

# How Political Repression Shapes Attitudes Toward the State: Evidence from Zimbabwe\*

Omar García-Ponce  
garcia.ponce@nyu.edu

Benjamin Pasquale  
ben.pasquale@nyu.edu

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## Abstract

How does political repression influence civilian attitudes toward the state? We study Zimbabwe, a context marked by government repression, physical and economic insecurity, but also regular electoral competition. To isolate the causal effect of indirect exposure to violent political repression, we employ a novel identification strategy that exploits the timing and location of state-led violent events, relative to the timing and location of nationally-representative household surveys. This strategy compares individuals interviewed just days after repressive conflict events, to observably similar individuals interviewed just days before such events. Individuals indirectly exposed to repression self-report substantially higher trust in the president and ruling party. We find evidence consistent with a mechanism of preference falsification, where respondents report higher trust in the state due to fear of the Mugabe regime. The effects of repression increase approaching elections, bolstering the idea that autocrats employ violence in order to win elections.

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\*Omar García-Ponce is a Post-Doctoral Fellow at the Center for Global Development. Benjamin Pasquale is a Post-Doctoral Researcher at the Center for International Studies, University of Southern California. We are grateful to Chris Blattman, Julia Cagé, Stephen Chaudoin, Christian Davenport, Oeindrila Dube, Willa Friedman, Guy Grossman, Jennifer Hill, Macartan Humphreys, Horacio Larreguy, Will Moore, Alex Scacco, and Cyrus Samii for particularly helpful comments. We also thank participants at the 2013 Working Group in African Political Economy (WGAPE) at MIT, the 2013 International Political Economy Seminar (IPES) at Claremont Graduate University, the 2013 Annual Meeting of the Midwest Political Science Association, and the Conflict Consortium's Virtual Workshop (CCVW) for providing valuable feedback on earlier versions of the paper. All interpretations and any errors are ours alone.

# 1 Introduction

Since at least Niccolò Machiavelli's *The Prince* (1532), political observers have questioned and theorized how and why political leaders use force for political ends. Citizens, targeted either directly or indirectly by state repression, may exhibit either greater allegiance or increasing hostility towards state leaders and institutions. Understanding systematically how and why citizens respond to violence is therefore central to understanding how and why governments use repression. Yet, there has been little systematic analysis of how political violence or intimidation influences individual-level attitudes and behavior. The paucity of work in this area stands in sharp contrast with the prevalence of violent conflict during the past half-century. While civil wars have afflicted a third of all nations, according to Blattman and Miguel, "[t]he social and institutional legacies of conflict are arguably the most important but least understood of all war impacts" (Blattman and Miguel, 2010, 42).

This gap in the literature makes it difficult to understand why states use violence outside of civil or interstate conflicts and why leaders promote (or allow) small-scale violence and intimidation by police, security forces, parties, mobs, vigilantes or other armed groups. In a review of the study of government repression, Davenport asks: "[w]hat are the 'benefits' of repression? Why do authorities believe that repressive action will lead them to their objectives, and does repression actually produce intended benefits? The answers are not clear" (Davenport, 2007, 17). Despite the fact that coercion and repressive behavior remain pervasive across a range of governmental regimes, current literature in political science offers little clear evidence for why states engage in repression.

In this paper, we address the question of how, and to what extent, incidents of state-led violence influence citizen attitudes toward the state – and in particular – trust in institutions. We study the Zimbabwean context, an environment marked by an abusive incumbent government, economic instability, and poor socio-economic conditions. Zimbabwe is an ideal case for testing the relationship between political repression and citizens' attitudes for at least two reasons. First, Robert Mugabe's regime has been characterized as a militarized form of electoral authoritarianism. Elections have been held on regular basis since independence in 1980. Yet elections have been neither free nor fair, and the state has routinely used violence to repress opposition groups and their supporters.

Second, the case of Zimbabwe provides rich micro-level data on violent repression and

political attitudes, which may be leveraged to evaluate the attitudinal impact of violent political repression. The recurrent use of violence in Zimbabwean politics has been documented by civil society organizations such as Zimbabwe Human Rights NGO Forum, but also with a geocoded-event dataset by the Armed Conflict Location & Event Data Project (ACLED).<sup>1</sup> Additionally, despite the constant threat to public security, a series of household surveys aimed at measuring political attitudes was conducted by the Afrobarometer over the same time period for which there is data on repression. After geocoding and spatially merging multiple waves of the Afrobarometer household survey in Zimbabwe, the combined dataset allows a unique opportunity to analyze how political repression affects political attitudes using fine-grained temporal and spatial variation.

We make two contributions: one methodological, and one substantive. First, we develop a new method for identifying the short-term causal effects of indirect exposure to political violence. Any study measuring the effects of conflict or violence must address the central challenge that violence is not random. Perpetrators of violence make strategic choices, usually conditioned on specific information or structural factors – the identity of their targets, the geographic area, the availability of resources, the choice of tactic, and so on. Our strategy exploits plausibly exogenous variation in exposure to violence generated by the timing and location of conflict events, relative to the timing and location of Afrobarometer surveys in Zimbabwe – a strategy with potential for other research settings where the timing of surveys and political events (conflict-related or otherwise) overlap.

Given the unanticipated nature of conflict events during the Afrobarometer fieldwork, some respondents were interviewed just days after repressive conflict events, and others just days before. This quasi-random variation in recent, indirect exposure to violent political repression allows us to credibly isolate the causal effect of such exposure on attitudes toward the state. We identify the effect of short-term, district-level exposure to violent political repression by comparing individuals whose districts experienced violent events within a 30-day, 15-day, 10-day, 7-day, or 5-day windows *prior* to the interview. These interviewees are compared to other survey respondents interviewed within the same district in the equivalent temporal window *after* the violent events.

By matching citizens' survey responses with violent events (both sub-nationally and within

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<sup>1</sup>For more information, visit: [www.acleddata.com/](http://www.acleddata.com/)

narrow temporal windows), our analysis is unlikely to be confounded by omitted variables. Any such bias would require unobserved variables that co-varied both within roughly the same week-period of the violent events and occur in the same sub-national districts where the events took place. We assess these possibilities by examining survey measures of respondents approaching the timing of repression events. As presented below, we find little to no evidence that (1) survey enumerators changed their behavior approaching or following conflict events, (2) the profile of respondents changed approaching or following conflict events, or (3) those individuals interviewed before or after conflict events were systematically different based on observable demographic, identity, or economic factors.

Our second contribution is to demonstrate how indirect exposure to violent repression in Zimbabwe affects citizen attitudes towards the state. We find strong evidence that indirect exposure to state repression leads citizens to report higher trust in the state institutions – the president, the ruling party, the parliament, local officials, and the police. Respondents interviewed within 10 days after a state-led violent event took place in their district are 15% more likely to say that they “trust the president a lot,” and 18% less likely to declare that they “do not trust the president at all,” relative to respondents interviewed 10 days before a state-led violent event in the same district.

We examine two mechanisms that could mediate how repression affects citizen support for the government. In an environment of high political instability and insecurity, one mechanism is *state-imposed political order*. According to this mechanism, state security operations that impose political order are valued by citizens, leading them to report higher support for the state. On the other hand, according to a second mechanism of *preference falsification*, citizens report higher support for the government in response to growing fear for their personal security. We find evidence consistent with the preference falsification mechanism as exposure to state repression increases both (a) fear of electoral violence and intimidation, and (b) the probability the respondents believe the state organized the household survey.

We argue the short-term effects of repression, and patterns of preference falsification, are important precisely because governments use force instrumentally for political ends. New research has shown electoral violence in Africa to be (a) common, (b) generally perpetrated by the incumbent, and (c) more frequent prior elections than afterwards (Straus and Taylor, 2011). Our empirical results provide causal evidence to help explain these patterns and ad-

judicate between rival hypotheses that might predict electoral violence. In the Zimbabwean context, as we show below, the Robert Mugabe regime has historically used violent repression to increase support for the ZANU-PF party leading up to, and sometimes following, competitive elections. In line these electoral implications of our findings, our results demonstrate that repression increases *reported* support for the state increases as the timing of the survey approaches the 2005 Presidential election in Zimbabwe.

## 2 Literature on Violence and Repression

Under what conditions will exposure to state-led political violence shape the attitudes of ordinary individuals? To date there has been relatively little systematic micro-empirical research on the effects of violence and repression on individuals. This lack of evidence on the effects of violence has also been identified in conflict-related subfields. In a review of the study of riots Steven Wilkinson details many important findings from and new challenges for studies on the causes of violence, but concludes by stating "we also ought to do more studies that use riots as an independent variable" (Wilkinson, 2009, 341). In this paper we focus on how short-term and indirect exposure to violence and repression affect ordinary citizens. In this way we study a numerically large category of individuals that to some extent includes (to a greater or lesser degree) every individual living in a country that experiences, or recently experienced, a serious armed conflict.

