as deterring specific activities and/or beliefs perceived to be challenging to government personnel, practices or institutions” ([Goldstein, 1978](#), xxvii). This definition thus focuses on coercive applications of the state that violate First Amendment rights like freedom of speech, freedom of association, as well as personal integrity, and excludes the use of coercion to deter or punish theft or violent crime

(Davenport, 2007).

Ruling parties use acts of repressive violence to send clear signals to citizens that they will be punished for publicly expressing dissent by casting the wrong vote, chanting the wrong slogan, or wearing the wrong t-shirt. Yet there is considerable variation in the extent to which such strategies of intimidation are successful: even in extremely repressive contexts some people are usually willing to resist, while others acquiesce to much lower levels of intimidation. We also see dramatic shifts in when these strategies work: repression can effectively keep unpopular regimes in power for decades, and then suddenly catalyze mass protest. Indeed, one of the most puzzling patterns in the repression literature is why states continue to regularly resort to repression when there is so little evidence that it works (Davenport, 2007).

This variation in outcomes creates a puzzle: for some, the threat of repression pressures people to stay out of politics or to feign support for the repressive regime. For others, however, the same level of repression is not convincing, or can even increase the desire to dissent. What determines when coercive violence works? What type of citizens are more likely to submit to the will of the regime in the face of a repressive threat? Under what conditions might repression actually lead to more resistance?

I argue that voters who are particularly vulnerable to violence are the most likely to capitulate in the face of violent threats. I argue that the poor are more likely to capitulate to threats because they are both more physically and psychologically vulnerable to violence. Physical vulnerabilities include factors that influence voters' abilities to protect themselves against violence, such as the ability to invest in security or flee. Psychological vulnerabilities include factors that make voters more likely to perceive an elevated personal risk or process information about political risk in a sub-optimal way.

This paper brings micro-level evidence to bear on the question of when intimidation effectively causes citizens to hide their true anti-regime preferences. My goal in this analysis is thus to identify the causal effect of both violence and poverty on the expression of dissent. It is important in such an analysis to deal with the potential for confounding factors that are correlated with poverty and

the expression of dissent, or selection into exposure to violence. In an ideal world, I would exploit random variation in both poverty and violence to identify the causal effects of these two factors on dissent. In this paper, I use two empirical tests to identify the causal impact of violence and poverty in separate analyses. At the individual level, I build on the analysis of [Garcia-Ponce and Pasquale \(2014\)](#) to use random variation in recent exposure to violence to identify the causal effect of violence. At the constituency level, I exploit random rainfall shocks in areas dependent on agriculture as shocks to economic scarcity.

I find that pre-election repression is associated with significant increases in the vote share of the ruling party. This effect is concentrated in poor constituencies, and is consistent when I operationalize poverty both with the endogenous measure based on the weight-for-height measures of children under five and the random variation in the quality of the rainy season rainfall. Furthermore, I find using the research design of [Garcia-Ponce and Pasquale \(2014\)](#) that the very poor are significantly more likely to hide their support for the opposition after state violence, while the behavior of citizens who are better-off is relatively unaffected.

This project relates to the large literature that argues that underdevelopment causes poor and undemocratic institutions ([Lipset, 1959](#); [Przeworski, 2000](#); [Svolik, 2008](#)). If the poor are easier to repress, then this suggests another channel through which poverty stymies the development of responsive democratic institutions. Furthermore, the relatively high ability of politicians to get votes from the poor using violence may help explain why democratic states in countries with large populations of poor voters can get away with failing to redistribute wealth. In this way, my argument is related to [Bates \(1981\)](#), who argues that leaders redistribute away from rural areas because they are less likely to rebel. If the poor are more easily repressed than the rich, politicians in semi-democratic systems have less of an incentive to be responsive to their demands.

XX election violence literature XX

Finally, this project has implications for interventions to prevent and mitigate election violence. Identifying where election violence should have the largest effects on voting behavior can help target efforts to deter malfeasance such as election monitoring to maximize its impact on the legitimacy of

the election.

2 Theory

In this section I discuss theoretical explanations regarding how poverty at the national, subnational, and individual level might affect the incidence and effectiveness of election violence. I divide explanations into three main types: those related to constraints on election violence, those related to the supply of violence, and those related to the effectiveness of violence in influencing voters. I contend that variation in the effectiveness of violence due to poverty have largely been overlooked, but that violence might be more effective in changing the behavior of the poor because they poor are more vulnerable to the physical and psychological effects of violence.

There is mixed evidence about how election violence may be affected by poverty. First, there is considerable evidence that election violence is more common in poor countries. For example, the NELDA dataset developed by [Hyde and Marinov \(2012\)](#) shows a very strong negative relationship between GDP per capita and the incidence of pre- and post-election violence. This relationship does not hold up to the inclusion of country fixed effects, however, meaning that the same country is not more likely to experience election violence in years when its GDP per capita is lower. In addition, [Taylor et al. \(2013\)](#) do not find a correlation between GDP per capita and the severity of election violence within sub-Saharan Africa.¹

There are many possible reasons for this pattern, but existing research has tended to focus on how poverty reduces constraints to the use of violence. At the national level, poor countries tend to have weaker institutions that could constrain violence such as the security sector, judiciary or press, all of which have been linked at a cross-country level to the use of illicit election tactics including violence ([Collier and Hoeffler, 2009](#); [Hafner-Burton et al., 2014](#)). Some have argued that democratic institutions as a whole constrain the use of electoral intimidation ([van Ham and](#)

¹[Taylor et al. \(2013\)](#) only include GDP per capita as a control variable, however, and their regressions include a number of controls that are plausibly part of the mechanism linking poverty to election violence such as social conflict, democratization, and previous election violence.

Lindberg, 2015). Similarly, Hoglund (2010) argues that low rule of law and a “culture of impunity” are “conditions enabling the use of electoral violence” (423).

The number of people willing to participate in election violence may also be affected by poverty. In particular, poverty may reduce the opportunity cost of violence such that more people are willing to participate, or foster grievances that politicians can take advantage of around elections (Boone and Kriger, 2010). This relationship may depend on the strategic logic of violence. In Kenya, election violence was used to displace opposed voters and perpetrators were rewarded with the land and assets that the victims left behind (Boone, 2011). In this case, there is evidence that election violence was more likely in areas with fewer people below the poverty line (Kasara, 2016).

At the individual level, there is fairly consistent evidence that the poor are more likely to be afraid of election violence, at least in Africa. Analyses using the Afrobarometer data consistently find that across all countries in the Afrobarometer sample (Arriola and Travaglianti, 2015) and in those with the highest incidences of fear of election violence, the poor are more likely to report being afraid that they personally will be targeted with violence during elections (Mares and Young, 2016). Analysis of data from individual countries has found mixed results, with some showing that the poor are more likely to experience violence (Sacramone-Lutz, 2013), while others find no significant effect (Bratton, 2008; Dercon and Gutierrez-Romero, 2011; Gutiérrez-Romero, 2014).

Although there is strong evidence that the poor report that they are more afraid of violence, there has been relatively little theory to explain why poor voters might be more exposed or responsive to violence. I suggest two reasons that they might be. First, the poor are more physically vulnerable to the effects of violence. The non-poor can mitigate and cope with the effects of violence by making investments in defense, flight, or healthcare if they are victimized. For example, many Nairobi residents in areas that had been affected by violence in 2008 left the city for the countryside during the 2013 election to protect against the possibility of violence. While the rich in developing countries tend to live behind large fences and can wait out short periods of instability, the very poor often live in slums without secure walls or locks on their doors. Finally, while most people presumably want to avoid violence, for someone living in extreme poverty who depends on daily

wages or cannot afford to pay a hospital bill, violence has particularly negative effects. For all of these reasons, the threat of violence may be a particularly strong incentive for poor voters.

Furthermore, the poor may be more psychologically vulnerable to the effects of violence. In other work, I show that there is considerable variation in how citizens interpret signals about their own risk of facing repressive violence. In most cases, citizens change their behavior not because they are physically incapacitated by violence but because of the perceived threat of violence if they take certain actions. The perceived threat of violence must be estimated on the basis of rare, noisy and potentially biased signals (Stern and Hassid, 2012; Stern and O'Brien, 2012). As a result, variation in how information about the risk of repression is perceived and processed can have a large mediating effect in how exposure to repression influences citizen behavior. I show in other work that variation in self-efficacy, or beliefs about one's general ability to cope with difficult situations, is strongly related to higher levels of fear, a higher perceived risk of repression, and lower pro-opposition political participation (Young, 2016). In many contexts, marginalized individuals such as the poor tend to have lower self-efficacy than the better-off. In addition, living in a state of scarcity may create a cognitive load that impedes rational decision-making or higher time discount rates. All of these factors may make it harder for the poor to process information about the risk of repression, and more likely that they will have a higher estimation of their personal risk of repression than a person who is not living in a state of scarcity. Indeed, the fact that the poor do not seem more likely to report actual exposure to violence in Kenya (Dercon and Gutierrez-Romero, 2011; Gutiérrez-Romero, 2014), but do report higher fear of election violence in the Kenya Afrobarometer after the 2008 election (Mares and Young, 2016) could be explained by variation in how fearfully citizens react to incidents of repression.

Finally, it is important to point out that showing that violence is more effective against the poor does not necessarily imply that we should see violence used more frequently or severely against poor voters. Political violence is one of a number of illicit electoral strategies, including vote buying and fraud. If politicians substitute between these strategies to use an optimal mix of positive and negative inducements and fraud against different subgroups, then it is the relative effectiveness

of violence compared to alternative strategies that should influence politicians' decisions. Most importantly, given the strong theory and evidence that vote-buying is more effective with poor voters, as poverty increases violence may become more effective in shaping voter behavior but still increasingly less effective in shaping the vote share relative to vote buying.

3 The Zimbabwean Case

Since gaining independence in 1980, Zimbabwe has been an electoral autocracy (Kriger, 2005). It holds regular elections but these have not resulted in any peaceful transitions of power between parties, in part because of the ruling party's use of violent force. ZANU-PF grew out the independence struggle and enjoyed widespread popular support in the 1980s and 1990s that diminished in the 1990s in part due to a severe structural adjustment program (LeBas, 2011). Beginning around 1999, an opposition party that grew out of the country's major trade union has credibly challenged the ruling party ZANU-PF. The first sign of real trouble for the ruling party came in a constitutional referendum in 2000, when the regime failed to secure the votes to pass a constitution that would have expanded its powers. The referendum process was poorly managed by the regime, with squabbles over unpopular clauses covered extensively in the media (LeBas, 2011, 138). The referendum was not preceded by significant intimidation or threats, and is largely interpreted as an expression of voters' true preferences.

Zimbabwe's current period of serious electoral violence began in 2000. The opposition party MDC had just orchestrated the unexpected defeat of ZANU-PF's proposed constitution in a referendum. Shortly thereafter, the government stopped preventing and ultimately began encouraging extra-judicial efforts to expropriate land from white farmers. (LeBas, 2006) describes the events that set off these attacks in the constituency of Bindura, where an MDC supporter was killed, the MDC offices were petrol-bombed, and two MDC vehicles were burned within a few days of the governor announcing at a rally that ZANU-PF members "must warn supporters of opposition parties that Zanu PF is well known for spilling blood" (427). Continuing into 2000 after

the referendum, violence took two main forms. First, there were clashes between land invaders, often led by self-claimed veterans of Zimbabwe's independence struggle, and white commercial farmers and their workers. Second, there were violent brawls in communal and urban areas that seemed to be opportunistic and disorganized skirmishes between low-level supporters of the MDC and ZANU-PF (LeBas, 2006).

