Preying on the poor: Repressive violence and citizen behavior

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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, although there is enormous heterogeneity in how citizens respond. I argue that repression is most effective against the poor, and that this greater vulnerability to coercion may be driven by both psychological and physical factors. I test this prediction using data on pre-electoral state repression in Zimbabwe and two empirical strategies at the constituency and individual level that draw on exogenous variation in poverty and exposure to repressive violence. I find results in support of this hypothesis that are robust across three analyses. I also rule out alternative explanations by presenting evidence that repression and scarcity are not accompanied by changes in preferences, differences in the type of repression, or differences in the effectiveness of clientelism.

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1 Introduction

In March 2008, Zimbabweans went to the polls. Economic mismanagement had given many citizens reason to vote against the incumbent regime, despite the history of state-sponsored violence against opposition voters. In the days after the election, the announcement of the results were delayed and police were deployed in the streets, leading opposition voters to fear that the government was preparing to release rigged results and crack down violently on protest. 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 different citizens to the same intimidating events are highly diverse. Fear, indifference, and anger are all common reactions by citizens to the threat of state coercion. Some citizens seem sure to demobilize in response to the threat of repression, while others would be spurred to greater expressions of dissent. What explains this heterogeneity in voter reactions to the threat of repression?

State repression, or the use of coercion by political authorities against those under their jurisdiction, is frequently used by political authorities to deter resistance to the regime (Goldstein, 1978; Davenport, 2007*a*). According to the CIRI dataset, three in five countries in the world frequently used repression during at least one year between 2007 and 2011.[1] Although repression is used in many democracies, often against minority groups disliked or distrusted by majority voters, it is more common and more widespread in non-democracies (Poe and Tate, 1994; Davenport, 1999, 2007*b*). In non-democracies, most citizens are excluded from the political process (Acemoglu and Robinson, 2006). Assuming that most citizens want access to political power and the policy benefits that it can bring, the use of repression is thus almost inherent in the definition of autocracy.

[1] The CIRI data tracks four types of state repression: disappearances, extrajudicial killing, political imprisonment, and torture, and codes whether each was used frequently, occasionally, or never during a given year between 1981 and 2011 (Cingranelli, Richards and Clay, 2014).

Despite the prevalence of state repression, little is known about how and why it works. Although some have argued that the purpose of repression is to actually physically incapacitate key activists who are directly targeted (Siegel, 2011), most view the primary effect of repression as an informational signal to a larger body of citizen bystanders who are considering whether or not to engage in collective dissent. Citizens living under repressive regimes must weigh the costs and benefits of expressing dissent against the regime, and repression can affect perceptions of both of those parameters. Most obviously, repression affects dissent decisions by increasing the perceived costs of dissent. However, it may also lead citizens to update their beliefs about the disutility of living under the regime (Lohmann, 1994), increase the moral or social benefits that citizens receive from participation (Opp and Roehl, 1990; Pearlman, 2016), or cause emotional reactions that affect decision-making processes and behavior (Aytaç, Schiumerini and Stokes, 2017; Young, 2017*b*).

Empirical studies of the effects of repression have come to conflicting conclusions. Studies have found that state repression effectively decreases citizen opposition to the regime (Hibbs, 1973), increases citizen opposition (Muller and Opp, 1986), or increases it up to a point and then decreases it (Muller and Weede, 1990). Most early research on how citizens respond to repression was cross-national, aggregating the behavior of the state and citizens up to annual or even decadal national averages (Hibbs, 1973; Muller and Weede, 1990; Gupta, Singh and Sprague, 1993; Moore, 1998). A few have taken a more fine-grained approach, and have also found fairly mixed results. Francisco (1995) and Rasler (1996) analyze weekly data from a range of repressive regimes and find that although repression decreases protest in the short run, it increases it over a longer time period. Other scholars have examined heterogeneity in the effects of repression by regime type (Gupta, Singh and Sprague, 1993) and severity of repression (Muller and Weede, 1990).

