Appendix

The psychology of state repression: Fear and dissent decisions in Zimbabwe

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A   Measurement

The variables used to measure each outcome and control variable are described in Table A.1. The survey instrument used in the study is available at `http://www.laurenelyssayoung.com/wp-content/uploads/2016/09/Final-Questionnaire.pdf`.

Table A.1: Measurement of outcomes, manipulation checks, and controls

	Measure	*Description*	*Responses*
Outcomes	Perceived risk of repression	Index of 12 questions measuring perceptions of six political risks in two periods (now and before the next election), conditional on attending an opposition rally	5-point likelihood scale
	Perceived propensity of others' to dissent	Index of 12 questions measuring perceptions of the proportion of other opposition supporters who would engage in six types of dissent (threats, assault, destruction of property, sexual abuse, abduction and torture, and murder) in two periods (now and before the next election)	5-point proportion scale
	Risk aversion	Spread of a 50-50 lottery selected for real cash rewards adapted from Eckel and Grossman (2002)	5 levels of risk
	Hypothetical propensity to dissent	Index of 12 questions measuring self-reported likelihood of engaging in six types of dissent (threats, assault, destruction of property, sexual abuse, abduction and torture, and murder) in two periods (now and before the next election)	5-point likelihood scale

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Manipulation Check	Behavioral propensity to dissent	Whether the respondent chose a wristband with a pro-opposition, anti-violence slogan on it over an otherwise similar plain wristband	Binary
	Current emotional state	The extent to which participants felt each of six primary emotions (anger, fear, disgust, sadness, surprise, and happiness)	4-point scale
Controls	Asset index	Includes quality of housing, land ownership, major assets like generators and cars, small assets like mobile phones and radios, and livestock. Adapted from the last Zimbabwean Demographic and Health Survey (DHS) and calculated separately for urban and rural households	First principal component
	ICT asset index	A subset of the assets in the DHS asset index that measure access to information and communication technology (ICT), including a cell phone, smartphone, computer, television and radio	First principal component
	Closeness to opposition	Self-reported closeness to the respondent's preferred opposition party	4-point closeness scale
	Self-efficacy	Index of 10 questions based on Jerusalem and Schwarzer (1995)	5-point agreement scale
	Past exposure to political violence	Version of Harvard Trauma Questionnaire adapted to Zimbabwean context that measures whether the respondent has personally experienced 8 types of political violence since the year 2000	Binary
	Past participation in dissent	Frequency of participation in eight acts of dissent since 2000	4-point frequency scale

Survey modules appeared in the following order:

1. Demographics
2. Self-efficacy
3. Risk attitudes (lottery) instructions
4. Emotion induction parts 1 and 2
5. Perceived risk of repression (order randomized)
6. Behavioral dissent (wristband)
7. Hypothetical dissent (order randomized)
8. Expectations of others (order randomized)
9. Emotion induction part 3
10. Perceived economic risk (order randomized)
11. Risk attitudes (lotteries)
12. Manipulation check (emotions)
13. Conjoint experiment (repression scenarios)[1]

[1]The conjoint scenarios were part of a separate study and are not presented in this paper.

14. Political participation
15. Past repression

B Measure validation

It is also important to validate the choice of the political wristband as a measure of propensity to take political action.[2] To do so, I look at the distributions of the hypothetical and behavioral measures of propensity to take risky political action. The primary goal of this exercise is to test whether taking a political wristband does in fact seem to be an indicator of willingness to take pro-democracy action. In fact, the hypothetical index is strongly predictive of the binary wristband measure. Figure B.1 shows that responses on the hypothetical measure of political action are strongly predictive of taking the wristband. It plots the predicted probability of taking the political wristband at different levels of the hypothetical propensity to act index. The plotted coefficients are from a probit model with the binary indicator for taking the political wristband as the dependent variable, the hypothetical dissent index as the explanatory variable, and gender, age, education, and assets as control variables.

Figure B.1: Validation of wristband as a measure of propensity to take political action
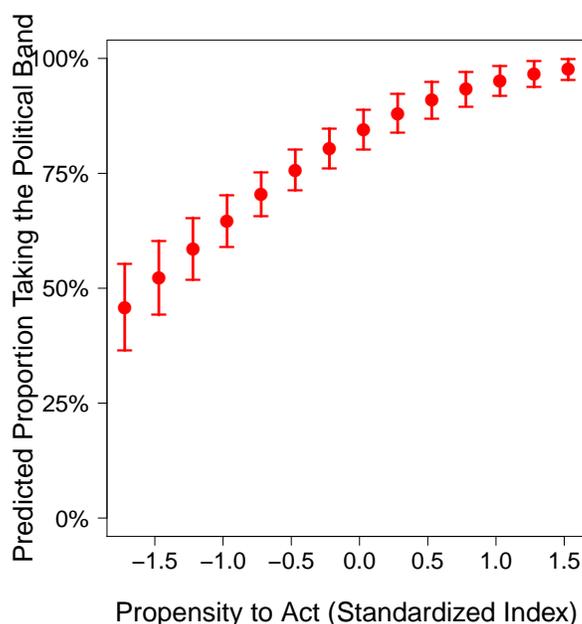
The predicted probability of taking the wristband for a respondent who is at the bottom extreme of the distribution on the hypothetical measures, which means that they responded that it is "not at all likely" that they would take the twelve political actions, is 0.46. For someone at the high extreme of the distribution, meaning that they are "sure" that they would take all twelve actions, the

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[2]Because we increased the sample size of the study after piloting (but before pre-registration), we did not have enough wristbands for the full sample. The analyses with the wristband outcome presented throughout this paper are from a restricted sample of only people surveyed on days where we offered real wristbands. On days after the wristbands had run out, we still collected a hypothetical measure of whether they would prefer a wristband with a political message or a plain wristband.

predicted probability of taking the wristband is 0.98. 95% confidence intervals are displayed around the predicted probabilities.