Our study joins only several to provide causal evidence of how some form of exposure to political conflict affects political attitudes and behavior. While a growing body of empirical research identifies the causes of political violence, little research has measured the effect of violence or conflict on the attitudes of citizens.<sup>2</sup> Among those studies identifying the effect of exposure to conflict, several important studies have measured the attitudes of former combatants. Blattman (2009), for instance, presents evidence that participation in a the Lord's Resistance Army in Northern Uganda increases engagement in politics among ex-combatants. Studying members of the Israel Defense Forces, Grossman et al. (2015) find that ex-combatants exhibit hardened attitudes towards rivals, and are more likely to vote for

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<sup>2</sup>More specifically, recent research finds that dynamics of political violence are driven by economic growth shocks (Harari and Ferrara, 2012; Edward Miguel and Sergenti, 2004), geographic conditions (Burke et al., 2009), commodity prices (Bazzi and Blattman, 2011; Dube and Vargas, 2013), foreign aid (Nunn and Qian, 2012), and institutions (Besley and Persson, 2012).

hardline parties (2013).<sup>3</sup>

Beyond political attitudes and behavior, new studies have also examined how conflict-exposure affects cooperation and social cohesion. [Bellows and Miguel \(2009\)](#) find in Sierra Leone that war victimization increased self-reported political mobilization and participation in local collective action, which they interpret as a psychological legacy of exposure to political violence. In Burundi, [Voors et al. \(2012\)](#) find that individuals indirectly exposed to civil war violence were more altruistic, while [Gilligan et al. \(2014\)](#) in post-war Nepal find that indirect exposure to violence led to greater within-community trust, cooperation, and altruism. Studying fairness, [Whitt and Wilson \(2007\)](#) find lower than expected out-group ethnic bias in post-war Bosnia. However not all post-war studies using lab in the field methods find an increase in social cohesion: [Cassar et al. \(2011\)](#) find that exposure to conflict in post-war Tajikistan lowered trust and fairness within local communities, decreased the likelihood of impersonal exchange, and increased norms of within-group morality.<sup>4</sup> In this study we complement these within- and across- community findings with new evidence on how exposure to conflict affects attitudes towards government actors. Additionally, while the studies above describe post-war contexts, here we focus a political environment where state repression is frequent and ongoing – particularly during electoral campaigns.

Two features of our research design contribute to a large literature on repression and dissent. First, we measure citizen attitudes towards government actors in response to repression. Second we focus on the short term effects of violence – relevant for understanding the incentives of actors who might engage in violence or repression.<sup>5</sup> With respect to understanding patterns of repression and dissent, [Lichbach \(1987\)](#) predicts a substitution effect where violent repression leads an opposition to use non-violent methods (and vice-versa). [Moore \(1998\)](#) finds empirical support for Lichbach’s model, which is also broadly in line with [Lyll](#)

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<sup>3</sup>While not explicitly related to how exposure to political violence affects political attitudes and behavior, [Humphreys and Weinstein \(2009\)](#) study the demobilization and reintegration of former combatants in Sierra Leone, while [Bateson \(2012\)](#) finds that individuals who report being the victim of a crime are more likely to participate in politics.

<sup>4</sup>While in our study we focus on short-term effects, other studies examine long-run outcomes. [Garcia-Ponce and Wantchekon \(2013\)](#) find a persistent legacy of colonial repression in Madagascar where individuals living in districts affected by colonial-era repression exhibit lower perceptions of freedom of expression in society today. [Nunn and Wantchekon \(2011\)](#) find that individuals whose ancestors were heavily raided during the slave trade are less trusting, arguing this legacy was driven by changing cultural norms, beliefs, and values (2011). [Jha and Wilkinson \(2012\)](#) find that, during the partition of South Asia, the combat experience driven by the ethnic composition of districts affected legacies of co-ethnic immigration and ethnic cleansing.

<sup>5</sup>For reviews of this literature see [Davenport \(2007\)](#) and [Davenport and Moore \(2012\)](#).

(2009), who finds that indiscriminate bombing by the Russian government led to a reduction in insurgent attacks in Chechnya.<sup>6</sup>

While we do not address the question of whether repression causes an increase in protest or the reverse, we believe our findings here do inform this important question. We find that government repression leads to (1) an increase in *self-reported* support for the state and (2) evidence that citizens falsify their true preferences to maximize their personal security. Taken together, these findings provide a micro-foundation support for what [Davenport \(2007\)](#) calls the "Law of Coercive Responsiveness." That is, our results help explain why state authorities will respond with violence force when their survival is threatened.

### 3 The Zimbabwean Political Context

The state of Zimbabwe (then 'Rhodesia') was created when Ian White unilaterally declared independence from the United Kingdom in 1965. After 14 years as an unrecognized state, the Lancaster House Agreement in 1979 facilitated electoral competition. In February 1980, Robert Mugabe's ZANU won an overwhelming electoral victory and since this time he and his party have remained in de-facto control of the government.<sup>7</sup>

Following the war for independence, Mugabe integrated ZIPRA, ZANLA armed organizations, and the Rhodesian army to form the Zimbabwe National Army. Since independence, Mugabe's ZANU-PF created a war veteran's militia, youth militia, in addition to capturing the Zimbabwean police and intelligence agency. ZANU-PF utilizes these "security structures to mobilize support, campaign and organize elections" ([Moyo, 2013](#), 70). Mugabe's rule has been described as "a militarized form of electoral authoritarianism" ([Masunungure, 2009](#), 8) where violence is used to ensure the survival of the ZANU-PF regime.

Citizens of Zimbabwe have been exposed to multiple forms of repression over the past several decades, generally instigated by the Mugabe's ZANU-PF regime. In 1988 the government organized violence and torture after the Food Riots. The government used military operations in response to rebellions in Matabeleland South and Midlands in 2000. The government invaded and expropriated farms in the name of land reform in 2000 and 2001. Catherine Boone

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<sup>6</sup>Though see [Shellman \(2006\)](#) for an alternative theory where dissent increases in the level of government repression.

<sup>7</sup>ZANU was renamed ZANU-PF in 1988 after a merger with Zimbabwe African People's Union (ZAPU).



has argued that this expropriation, in threat or reality, has provided a material base for an exclusionary nationalism in Zimbabwe (Boone, 2009, 183).

The Zimbabwe Human Rights NGO documented human rights abuses against teachers in 2002, and incidents of torture through 2012. In May 2005 the government began *Operation Murambatsvina* (in English: "Operation Drive Out Rubbish") a wide-spread effort to forcibly clear slums in urban areas of the country – areas believed to house much of the internal opposition to the Mugabe regime (Bratton and Masunungure, 2006). The United Nations estimated that *Operation Murambatsvina* caused the loss of home or livelihood for 700,000 citizens and indirectly affected as many as 2.4 million people (Tibajjuka, 2005). Scholars of this context have identified political violence as a new way of mobilizing people – as well as a method, through violence and exclusionary rhetoric, of demobilizing people. In this way *Operation Murambatsvina* served as redistricting by other means. As Boone writes, the state uses political violence instrumentally to win the support of key electoral constituencies (Boone, 2009, 183).

### 3.1 Opposition

The first significant political party challenge to Mugabe's dominance was founded by Morgan Tsvangirai in 1999. The Movement for Democratic Change (MDC), built on the politicization of trade unions (notably the Zimbabwe Congress of Trade Unions), civil society organization and religious groups (e.g. National Constitutional Assembly), creating an organized front to challenge Mugabe's ZANU-PF government. The MDC engaged in peaceful meetings and protests, but also created action committees (or parallel structures) with the aim of fostering mass action against the Mugabe regime. While the MDC rarely engaged in violence, Adrienne LeBas argues that the MDC matched ZANU-PF's exclusionary nationalism with a nationalist anti-state identity of its own creation (LeBas, 2006).<sup>8</sup>

In turn, ZANU-PF accused the opposition MDC as wanting to bring back 'white rule,' representing the West and particularly British interests. When confronted with opposition of any sort, the Mugabe regime has been quick to utilize intimidation and aggression. ZANU-PF's short-run strategies intend to create us-vs-them boundaries. Following the formation of the MDC, the state increasingly targeted teachers, ethnic minorities, and others suspected

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<sup>8</sup>A split in the MDC in 2006 caused sporadic violence between the MDC-Tsvangirai and MDC-Ncube factions (Compagnon, 2011, 114-116).



of opposition sympathies with violence and intimidation. According to LeBas, where party affiliation was not chosen, it was imposed (LeBas, 2006, 420-421).

### **3.2 Elections and Violence**

Elections have always been held regularly since Independence in 1980, but have never been free, fair, nor without violence (Compagnon, 2011, 37, 47). From independence, the ruling ZANU-PF has been accused of vote-buying, vote-rigging and last minute redrawing of districts (LeBas, 2006, 430). This instrumental use of violence for political ends was not new, but common since Zimbabwe's liberation war where "routine coercion was an important and in some areas crucial resource to obtain political support from the 'masses'" (Compagnon, 2011, 49). According to (LeBas, 2006, 429), violence followed elections as a form of punishment for opponents of the ruling party – "violence began to follow the election calendar, appearing in constituencies from which it had been absent in the period immediately preceding the polls." Moyo (2013, 69) argues that electoral violence in Zimbabwe is a tactic used effectively to whip up support base.

Electoral violence preceded parliamentary elections in 2000, in addition to physical assaults and intimidation of voters suspected of supporting the opposition MDC following the election (Makumbe, 2002, 89). Opposition parties nearly won a majority of parliamentary seats in 2000. Since then, Mugabe's government has used vote-rigging, intimidation, fraud and violence in the 2002, 2005, and 2008 elections. Regarding this period, (Makumbe, 2002, 87) writes, "the regime deployed tactics whose sheer brutality and underhandedness were without precedent even in the troubled post-independence history of this southern African republic." Compagnon (2011) argues that violence was important to ZANU-PF's electoral victories in parliamentary elections of 2000, presidential election in 2002 and again in the presidential run-off of 2008. According to two exit poll analyses, "up to 12 percent of the voters changed their vote from MDC to ZANU-PF as a consequence of the political violence inflicted on them during the electoral campaign" (Compagnon, 2011, 46).

Following the disputed March 2008 presidential election between Mugabe and Tsvangirai, a run-off election was called for June 27. During this period, ZANU-PF intensified its use of violence. According to the *Anatomy of Terror* report written by civil society organizations and analyzed by Kriger (2012), during this period ZANU-PF organized violence through a network

of politicians, state administrators and local militia.<sup>9</sup> Consider the constituency of Maramba Pfungwe (in Mashonaland East province), where ZANU-PF operated 15 bases prior to and during the election run-off campaign in 2008. From these bases, army leaders directed politicians, police, local Chiefs, village and kraal heads, and elected ward councillors to mobilize youth militia to perpetrate violence against local MDC activists, and protect them from arrest (Kriger, 2012, 18). Despite an agreement to form a power-sharing government led by Mugabe and Tsvangirai in September 2008, the ZANU-PF's use of violence for political ends has not ended. According to Kriger, the incorporation of 'opposition' parties in government from 2008 has not changed "ZANU PF's de facto rule through violence, intimidation, and repression against the 'opposition' in government" (Kriger, 2012, 11).