Existing research on the relationship between repression and dissent has two shortcomings that I address in this article. First, theoretically, there has been relatively little focus on how variation at the individual or local level may condition how citizens react to repression, despite the individual-level nature of most theories. There are some exceptions: an early study by Opp (1994) uses post-dissent survey data on social network composition to test whether repression has a

greater effect on people embedded in critical or activist networks, and a recent working paper by Young (2017*a*) uses survey experiments and qualitative interviews to argue that repression mobilizes citizens who are high in the psychological characteristic of self-efficacy. Nevertheless, most theories do not attempt to identify characteristics of citizens or groups that would make them more or less likely to respond to repression by increasing or decreasing dissent. This oversight is important in part because this heterogeneity could help scholars and policymakers form a better understanding of the mechanisms by which repression shapes behavior and how it might be targeted by a strategic regime. In this article, I contribute a new argument about why repression should have a bigger negative effect on the behavior of the poor.

The second shortcoming that I address is empirical. Few existing studies have applied research designs that enable causal claims. Repression is presumably strategically targeted on particular groups of citizens in most cases, and this creates a problem of selection bias because the characteristics that make such groups attractive targets may also affect their dissent behavior. Garcia-Ponce and Pasquale (2015) use the plausibly exogenous timing of violent events around a survey to shut down this potential selection bias. I use two different research designs in this article to address endogeneity issues, including an extension of Garcia-Ponce and Pasquale's (2015) study at the individual level and a second difference-in-differences design in which I test empirically for selection bias in the incidence of repression.

In this article, I argue that voters living in a state of economic scarcity are the most likely to capitulate in the face of violent threats. This greater vulnerability could be driven by physical or psychological differences in the way poor voters are affected by repression. 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.

Empirically, I bring micro-level evidence to bear on the question of when repression causes citizens to falsify their true anti-regime preferences. The goal of this research design is to identify

the effect of both repression and economic scarcity on the expression of dissent. This implies that it is important to deal with the potential for confounding factors that are correlated with scarcity and the expression of dissent, and selection into exposure to repression. An ideal research design would exploit random variation in both scarcity and repression to identify the causal effects of these two factors on dissent. In this article, I draw on as-if random variation in each of these factors in two separate empirical tests. The constituency level analysis uses a difference-in-differences design that exploits random, short-term rainfall shocks in areas dependent on agriculture as shocks to economic scarcity. The individual level analysis addresses the endogeneity problem of selection into violence exposure by using as-if random variation in the timing of recent exposure to repression around a survey. In addition to the identification benefits of this research design, this dual research design also enables consideration of two different operationalizations of the dependent variable of citizen dissent. In the individual-level analysis, the measure of dissent is whether an individual reveals to a surveyor that they prefer the opposition party. In the constituency-level analysis, dissent is measured with the share of the vote captured by the opposition relative to the ruling party.

At the constituency level, 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 an endogenous, long-term measure of poverty and using random, short-term variation in the quality of the rainy season rainfall. At the individual level, I find that the very poor are significantly more likely to hide their support for the opposition after state violence, while the responses of better-off citizens are relatively unaffected. These results are highly consistent across the two research designs, and strongly in line with the proposed theory that economic scarcity makes citizens more likely to capitulate in the face of repressive threats.

In addition to the literature on repression, this article is related to research on election violence. In the Zimbabwean context, state repression is strongly linked to electoral outcomes and support for opposition political parties. While this is not the case in all repressive regimes, empirical trends suggest a high degree of overlap between election violence and repression. The vast majority of non-democracies hold elections (Hyde and Marinov, 2012), and incumbents perpetrate violence

in a large proportion of these non-democratic elections. Indeed, in the 638 elections held in non-democracies between 1981 and 2008, incumbents perpetrated pre-electoral violence in 43% and post-election violence in 10%.[2]

At a more general level, this project relates to a large literature arguing that underdevelopment stymies democracy (Lipset, 1959; Przeworski, 2000). If the poor are easier to repress, it may make democratization and democratic consolidation less likely in places with more poor citizens. Furthermore, the relatively high ability of politicians to get votes from the poor using violence may help explain why leaders in countries with large populations of poor citizens can manage to preserve wealth within the hands of a small economic elite. In this way, this 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 may have less of an incentive to be responsive to their demands.