By 2002, violence was more directly organized by party elites. In 2001 the government initiated a national youth training program which created a nationwide militia for the party. These militia set up bases around the country and began using more sophisticated forms of violence such as torture (LeBas, 2011; Reeler, 2003). State-sponsored violence has subsequently been used to directly target opposition candidates and voters around elections. The worst cases and highest rates of violence occurred in rural areas, particularly in ZANU-PF strongholds. Party agents, youth wing members, members of the association of war veterans from Zimbabwe's independence struggle, soldiers, and traditional leaders have all played a role in organizing intimidation campaigns around recent elections (Bratton and Masunungure, 2008). This type of violence peaked with the popularity of the MDC in 2008, when hundreds were killed and hundreds of thousands displaced after the MDC won the first round of the presidential election.

How was violence targeted during this period? It is initially puzzling that violence was most concentrated and severe in areas with high levels of electoral support for ZANU-PF where the ruling party candidates' positions were at little risk of being lost. For example, Mashonaland Central and Mashonaland East, the two provinces that had seen the highest rates of support for the government's constitution during the 2000 referendum, experienced the most violence during the 2000 parliamentary elections. LeBas (2006) argues that violence occurred largely as a function of ZANU-PF's internal politics. LeBas documents through interviews with ruling party elites that they themselves saw the political violence during this period as a way for "particular elites within the party to expand their control over decision making, threaten those they suspected of opposing the centralization of party, and pursue specialized, somewhat reactionary ideological agendas" (429). This strategy, which was ultimately successful in consolidating power within ZANU-PF, emerged in

large part because by the early 2000s some members of the ruling party leadership had lost control of party structures in their constituencies. The use of youth militias and party-affiliated civil society groups enabled them to retake control and threaten local challengers (LeBas, 2006).

In 2005, pre-election violence was significantly reduced, with most analysts arguing that the ruling party aimed to avoid the international condemnation that had followed the brutality of the 2002 elections (ZHRNGOF, 2005). However, low-level, opportunistic violence continued to target MDC activists and supporters, and by many accounts served to remind people of the capacity of the ruling party to exact the kind of brutal campaign that they had carried out in 2002.

4 Methodology

My theory makes predictions about the causal effect of both repressive violence and poverty on the expression of dissent. Testing for such a causal relationship is difficult because evidence based on correlations is likely to be biased by confounding factors, reverse causality, and selection bias. The ideal design from a methodological perspective would randomly assign both poverty and exposure to violence to establish a design-based counterfactual. In this case, that is not possible for both ethical and logistical reasons. The purpose of this project is to bring evidence to bear on how a government policy of repression is related to subsequent changes in citizen behavior at a significant scale.

My research design combines two identification strategies applied in two separate analyses that draw on exogenous variation in poverty and exposure to state repression to address the major challenges to inference that testing such a theory implies. The first challenge is that it is difficult to separate out the role of poverty from the role of other factors such as education, ethnicity, or occupation that may be correlated with socioeconomic status. In a constituency-level analysis, I draw on random variation in poverty caused by bad rainy season rainfall to identify the causal effect of variation in poverty. The second challenge is that citizens with certain characteristics may select into exposure to violence, and this propensity may be correlated with willingness to express

dissent. I extend an individual-level analysis by [Garcia-Ponce and Pasquale \(2014\)](#) to exploit random variation in the order in whether survey respondents were surveyed immediately before or after a repressive event in their district. Table 1 describes for each analysis which explanatory variable is identified and which is endogenous.

Table 1: Level of analysis and exogeneity of key explanatory variables

		Variable	
		Poverty	Repression
Level	Individual	Endogenous	Exogenous
	Constituency	Exogenous	Endogenous

My methodology makes several contributions. First, I provide both a constituency- and individual-level test of my theory and show that it applies to both voting for a less-preferred party and lower-level forms of preference falsification like revealing anti-regime political preferences to a stranger. Second, in different sections of the analysis, I provide causal estimates of the effect of scarcity due to random variation in the quality of the rainy season and violence due to random variation in whether an individual was surveyed before or after a violent event in his district. Taken together, these results provide strong evidence that there is a causal relationship between economic scarcity and the propensity to capitulate in the face of violent threats.

4.1 Constituency-level analysis

4.1.1 Research design

In the first section of this paper, I analyze how pre-election violence is related to changes in the vote share of the ruling party. I use several strategies to create a counterfactual for the ruling party's vote share in the constituency. First, I leverage a constitutional referendum that occurred in March 2000 that occurred before ZANU-PF started using significant levels of violence against voters. This poll took ZANU-PF by surprise: from independence in 1980 until that 2000 election, ZANU-PF easily won supermajorities in elections based on their popularity. At the end of the 1990s, they lost a significant amount of support as the economy began to slow down but were unprepared to

lose a constitutional referendum that was essentially a referendum on the president's core policies and leadership. Thus, I use the level of support for ZANU-PF's policies from this referendum as a baseline measure and analyze whether violence increases support for ZANU-PF from this baseline.

Second, I use time and region-specific fixed effects to control for all time- and province- or constituency-specific factors that influence the degree to which ZANU-PF's support deviates from this baseline. Indeed, support for ZANU-PF was overall increasing from 2000 to 2005. Similarly, some constituencies, such as those in the Matabeleland region that is populated by the Ndebele minority ethnic group, have lower support for ZANU-PF. In my preferred specification, I use fixed effects for each constituency and election to implement a difference-in-difference design where the coefficients are estimated using the variation in the severity of violence in a specific constituency over time.

Last, I use random variation in the quality of the rainy season to identify the impact of scarcity on the effectiveness of violence. I hypothesize that in poor constituencies, violence should be associated with a higher increase in ZANU-PF's vote share. For my first set of results, I measure scarcity using data from the Zimbabwe Demographic and Health Survey (DHS) on wasting, a measure of child malnutrition. However, wasting is correlated with a number of other factors that might influence the effectiveness of violence such as ethnicity or flows of patronage from ZANU-PF.

To isolate the random variation in scarcity, I use variation in the quality of the rainfall in each constituency during the rainy season preceding the election. Specifically, for each constituency I calculate the 20-year average rainfall during the rainy season from 1990 to 2010 using data collected by satellite by the Climate Hazards Group InfraRed Precipitation with Station (CHIRPS). Average rainfall by constituency is not random, as it varies in predictable ways across constituencies. However, whether the rainfall in a given December-February rainy season is above or below the constituency average is as-if random as it is orthogonal to the political characteristics that might be correlated with my dependent variable. Thus, I create a dummy variable for whether the previous rainy season's rainfall was below average and use this as a random shock to the economic status of Zimbabwean voters.

4.1.2 Specifications

For this section of the analysis I estimate the following specification to test whether state-sponsored pre-election violence increases the ruling party's vote share:

$$Y_{it} = \beta violence_{it} + \gamma_i + \lambda_t + \varepsilon_{it}$$

where Y_{it} is the difference between ZANU-PF's vote share in an election post-March 2000 and its vote share in the March 2000 referendum. γ_i is a constituency-specific fixed effect (in some specifications I use a province fixed effect, but in my preferred specification it is at the level of the individual unit, the constituency) and λ_t is an election-specific fixed effect. $violence_{it}$ is the number of violent events that have occurred in a constituency during the three-month period immediately prior to the election. For specifications where I include fixed effects for each province instead of each constituency, I also estimate the effect of some constituency-level controls.

To estimate the heterogeneous effects I test for the interaction of pre-election violence and a measure of scarcity in a given constituency:

$$Y_{it} = \beta_1 violence_{it} \times poverty_{it} + \beta_2 violence_{it} + \beta_3 poverty_{it} + \gamma_i + \lambda_t + \varepsilon_{it}$$

where the coefficient of interest β_1 is on the interaction of the extent of violence $violence_{it}$ and the extent of economic scarcity $poverty_{it}$ in a constituency.²

4.1.3 Data

I construct a time series dataset of constituency-level characteristics using four primary data sources. First, I build a time series of ZANU-PF's vote share in all elections taking place between 2000 and 2005. During this period there were four elections: the March 2000 constitutional referendum,

²This specification does not always include the direct effect of $poverty_{it}$ because in some cases I use non-time varying measures of poverty and they are subsumed by the constituency fixed effects. Specifically, the exogenous measure of poverty (deviation from average rainfall) is time-varying and constituency-specific, while the endogenous measure (wasting in children under five) is measured just once per constituency.

a June 2000 parliamentary election, the March 2002 presidential election, and a March 2005 parliamentary election. During this period Zimbabwe had 120 unique constituencies across 10 provinces; I stop at 2005 because Zimbabwe had a major redistricting before the 2008 election that increased the number of constituencies to 210 that makes it very difficult to trace continuous vote shares from the prior period. Zimbabwe also underwent two minor redistricting exercises in 2000 between the referendum and parliamentary elections and between the 2002 and 2005 elections. Both of these were highly politicized, and to deal with the threat that these changes pose to my analysis I create measures of the extent to which the constituency boundaries changed during each of these delimitation exercises. I include these as controls in some analyses and in others drop constituencies that had more than a minor change to their boundaries.

To measure pre-election violence, I coded text reports of cases of state violence against civilians during the three months leading up to the elections in 2000, 2002, and 2005. These reports were collected by the Zimbabwe Human Rights NGO Forum (ZHRNGOF), a network of 17 human rights NGOs that pooled data on cases of violence and collaborated to conduct research and lobby for justice. ZHRNGOF produced monthly reports on major violent events from 2000 to 2009 with descriptions of the event and information on its location (meaning the constituency it occurred in) and date. I coded these for the perpetrator and victim types (ZANU-PF vs. MDC), type of abuse, and number of victims. The majority of their cases are reported from member organizations that had an active monitoring presence in the constituencies, although in some cases they also drew information from local media reports.

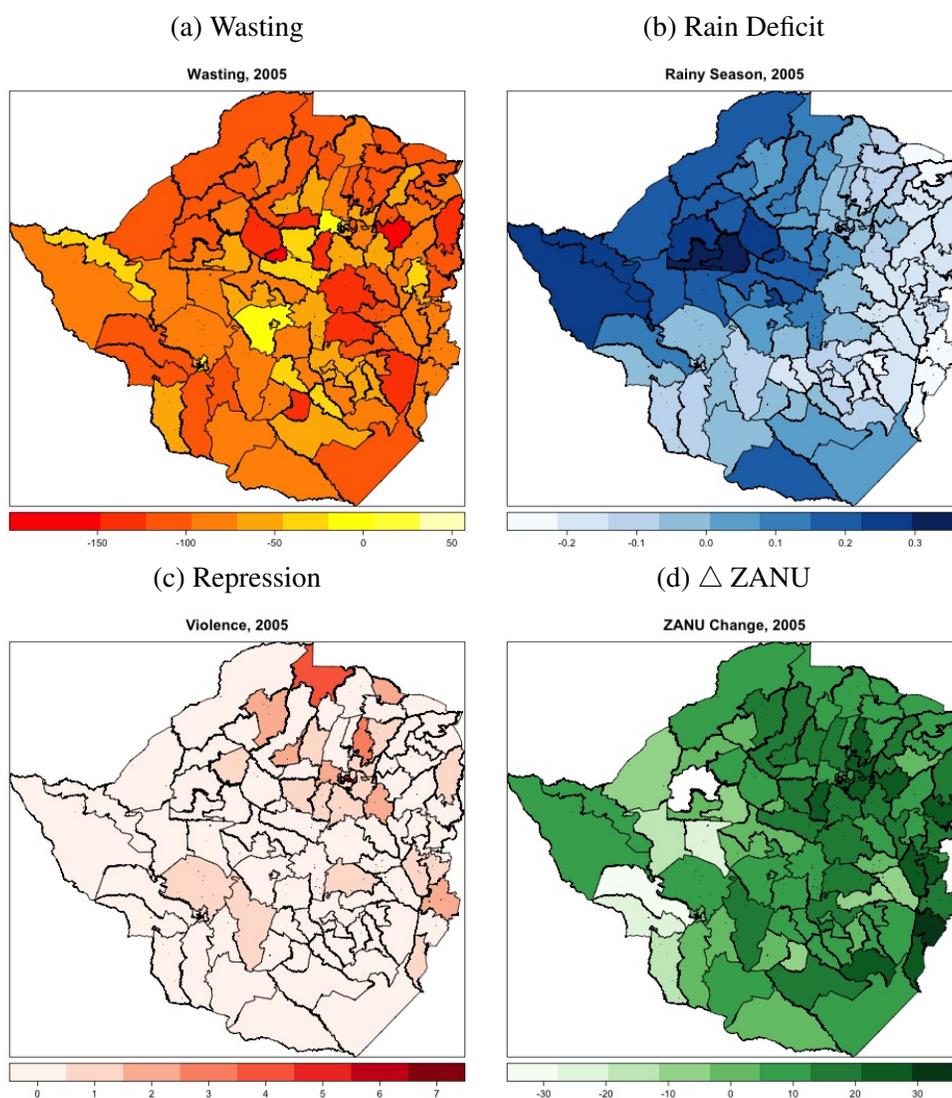
Third, I measure poverty by constituency using data collected in the 2005 Zimbabwe DHS on children's nutritional status. Specifically, I calculate using ArcGIS the average weight-for-height z-score of children surveyed by the DHS in each constituency. The weight-for-height z-score compares surveyed children to an international healthy standard taken from children in the US. From this, I calculate a normalized measure of wasting severity that is the inverse of the weight-for-height z-scores.