## 4 Theoretical and Empirical Implications

Given the history of state repression and protest in Zimbabwe, we hypothesize that indirect exposure to repression will increase citizens reported support for the actor that perpetrates a given violent event. Consider a political environment where there state is minimally democratic – where elections are held but are unlikely to be free, fair, or without violence. Such political contexts are common in authoritarian regimes where the incumbent government is unlikely to lose power, but these patterns of violent electoral competition may also arise in contexts of 'fair' elections where all parties agree on the necessity of elections but electoral violence is still common. In such an environment, the state does not necessarily have a monopoly over the use of force, but competing groups use force and intimidation in an attempt to gain citizen support.

- **Hypothesis:** *Exposure to repression increases citizens reported support for the state*

This hypothesis is consistent with the most relevant survey evidence on support for the state in Zimbabwe. Analyzing a cross-section of survey responses in 2004, Chikwanha et al. (2004) find that despite widespread violence, political support for the incumbent government increases. The authors explain this result as reflecting a combination of patronage from regime supporters, self-censorship and propaganda from the state. Given the open question of

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<sup>9</sup>More specifically, politicians included Members of Parliament, parliamentary candidates, provincial governors and party officials, administrators included retired military personnel, police, state intelligence agents and district administrators, and militia were mainly youth-led but also included war veterans (Kriger, 2012, 15-17).

*why* violence should increase reported support from the state we now suggest several channels that may explain this prediction.

In practice, cases of political violence involve perpetrators, victims, and tactics. We focus on state-directed political repression, easily the most common type of political violence in Zimbabwe in our period of study. In Zimbabwe, state actors and their proxies engage in violent repression in particular areas to mobilize (or coerce) local support. The mechanisms by which repression leads to public support may be several. For instance, individuals may report higher support for the state if they prefer the state engage in repressive action in order to create political order and improve local security.

- **Mechanism 1:** *State repression increases perceived state-provision of political order*

Alternatively, citizens may be angry at the state for engaging in violent repression in their geographic area but feel fearful of, and powerless against the regime. Given such fear of repression, citizens will hide their true feelings and report higher support for the state to protect themselves. [Kuran \(1997\)](#) describes the expression of attitudes where (true) private preferences diverge from public preferences as *preference falsification*.

- **Mechanism 2:** *State repression increases the falsification of political preferences*

In a 2008 survey following power sharing agreement, [Bratton \(2011\)](#), asks whether individuals prefer peace or justice for human rights abusers. Consistent with this second mechanism, [Bratton \(2011, 354, 378\)](#) finds that ordinary citizens “prefer peace above [transitional] justice,” due to fear of state-sponsored backlash. In short, an increase in reported support for the state may simply reflect individuals’ attempts to maximize their own security by falsifying their true political attitudes towards the state.

## 5 Data Construction

In order to precisely measure how exposure to political violence affects support for the government, we constructed an original dataset that leverages daily temporal and sub-national geographic information on repression and political attitudes. We combine three waves of the Afrobarometer, a nationally-representative household survey, conducted in Zimbabwe in 2004, 2005, and 2009 with conflict data from the Armed Conflict Location Event Dataset (ACLED).

Each Afrobarometer observation includes the district and date of the interview, while each conflict event in ACLED includes the date, the longitude and latitude, and perpetrator of the violent event.

In order to analyze how political conflict events affect the responses of individuals in Zimbabwe we required a method to map events to respondents. First we combined rounds 2, 3 and 4 from Afrobarometer survey in Zimbabwe.<sup>10</sup> Next we located geographic coordinates (longitude and latitude) for the district in which each respondent was surveyed across the three survey waves. We successfully located 96% of the district names from the Afrobarometer surveys. We then spatially merged all Afrobarometer respondents (by district) with ACLED conflict events (using the coordinates included in each ACLED country dataset) to publicly available GIS administrative boundaries.

The final result is a time-series, cross-sectional dataset with a district-day unit. Table 1 presents a summary of descriptive statistics from the merged dataset. Between January 1, 1997 – December 31, 2013, ACLED coded 4,850 violent events in Zimbabwe. More than three-quarters of these events are coded as “violence against civilians” (3,757 events or 77.5%). Of those events coded as “violence against civilians,” in 87.5% of cases ACLED codes the perpetrated as ZANU-PF or other government actors. In the three years from which we have Afrobarometer survey data (2004, 2005, 2009), ACLED records 819 events, of which 660 (80.6%) ACLED codes as “violence.” Given our focus in this paper on violent repression and the Zimbabwe state, we focus on political conflict events perpetrated by the state and coded as “violence against civilians.” For ease of exposition, we describe these events as “political repression.”

## 6 Empirical Strategy

Violence is not random. Since government regimes use repression strategically, estimates of the effect of political violence in any study will be biased if unobserved factors jointly predict exposure to violence and the outcome of interest. For instance, our results will be biased if ZANU-PF targets particular geographic areas where they believe opposition supporters live and we then observe a change in support in these areas. To address this fundamental

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<sup>10</sup>We excluded the first round of the Afrobarometer in Zimbabwe as many of the questions of interest are excluded and more generally implementation in first round appeared to be of a substantially lower quality.

empirical challenge, we employ a unique empirical design that exploits plausibly exogenous variation in indirect exposure to violence. We identify this variation by comparing the timing and location of conflict events with the timing and location of the Afrobarometer surveys.

Our identification strategy approximates as-if random exposure to violent events by comparing respondents who were interviewed right before a small-scale violent event took place in their district, to nearly identical respondents – from the same ethnic group – who were interviewed within the boundaries of the same district, but shortly after the event. The underlying assumption is that, within a given district, the timing of violent events is exogenous to the timing of survey interviews. To better approximate as-if random exposure to violence, we focus on individuals in districts who were exposed to violent events within a narrow temporal window of their interview (5, 7, 10, 15, or 30 days).<sup>11</sup>

Two features of the Afrobarometer household survey bolster the credibility of this identification strategy. First, households are selected using a systematic random sampling method.<sup>12</sup> Second, based on discussions with those familiar with the implementation the Afrobarometer survey we learned that the least safe geographic areas are typically not included in the sample of the household survey. This fact suggests that individuals living in areas exposed to violent events during the survey are unexpected. However, rather than simply assuming exogeneity, we next assess the viability of this research design in capturing unbiased estimates of indirect exposure to state repression.

## 6.1 Assessing Potential Confounders

Our identification strategy requires that conflict events did not alter the implementation of the Afrobarometer household surveys. Ordinarily when assessing the credibility of whether an independent variable can be considered *exogenous*, researchers conduct an analysis of balance: whether control variables appear similar between treated and control units. Here, we

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<sup>11</sup>After completing our analysis we learned of two other studies, related to crime and public health in Chicago, that use a similar method to ours, (Sharkey, 2010; Sharkey et al., 2014). However, to the best of our knowledge, our study is the first to use such a methodology in political science as well as the only such study to examine the effects of political conflict or violence.

<sup>12</sup>The Afrobarometer surveys conducted in Zimbabwe follow a random selection of citizens based on a stratified multistage cluster sampling design. Starting points (households) are randomly selected, followed by a systematic random walk pattern using 5/10 intervals. Within the household, a gender quota is filled by alternating interviews between males and females, and a numbered card is used to select the respondent. For additional details, see the Survey Technical Information of the 2005 Round in Zimbabwe, available here: [http://www.afrobarometer.org/files/documents/survey\\_technical\\_information/zim\\_r4\\_tif.pdf](http://www.afrobarometer.org/files/documents/survey_technical_information/zim_r4_tif.pdf).

go a step further and conduct a graphical analysis of balance for various time-sensitive treatment definitions. The idea is to evaluate balance across treatment conditions, and to assess whether such balance is sensitive to the temporal window used to define the treatment.

We exploit the timing of survey interviews with respect to a violent repression event to compare individuals who are *treated* (i.e. those interviewed just after the violent event) and *control* (i.e. those interviewed just before such event). We compare the profiles of these two sets of individuals across a number of characteristics that might confound an association between exposure to violence and attitudes toward the state. We focus on the following individual-level characteristics as potential confounding factors: the respondent's sex (*Female* indicator), Age in years, education (an indicator for *Secondary School* completion), religion (indicators for *Catholic* and *Protestant*), *Unemployed* status at the time of the interview, and ethnic group (*Shona* and *Ndebele*). We also include an index of the respondent's perceptions of local public services (*Shortage of Public Services*).<sup>13</sup> We regress each covariate on a dummy capturing indirect exposure to violent state repression. This variable is equal to 1 if a repression event occurred in the survey respondent's district in the past 5, 7, 10, 15 or 30 days, and equal to 0 if the event occurred in the 5, 7, 10, 15, or 30 days following the survey interview date. All estimates are based on OLS regressions using district and survey wave fixed effects. Robust standard errors are clustered by district.

In Figure 2, we plot the coefficients and 95% confidence intervals from the regressions of each covariate on exposure to repression within 5, 7, 10, 15 and 30 days. We observe a violation of our identification strategy if the estimated coefficient varies systematically approaching the date of the conflict event. Changes in the coefficient approaching the event imply that (1) survey enumerators strategically changed their behavior in response to, or anticipation of, the conflict event, or (2) respondents were less likely to participate in the interview approaching or after the conflict event. A second type of violation occurs if the coefficient is significantly and substantially different from 0 – this would imply that the profile of those interviewed was different after the event occurred than before. This second type of violation is the type ordinarily examined with use of a balance table.