2 Theoretical Framework

In this section I formulate predictions about how poverty might shape the effectiveness of coercive violence. I predict that violence might be more effective in changing the behavior of the poor because they poor are more vulnerable to its physical and psychological effects.

Patterns reported in existing studies suggest that the poor may be more concerned about some forms of coercive violence than the better off, even when they do not seem more likely to be personally affected by it. 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 the ten countries with the highest incidences of fear of election violence (Mares and Young, 2016), the poor are more likely to report being afraid that they personally will be targeted with violence during elections. However, several studies based on surveys in Nigeria and Kenya

[2] Author's own calculation from the Hyde and Marinov (2012) replication data.

have found that the poor are not actually more likely to experience election violence (Bratton, 2008; Dercon and Gutierrez-Romero, 2011; Gutiérrez-Romero, 2014). This pattern may suggest that the same level of violence exposure causes the poor to perceive a greater prospective risk. I suggest two theoretical reasons why this might be the case.

First, the poor may be 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 or flight before violence occurs, or in healthcare once they are victimized. 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.

Second, the poor may be more psychologically vulnerable to violence. 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 on how exposure to repression influences citizen behavior. Low self-efficacy, or beliefs about one's general ability to cope with difficult situations, is strongly related to higher levels of fear and higher perceived personal risks in a number of contexts (Bandura, Reese and Adams, 1982; Gamson, 1968). Recent research in Zimbabwe has also shown that people who are low in self-efficacy are more afraid of repression, perceive a higher risk of repression, and are less likely to participate in pro-opposition acts of dissent (Young, 2017*a*). In many contexts, the poor tend to have lower self-efficacy than the better-off, and this may lead them to react more fearfully and more pessimistically to the same information about a repressive threat. Although self-efficacy is typically viewed as akin to a personality trait formed over time, particularly through experiences of mastery early in life, there is also evidence that is is affected by later and shorter-term economic shocks like job loss (Shaver, 2017).

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, 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 less effective in shaping the vote share relative to vote buying.

3 The Zimbabwean Case

Since gaining independence in 1980, Zimbabwe has held fairly regular elections. However, elections have never resulted in a peaceful transition of power between parties, in part because of the ruling party's use of violent force (Kriger, 2005). ZANU-PF grew out the independence struggle and enjoyed widespread initial popular support 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' unmanipulated partisan preferences.

Zimbabwe's period of serious electoral violence began shortly afterwards with the legislative elections in 2000. Shortly after the referendum, 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? In fact, evidence suggests that violence was most concentrated and severe in areas with high levels of electoral support for ZANU-PF. 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 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 Research Design

The theory laid out in Section 2 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, but in this case random assignment is not possible for both ethical and logistical reasons. The purpose of this analysis 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.

This research design combines two identification strategies applied in separate analyses that draw on exogenous variation in poverty and exposure to state repression to address the major challenges to inference involved in testing such a theory. 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 poverty. The constituency-level analysis draws 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. To compare citizens with equal propensities for dissent but who vary in their exposure to repression, I extend an individual-level analysis by Garcia-Ponce and Pasquale (2015) to exploit random variation in whether survey respondents were surveyed immediately before or after a repressive event in their district. Table 1 describes which explanatory variable is identified and which is endogenous in the two analyses.

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

		Variable	
		Poverty	Repression
Level	Individual	Endogenous	**Exogenous**
	Constituency	**Exogenous**	Endogenous

This methodology makes several contributions. First, it provides both a constituency- and individual-level test of the theory and shows 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, the different analyses provide identified estimates of the effect of poverty due to random variation in the quality of the rainy season and the effect of violence due to random variation in whether an individual was surveyed before or after a violent event in her district. Taken together, these results provide strong evidence that there is a causal relationship between poverty and the propensity to capitulate in the face of violent threats.