Last, as a second measure of poverty I calculate the extent to which the December to February

rainy season preceding each election deviates from the 20-year average rainy season rainfall for that constituency. I again calculate this measure using ArcGIS from monthly rainfall satellite images collected by the UC Santa Barbara CHIRPS project. I calculate the rainy season rainfall for each constituency for each year, subtract out the 20-year average rainy season rainfall, and then create a dummy for whether the rainy season preceding each election was above or below average.

Figure 1 displays the main variables in 2005 visually.

Figure 1: Main explanatory and dependent variables in the constituency-level analysis, 2005



4.2 Individual-level expression of anti-regime preferences

4.2.1 Research design

Second, I test my theory of the effectiveness of repression using opinion data from five rounds of Afrobarometer surveys in Zimbabwe conducted between 1999 and 2012. The strategy that I employ to test this theory is adapted from [Garcia-Ponce and Pasquale \(2014\)](#). I subset the survey data to only communities that have experienced violence in the week before or after being surveyed. Thus, this strategy controls for the characteristics that determine whether communities are targeted with violence and isolates the plausibly random variation in timing within a very small window.

I first test whether recent pre-survey violence is associated with a decline in willingness to publicly reveal support for the opposition. Because some Zimbabwean respondents are falsifying their preferences in the Afrobarometer surveys, it is impossible to determine the true level of support for the two main parties or which voters actually support the opposition or the ruling party. However, comparing rates of support for the ruling party areas with pre-survey violence to those with post-survey violence allows us to measure the proportion of voters who switch their revealed preference as a result of the treatment.

Second, I interact pre-survey violence with a respondent's self-reported poverty level, which I predict makes them more likely to be influenced by pre-survey violence. I operationalize poverty by creating a composite measure of how frequently a respondent goes without food, and how frequently she goes without a cash income.

This strategy has three key identifying assumptions that are important to clarify:

1. The timing of the survey within a district is orthogonal to violence
2. Repression does not change preferences toward the regime
3. Repression is not correlated with other events that affect preferences or falsification

The first assumption implies that the surveyors do not include or exclude households from the survey based on whether they were recently exposed to repressive violence. Interviews with the head of the survey firm that conducts the Afrobarometer in Zimbabwe and the Afrobarometer's

sampling protocols confirm that this is the case.

The second assumption is necessary to identify variation in willingness to express dissent because two parameters fundamentally shape the expression of dissent: preferences over the regime and willingness to express them. Citizens who do not express dissent may do so either because they truly like the regime, or because they do not but are not willing to run the risk of expressing it. Therefore, to identify changes in willingness to express dissent, preferences over the regime must be held constant. In this case, and particularly during the period under study, voters have strong partisan identities that are unlikely to be affected by a single recent experience (LeBas, 2011).

Last, we must assume that repression is not correlated with other events that might affect preferences or willingness to express dissent. To the extent that repression occurs in response to opposition mobilization, for example, the results would be biased by the effect of these other recent events on expression of dissent. I test this assumption empirically by controlling for recent opposition mobilization.

Given these assumptions, I am able to identify the causal effect of violence on willingness to reveal support for the opposition. However, the key tests of my hypotheses come from the interactions of this causal effect with covariates that are not randomly assigned. While these interactions do show that certain types of individuals are more likely to respond to violence in certain ways, we cannot interpret the effects as the causal impact of economic vulnerability on the propensity to comply with repression. Nevertheless, they provide important insight into when intimidation has the intended effect on citizen behavior.

4.2.2 Specification

To test my theory at the individual level, I run a regression of revealed support for the opposition (or, in alternative specifications, support for the ruling party, no party preferences, or refusal to answer the question about party identification) on exposure to pre-survey violence. The main variable of interest is the interaction between individual-level characteristics and district-level exposure to pre-survey violence. To take into account the propensity of the respondent to be exposed to violence,

I include district fixed effects. This implies that the estimated coefficient on pre-survey violence comes from within-district variation in whether someone was surveyed just before or just after a violent events, which I argue is orthogonal to the political beliefs that I use as an outcome variable.

The specification that I estimate in this section is therefore:

$$Y_i = \beta_1 violence_i \times poverty_i + \beta_2 violence_i + \beta_3 poverty_i + \gamma_j + \lambda_t + \varepsilon_i$$

where i represents each individual, j each district, and t each Afrobarometer round (i.e. a dummy for being surveyed in 2004, 2005, 2009, etc). The main coefficient of interest is on the interaction between whether an individual was exposed to pre-survey state-sponsored violence and their individual level of experienced poverty. I control for each district with the fixed effect γ_j , and for each Afrobarometer round with λ_t . I also include the direct effects of exposure to violence and poverty.

4.2.3 Data

I analyze the impact of violence on revealed support at the level of the individual Afrobarometer respondent using data on violence at the level of the district, the second-lowest administrative unit in Zimbabwe. The ACLED data includes geo-coordinates that could enable more specific targeting of violent events, but these geo-coordinates are unreliable as events that are identified with a low level of precision (such as an event that took place in the city of Harare) are geo-localized at the center of the geographical area. Thus, the lowest consistent level of geo-localization for all the events is at the district level. The Afrobarometer data is identified at the constituency level, which I aggregated up to the district. In the case of Harare, I followed the convention in the ACLED data of splitting the district into several large urban areas (Harare, Chitungwiza, and Epworth) that are included within the district of Harare.

To measure exposure to violence, I use violent events from the ACLED dataset. ACLED measures violence from a variety of public sources including newspapers and NGO reports. Although recent several studies have shown that the ACLED data is not an exhaustive measure of violence,

this is not a major concern for this particular study for several reasons. First, because I am looking at the effect of violence on citizens at the district level, the type of violent events that are relevant for this analysis are fairly large-scale, public events. Second, compared to many conflict-affected countries covered in the ACLED data such as the Democratic Republic of Congo and Afghanistan, Zimbabwe has a robust independent media and civil society that tracks state violence. Many of the ACLED records draw from these sources, including the ZHRNGOF.

From the ACLED data, I excluded events that were not “violence against civilians” or “riots/protests.” I constructed measures of the date of violent events and the perpetrator’s affiliation (government or MDC) based on the identity of the perpetrators or their allies. A table of the coding of the perpetrator’s affiliation is in Appendix B.2.

I chose the measures in the Afrobarometer data to use to examine heterogeneous effects based on the predictions of my theory and the availability of the data. The Afrobarometer was fielded in Zimbabwe over five different rounds, with only a handful of questions asked in the same way in multiple rounds. The measures for gender, age, education, and employment were straightforward. The measure of poverty of food and a cash income are strong indicators of economic vulnerability and were included in all five rounds in the same way.

5 Results: Constituency-level analysis of voting

5.1 Main effects: Pre-election violence and vote share

In this section I present the results of my tests at the constituency level. As described in Section 4.1, the unit of analysis is the constituency-election. The dependent variable is the deviation in a given election from the baseline measure of support in the referendum vote in 2000. The independent variables include dummies for whether the delimitation exercises in 2000 and 2005 resulted in major, minor, or no changes in the constituency boundaries. All specifications include election (time) fixed effects, and progressively include province or constituency level fixed effects. Finally, Table 2 is split between results from two different datasets. The first dataset used for Columns

1-4 includes all constituencies, regardless of the extent of redistricting that occurred. The second dataset used for Columns 5-8 excludes constituencies that underwent major changes during either redistricting process.

All models are estimated using OLS, and the standard errors are clustered at the level of the constituency because constituency results are correlated across years.

Table 2: Pre-election violence and changes in ZANU-PF vote share

	<i>Dependent variable:</i>							
	Change in ZANU vote share							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Repression Events	3.94*** (0.68)	1.81*** (0.46)	1.70*** (0.46)	1.72*** (0.53)	4.15*** (0.78)	2.21*** (0.48)	2.04*** (0.48)	1.93*** (0.57)
2000 Redist - Minor			-0.62 (2.46)					
2000 Redist - None			-2.38 (2.23)				-1.49 (1.37)	
2005 Redist - Minor			-0.19 (1.82)					
2005 Redist - None			-3.15* (1.88)				-3.14*** (1.21)	
Intercept	-9.90*** (1.75)	-14.41*** (1.17)	-11.97*** (2.76)	-9.92*** (0.69)	-9.61*** (1.90)	-13.47*** (2.09)	-10.72*** (2.63)	-9.50*** (0.73)
Election FE	✓	✓	✓	✓	✓	✓	✓	✓
Province FE		✓	✓			✓	✓	
Constituency FE				✓				✓
Observations	369	369	369	369	279	279	279	279
R ²	0.23	0.61	0.62	0.85	0.22	0.62	0.64	0.86
Clusters	119	119	119	119	93	93	93	93
Sample	All				Minor or no redistricting			

Standard errors clustered at the constituency level in parentheses.

*p<0.1; **p<0.05; ***p<0.01

Models estimated using OLS. Columns 1-4 are estimated using data from all the constituencies, and 5-8 are estimated using a subset of constituencies that experienced minor or no changes during the delimitation exercises in 2000 and 2005. The dependent variable is the change in ZANU-PF's vote share from the 2000 referendum to the election of interest in 2000, 2002, or 2005. The main coefficient of interest is on Repression Events, which is a measure of the number of events of state violence against the opposition in a given constituency in the three months leading up to an election, and has been logged. All columns include fixed effects for the election. Columns 2-3 and 6-7 add fixed effects for each of Zimbabwe's ten provinces. Columns 4 and 8 replace the province fixed effects with fixed effects for each constituency (the individual unit of analysis).

The results shown in Table 2 show that pre-election violence is strongly associated with increases in ZANU-PF's vote share. This result is largely driven by variation in the number of violent events within a constituency over time: the results from Columns 4 and 8 with both election

and constituency fixed effects are not very different from the results in Columns 2-3 and 6-7 with election and province fixed effects.

To deal with the threat that the redistricting exercises pose to this kind of time series inference, I use two strategies. First, I include controls for dummy variables indicating whether the constituency underwent a major, minor or no boundary changes during the redistricting process. The coefficients on these variables show logical patterns in light of the significant anecdotal evidence that these redistricting processes were politically motivated: constituencies that underwent no redistricting (both in 2000 and 2005) have the smallest changes in ZANU-PF's vote share, while constituencies that underwent minor changes also have smaller changes than those that underwent major boundary changes. While most of these coefficients are not significant, the coefficient on the dummy indicating that a constituency underwent no boundary changes in 2005 is significant in both Columns 3 and 7.³

The second strategy to deal with redistricting is to simply exclude all constituencies that underwent major changes during either of the redistricting processes. When I exclude the constituencies that underwent any major redistricting changes in Columns 5-8, the coefficient of interest on Repression Events actually increases in magnitude.