As shown in Figure 2, we find little evidence that: (1) survey enumerators changed their

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<sup>13</sup>This index was constructed via principal components analysis using answers to questions on how often respondents have gone without fuel, water, and medicines or medial treatment over the past year. The resulting index was normalized rescaling by the minimum to make all the elements lie between 0 (never) and 1 (always).

behavior approaching or following conflict events, (2) that the profile of respondents changed approaching or following conflict events, or (3) those individuals interviewed before or after conflict events were systematically different based on observable demographic, ethnic identity, or economic covariate data. The clearest exception to these general patterns is the dummy for whether a respondent completed *Secondary School*, our proxy for education. While this covariate does not meaningfully increase or decrease approaching the conflict event, we do observe that individuals interviewed after the conflict events are less likely to have completed secondary school. All of the results in this paper are robust to the inclusion of *Secondary School* and the other covariates (as well as their exclusion), which we incorporate in separate model specifications below.

As an additional test of the identification strategy, we plot the survey responses of individuals who were interviewed just prior to repression events in their district (individuals in our *control* condition). We examine whether these individuals report an increase in self-reported “Fear of Violence” (exact wording of the survey question is included in the Supplementary Online Appendix). If these respondents were more likely to report fear of violence as date of repression neared the survey date we would consider this a violation of the identification strategy – a type of *anticipation effect*. As shown in the left plot in Figure 3, we find no evidence that respondents in the *control* condition anticipated the act(s) of repression 5 or more days after they were interviewed. We also examine non-response to the fear question among this same group of individuals (the *control* group). We find no evidence, as shown in the right graph in Figure 3, that our identification strategy was violated – individuals whose area would soon be affected by an act of repression were no more likely to refuse to answer this sensitive question.

## 6.2 Estimation Framework

We estimate the effect of district-level, recent exposure to violent political repression on various measures of an individual’s trust in the state institutions. Our basic estimation equation is given by:

$$TRUST_{i,e,d,t}^w = \beta_0^w + \beta_1^w REPRESSION_{i,e,d,t} + \alpha_e^w + \gamma_d^w + \psi_t^w + \mathbf{X}'_{i,e,d,t} \phi^w + \varepsilon_{i,e,d,t}^w \quad (1)$$

Here,  $TRUST_{i,e,d,t}^w$  is the level of trust reported by individual  $i$ , from ethnic group  $e$ , inter-



viewed in district  $d$ , during the Afrobarometer wave  $t$ . In this specification,  $\beta_1$  measures the effect of exposure to violent political repression on trust.  $REPRESSION_{i,e,d,t}$  is a binary measure equal to 1 if an individual was interviewed after a violent event of political repression took place in her district within a particular temporal window.  $REPRESSION_{i,e,d,t}$  takes a value of 0 if the interview took place before the violent event within the same temporal window.

The superscript  $w$  refers to the temporal length window: alternatively 5-days, 7-days, 10-days, 15-days, or 30-days. We include ethnicity fixed effects,  $\alpha_e$ , district fixed effects,  $\gamma_d$ , as well as survey-wave fixed effects,  $\psi_t$ .  $\mathbf{X}'_{i,e,d,t}$  is a set of personal characteristics, some of which are slightly imbalanced across individuals interviewed after and before the conflict events, and which could potentially affect trust in institutions. The results we report below are robust to their inclusion or exclusion. The individual level error term is denoted  $\varepsilon_{i,e,d,t}^w$ .

Estimation of Equation (1) is constrained by the fact that we cannot directly observe the latent continuous measure of  $TRUST_{i,e,d,t}$ . Instead, we observe four ranked response outcomes that come from the following question:

- "How much do you trust each of the following, or haven't you heard enough about them to say: [the President]?"

Response options include: (1) "Not at all," (2) "Just a little," (3) "Somewhat," and (4) "A lot." To maintain the ordinal categorical nature of these answers, we estimate ordered logit models. An alternative strategy is to ignore the categorical nature of the answers and instead estimate OLS regressions. In the Online Appendix, we show that estimates are statistically and substantively similar if we pursue this alternative strategy.

## 7 Results

In this section we present substantively meaningful and statistically robust evidence that indirect exposure to state repression leads to higher self-reported trust in the state. Panel A of Table 2 shows estimates from ordinal logit regressions of Trust for the President on indirect exposure to state repression for 5, 7, 10, 15, and 30-day windows (without controls). We find that recent indirect exposure to violent political repression leads to an increase in reported trust in the president. Interestingly, the data also reveal that the effect is larger in narrower

temporal windows of exposure, which underscores the validity of our empirical strategy to causally isolate the effect of indirect exposure to repression.

In Panel B, we focus on the 10-day treatment, which is used as a benchmark specification throughout the analysis. We include district, survey wave, and ethnicity fixed effects in all regressions. Additionally, we incorporate the following host of individual-level controls from the survey: female, age, whether the respondent completed secondary school, an index of shortage of public services, religion indicators (Catholic and Protestant), and whether the respondent was unemployed at the time of the interview. Across all specifications, we find positive and statistically significant evidence at the 1% level that recent indirect exposure to violent political repression leads to an increase in reported trust in the president.<sup>14</sup>

To interpret the magnitude of our treatment effect, we transform the ordinal logistic estimates from our benchmark model (reported in column (3) of Panel A in Table 2) into first differences of the predicted probabilities of observing each of the four outcomes of Trust in the President. That is, we compute  $P(Y = j|Treated) - P(Y = j|Control)$ , where  $j$  represents each of the four possible answers to the question on trust in the president (“a lot,” “some-what,” “just a little,” “not at all”). We use *Zelig* (Imai et al., 2012) to compute the simulated first differences, holding all covariates at their sample means, and plot the densities in Figure 4. We observe that respondents interviewed within 10 days following a state-led violent in their district are, on average, about 14 percentage points more likely to say that they “trust the president a lot,” relative to respondents who were interviewed within 10 days prior a state-led violent event in the same district. These respondents are 19 percentage points less likely to declare that they “do not trust the president at all.” By considering that 29% of the respondents in our sample “do not trust the president at all,” and about 23% trust him “a lot,” we conclude that these are sizable effects.

## 7.1 Alternative Measures of Trust in the Government

Following our theoretical predictions, we expect an increase in support for the state following repression due to (1) increased perceptions that the state provides political order, or (2) because individuals falsify their responses to maximize their personal security. In either case we should observe stronger effects for those state institutions that are more closely related to

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<sup>14</sup>We replicate Table 2 using OLS in Table 1 of the Online Appendix and find the same positive relationship and similarly precise estimates ( $p < .01$ ).

the state’s production of violence and coercion. In Zimbabwe, we expect state-led repression to affect perceptions of the ruling party and security forces more than the perceptions of the parliament or local officials. In Table 3 we again use ordinal logistic regression to examine the effect of indirect exposure to state repression events within a 10-day window on trust for the Ruling Party (ZANU-PF), the Parliament, Local Officials, and the Police.<sup>15</sup>

In Panel A of Table 3 we present four models (one for each institution) without controls, and in Panel B we include the full set of covariates described above. In each model we observe a positive and statistically significant relationship between indirect exposure to state repression and trust in government institutions.<sup>16</sup> In Figure 5 we present the simulated first differences of the predicted probabilities for trust in each institution when changing the treatment status (based on the benchmark specification using the 10-day temporal window).

We find substantively larger effects for the outcome *Trust in ZANU-PF* and *Trust in the Police*, both closely associated with President Mugabe and the use of state coercion. The plot in the top-left graph of Figure 5 indicates that, on average, respondents interviewed within 10 days after a violent event are about 14 percentage points more likely to say that they “trust the ZANU-PF a lot,” and 24 percentage points less likely to declare that they “do not trust the ZANU-PF at all,” relative to respondents who were interviewed within 10 days before a state-led violent event took place in the same district. Similarly, respondents exposed to repression are 15 percentage points less likely to respond that they “do not trust the police at all.”

## 7.2 The Effect of Repression Approaching the 2005 Senate Elections

Next, we examine whether there are differential effects of political repression on trust in the president, based on temporal proximity to an upcoming election. The 2005 wave of the Afrobarometer was conducted between one and two months prior to the 26 November Senate Elections. To capture the extent to which the effect of political repression on affects levels of trust as the date of the interview approaches election day, we interact the number of days until that election with (alternatively) the 10-day, 15-day, and 30-day treatment windows.<sup>17</sup>

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<sup>15</sup>The exact wording of these survey questions is provided in Section 1 of the Online Appendix.

<sup>16</sup>In the Online Appendix, we replicate Table 3 using OLS and find similar results.

<sup>17</sup>As this is a relatively demanding specification for which we can only use one survey wave, we did not have enough observations to reliably estimate 5-day and 7-day windows.

The estimation equation is then given by:

$$\begin{aligned}
TRUST_{i,e,d}^w &= \beta_0 + \beta_1^w REPRESSION_{i,e,d} + \beta_2^w ELEC\_DAYS_{i,e,d} & (2) \\
&+ (REPRESSION_{i,e,d} \times ELEC\_DAYS_{i,e,d}) \beta_3^w \\
&+ \alpha_e + \gamma_d + \varepsilon_{i,e,d}
\end{aligned}$$

Variables included here come from the 2005 wave of the Afrobarometer and are coded precisely as they are in Equation (1). There are two additional variables:  $ELEC\_DAYS_{i,e,d}$ , which is the number of days between the interview date and the upcoming election, and its interaction with the repression treatment ( $REPRESSION_{i,e,d} \times ELEC\_DAYS_{i,e,d}$ ). In this specification we also include ethnicity ( $\alpha_e$ ) and district ( $\gamma_d$ ) fixed effects.