4.1 Constituency-level analysis

4.1.1 Research design

The first section of this article analyzes how pre-election violence is related to changes in the vote share of the ruling party. This analysis uses several strategies to create a counterfactual for the ruling party's vote share in the constituency. First, it leverages the constitutional referendum 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. As discussed in Section 3, because the loss was unanticipated, the referendum was not preceded by violence and can thus be used as a baseline measure of support for ZANU-PF. I analyze whether violence increases support for ZANU-PF from this baseline.

Second, this analysis uses time and region 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. ZANU-PF's vote share increased overall 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. The preferred specification uses 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 short-term poverty on the effectiveness of violence. I hypothesize that in poorer constituencies, violence should be associated with a higher increase in ZANU-PF's vote share. For the first set of results, I measure scarcity using data from the Zimbabwe Demographic and Health Survey (DHS) on wasting, a measure of child malnutrition that is responsive to relatively short-term shocks. 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 poverty, 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 is 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 the dependent variable. Thus, I create a dummy variable for whether the previous rainy season's rainfall was below average for that particular constituency and use this as a random shock to the economic status of the constituencies' voters.

4.1.2 Specifications

In 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 (some specifications include province fixed effects, but in the preferred specification the fixed effect is at the level of the individual unit, the constituency) and λ_t is an election-specific time fixed effect. $violence_{it}$ is the number of violent events that occurred in a constituency during the three-month period immediately prior to the election. For specifications that include fixed effects at the province level, 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 constituency-level measure of poverty:

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

where the coefficient of interest β_1 is on the interaction of the extent of violence and the extent of poverty in a constituency.[3]

[3]This 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.

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, 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, which 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 the 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

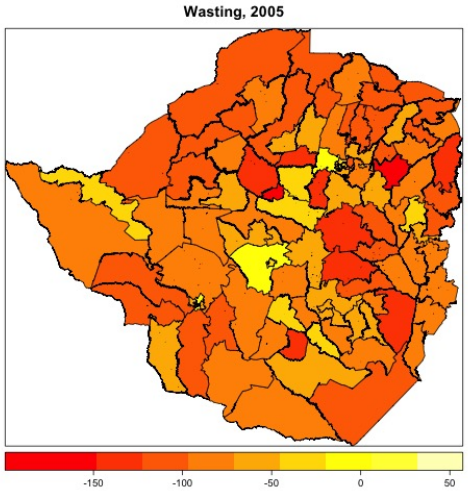
compares surveyed children to an international healthy standard taken from children in the US. From this, I calculate a normalized measure of wasting severity that is the inverse of the weight-for-height z-scores.

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.

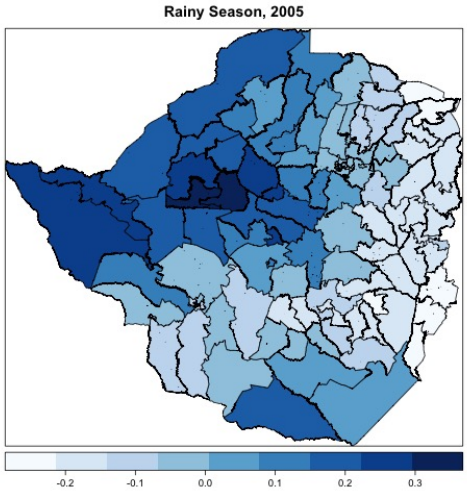
Figure 1 displays the main variables in 2005 visually.