Substantively, these results imply that a ten percent increase in pre-election repression events is associated with a 0.193 percentage point increase (from the preferred specification in Column 8) in the change in ZANU-PF's vote share in an election over their vote share in the constitutional referendum in 2000. This implies that going from no violent events to one violent event is associated with a 1.3 percentage point increase in ZANU-PF's vote share.

If we interpret this result causally, this implies that if there had been no violence during the 2000-2005 elections, ZANU-PF's national share of the vote would have dropped by 3.5 percentage points in 2000, 1.3 percentage points in 2002, and 0.5 percentage points in 2005. Assuming that this vote share would have gone to the main opposition party, this estimate implies that ZANU-PF would have lost seven constituencies that it narrowly won: Bindura, Chinhoyi, Chiredzi East, Makoni East,

³In Column 3, the coefficient is estimated in comparison to the base case of major boundary changes, while in Column 7, the coefficient is estimated in comparison to the base case of minor boundary changes because all major cases are excluded from the analysis.

Marondera East, and Masvingo North in 2000, and Tsholotsho in 2002.

Robustness checks in Appendix A show that these results are not driven by outlier values of the main independent variable as they are robust to using a binary and rank measure of Repression Events as the key explanatory variable.

5.2 Heterogeneous effects: Endogenous poverty

In this section I test my hypothesis that poor voters are most likely to be influenced by pre-election violent repression. I do this by testing for the heterogeneous effects of pre-election repression events by estimating the coefficient on the interaction of repression events and economic poverty.

I use two primary measures of economic poverty in this analysis. First, I use a measure of wasting of children under five from the Demographic and Health Survey in Zimbabwe in 2005. This is a direct measure of my concept of poverty as it measures the inability of families to provide sufficient calories for their children. However, wasting is endogenous to a range of factors, including others that could theoretically influence the effectiveness of violence. Therefore, as a second measure I also use the rainfall deficit in the last rainy season before the election in question in Section 5.3.

As in Section 5.1, the unit of analysis is the constituency-election. I again run the analysis using both a complete dataset of Zimbabwean constituencies and a second dataset that excludes constituencies affected by major boundary changes during the 2000 or 2005 delimitation exercises. The dependent variable is the change for a given constituency-election from the constituency's results in the pre-violence constitutional referendum in 2000. Table 3 shows the results of this analysis.

Table 3 shows that the impact of pre-election violent repression is higher in constituencies with higher levels of wasting in children under five. The positive coefficient on the interaction term Repression Events \times Wasting indicates that the effect of an increase in the coefficient on the logged measure of repression events is larger in constituencies with more wasting. However, this coefficient decreases slightly in magnitude and loses significance when I include constituency

Table 3: Heterogeneous effects of pre-election violence in poor constituencies

	<i>Dependent variable:</i>							
	Change in ZANU vote share							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Repression Events × Wasting	0.63*	0.54**	0.55*	0.42	0.99**	0.73**	0.76**	0.53
	(0.36)	(0.27)	(0.28)	(0.33)	(0.44)	(0.30)	(0.30)	(0.41)
Repression Events	3.86***	1.71***	1.59***	1.61***	4.05***	2.13***	1.95***	1.85***
	(0.68)	(0.44)	(0.44)	(0.50)	(0.78)	(0.48)	(0.47)	(0.55)
Wasting	0.90	−0.86	−0.65		0.35	−1.13	−0.86	
	(0.87)	(0.69)	(0.70)		(1.02)	(0.75)	(0.77)	
2000 Redist - Minor			−0.03					
			(2.52)					
2000 Redist - None			−2.28				−2.19	
			(2.23)				(1.59)	
2005 Redist - Minor			−0.29					
			(1.88)					
2005 Redist - None			−3.17*				−3.09**	
			(1.89)				(1.23)	
Intercept	−9.75***	−14.82***	−12.20***	−9.88***	−9.33***	−14.25***	−10.49***	−9.43***
	(1.77)	(1.20)	(2.76)	(0.69)	(1.94)	(1.94)	(2.96)	(0.73)
Election FE	✓	✓	✓	✓	✓	✓	✓	✓
Province FE		✓	✓			✓	✓	
Constituency FE				✓				✓
Observations	366	366	366	366	276	276	276	276
R ²	0.25	0.61	0.62	0.85	0.24	0.63	0.64	0.86
Clusters	118	118	118	118	92	92	92	92
Sample			All			Minor or no redistricting		

Standard errors clustered at the constituency level in parentheses.

*p<0.1; **p<0.05; ***p<0.01

Models estimated using OLS. Columns 1-4 are estimated using data from all the constituencies, and 5-8 are estimated using a subset of constituencies that experienced minor or no changes during the delimitation exercises in 2000 and 2005. The dependent variable is the change in ZANU-PF's vote share from the 2000 referendum to the election of interest in 2000, 2002, or 2005. The main coefficient of interest is on the interaction term Repression × Wasting. Repression Events is a measure of the logged number of events of state violence against the opposition in a given constituency in the three months leading up to an election. Wasting is a standardized measure of the weight-for-height z-scores of children surveyed during the 2005 DHS in a given constituency. All columns include fixed effects for the election. Columns 2-3 and 6-7 add fixed effects for each of Zimbabwe's ten provinces. Columns 4 and 8 replace the province fixed effects with fixed effects for each constituency (the individual unit of analysis).

rather than province fixed effects. Nevertheless, these results, although they appear to be driven by variation between constituencies rather than within constituencies over time and are thus more vulnerable to concerns of endogeneity, suggest that there might be a positive relationship between wasting and the strength of the relationship between pre-election violence and ZANU-PF's vote share.

Figure 2 plots the effect of a one-unit increase in the logged number of violent events at different levels of wasting in children under five.

Figure 2: Relationship between pre-election violence and ZANU-PF vote share at different levels of wasting

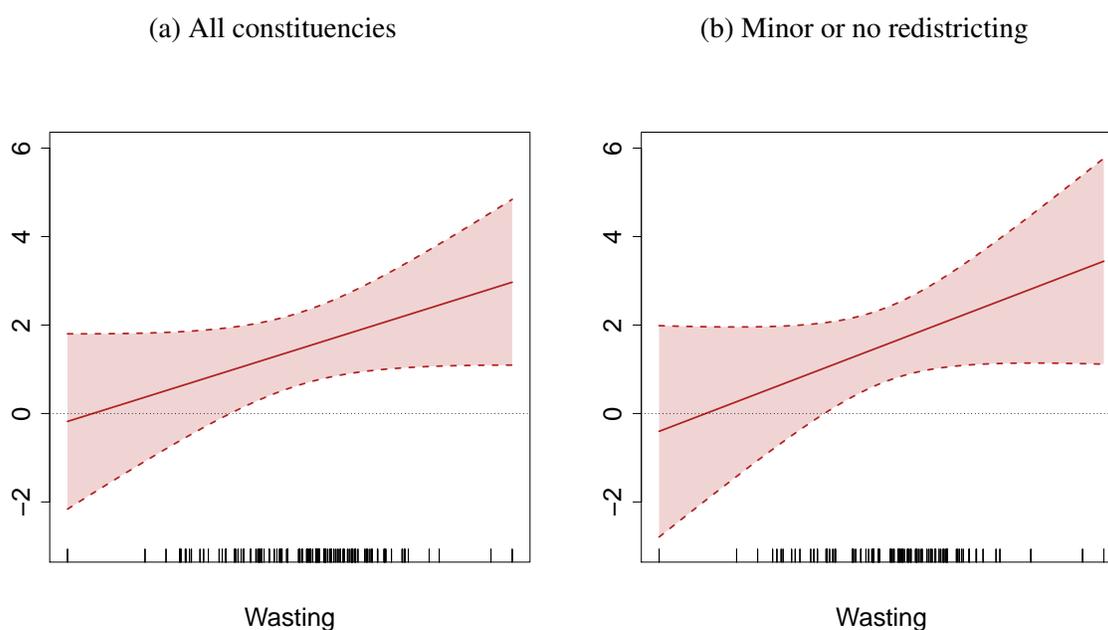


Figure 2 shows that additional pre-election violent events have little or no effect on ZANU-PF's vote share in constituencies that have very low levels of wasting in children under five. For constituencies that are high in this measure of poverty, however, additional violent events have a large and significant effect on electoral outcomes.

5.3 Heterogeneous effects: Exogenous poverty

In the next section I discuss the results of my second test of the heterogeneous effects of pre-election violence based on exogenous variation in poverty. Specifically, instead of using child malnutrition as a measure of poverty, I use exogenous variation in the quality of the annual rainy season for each constituency. As discussed in Section 4.1.3, these measures both proxy for the underlying state of poverty that households are living in. Appendix A.5 shows that the two measures are positively correlated with each other, suggesting that bad rainy seasons do indeed negatively affect households' economic situations.

Whether or not a given rainy season is better or worse than the historical average for a constituency is random – it is not influenced by local political or economic factors that could influence the effectiveness of violence. Each constituency has an equal likelihood of having an above or below average rainfall during a given year. When the rains are bad, it affects not only Zimbabwean families' abilities to produce food for consumption but is also a shock to the incomes of households that produce crops to sell. Thus, this analysis allows me to show that economic poverty has a causal impact on the relationship between repression and the ruling party's vote share.

As in the previous section, the unit of analysis is the constituency-election and the dependent variable is the deviation in a given election of ZANU-PF's vote share from its vote share in the 2000 referendum. Table 4 shows the results of this analysis.

Table 4 shows that the relationship between pre-election violence and ZANU-PF's vote share in constituencies in which the previous rainy season was below-average is significantly more positive than constituencies with normal or above-average rainy seasons. This significant positive coefficient on the interaction term $\text{Repression Events} \times \text{Rain Deficit}$ is robust to the inclusion of controls for the extent of redistricting that occurred in 2000 and 2005 (Columns 3 and 7) as well as fixed effects for each province (Columns 2-3 and 6-7) or for each individual constituency (Columns 4 and 8). There is no significant effect, however, if we do not include geographic controls and the province or constituency level. Importantly, once we exclude constituencies whose boundaries have gone through major changes due to the politicized delimitation process, the relationship between

Table 4: Heterogeneous effects of pre-election violence in constituencies with below-average rainfall

	<i>Dependent variable:</i>							
	Change in ZANU vote share							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Repression Events × Rain Deficit	0.13	2.18**	2.27***	2.16**	0.20	2.84***	2.83***	2.55**
	(1.30)	(0.87)	(0.86)	(0.93)	(1.45)	(1.06)	(1.00)	(1.11)
Repression Events	3.72***	1.24**	1.14**	1.23**	3.89***	1.52***	1.35**	1.22*
	(0.76)	(0.51)	(0.53)	(0.60)	(0.86)	(0.54)	(0.54)	(0.63)
Rain Deficit	-0.58	0.11	0.30	-0.38	-0.53	-0.34	-0.13	-0.34
	(1.94)	(1.35)	(1.26)	(1.52)	(2.36)	(1.47)	(1.38)	(1.75)
2000 Redist - Minor			0.48					
			(2.47)					
2000 Redist - None			-2.85				-3.34***	
			(2.39)				(1.23)	
2005 Redist - Minor			0.07					
			(1.89)					
2005 Redist - None			-3.06				-3.25**	
			(1.93)				(1.30)	
Intercept	-9.25***	-13.75***	-11.39***	-10.68***	-8.86***	-13.13***	-8.83***	-10.12***
	(1.85)	(1.27)	(3.01)	(0.84)	(1.98)	(2.02)	(2.43)	(0.89)
Election FE	✓	✓	✓	✓	✓	✓	✓	✓
Province FE		✓	✓			✓	✓	
Constituency FE				✓				✓
Observations	347	347	347	347	265	265	265	265
R ²	0.24	0.61	0.63	0.86	0.22	0.64	0.65	0.87
Clusters	115	115	115	115	91	91	91	91
Sample			All			Minor or no redistricting		

Standard errors clustered at the constituency level in parentheses.