We find that recent exposure to violent political repression more strongly predicts self-reported trust in the President as one approaches an upcoming election. In Figure 6, we again present first differences in the predicted probabilities of observing the four outcomes of *Trust in the President*. The first differences are a result of changing the treatment status, conditional on the number of days until the next election, for those respondents living in a district affected by repression 10 days prior to or following the interview. The top-left plot in Figure 6 indicates that the probability that a *treated* respondent reports that she “does not trust the president at all” decreases abruptly as one approaches the Senate elections. For instance, treated respondents are about 60 percentage points less likely to declare that they “do not trust the president at all,” relative to control respondents, if interviewed less than a month from election day. If interviewed 45 days from election day, they are about 40 percentage points less likely to report the same.

By contrast, the probability of reporting “trust a lot” or “somewhat” does not seem to be significantly affected by proximity to the election. Put another way, exposure to repression does not appear to affect supporters of the regime (bottom-left and bottom-right plots in Figure 6). Rather, the results appear to be driven by individuals who are not supporters of the regime – individuals who are particularly vulnerable to state repression during the election season. Recall our predictions for the *preference falsification* mechanism, that regime opponents will maximize their security by hiding critical views of the government when answering the survey. Indeed we find that the amplification of our results in the lead up to an election is driven by individuals who shift their responses away from the answer they Trust the government

“not at all” as the election approaches.

An normatively unfortunate implication of this finding is that authoritarian leaders may indeed improve their electoral performance by using repression in the short-term. Further, these results are consistent with the state’s use of repression to mute the criticism of regime opponents. While these results suggest the preference falsification mechanism may be operative as respondents fear giving the ‘wrong’ answer approaching elections, in the following section we use additional survey evidence to distinguish between the *political order* and *preference falsification* mechanisms.

### 7.3 Why Does Repression Increase Self-reported Trust?

Multiple mechanisms could explain why state-led repression causes an increase in self-reported support for the ruling government party. In an insecure and unstable political environment, citizens may perceive the state’s use of force as enhancing local security – a mechanism of *perceived state-provided political order*. Alternatively, in the face of a government that regularly perpetrates violence and repression for electoral ends, citizens may *falsify their political preferences*, with the aim of enhancing their personal security. In order to better adjudicate between these two mechanisms we analyze two additional survey questions. The first question asks respondents about their fear of violence during electoral periods:

- "During election campaigns in this country, how much do you personally fear becoming a victim of political intimidation or violence?"

Again utilizing our main specification from Equation (1), in Table 4, Panel A we show that indirect exposure to repression leads respondents to report substantially higher fear of violence during election campaigns. First differences in the predicted probabilities of observing each of the four outcomes of *Fear of Violence* as a result of a change in the treatment status are shown in Figure 7 (top panel). The plot shows that treated respondents are 47 percentage points more likely to say that they fear “a lot” becoming a victim of political intimidation or violence. This positive association between fear of violence and indirect exposure to state repression is inconsistent with a mechanism that citizens report higher trust in the state due to their perception that the state uses force to successfully provide political order. However, this positive association is consistent with a mechanism of *preference falsification*, where citizens

hide their true political preferences due to fear of the Mugabe regime and its use of political repression.

As an additional test of the *preference falsification* mechanism, the Afrobarometer survey includes a question asking respondents who they believe organized (or sponsored) the household survey. We re-code this open ended question with a simple binary variable by categorizing survey responses that identify a state actor (the president, the ruling party, or the secret service) with a 1, while coding all other actors as a 0. The exact wording is as follows:

- "Who do you think sent us to do this interview?"

In Table 4, Panel B we present evidence from a series of logit regressions that repression increased the probability that interviewees report that the state sponsored the Afrobarometer survey. Accordingly, a density plot of simulated first differences in the predicted probability that the respondents believe the survey was sponsored by the state is shown in Figure 7 (bottom panel). Based on our most conservative estimate, column (3), treated respondents are 4 percentage points more likely to say that the survey was sponsored by either the president, the ruling party, or the secret service.<sup>18</sup> Since only 8% of the respondents in our sample identified a state actor as the survey sponsor, this finding also provides support for the *preference falsification* mechanism. If respondents believe the state is using the survey to guide its (often violent) tactics and campaigns, respondents should maximize their personal security by avoiding survey responses that are critical of the Mugabe regime or state more generally.

## 8 Conclusion

In this paper we provide robust, causal evidence that indirect exposure to state repression increases self-reported trust in the state. We find that individuals interviewed shortly after small-scale, state-led violent events are substantially *more* likely to report trust in state institutions, than observably similar individuals interviewed just before. Our main finding leverages a new method to measure the causal effect of political violence when outcomes are measured with high frequency. Given the growth of micro-comparative studies of conflict and

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<sup>18</sup>Using a linear probability model, as reported in Table 5 in the Online Appendix, treated respondents are 7 percentage points more likely to say that the survey was sponsored by the state.

violence, as well as survey research in conflict zones, we hope this method may be useful to other researchers.

We present three pieces of additional evidence to argue that higher reported support for the state in exposed districts actually reflects respondents' attempts to hide their genuine political preferences. First, we show that the effect of exposure to repression is magnified for individuals interviewed approaching the 2005 election. We show that this election timing result is driven by a lower probability that exposed respondents will report low levels of support for the President, as an election approaches. Second, we show that individuals exposed to repression are much more likely to report being fearful of becoming victims of political violence or intimidation during election campaigns. Third, we show that indirectly exposed individuals are more likely to believe that the state has sponsored the Afrobarometer survey.

Our evidence is consistent with scholarly analysis of Mugabe's ZANU-PF regime as exclusionary, repressive and frequently perpetrates violence as an electoral tactic (Boone, 2009; Compagnon, 2011; LeBas, 2006; Makumbe, 2002; Moyo, 2013). Our results are also broadly consistent with a country-level analysis of survey results in Zimbabwe that broad patterns of reported support for the state have increased despite widespread and growing hardship and vulnerability for ordinary citizens (Chikwanha et al., 2004). Our findings in this paper suggest that increases in self-reported support for the state in politically sensitive environments (such as Zimbabwe) should not be taken as given. Rather researchers should consider the possibility that such patterns are driven by respondents strategically hiding their true preferences in order to protect themselves.

Finally, our results shed some light on how and why authoritarian leaders benefit from the strategic use of violence both against opposition groups as well as against civilians. A substantial literature investigates the link between state repression and dissent (Davenport, 2007; Davenport and Moore, 2012; Lichbach, 1987; Lyall, 2009; Shellman, 2006). We contribute to this literature by showing that repression can increase *reported* support for the state, at least in the short term. A key part of this finding is that citizens operate as strategic actors who are careful to conceal their true political preferences when necessary. To be sure, citizens may shift their behavior quite quickly as new social or political conditions unfold. A next step for this research agenda is to systematically observe how citizens respond to other political shocks – such as opposition protests, rallies, and demonstrations. In this way we can



theorize more fully the conditions under which citizens will demonstrate publicly or hide in response to the behavior of state and non-state actors.