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

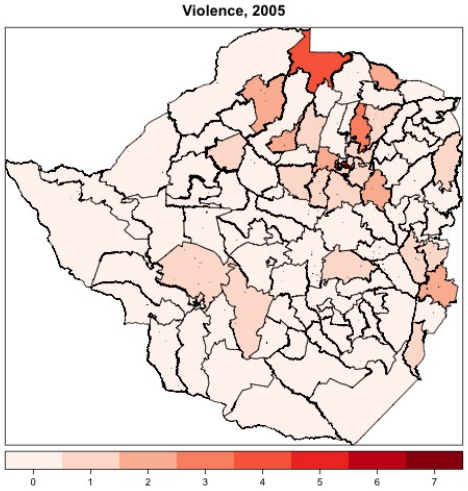
(a) Wasting



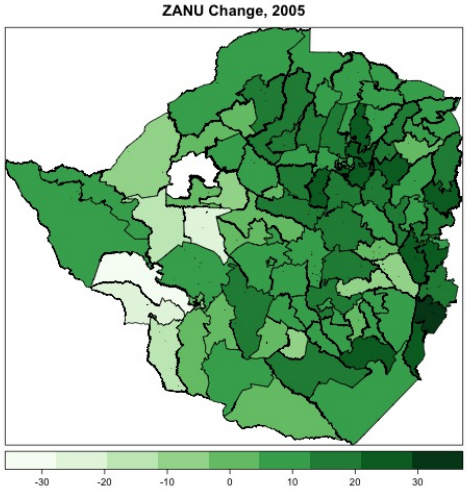
(b) Rain Deficit



(c) Repression



(d) $\triangle$ ZANU



4.2 Individual-level expression of anti-regime preferences

4.2.1 Research design

The second analysis tests the predictions about the effectiveness of repression using opinion data from five rounds of Afrobarometer surveys in Zimbabwe conducted between 1999 and 2012. This empirical strategy is adapted from Garcia-Ponce and Pasquale (2015). 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. I operationalize poverty by creating a composite measure of how frequently a respondent goes without food and 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 the prediction 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 the predictions about the interaction of violence and poverty 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 event, which is plausibly orthogonal to the political beliefs that I use as an outcome variable.

I then estimate the following specification:

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

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 willingness to reveal support for the opposition at the level of the individual Afrobarometer respondent using ACLED 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 linked to constituencies, which I aggregate up to districts. In the case of Harare, I followed the ACLED convention of splitting the district into several large urban areas (Harare, Chitungwiza, and Epworth) that are included within the district of Harare.

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 examine heterogeneous effects based on my predictions 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

This section presents the results of the constituency level analyses. 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. All specifications include election (time) fixed effects, and progressively include province or constituency level fixed effects. Some include dummies for whether the delimitation exercises in 2000 and 2005 resulted in major, minor, or no changes in the constituency boundaries. 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 with standard errors clustered by constituency.

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

	Dependent variable:							
	Change in ZANU vote share							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Repression Events	3.94***	1.81***	1.70***	1.72***	4.15***	2.21***	2.04***	1.93***
	(0.68)	(0.46)	(0.46)	(0.53)	(0.78)	(0.48)	(0.48)	(0.57)
2000 Redist - Minor			−0.62					
			(2.46)					
2000 Redist - None			−2.38				−1.49	
			(2.23)				(1.37)	
2005 Redist - Minor			−0.19					
			(1.82)					
2005 Redist - None			−3.15*				−3.14***	
			(1.88)				(1.21)	
Intercept	−9.90***	−14.41***	−11.97***	−9.92***	−9.61***	−13.47***	−10.72***	−9.50***
	(1.75)	(1.17)	(2.76)	(0.69)	(1.90)	(2.09)	(2.63)	(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 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 poses to this kind of time series inference,

I use two strategies. First, I include controls 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.[4] 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. Interpreting this result causally 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, 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

[4]In 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.

Events as the key explanatory variable.

5.2 Heterogeneous effects: Endogenous poverty

In this section I test the 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 poverty.

I use two primary measures of 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 where the average child under five is more underweight. 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 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

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

	Dependent variable:							
	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 Events × 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).