*p<0.1; **p<0.05; ***p<0.01

Models estimated using OLS. Columns 1-4 are estimated using data from all the constituencies, and 5-8 are estimated using a subset of constituencies that experienced minor or no changes during the delimitation exercises in 2000 and 2005. The dependent variable is the change in ZANU-PF's vote share from the 2000 referendum to the election of interest in 2000, 2002, or 2005. The main coefficient of interest is on the interaction term Repression Events × Rain Deficit. Repression Events is a measure of the number of events of state violence against the opposition in a given constituency in the three months leading up to an election. Rain Deficit is a dummy that takes a value of 1 if the rainy season rainfall in a given constituency was below the 20-year average for that constituency. All columns include fixed effects for the election. Columns 2-3 and 6-7 add fixed effects for each of Zimbabwe's ten provinces. Columns 4 and 8 replace the province fixed effects with fixed effects for each constituency (the individual unit of analysis).

pre-election violence and the heterogeneous effect of bad rainfall increase in magnitude (Columns 1-4 compared to 5-8).

Figure 3 displays graphically how the marginal effect of pre-election violence depends on whether the last rainy season was above or below average.

Figure 3: Relationship between pre-election violence and ZANU-PF vote share after good and bad rainy seasons



5.4 Robustness and Mechanisms

In this section I run a series of robustness checks and test for evidence consistent with my argument and several alternative explanations for the observed patterns. To recap, in the previous sections I found first that violence appears to work – it does effectively increase the vote share of the ruling party. First, I test that the results are not driven by outliers in the right-skewed measure of repression events. The results are robust to different transformations of the key measure of repression events, including a binary measure of whether any events occur and a ranked measure. These results are shown in Appendix A.1. This test should assuage some concerns that the results are driven by variation in the quality of the conflict data, an important concern in several recent analyses (Dafoe

and Lyall, 2015; Weidmann, 2015). If some constituencies that do experience repression events are erroneously coded as zeros in the binary coding, this would lead to under-estimation of the coefficient of interest, but the robustness to a binary version of repression events suggests that under most plausible types of reporting bias we would not expect to over-estimate the relationship between repression and change in the ruling party's vote share.

Second, if poverty is making citizens more vulnerable to repression, we should expect to see that increases in poverty matter more when people are relatively more poor. For instance, being extremely poor versus somewhat poor should have a greater relationship with the marginal effect of repression than being well-off versus average because being above a certain threshold additional wealth should not imply more physical or psychological resources to cope with violence. I test whether there is a threshold of poverty below which the marginal effect of violence begins to increase. To test for a threshold effect of poverty, I create categorical versions of the measures of wasting and bad rainfall in Appendix A.2. Indeed, I find that my results are driven in variation in poverty above the mean, while decreases below the mean have no effect. This is consistent with a theory in which extreme poverty makes citizens vulnerable to repressive threats.

I also test the plausibility of several alternative mechanisms. First, I test whether violence might be more severe or more frequent in poor areas. To test this potential confound, I estimate the relationship between the severity of violence and the level of wasting. In Appendix A.3 I show that violence overall and more severe types of violence like murder, rape, and abduction are not more likely in poorer constituencies. If anything, less severe types of violence like assault may be less common in poorer areas, which would lead me to under-estimate my coefficients, although there is no statistically significant evidence of any relationship.

As discussed in the methodology section, my difference-in-difference design controls for all time- and constituency-specific factors that might bias my estimates. However, to the extent that there are confounding factors that vary over time within each constituency, my results might be biased. One such factor that might affect the analysis of the effect of rainfall on the effectiveness of violence in Section 5.3 is any change in citizen preferences caused by rainfall shocks. My theory

implies that changes in poverty affect the willingness to reveal or act on preferences under the threat of violence, which means that empirical tests must hold the actual preferences constant. The existing literature suggests that bad rainfall might cause citizens to prefer the government less (Achen and Bartels, 2002; Healy and Malhotra, 2010), which would lead me to underestimate the effect of poverty on the effectiveness of repression. I conduct an empirical test of the relationship between bad rainfall and citizen preferences in Zimbabwe in Appendix A.4. This analysis shows that there is little to no evidence that bad rainfall affects citizen preferences, and if anything this would indeed lead me to under- rather than over-estimate my coefficient of interest.

Another potential time-variant and constituency-specific confound is the effectiveness of clientelism, or offers of positive inducements in exchange for electoral support. Clientelism is difficult to measure as it is fundamentally based on perceptions, or whether citizens believe that their ability to access transfers is contingent on how they vote. In Zimbabwe, the supply of clientelistic goods is near constant as the Zimbabwean state is relatively high-capacity and has control over the distribution of a range of important transfers including food aid, agricultural inputs from seeds and fertilizer to machinery, farmland, permits to sell goods in markets, all the way up to commercial farmland, banking licenses, and key positions in state-owned enterprises (Davies, 2004; Dawson and Kelsall, 2012). If the supply of clientelism is essentially constant, then if clientelism is more effective during poor periods we should expect to see a positive relationship between overall poverty and increases in ZANU-PF's vote share. In fact, in both Table 3 and Table 4 the direct effect of the two measures of poverty are both indistinguishable from zero, and often negatively signed. This suggests that it is unlikely that increases in the effectiveness of clientelism are driving the results observed here.

Thus, this analysis provides strong support for my theory that violence is most effective against the poor. At very low levels of wasting and during good rainy seasons, pre-election violence has little or no significant effect on ZANU-PF's vote share. The estimates using rainfall deviations suggest that the relationship between poverty and responses to repression is indeed causal.

6 Results: Individual-level analysis of survey response

In this section I present the results of an individual-level analysis that exploits [Garcia-Ponce and Pasquale's \(2014\)](#) strategy to identify the effect of violence on citizen behavior by using random variation in the timing of violent events around an opinion survey. One concern with the previous results based on a difference-in-difference design is that there could be an omitted factor that varies both with time and by constituency and is confounded with the incidence of violence and ZANU-PF's vote share. In this section of the analysis, I use random variation in violence to test whether the relationship between exposure to violence, poverty, and capitulation still exists when I use a research design that eliminates the possibility for that type of confounding factor.

I extend the results of [Garcia-Ponce and Pasquale \(2014\)](#) to show how pre-survey violence affects the propensity of respondents to reveal the most sensitive political opinion in Zimbabwe: political party preference. I show how respondents in this case choose to falsify a preference by refusing to reveal their preference for the opposition rather than falsely claiming a preference for the ruling party. Last, I test my theory that the poor are more likely to respond to violent threats with submission by showing that these effects are driven by the responses of the very poor.

6.1 Main effects: Violence and revealed party preferences

First, I show that in line with the results of [Garcia-Ponce and Pasquale \(2014\)](#) that citizens exposed to pre-survey state repression are more likely to hide their party preferences. Table 5 shows the results of a regression of exposure to pre-survey state-sponsored violence on the revealed party preferences of Afrobarometer respondents.

In this analysis the data is subset to only districts that have an episode of state repression during the course of the survey in a particular district. Surveying in any particular district lasted between one and six days. I include district fixed effects which means that my estimation uses the variation within a district between individuals who were surveyed immediately before and immediately after one of these violent events. There are fifteen districts with a total of 552 respondents in which a

violent event occurred during the course of the survey in that district. Because surveying in most rural districts only lasted for a single day, this sample is disproportionately urban. To take into account the fact that the treatment is assigned at the district level, I cluster my standard errors at the district level. Given the small number of clusters, I use [Cameron et al.'s \(2008\)](#) strategy for wild cluster bootstrapped standard errors.⁴

In the table that follows, Columns 1 and 2 use a dummy for whether the respondent revealed support for the MDC as the dependent variable. In Columns 3 and 4 the dependent variable is a dummy for whether the respondent revealed support for ZANU-PF, in 5 and 6 it is a dummy for whether the respondent revealed that she supports no party, and in Columns 7 and 8 it is a dummy for whether the respondent refused that question. In the even columns I also include controls for age, gender, education, employment, poverty, and ethnicity. Table 5 shows the results of this analysis.

⁴[Cameron et al. \(2008\)](#) show that traditional clustered standard errors are biased upward for data with less than 30-50 clusters.

Table 5: Pre-survey ZANU-PF violence and willingness to reveal party preferences

	<i>Dependent variable:</i>							
	MDC		ZANU		None		Refuse	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PSV - ZANU	-0.109 (0.069)	-0.099 (0.062)	-0.032 (0.020)	-0.035 (0.034)	0.129*** (0.039)	0.128*** (0.034)	0.005 (0.020)	-0.001 (0.006)
Age		-0.019 (0.021)		0.0002 (0.018)		0.054*** (0.018)		-0.034*** (0.009)
Female		-0.080*** (0.019)		-0.049 (0.033)		0.102*** (0.033)		0.021 (0.018)
Education		0.025 (0.018)		-0.048*** (0.014)		0.056*** (0.014)		-0.025** (0.011)
Employed		0.027 (0.019)		0.010 (0.012)		-0.046*** (0.012)		0.007 (0.013)
Poor	0.004 (0.023)	0.020 (0.026)		-0.030** (0.014)		-0.008 (0.014)		0.020 (0.016)
Ndebele	-0.036 (0.039)	-0.032 (0.039)	-0.077 (0.069)	-0.090 (0.069)	0.064 (0.103)	0.068 (0.069)	0.028 (0.067)	0.034 (0.063)
Other	-0.003 (0.072)	-0.004 (0.066)	0.071 (0.044)	0.065 (0.046)	-0.081 (0.055)	-0.087* (0.046)	0.015 (0.044)	0.027 (0.032)
Intercept	0.271*** (0.051)	0.296*** (0.050)	0.253*** (0.050)	0.297*** (0.062)	0.544*** (0.063)	0.471*** (0.062)	-0.081* (0.048)	-0.079*** (0.028)
Observations	552	552	552	552	552	552	552	552
R ²	0.109	0.130	0.221	0.238	0.110	0.138	0.053	0.072

Cameron et al.'s (2008) wild cluster bootstrap standard errors clustered at the district level in parentheses.

*p<0.1; **p<0.05; ***p<0.01

Models are estimated using OLS. The dependent variable in Columns 1-2 is a dummy for whether the respondent revealed that she supports the main opposition party MDC; in Columns 3-4 it is a dummy for whether the respondent revealed support for ZANU-PF; in Columns 5-6 it is a dummy for whether the respondent revealed that she supports no party; and in Columns 7-8 it is a dummy for whether the respondent refused to answer the question about party identification. The main explanatory variable of interest is PSV - ZANU, which is a measure of whether the a violent event perpetrated by ZANU-PF occurred in the respondent's district during the seven days before she was surveyed. The variable Poor is the standardized sum of the responses to the Afrobarometer questions about the scarcity of food and the scarcity of a cash income. Age, Female, Education, Employed, and the ethnicity dummies also come from the Afrobarometer survey questions. The data is subset to only districts with a violent event in the seven days before or seven days after the respondent was surveyed.

These results extend the analysis by [Garcia-Ponce and Pasquale \(2014\)](#) by showing that revealed party identification is also affected by exposure to pre-survey state repression. Interestingly, revealed support for the opposition and the ruling party goes down by approximately ten percentage points after violence, although this coefficient is insignificant. Instead, respondents are more likely to say that they support no party at all.