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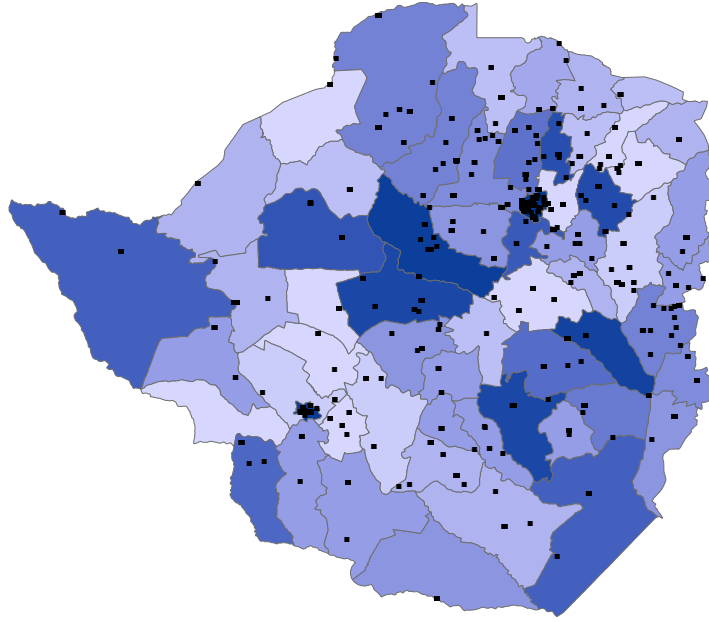
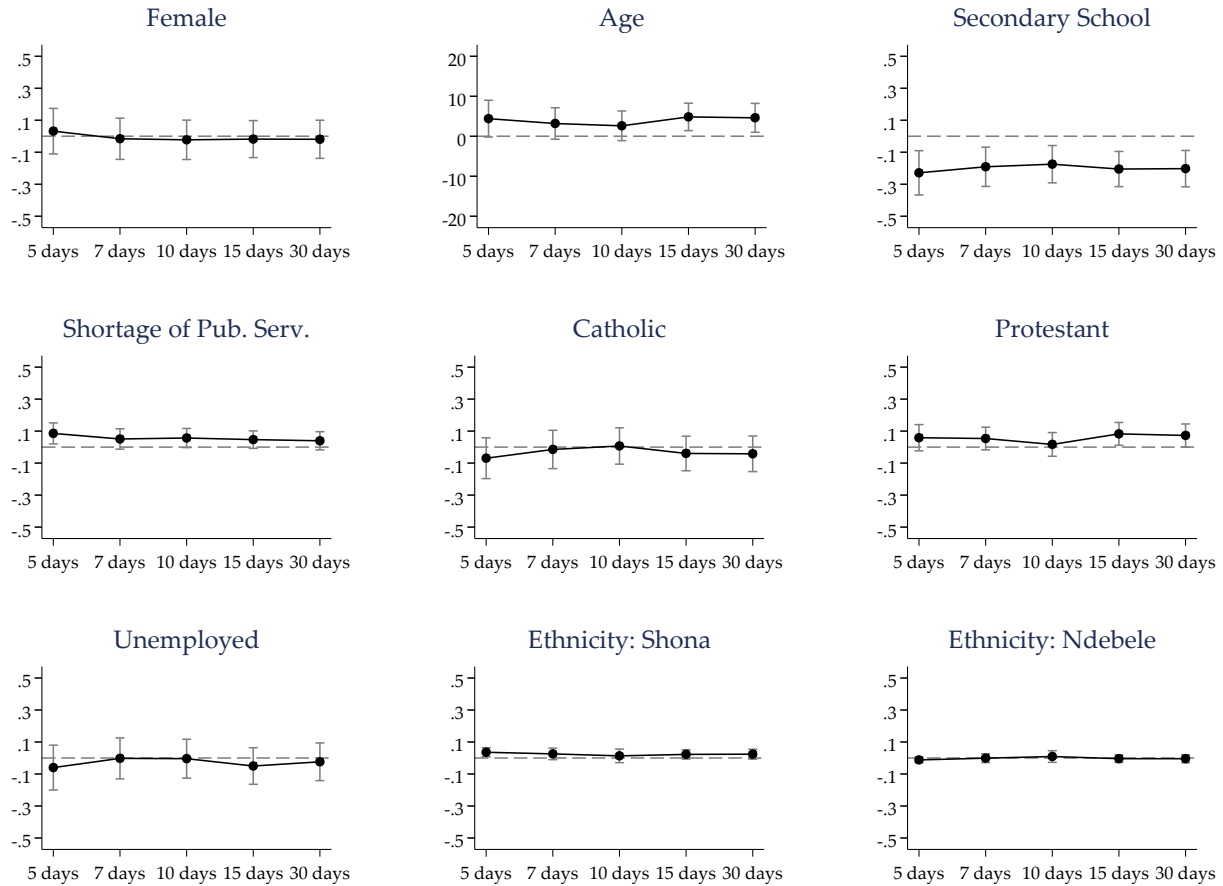
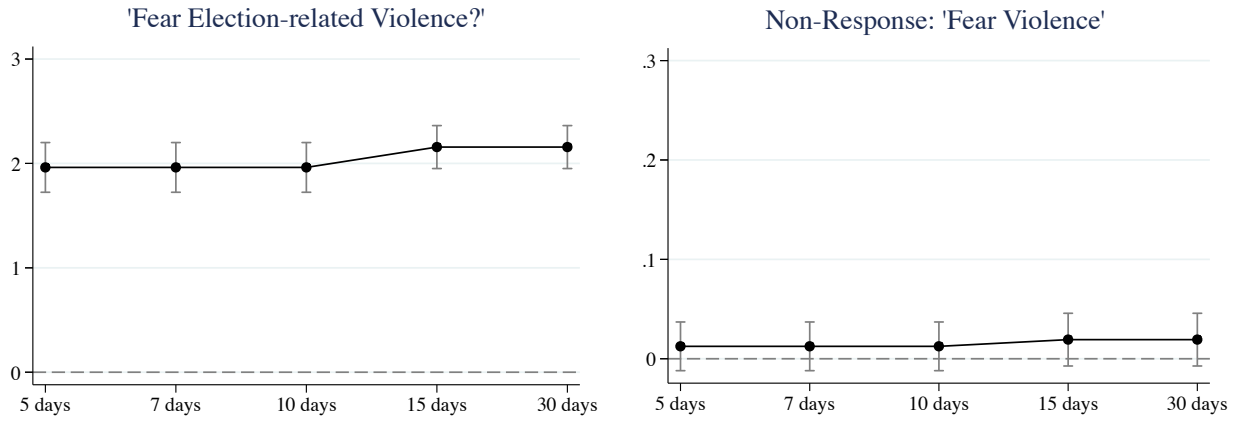


Figure 1: **Map of Zimbabwe:** Districts shaded according to the quantile distribution of the number of Afrobarometer respondents. Darker colors denote higher number of respondents. Squares indicate geo-referenced events of state repression reported in the Armed Conflict Location & Event Data Project (ACLED).



**Figure 2: Assessing Potential Confounders:** Each graph plots the coefficient and 95% confidence interval from five regressions of a covariate on the treatment of state repression. State repression is coded with a 1 if the respondent was exposed to a conflict event prior to the interview and a 0 if the respondent was exposed following the interview. Regressions vary with regards to the period of time between the repression event and the interview: 5, 7, 10, 15, or 30 days. All estimates are based on OLS regressions using district and survey wave fixed effects. Robust standard errors are clustered by district.





**Figure 3: Do Respondents Anticipate Acts of State Repression?** In this figure we plot mean survey responses for those individuals who answered the survey 5, 7, 10, 15, or 30 days prior to act(s) of repression in their district. Vertical bars represent the standard deviation.

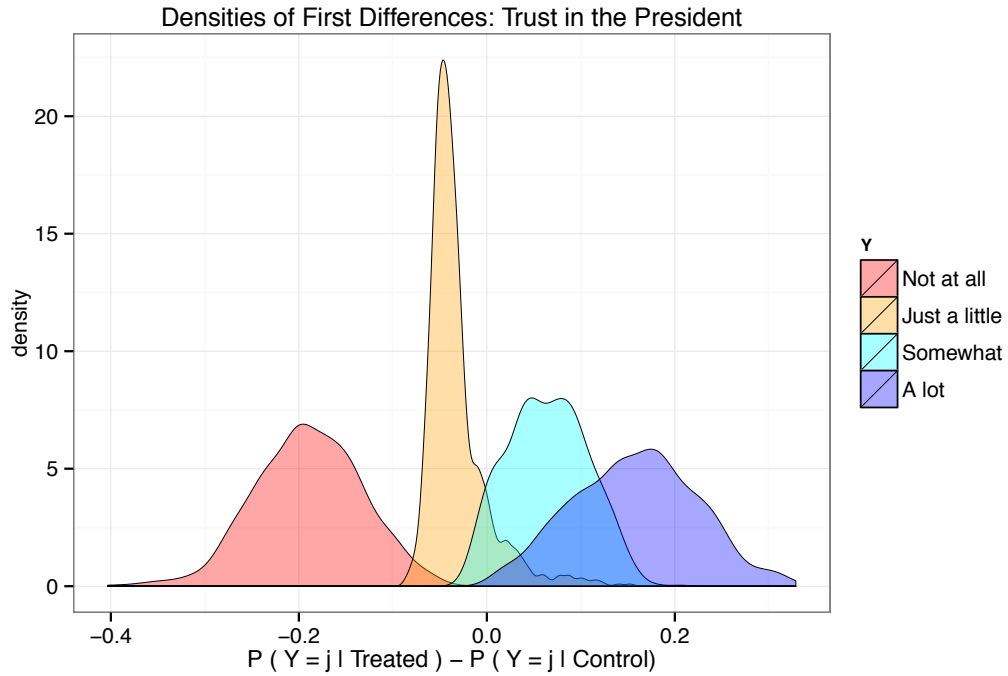


Figure 4: **The Effect of Repression on Trust in the President:** Densities of simulated first differences in the predicted probabilities of observing each of the four outcomes of *Trust in the President* as a result of a change in the treatment status, i.e.  $P(Y = j | \text{Treated}) - P(Y = j | \text{Control})$ , holding other attributes at their sample means.