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

(a) All constituencies

(b) Minor or no redistricting

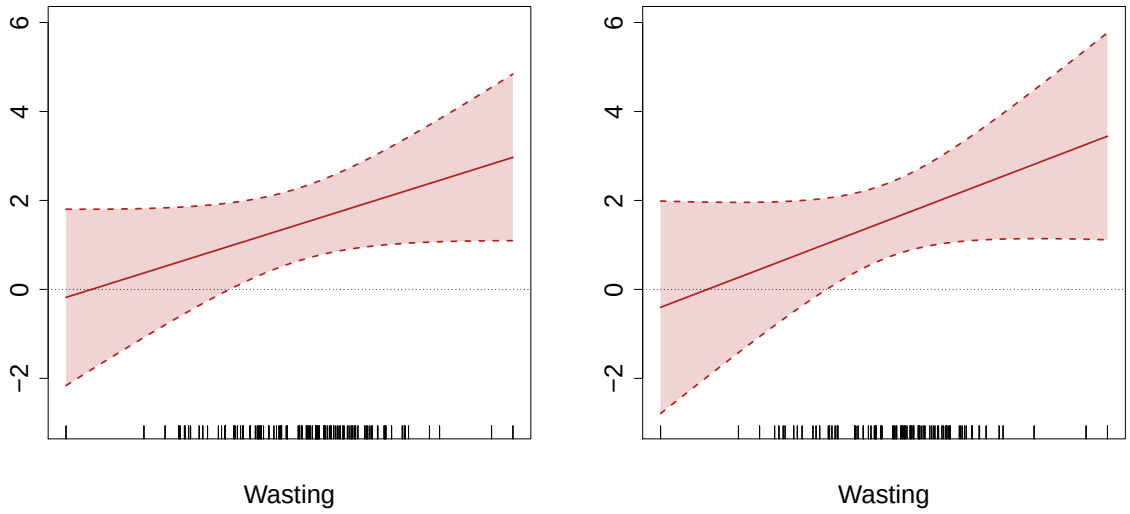


Figure 2 shows that additional pre-election violent events have little or no effect on ZANU-PF's vote share in constituencies that have very low levels of wasting in children under five. For constituencies that are high in this measure of poverty, however, additional violent events have a large and significant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

5.3 Heterogeneous effects: Exogenous poverty

In this section I discuss the results of the second test of the heterogeneous effects of pre-election violence based on exogenous variation in poverty. Specifically, instead of using child malnutrition to 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 test whether 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 Repression Events $\times$ 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 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.

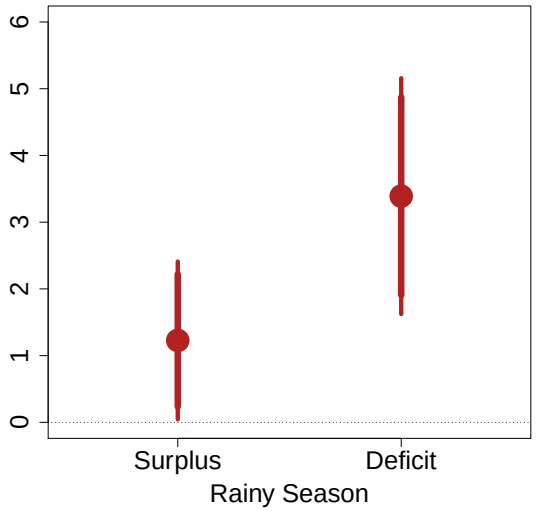
Table 4: Heterogeneous effects of pre-election violence in constituencies with below-average rainfall

	Dependent variable:							
	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			

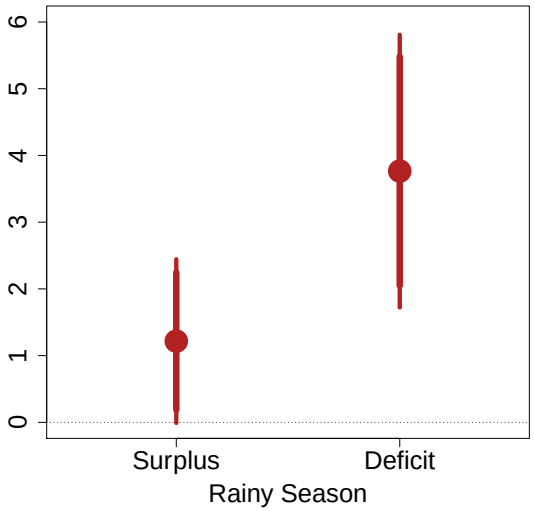
Standard errors clustered at the constituency level in parentheses.
*p<0.1; **p<0.05; ***p<0.01
Models estimated using OLS. Columns 1-4 are estimated using data from all the constituencies, and 5-8 are estimated using a subset of constituencies that experienced minor or no changes during the delimitation 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).