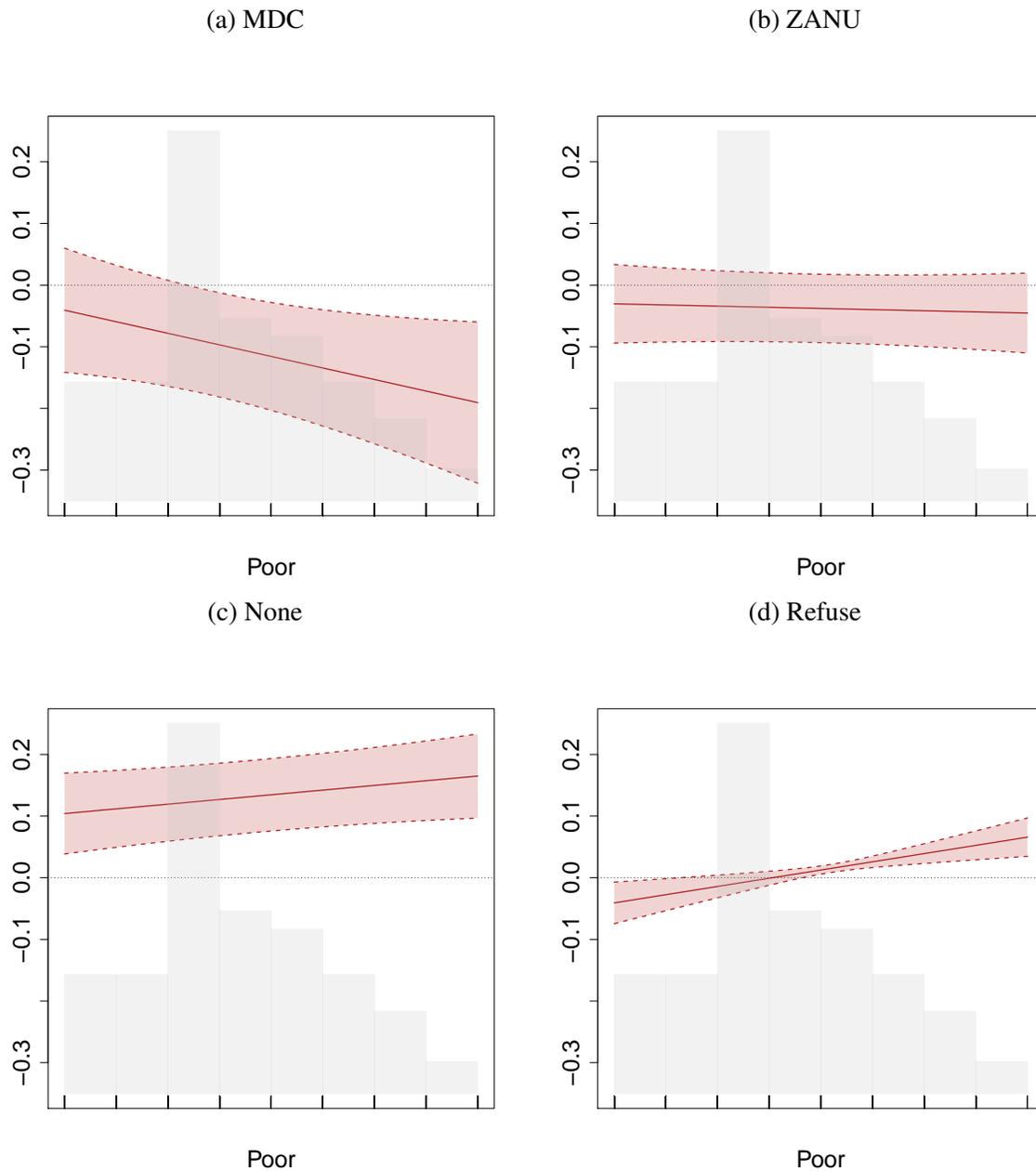
One limitation of this methodology is its inability to separate changes in the willingness to reveal preferences from changes in actual preferences. For the analysis to identify changes in preference falsification, we must assume or prove that changes in preferences truly do not change after state repression. In this case, it is plausible that some supporters of ZANU-PF change their preferences after exposure to their party's use of violence. Empirically, however, there does not appear to be a relationship between

6.2 Heterogeneous effects: Poverty and pre-survey violence

Next, I test for the heterogeneous effects of poverty on the effect of pre-survey ZANU-PF violence. My theory predicts that the poor should be even more likely than the better-off to hide their preferences for the opposition and tell a surveyor that they support ZANU-PF, no party at all, or refuse to answer the question on party identification. Thus, I predict a negative interaction when the dependent variable is support for the MDC, and a positive interaction on support for ZANU, support for no party, and refusal to answer. [Figure 4](#) plots the marginal effects of pre-survey ZANU-PF violence by severity of poverty. The full table of results is in [Appendix B.1](#).

[Figure 4](#) shows that the marginal effect of pre-survey violence is significant and substantively large only for the poor in most cases. Respondents who say that they frequently go without food or a cash income are significantly more likely to hide their preference for the MDC and refuse to respond to the question on party affiliation after an event of state repression. There is no significant effect of violence on respondents who are better off than average on their propensity to reveal pro-MDC preferences or to answer the question on party affiliation. The magnitude of the effect on the propensity to respond that you have no party affiliation is also larger for the poor, although

Figure 4: Relationship between pre-survey violence and revealed party preferences by poverty



even the better-off are significantly more likely to respond that they have no party affiliation after a pre-survey violent event. Last, no respondent is significantly more or less likely to respond that they prefer ZANU-PF after an episode of state-sponsored violence.

These results, despite the limitation that we cannot cleanly separate a change in preference falsification from a change in real preferences, do provide suggestive evidence that supports

the common belief that Afrobarometer respondents in Zimbabwe are falsifying their political preferences due to fear of violence. Switching from revealing support for the opposition to revealing no support for any party or refusing to answer the question about party identification is a pattern that is much more in line with preference falsification than changing preferences. More importantly, these results provide strong evidence at the individual level that it is in fact poor individuals who change their behavior after violence to a larger extent than the better off.

7 Conclusion

In this paper, I develop a theory that the poor, for either physical and psychological reasons, are more vulnerable to repressive violence. The poor are both less capable of physically defending themselves against personal attacks by investing in security, and less capable of mustering the mental bandwidth or self-efficacy to coolly process frightening signals about the risk of repression.

I test this theory using two empirical strategies at the constituency and individual level. First, I use a difference-in-difference design that controls for all characteristics that vary by time period and constituency to test whether pre-election repressive violence during the 2000-2005 period in Zimbabwe resulted in increases in ZANU-PF's vote share. I find that violence is significantly associated with increases in the vote share of the ruling party, and that these increases are concentrated in poor constituencies. The relationship between poverty and the effectiveness of violence holds true when I use an endogenous measure based on child nutritional outcomes or an exogenous measure based on random variation in the quality of a given rainy season compared to the constituency's 20-year average.

I then apply an empirical strategy at the individual level that exploits variation in recent exposure to violence to test whether the impact of violence on citizen behavior is causal. I find again that violence reduces the share of citizens who state preferences for the opposition, and that this effect is driven by people who say they always or often go without enough to eat.

This paper addresses a gap in the literature on state repression by developing a theory that

may explain why the poor are consistently more likely to be afraid of electoral violence across a number of cases in Africa (Mares and Young, 2016). I address a gap in the literature on electoral violence, which has so far focused largely on the strength of party identification and ethnicity to explain which voters are targeted with intimidation during elections.

The results show that repressive threats do not affect all citizens equally. Violence is most effective in coercing support from the poor. This result puts forward another potential channel through which poverty may prevent the development of responsive democratic institutions. It implies that dictators in poor countries should be better able to prevent democratization. Furthermore, this may explain why semi-democratic leaders in very poor countries redistribute so little, despite having large populations of poor voters. If these leaders are willing to use repressive tactics to remain in power, they may worry least about the votes of the poor as those voters are the easiest to manipulate with threats.

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Appendix

A Constituency analysis

A.1 Robustness: Outliers

Table 6 shows that the results in Table 2 are not driven by outlier values of the main variable of interest. The number of pre-election repression events by constituency is strongly right skewed. In this analysis I use a binary or ranked measure of repression events in each constituency-election as the main explanatory variables.

Table 6: Binary or ranked pre-election violence and changes in ZANU-PF vote share

	<i>Dependent variable:</i>			
	Change in ZANU vote share			
	(1)	(2)	(3)	(4)
Repression Events - Binary	2.49** (1.08)		3.16*** (1.09)	
Repression Events - Rank		0.01*** (0.004)		0.01*** (0.004)
Intercept	-9.15*** (0.60)	-11.34*** (1.11)	-8.72*** (0.58)	-11.41*** (1.17)
Election FE	✓	✓	✓	✓
Constituency FE	✓	✓	✓	✓
Observations	369	369	279	279
R ²	0.85	0.85	0.86	0.86
Clusters	119	119	93	93
Sample	All		Minor or no redistricting	

Standard errors clustered at the constituency level in parentheses.

*p<0.1; **p<0.05; ***p<0.01

Models estimated using OLS. Columns 1-2 are estimated using data from all the constituencies, and 3-4 are estimated using a subset of constituencies that experienced minor or no changes during the delimitation exercises in 2000 and 2005. The dependent variable is the change in ZANU-PF's vote share from the 2000 referendum to the election of interest in 2000, 2002, or 2005. The main coefficient of interest is on Repression Events, which is a measure of the number of events of state violence against the opposition in a given constituency in the three months leading up to an election. In this table the number of repression events is transformed to reduce the influence of outliers: in Columns 1 and 3 it is made into a binary measure of whether there were any repression events in a constituency-election, and in Columns 2 and 4 it is the rank of violent events. All columns include fixed effects for the election and for the constituency as in the preferred specification in Columns 4 and 8 of Table 2.

The coefficients on Repression Events remain strongly significant after creating both a binary or ranked version of this variable, suggesting that the results are not driven by outlier values.

A.2 Robustness: Levels of poverty

Another question that we may have is whether the effect of violence on support for ZANU-PF really has a linear relationship with poverty. This is a robustness check because we would want to see a somewhat linear relationship between poverty and the effectiveness of violence to justify our choice to analyze poverty as a continuous variable. However, if violence becomes more effective at a certain point, it could give us relevant substantive information about how this phenomenon works.