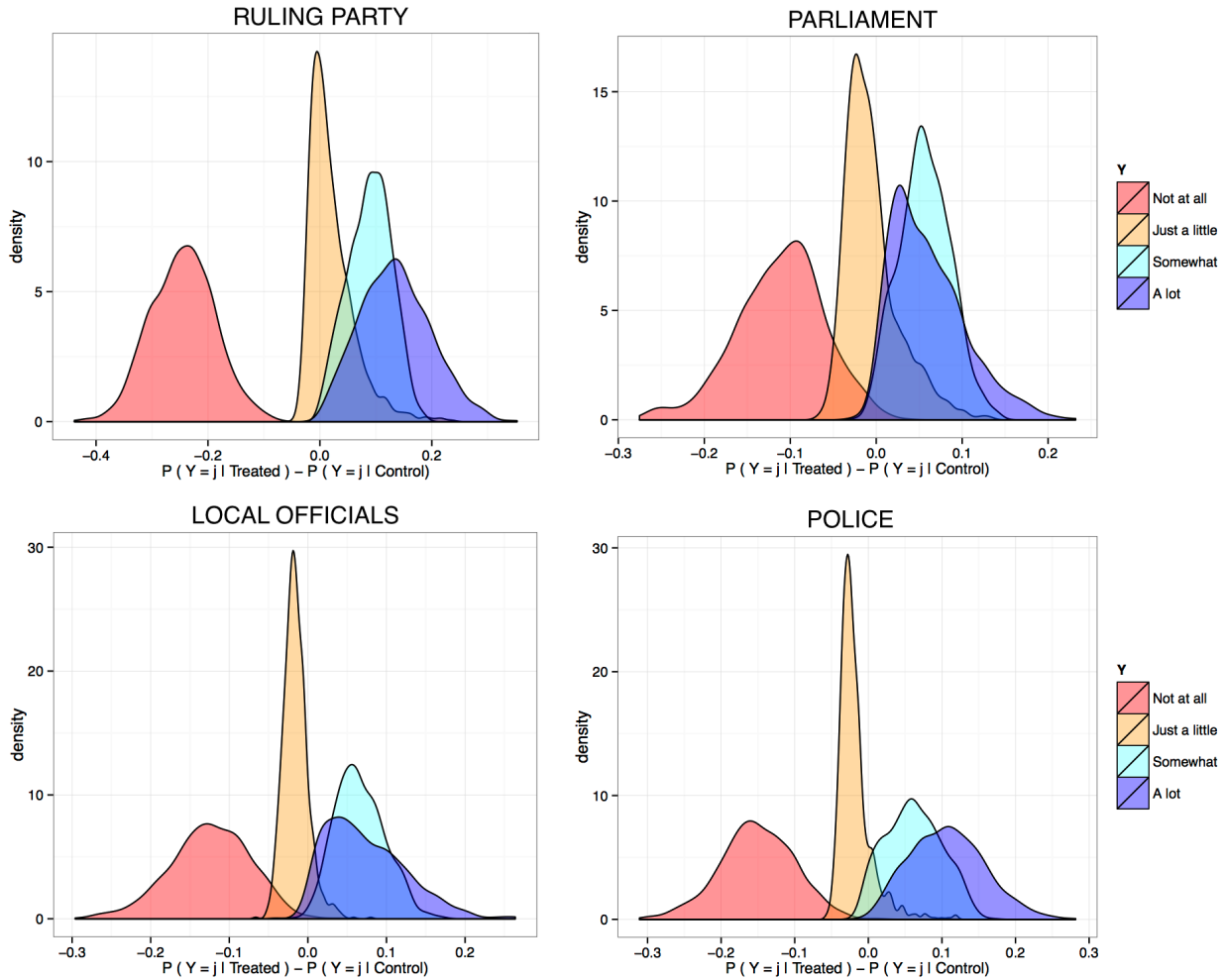
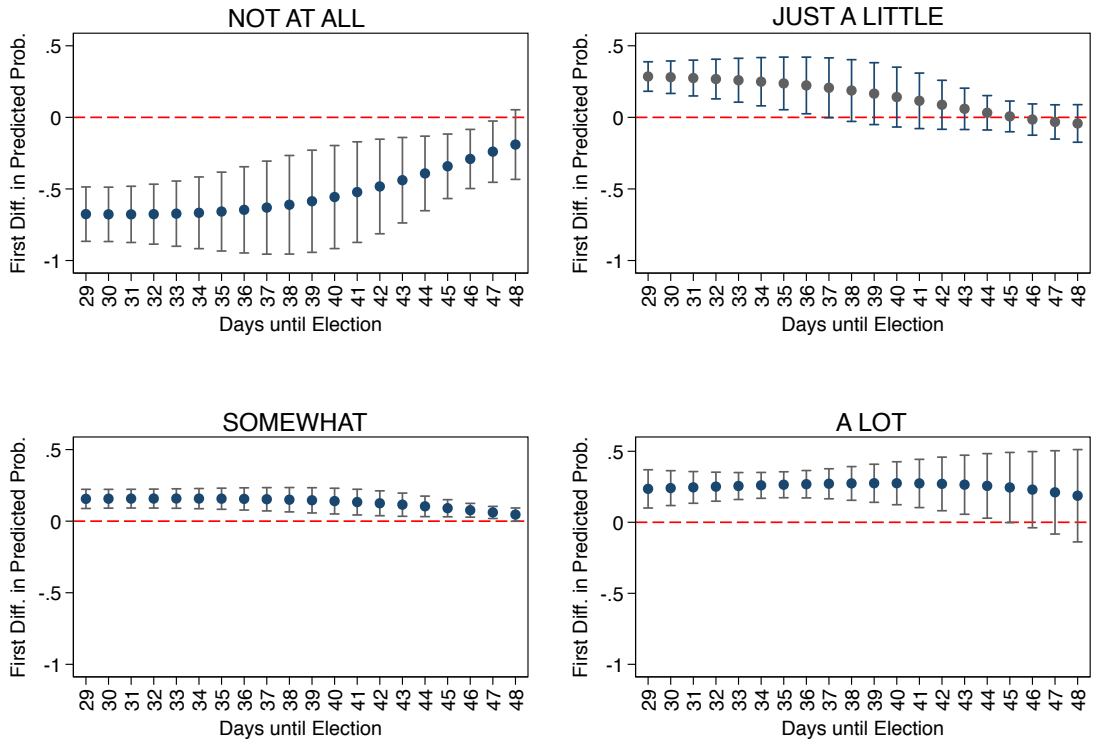


Figure 5: **The Effect of Repression on Trust in the State Institutions:** Densities of simulated first differences in the predicted probabilities of observing each of the four outcomes of *trust* in the ruling party, the parliament, the local officials, and the police, as a result of a change in the treatment status, holding other attributes at their sample means.



**Figure 6: The Effect of Repression on Trust in the President Approaching Elections:** In each plot, the dots represent the first differences in the predicted probabilities of observing one of the four outcomes of *Trust in the President* as a result of a change in the treatment status, conditional on the number of days until the next election. Vertical capped spikes indicate 95% confidence intervals.

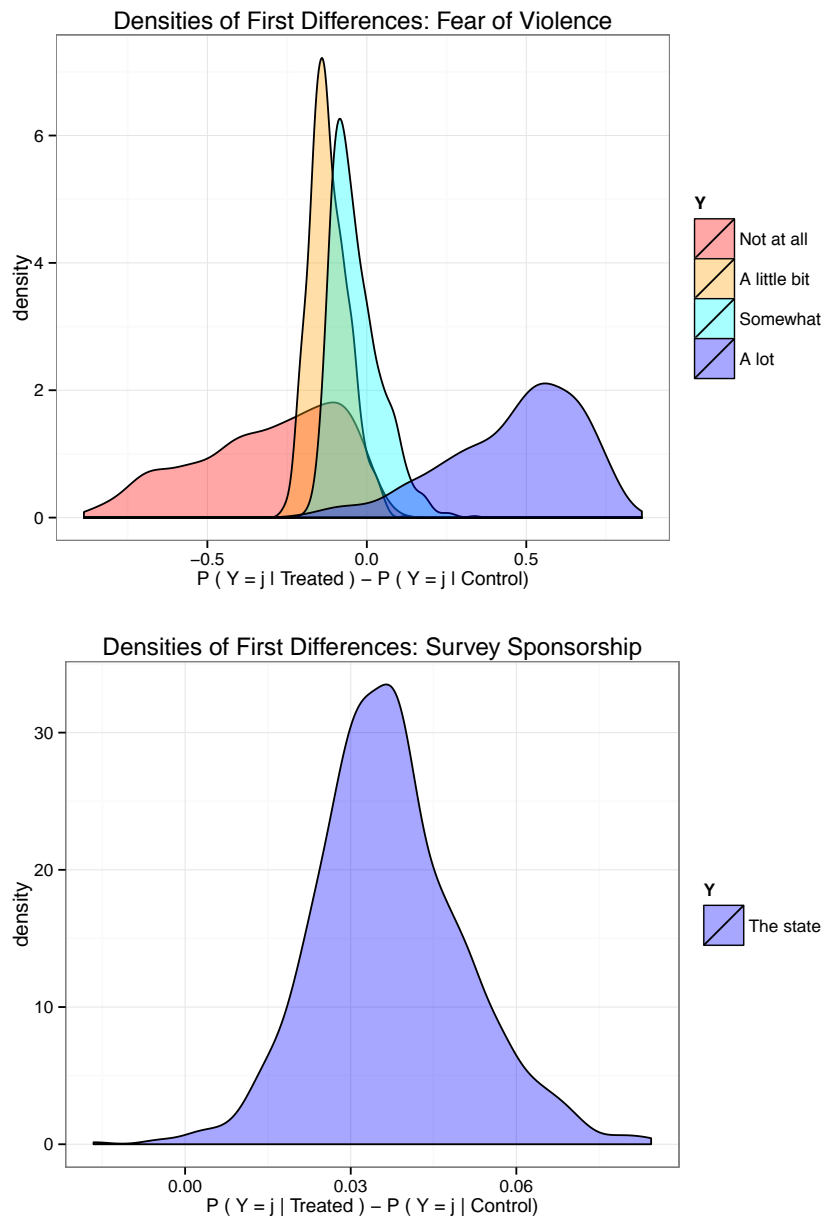


Figure 7: **Mechanisms: The Effect of Repression on Fear of Violence and Survey Sponsorship.** Top: Densities of simulated first differences in the predicted probabilities of observing each of the four outcomes of *Fear of Violence* as a result of a change in the treatment status, holding other attributes at their sample means. Bottom: Density plot of simulated first differences in the predicted probability that the respondents believe the survey was sponsored by the state.

Table 1: Summary Statistics

Variable	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Obs.
	<b>Full Sample</b>			<b>10-Day Sample</b>		
<i>Trust in the State Outcomes</i>						
Trust in the President	1.337	1.110	3,099	1.371	1.126	720
Trust in the Ruling Party	1.163	1.105	3,183	1.147	1.121	746
Trust in the Parliament	1.484	0.981	3,073	1.446	1.007	724
Trust in Local Officials	1.360	1.034	2,155	1.279	1.026	466
Trust in the Police	1.435	1.057	3,275	1.380	1.106	772
<i>Mechanism Outcomes</i>						
Fear of Violence	2.406	0.980	1,192	2.415	0.951	258
Fear of Violence (non-response)	0.007	0.081	1,200	0.008	0.088	260
Sponsor of the Survey	0.091	0.288	3,352	.083	0.276	770
<i>Control Variables</i>						
Female	0.501	0.500	3,352	0.505	0.500	794
Age	36.465	14.759	3,332	35.076	14.022	793
Catholic	0.261	0.439	3,352	0.338	0.473	794
Protestant	0.259	0.438	3,352	0.175	0.380	794
Ethnic: Ndebele	0.152	0.359	3,352	0.099	0.300	794
Ethnic: Shona	0.789	0.408	3,352	0.864	0.343	794
Secondary School	0.635	0.481	3,352	0.734	0.442	794
Unemployed	0.457	0.498	3,351	0.482	0.500	794
Shortage of Public Services	0.379	0.248	3,310	0.395	0.266	798
<b>Treatment Variables</b>						
Repression, 5 days	0.607	0.489	654	.	.	.
Repression, 7 days	0.642	0.480	718	.	.	.
Repression, 10 days	0.698	0.460	794	.	.	.
Repression, 15 days	0.655	0.476	974	.	.	.
Repression, 30 days	0.658	0.474	1,054	.	.	.