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

(a) All constituencies



(b) Minor or no redistricting



5.4 Robustness and Mechanisms

In this section I run a series of robustness checks and test for evidence consistent with my predictions and several alternative explanations for the observed patterns. To recap, in the previous sections I found first that repression appears to 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, 2016). 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. In other words, 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 the results are driven by 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 the coefficients, although there is no statistically significant evidence of any relationship. This suggests that reverse causality due to the targeting of violence on poor areas is unlikely to be driving the results.

As discussed in the methodology section, the difference-in-difference design controls for all time- and constituency-specific factors that might bias the estimates. However, to the extent that there are confounding factors that vary over time across constituencies, the 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 a change in citizen preferences caused by rainfall shocks. The 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 should hold 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. If anything, the negative changes in preferences associated with bad rainfall would indeed lead me to under- rather than over-estimate the 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 the prediction 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 identifies 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 the 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 recent exposure to violence to test whether the relationship between exposure to violence, poverty, and preference falsification 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 (2015) 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 the prediction 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 (2015) 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 the estimation strategy 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 the standard errors at the district level. Given the small number of clusters, I use Cameron, Gelbach and Miller's (2008) strategy for wild cluster bootstrapped standard errors.[5]

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.

[5]Cameron, Gelbach and Miller (2008) show that traditional clustered standard errors are biased for data with fewer than 30-50 clusters.

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

	Dependent variable:							
	MDC		ZANU		None		Refuse	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PSV - ZANU	−0.109	−0.099	−0.032	−0.035	0.129***	0.128***	0.005	−0.001
	(0.069)	(0.062)	(0.020)	(0.034)	(0.039)	(0.034)	(0.020)	(0.006)
Age		−0.019		0.0002		0.054***		−0.034***
		(0.021)		(0.018)		(0.018)		(0.009)
Female		−0.080***		−0.049		0.102***		0.021
		(0.019)		(0.033)		(0.033)		(0.018)
Education		0.025		−0.048***		0.056***		−0.025**
		(0.018)		(0.014)		(0.014)		(0.011)
Employed		0.027		0.010		−0.046***		0.007
		(0.019)		(0.012)		(0.012)		(0.013)
Poor	0.004	0.020		−0.030**		−0.008		0.020
	(0.023)	(0.026)		(0.014)		(0.014)		(0.016)
Ndebele	−0.036	−0.032	−0.077	−0.090	0.064	0.068	0.028	0.034
	(0.039)	(0.039)	(0.069)	(0.069)	(0.103)	(0.069)	(0.067)	(0.063)
Other	−0.003	−0.004	0.071	0.065	−0.081	−0.087*	0.015	0.027
	(0.072)	(0.066)	(0.044)	(0.046)	(0.055)	(0.046)	(0.044)	(0.032)
Intercept	0.271***	0.296***	0.253***	0.297***	0.544***	0.471***	−0.081*	−0.079***
	(0.051)	(0.050)	(0.050)	(0.062)	(0.063)	(0.062)	(0.048)	(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, Gelbach and Miller'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 show that self-reported party identification is also affected by exposure to pre-survey state repression. Stated support for the opposition goes down by approximately ten percentage points after violence, although this coefficient is statistically insignificant. Instead, respondents are more likely to say that they support no party at all.