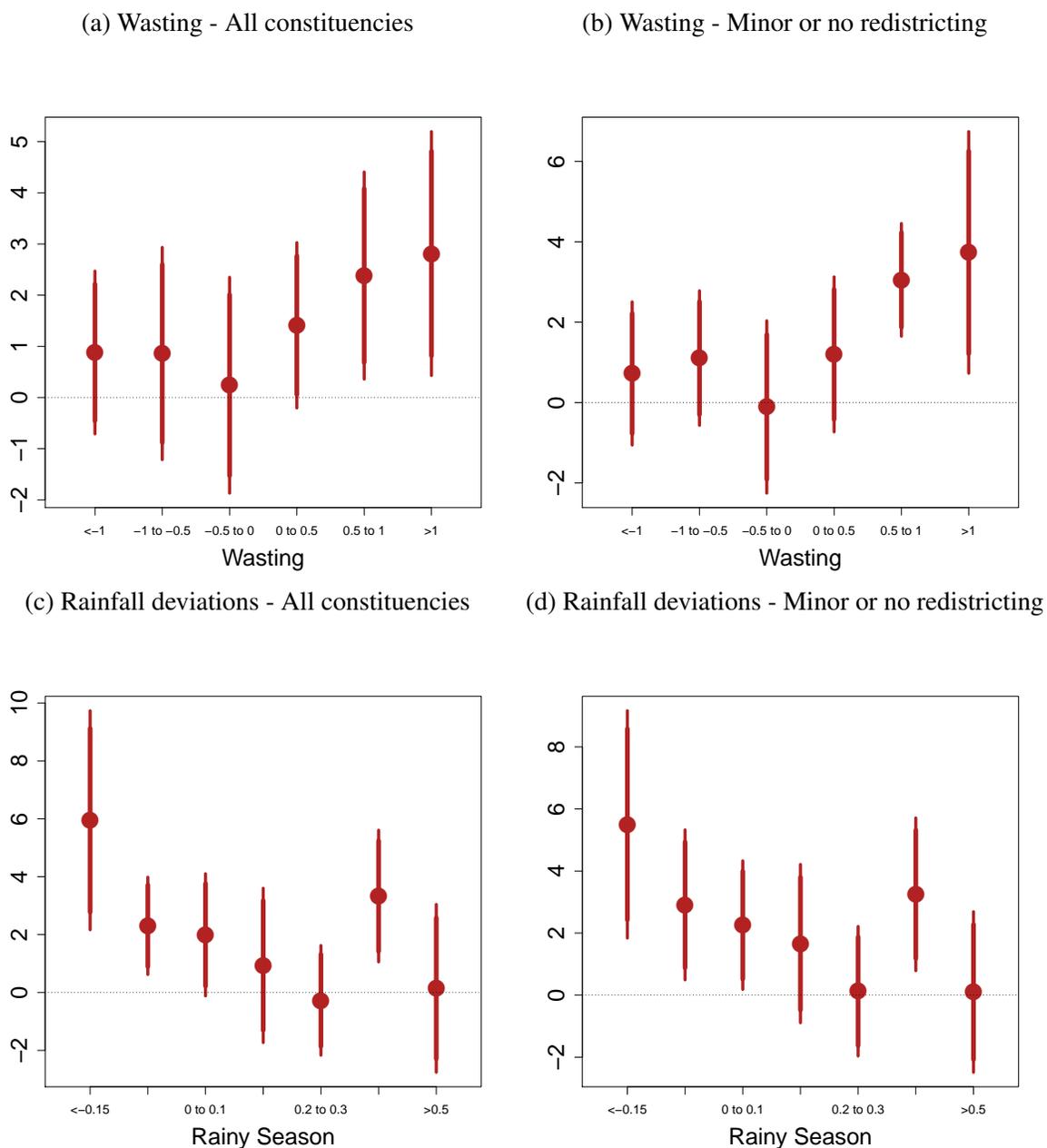
For this robustness check, I create categorical versions of both Wasting and Rain Deficit. For the factorial Wasting variable, I create six categories based on how wasted children are compared to other Zimbabwean children. In the worst (highest) category, wasting in the constituency is more than one standard deviation above the mean for Zimbabwe. In real terms, the average child in this category is 1.3 standard deviations below the average weight of the WHO's reference for a child of his or her age (a child with a weight-for-height z-score of -2 is considered malnourished). Three of the categories of this variable are above zero (which marks the average weight of a child in Zimbabwe), and three fall below it.

For the factorial measure of Rain Deficit, I also cut the measure of rainy season rainfall into meaningful and somewhat even categories. For this variable the worst or poorest category is below -0.15, which represents a constituency whose rainy season was more than 0.15 standard deviations below its 20-year average. Again, I split the data at zero, which marks the 20-year mean for each constituency. Two categories are below 0, indicating worse-than-average rainy seasons, and five are above it.

Figure 5 plots the heterogeneous effect of ZANU-PF violent events based on these categorical measures of poverty.

Figure 5 shows that both of the categorical variables show that variation in poverty seems to matter above a certain threshold of poverty. In Figures 5a and 5b, we see that the marginal

Figure 5: Relationship between poverty and type of violence



effect of exposure to violence is larger and becomes statistically significant for constituencies where the average child is above Zimbabwe's average level of wasting. The marginal effect gets increasingly large as we move from the category with constituencies that are between 0 and 0.5 standard deviations above the mean on wasting, to the next highest category where children are between 0.5 and 1 standard deviations above the mean on wasting, and then the highest category

where they are above 1 standard deviation above the mean.

Figures 5c and 5d show that the marginal effect of ZANU-PF violence seems to matter when rainfall is at or below the constituency average, with one exception. When rainy season rainfall is between 0 and 0.1 standard deviations above the mean, violence has a small and significant marginal effect on ZANU-PF's vote share. Between -0.15 and 0, it has a slightly larger effect, and the largest effect is in the category with the worst rainy season rainfall of below -0.15 standard deviations below the mean. The exception to this trend is in the category between 0.3 and 0.5 standard deviation above the mean, where violence again has a significant marginal effect. Nevertheless, the general pattern is clear when we analyze rainfall as a factor as well as a binary variable.

This analysis confirms that there is generally a linear relationship between poverty and the effectiveness of violence, particularly at high levels of poverty, meaning high wasting or particularly negative levels of rainfall.

A.3 Mechanisms: Is violence more severe in poor areas?

One explanation for the patterns uncovered here is that violence is simply more severe in poorer areas. In this section, I test whether different categories of violence that differ in their severity are more or less likely in areas that have high levels of wasting or bad previous rainy seasons.

To test for relationships between poverty and severity of violence, I regress each type of violence on my measures of poverty and a time fixed effect. I coded seven different types of violence from the ZHRNGOF data during this period: murder, rape, abduction, torture, assault, malicious destruction of property/theft, and intimidation. While it is impossible to create a clear ranking of the severity of these types of violence, there are some rankings that most people would agree on. For instance, it is generally agreed that intimidation is less severe than physical assault, and that murder is worse than physical assault that doesn't result in death.

In Figure 6, I plot the coefficients on my measures of poverty from these analyses.

Figure 6: Relationship between poverty and type of violence

(a) Wasting and types of violence by severity (b) Rain deficit and types of violence by severity

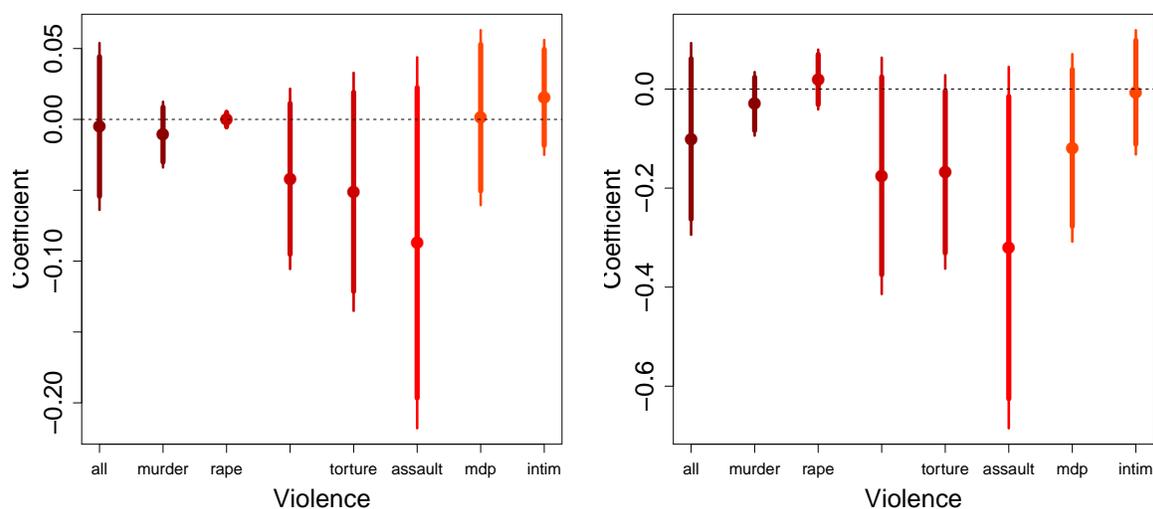


Figure 6 shows that the severity of violence does not seem to be related to the level of poverty in a constituency. The first coefficient plotted in the right of each graph shows that violent events overall are not significantly related to poverty, and if anything, there seems to be fewer violent

events in areas with bad rainfall. Furthermore, the most severe types of violence such as murder and abduction are not more likely in poorer constituencies; if anything, the least severe types of violence appear to be slightly less likely in poorer constituencies. Assault is almost significantly less likely in poorer places, while the more severe forms of physical violence like murder and rape are less negatively related to poverty.

A.4 Mechanisms: Does bad rainfall change citizen preferences?

Another challenge to the identification of the effect of poverty on the effectiveness of repression is that time-variant measures of poverty, particularly rainfall shocks, could also change citizen preferences. My theory is fundamentally about when citizens choose to express preferences, which implies that preferences should be held constant in any empirical tests in order to identify the impact of violence on willingness to reveal preferences.

One concern with my analysis in Section 5.3 is that rainfall shocks might be changing preferences as well as poverty levels. If rainfall shocks themselves have a negative effect on ZANU-PF's vote share, then I may in fact be underestimating the relationship between poverty and the effectiveness of violence.

One way to test whether this assumption that preferences are not changing after rainfall shocks is valid is to estimate empirically the relationship between bad rainy seasons and citizen preferences. One would expect based on a theory of “blind retrospective voting” (Achen and Bartels, 2002; Healy and Malhotra, 2010) that voters would like the ruling party less after bad rainy seasons. Unfortunately, as we demonstrated in Section 6.1, citizens do not always reveal their true preferences in opinion surveys due to the fear of violence. Therefore, we should expect that any analysis of how bad rainfall affects expressed opinions on a survey is a mix of both changes in preferences and changes in willingness to reveal preferences.

With those caveats, I analyze how bad rainfall affects expressed citizen preferences towards the ruling party. To take into account to some degree variation in willingness to reveal preferences, I analyze the impact of bad rainfall on attitudes towards the ruling party at different levels of fear

of electoral violence. Presumably, people who have less fear of electoral violence should be more likely to reveal their true opinions after rainfall shocks, while those with more fear should be more likely to express opinions out of fear.

To implement this analysis, I use opinion data from the Afrobarometer on both revealed support for ZANU-PF and reported trust in the president as the dependent variable. As the independent variable, I use the same dummy indicating whether a constituency is below its own 20-year average rainfall during the rainy season preceding the opinion survey. I also include a four-category factor measure of fear of violence as an independent variable, along with an interaction term between fear of violence and bad rainfall. This interaction allows me to estimate the impact of bad rainfall on opinion at different levels of reported fear. I also include district and year fixed effects such that the coefficient is estimated off of the variation within a district in attitudes from good to bad rainy seasons. The final specification that I estimate is:

$$Y_{ijt} = \gamma_j + \lambda_t + \beta \text{rain}_{jt} \times \text{fear}_{it} + \theta \text{rain}_{jt} + \delta \text{fear}_{it} + \varepsilon_{it}$$

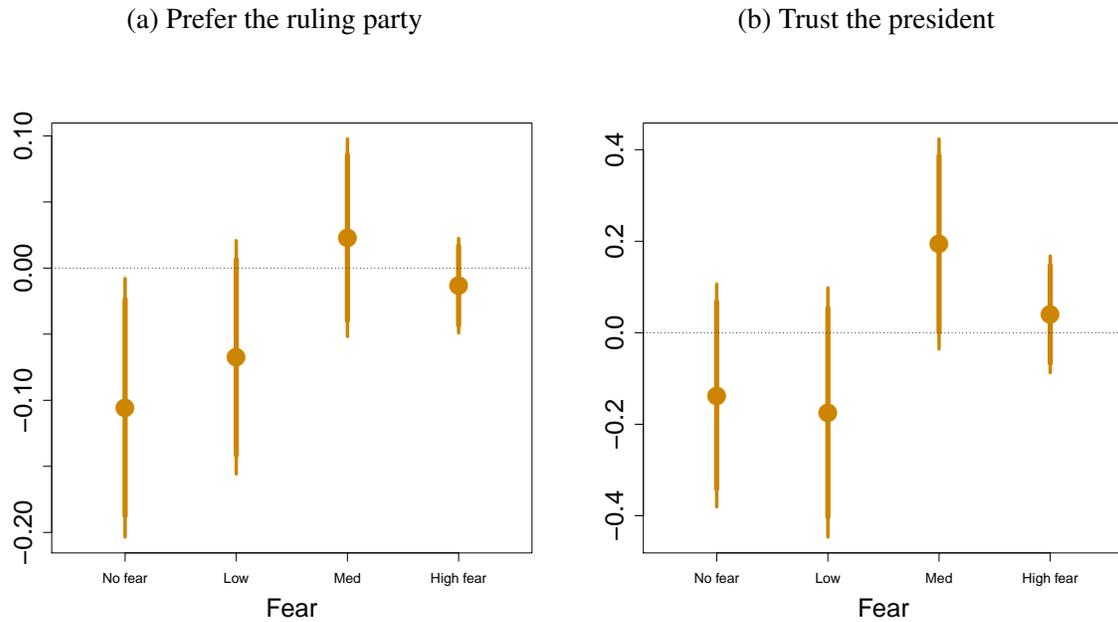
where i represents a respondent, j a district, and t a year. Standard errors are clustered at the district level.