Table 2: **The Effect of Repression on Trust for the President**

	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Varying Treatment Window</i>					
Repression, 5 days	1.05*** (0.16)				
Repression, 7 days		0.96*** (0.15)			
Repression, 10 days			0.89*** (0.15)		
Repression, 15 days				0.66*** (0.13)	
Repression, 30 days					0.60*** (0.12)
Observations	598	653	720	887	963
<i>Panel B: Adding Controls</i>					
Repression, 10 days	0.92*** (0.25)	0.86*** (0.25)	0.91*** (0.24)	0.91*** (0.24)	0.91*** (0.24)
Female	-0.02 (0.14)	-0.08 (0.14)	-0.10 (0.14)	-0.10 (0.14)	-0.09 (0.14)
Age	0.01*** (0.01)	0.00 (0.01)	0.00 (0.01)	0.00 (0.01)	0.00 (0.01)
Secondary School		-0.65*** (0.20)	-0.68*** (0.20)	-0.70*** (0.20)	-0.67*** (0.20)
Shortage of Public Services			-1.11*** (0.29)	-1.11*** (0.29)	-1.04*** (0.29)
Catholic				0.12 (0.16)	0.11 (0.16)
Protestant				0.06 (0.20)	0.06 (0.20)
Unemployed					-0.27* (0.15)
Observations	720	720	708	708	708

Ordinal logit estimates. Panel B includes district, survey wave, and ethnicity fixed effects. Robust standard errors are shown in parentheses.

\* p<0.1, \*\* p<0.05, \*\*\* p<0.01 (two-sided tests)

Table 3: **The Effect of Repression on Trust in the State**

	(1)	(2)	(3)	(4)
	ZANU-PF	Parliament	Local Officials	Police
<i>Panel A: No Controls</i>				
Repression, 10 days	0.81*** (0.15)	0.47*** (0.15)	0.53*** (0.17)	0.64*** (0.15)
Observations	746	724	466	772
<i>Panel B: With Controls</i>				
Repression, 10 days	0.97*** (0.22)	0.45* (0.23)	0.58*** (0.22)	0.72*** (0.20)
Female	-0.06 (0.14)	-0.12 (0.14)	-0.06 (0.18)	-0.14 (0.14)
Age	0.01 (0.01)	0.01 (0.01)	0.00 (0.01)	0.01 (0.01)
Secondary School	-0.49*** (0.19)	-0.60*** (0.19)	-0.98*** (0.23)	-0.73*** (0.18)
Shortage of Public Services	-1.08*** (0.30)	-1.01*** (0.30)	-1.59*** (0.35)	-1.44*** (0.31)
Catholic	0.00 (0.16)	0.16 (0.16)	0.49** (0.20)	-0.03 (0.15)
Protestant	0.45** (0.21)	0.28 (0.19)	-0.02 (0.26)	0.27 (0.20)
Unemployed	-0.16 (0.15)	-0.06 (0.15)	-0.09 (0.19)	-0.12 (0.15)
Observations	732	711	461	757

Ordinal logit estimates. Panel B includes district, survey wave, and ethnicity fixed effects. Robust standard errors are shown in parentheses.

\* p<0.1, \*\* p<0.05, \*\*\* p<0.01 (two-sided tests)



Table 4: **Mechanisms: Political Order and Preference Falsification**

	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Outcome is Fear of Violence</i>					
Repression, 5 days	2.90*** (0.88)				
Repression, 7 days		2.84*** (0.87)			
Repression, 10 days			2.73*** (0.91)		
Repression, 15 days				2.64*** (0.92)	
Repression, 30 days					2.64*** (0.95)
Observations	140	178	251	344	358
<i>Panel B: Outcome is State Sponsorship of Survey</i>					
Repression, 5 days	1.07** (0.43)				
Repression, 7 days		0.87** (0.42)			
Repression, 10 days			0.77* (0.42)		
Repression, 15 days				0.85** (0.40)	
Repression, 30 days					0.86** (0.41)
Observations	618	680	753	855	934

Ordinal logit estimates in Panel A. Logit estimates in Panel B.

Panel A includes individual controls, and district and ethnicity fixed effects.

Panel B includes individual controls, district, wave, and ethnicity fixed effects.

Robust standard errors are shown in parentheses.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$  (two-sided tests)

# Online Appendix—Not Intended for Publication

## A.1 Exact Wording of Afrobarometer Survey Items

### 1. Main Trust Outcomes

- How much do you trust each of the following, or haven't you heard enough about them to say: The President ?

Not at all | Just a little | Somewhat | A lot

- How much do you trust each of the following, or haven't you heard enough about them to say: ZANU-PF ?

Not at all | Just a little | Somewhat | A lot

- How much do you trust each of the following, or haven't you heard enough about them to say: Parliament ?

Not at all | Just a little | Somewhat | A lot

- How much do you trust each of the following, or haven't you heard enough about them to say: Local Officials ?

Not at all | Just a little | Somewhat | A lot

- How much do you trust each of the following, or haven't you heard enough about them to say: Police ?

Not at all | Just a little | Somewhat | A lot

### 2. Outcomes to assess mechanisms: 'Fear Violence'

- During election campaigns in this country, how much do you personally fear becoming a victim of political intimidation or violence?

Not at all | Just a little | Somewhat | A lot

### 3. Outcomes to assess mechanisms: 'Sponsor of Survey'

- Who do you think sent us to do this interview?

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## A.2 Robustness to OLS

Table 5: The Effect of Repression on Trust for the President

	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Varying Treatment Window</i>					
Repression, 5 days	0.61*** (0.09)				
Repression, 7 days		0.57*** (0.09)			
Repression, 10 days			0.53*** (0.09)		
Repression, 15 days				0.40*** (0.08)	
Repression, 30 days					0.37*** (0.08)
Observations	598	653	720	887	963
<i>Panel B: Adding Controls</i>					
Repression, 10 days	0.51*** (0.14)	0.46*** (0.14)	0.49*** (0.13)	0.48*** (0.13)	0.49*** (0.13)
Female	-0.04 (0.08)	-0.08 (0.08)	-0.09 (0.08)	-0.09 (0.08)	-0.09 (0.08)
Age	0.01*** (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)
Secondary School		-0.39*** (0.11)	-0.41*** (0.11)	-0.42*** (0.11)	-0.40*** (0.11)
Shortage of Public Services			-0.61*** (0.16)	-0.61*** (0.16)	-0.56*** (0.16)
Catholic				0.06 (0.09)	0.05 (0.09)
Protestant				0.01 (0.11)	0.02 (0.11)
Unemployed					-0.16* (0.08)
Observations	720	720	708	708	708

OLS estimates. Panel B includes district, survey wave, and ethnicity fixed effects.

Robust standard errors are shown in parentheses.

\* p<0.1, \*\* p<0.05, \*\*\* p<0.01 (two-sided tests)

**Table 6: The Effect of Repression on Trust in the State**

	(1) ZANU-PF	(2) Parliament	(3) Local Officials	(4) Police
<i>Panel A: No Controls</i>				
Repression, 10 days	0.47*** (0.08)	0.26*** (0.08)	0.30*** (0.09)	0.37*** (0.09)
Observations	746	724	466	772
<i>Panel B: With Controls</i>				
Repression, 10 days	0.52*** (0.12)	0.21* (0.12)	0.29** (0.11)	0.40*** (0.12)
Female	-0.05 (0.08)	-0.06 (0.07)	-0.03 (0.09)	-0.08 (0.08)
Age	0.01 (0.00)	0.01* (0.00)	0.00 (0.00)	0.00 (0.00)
Secondary School	-0.29*** (0.11)	-0.30*** (0.10)	-0.50*** (0.12)	-0.44*** (0.10)
Shortage of Public Services	-0.52*** (0.16)	-0.51*** (0.15)	-0.76*** (0.17)	-0.76*** (0.16)
Catholic	-0.00 (0.09)	0.07 (0.08)	0.25** (0.10)	-0.02 (0.09)
Protestant	0.23** (0.11)	0.15 (0.10)	-0.04 (0.13)	0.17 (0.12)
Unemployed	-0.10 (0.08)	-0.03 (0.08)	-0.08 (0.10)	-0.06 (0.08)
Observations	732	711	461	757

OLS estimates. Panel B includes district, survey wave, and ethnicity fixed effects.

Robust standard errors are shown in parentheses.

\* p<0.1, \*\* p<0.05, \*\*\* p<0.01 (two-sided tests)

Table 7: **Mechanisms: Political Order and Preference Falsification**

	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Outcome is Fear of Violence</i>					
Repression, 5 days	0.92*** (0.26)				
Repression, 7 days		0.91*** (0.26)			
Repression, 10 days			0.89*** (0.26)		
Repression, 15 days				0.87*** (0.26)	
Repression, 30 days					0.87*** (0.26)
Observations	140	178	251	344	358
<i>Panel B: Outcome is State Sponsorship of Survey</i>					
Repression, 5 days	0.12** (0.05)				
Repression, 7 days		0.09** (0.04)			
Repression, 10 days			0.07* (0.04)		
Repression, 15 days				0.07** (0.04)	
Repression, 30 days					0.08** (0.04)
Observations	642	704	777	951	1030

OLS estimates. Panel A includes individual controls, and district and ethnicity FEs. effects. Panel B includes individual controls, district, wave, and ethnicity FEs.

Robust standard errors are shown in parentheses.

\* p<0.1, \*\* p<0.05, \*\*\* p<0.01 (two-sided tests)