6.2 Heterogeneous effects: Poverty and pre-survey violence

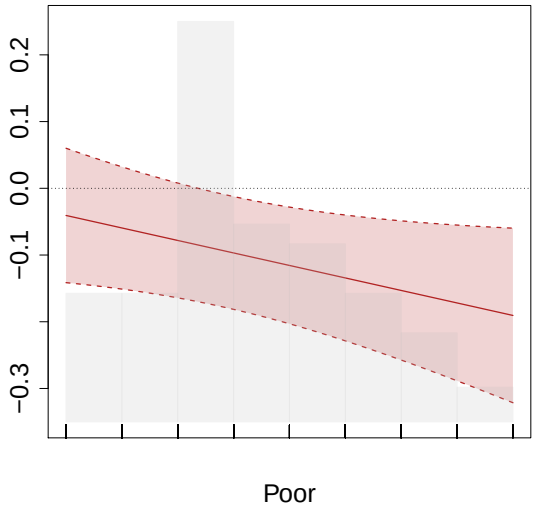
Next, I test for the heterogeneous effects of poverty on the effect of pre-survey ZANU-PF violence. The theory predicts that the poor should be even more likely than the better-off to hide their preferences for the opposition and tell a surveyor that they support ZANU-PF, no party at all, or refuse to 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 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.

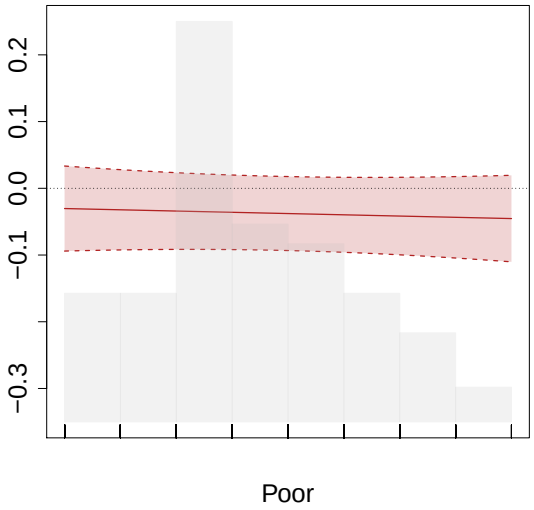
These results, despite the limitation that we cannot cleanly separate a change in preference falsification from a change in real preferences, do provide suggestive evidence that supports the common belief that Afrobarometer respondents in Zimbabwe are falsifying their political preferences due to fear of violence. Switching from revealing support for the opposition to revealing

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

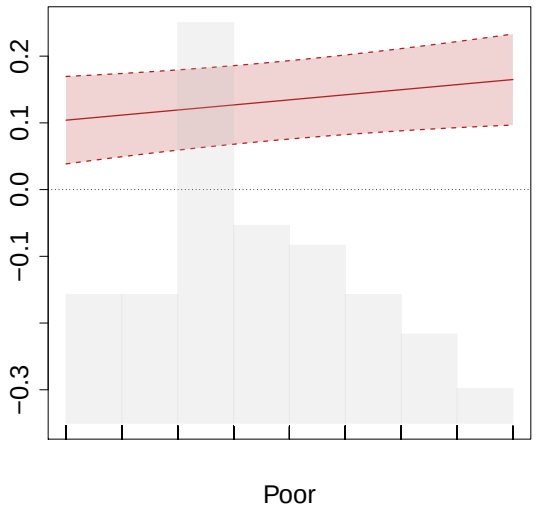
(a) MDC



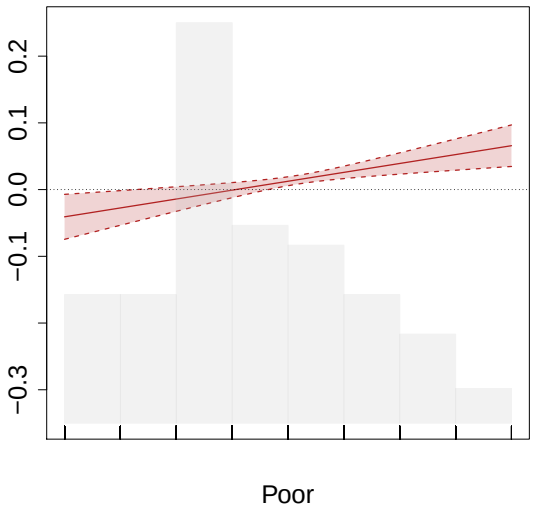
(b) ZANU



(c) None



(d) Refuse



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

This article develops a theory that the poor, for either physical and psychological reasons, are more vulnerable to repressive violence. The poor may be 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 prediction 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 article 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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