Figure 7 plots the marginal effects of bad rainfall at varying levels of fear of political violence.

Figure 7 shows that citizens at low levels of fear of political violence do indeed seem to have slight more anti-government preferences after bad rainy seasons. However, most of these marginal effects are not significant, with the exception of the responses on party preference for people who have no fear of political violence at all. For people who are afraid, however, there is no effect or a slightly positive effect of bad rainfall on preferences towards the government. This effect hovers around zero and is not statistically significant.

Taken together, these results suggest that bad rainfall may indeed have a small negative effect on citizen preferences towards the government, although this effect is statistically indistinguishable from zero. The negative coefficients for low-fear voters are a better test of respondents' true preferences, while the null effects on high-fear respondents may be driven by their unwillingness to

Figure 7: Relationship between bad rainfall and revealed citizen preferences



reveal their preferences more than the lack of a change in the preferences.

This analysis, while not totally conclusive, suggests that bad rainy seasons are probably not changing citizen preferences towards the ruling party. Furthermore, if bad rainy seasons do have any effect on preferences, it would lead me to underestimate the relationship between poverty and the effectiveness of violence on increasing ZANU-PF's vote share in the regressions in Section 5.3.

A.5 Validation: Measures of poverty

In this study I use both the weight-for-height z-scores of children under 5 and whether the previous rainy season was less than average for a constituency as measures of poverty in a constituency. I use both of these as proxies for the underlying concept of poverty, based on the arguments that wasting in children under five is a sign of poverty and that bad rainy seasons are a cause of it. If both of these measures are based on the same underlying concept, we would expect them to also be correlated. In other words, if bad rainy seasons do indeed cause poverty which leads to wasting in children under five, there should be a positive relationship between my measure of the quality of the rainy season and under-5 wasting. I test that assumption here by regressing wasting on the dummy indicating a bad rainy season and dummies for each year.

I test for the relationship between bad rainfall and wasting in two specifications. First, I test for the effect of a dummy variable indicating whether the rainy season was below the 20-year average. Second, I test for whether the deviation from the 20-year average has a positive effect on stunting when the rainy season was less than average. I test for the effect of this deviation condition on being in a below-average rainfall year with the interaction of Rain Deviation and Rain Deficit. Because I expect that positive deviations from average should have little or no deleterious effect on children's health, I expect that the direct effect of Rain Deviation should be close to zero and insignificant while the interaction effect $\text{Rain Deviation} \times \text{Rain Deficit}$ should be positive and significant.

This analysis supports my predictions that bad rainy seasons should be associated with increases in wasting in children under five. In Column 1 the coefficient on the dummy variable Rain Deficit (which takes a value of 1 during below-average rainy seasons) is positive and significant, indicating that bad rainy seasons lead to increased wasting in children under five. In Column 2 I test for whether the extent to which the rainy season rainfall deviates from the constituency's average affects wasting. Indeed, the coefficient on Rain Deviation when the Rain Deficit dummy is 0 is slightly positive but insignificant, suggesting that deviations from the average rainfall do not have a deleterious effect on children's weights during above-average rainfall years. The coefficient on the interaction term, however, is significant and positive, suggesting that in bad rainfall years when

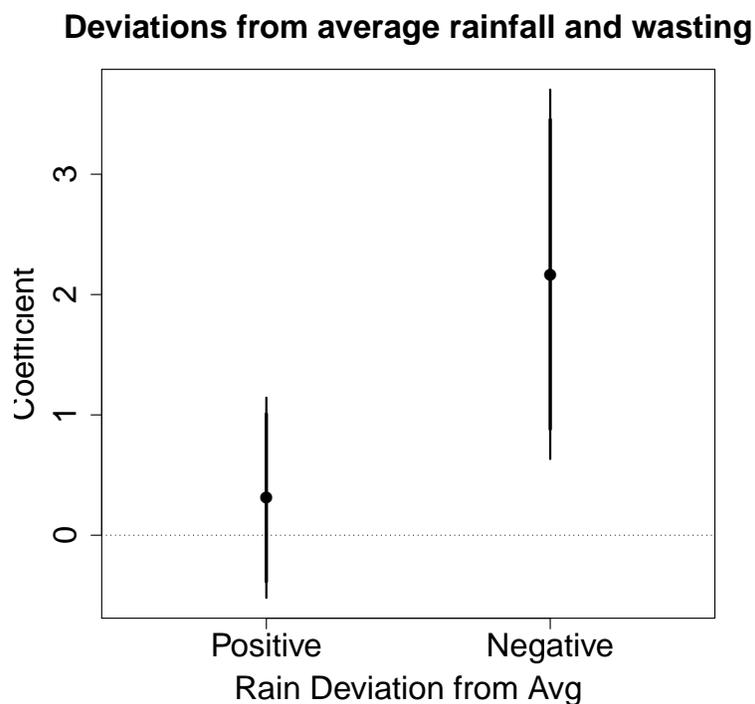
Table 7: Wasting in children under five and bad rainy seasons, 2000-2005

	<i>Dependent variable:</i>	
	Wasting	
	(1)	(2)
Rain Deficit	0.271** (0.123)	0.074 (0.177)
Rain Deviation		0.311 (0.425)
Rain Deficit × Rain Deviation		1.856** (0.887)
Intercept	0.007 (0.100)	-0.114 (0.171)
Year FE	✓	✓
Observations	347	347
R ²	0.014	0.028

*p<0.1; **p<0.05; ***p<0.01

Rain Deficit takes a value of 1 the size of the deviation from the average is positively associated with children's nutritional status. Figure 8 plots the effect of a rainfall deviation by the dummy indicating whether the constituency is in an above- or below-average year.

Figure 8: Positive and negative deviations from average rainfall and wasting



B Individual analysis

B.1 Heterogeneous effects: Full table

Table 8 shows the full table of results in the analysis of the individual-level heterogeneous effects of pre-survey violence on willingness to reveal party preferences.

Table 8: Pre-survey ZANU-PF violence and willingness to reveal party preferences

	<i>Dependent variable:</i>							
	MDC		ZANU		None		Refuse	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PSV - ZANU × Poor	-0.031 (0.027)	-0.040 (0.030)	-0.004 (0.009)	-0.004 (0.011)	0.010 (0.032)	0.016 (0.011)	0.027*** (0.009)	0.029** (0.011)
PSV - ZANU	-0.126** (0.058)	-0.119** (0.056)	-0.032 (0.021)	-0.037 (0.033)	0.135*** (0.038)	0.136*** (0.033)	0.016 (0.021)	0.014*** (0.004)
Age		-0.019 (0.020)		0.0003 (0.016)		0.054*** (0.016)		-0.034*** (0.009)
Female		-0.082*** (0.022)		-0.050* (0.029)		0.103*** (0.029)		0.022 (0.017)
Education		0.026 (0.017)		-0.048*** (0.014)		0.055*** (0.014)		-0.026** (0.011)
Employed		0.029 (0.020)		0.011 (0.013)		-0.047*** (0.013)		0.005 (0.012)
Poor	0.037 (0.044)	0.063 (0.049)	-0.011 (0.018)	-0.026 (0.021)	-0.020 (0.056)	-0.025 (0.021)	-0.007 (0.017)	-0.011 (0.020)
Ndebele	-0.039 (0.044)	-0.037 (0.038)	-0.077 (0.068)	-0.090 (0.069)	0.065 (0.101)	0.070 (0.069)	0.031 (0.068)	0.037 (0.064)
Other	-0.001 (0.071)	-0.001 (0.066)	0.069 (0.042)	0.065 (0.044)	-0.083 (0.057)	-0.088** (0.044)	0.016 (0.046)	0.025 (0.032)
Intercept	0.256*** (0.052)	0.277*** (0.052)	0.242*** (0.049)	0.295*** (0.057)	0.543*** (0.062)	0.478*** (0.057)	-0.058 (0.048)	-0.065** (0.032)
Observations	552	552	552	552	552	552	552	552
R ²	0.111	0.134	0.222	0.238	0.110	0.138	0.062	0.078
Number Clusters	15	15	15	15	15	15	15	15

Cameron et al.'s (2008) wild cluster bootstrap standard errors clustered at the district level in parentheses.

*p<0.1; **p<0.05; ***p<0.01

Models are estimated using OLS. The dependent variable in Columns 1-2 is a dummy for whether the respondent revealed that she supports the main opposition party MDC; in Columns 3-4 it is a dummy for whether the respondent revealed support for ZANU-PF; in Columns 5-6 it is a dummy for whether the respondent revealed that she supports no party; and in Columns 7-8 it is a dummy for whether the respondent refused to answer the question about party identification. The main explanatory variable of interest is PSV - ZANU, which is a measure of whether the a violent event perpetrated by ZANU-PF occurred in the respondent's district during the seven days before she was surveyed. The variable Poor is the standardized sum of the responses to the Afrobarometer questions about the scarcity of food and the scarcity of a cash income. Age, Female, Education, Employed, and the ethnicity dummies also come from the Afrobarometer survey questions. The data is subset to only districts with a violent event in the seven days before or seven days after the respondent was surveyed.

B.2 Perpetrator coding

Table 9 shows the actors and allies that I used to code whether a violent act was committed by the government or the opposition.

Table 9: Categories of actors and allies used to code perpetrator affiliation

	ZANU-PF	MDC
Actor	<ul style="list-style-type: none"> • ZANU-PF: Zimbabwe African National Union-Patriotic Front • ZANU-PF • ZRP: Zimbabwe Republic Police Militia • Police Forces of Zimbabwe (1987-) • Military Forces of Zimbabwe (1987-) • Military Forces of Zimbabwe (1987-) Presidential Security Unit • Mutiny of Military Forces of Zimbabwe (1987-) • ZNLWVA: Zimbabwe National Liberation War Veterans Association • Joint Operations Command (2008-) • CIO: Central Intelligence Organization • ZNYS: Zimbabwe National Youth Service 	<ul style="list-style-type: none"> • MDC-M: Movement for Democratic Change (Mutambara Faction) • MDC-N: Movement for Democratic Change (Ncube Faction) • MDC-T: Movement for Democratic Change (Tsvangirai Faction) • MDC: Movement for Democratic Change • MDC: Movement for Democratic Change Pro-Senate Militia
Ally	<ul style="list-style-type: none"> • ZNLWVA: Zimbabwe National Liberation War Veterans Association • CIO: Central Intelligence Organization • Farm invaders (Zimbabwe) • Military Forces of Zimbabwe (1987-) • Military Forces of Zimbabwe (1987-) Presidential Security Unit • Police Forces of Zimbabwe (1987-) • ZANU-PF: Zimbabwe African National Union-Patriotic Front 	<ul style="list-style-type: none"> • Farm Workers (Zimbabwe) • Farm Owners (Zimbabwe) • MDC-N: Movement for Democratic Change (Ncube Faction) • MDC-T: Movement for Democratic Change (Tsvangirai Faction) • MDC: Movement for Democratic Change • WOZA: Women of Zimbabwe Arise • ZCTU: Zimbabwe Congress of Trade Unions