Qualitatively, participants who did not take the political wristband reported that they were afraid to wear it, and there is no reason to expect that there are any financial or aesthetic reasons that participants would choose the plain wristband, or that if there are that these would be affected by fear.

C   Text of the emotion inductions used

Table C.1: Reflection Treatments

	Assignment	
Control	General	Political
$N = 350$	$N = 175$	$N = 175$
1. What are the 2-3 activities that you do to relax? Please tell me 2-3 sentences about each thing that you like to do. You might feel relaxed when you are watching tv, attending soccer matches, reading newspapers and magazines, fishing.	1. What are the 2-3 things that make you most afraid? Please tell me in 2-3 sentences about each thing that makes you afraid. You might be afraid of witchcraft, being alone on a dark street, being in a traffic accident, diseases like HIV or cholera, or dangerous animals like snakes or lions.	1. What are the 2-3 things that make you most afraid about politics and elections? Please tell me in 2-3 sentences about each thing that makes you afraid. You might be afraid of the ZANU-PF militia, being abducted, losing your home, or being raped.
2. Now we'd like you to describe in more detail the (another) way you most like to relax. Begin by telling me a description of your favorite relaxing activity. Examples of things you might describe include going to church, spending time with certain friends, watching football, eating a meal with your family, etc. **X2**	2. Now we'd like you to describe in more detail the one situation that makes you most afraid. This could be something you are presently experiencing or something from the past. Please tell me as if you?re trying to make me afraid as well. What is it like to be in this situation? Why is it so scary? **X2**	2. Now we'd like you to describe in more detail the one situation that makes you most afraid about politics and elections. This could be something you are presently experiencing or something from the past. Please tell me as if you?re trying to make me afraid as well. What is it like to be in this situation? Why is it so scary? **X2**

Enumerators were given a list of probes to use to follow up on the response, including "What makes you feel most relaxed / afraid?", "Why does it make you feel so relaxed / afraid?", and "What does it feel like to be relaxed / afraid?"

D   Ethical considerations

Given the sensitivity of the topic and context of this study, ethical questions concerning the participants and study team were first order. Although field research, including experiments, on sensitive topics in authoritarian (Guan and Green, 2006; Tsai, 2007) or violence-affected contexts (Wood, 2003; Humphreys and Weinstein, 2007; Lyall, Blair and Imai, 2013) has exploded in recent years, there remains a lack of consensus about how to apply the principles for protection of human subjects outlined in the Belmont Report in such settings, or whether additional ethical principles should be invoked. In addition, the ethics of field research in areas affected by conflict is highly context-dependent and requires researcher judgment, so simply adhering to a set of standard practices is almost certainly insufficient (Wood, 2006).

D.1   Process of developing ethical practices

The first phase of this project was focused on 1) assessing the risks of doing research on political violence in Zimbabwe were and 2) identifying the relevant ethical principles.

D.1.1   Risk assessment

To assess the risks of doing research on political violence in Zimbabwe, I carried out a long period of informal consultations and then created more formal relationships with a senior professor and research organization. I began by consulting a wide network of international and domestic researchers and policymakers over 22 months from my first exploratory trip to Zimbabwe in 2013 to the start of fieldwork in 2015. I also worked as a consultant on a politically sensitive survey that was carried out by one of the leading Zimbabwean survey firms as part of a USAID-funded evaluation of a governance program. These efforts provided me with a network of current and former PhD students, research professionals, and policymakers who could inform me of their experiences running programs and research on sensitive subjects related to political violence, provide their assessments of the current risks, and advise me on my research plans.

These initial consultations and experiences led me to a second phase of risk assessments in which I set up more formal relationships with both an academic advisor who had no personal stake in the project, and a data collection organization. Eldred Masunungure, a senior professor at the University of Zimbabwe and the head of the Zimbabwean research firm that carries out the Afrobarometer survey in Zimbabwe, began providing feedback on a regular basis as I developed a specific research design. As a prominent Zimbabwean academic who has overseen numerous surveys on political beliefs and experiences and a frequent commentator on elite politics and political risk in Zimbabwe, his insights into what was feasible were extremely important. He also provided a

review of my research protocol, including data collection materials, to my university's Institutional Review Board. His experience and independence as a senior scholar who had nothing at stake in the project gave me confidence that his assessments of the risks were well-informed and presented without bias.

The second source of information on risks that I solicited was from the organization that I hired to implement the research. This was more difficult because the organization was an interested party in the research, and I worried that they might have incentives to under-report risks in order to avoid losing the contract. However, there is no other source of information on highly localized or rapidly shifting risks, and VfD's local knowledge was one of the reasons I chose to work with them. As described in Section D.2, I took a number of steps to try to make the research team feel that it was in their best interest to share accurate information about risks with me.

The third source of information on risk that I consulted were quantitative data sources on political violence, particularly the monthly reports of the Zimbabwe Peace Project. I used these reports to assess where violence was occurring and who was targeted during the period preceding the fieldwork. These reports and the existence of an easily accessible independent media in Zimbabwe that covers political violence made it possible to cross-check the information that I was getting on the risk of violence from VfD and my local advisors with a totally independent and contemporary source.

D.1.2 Identifying appropriate ethical principles

A second step in this research was identifying the appropriate ethical principles to follow, and reviews to conduct. I took the position that the review of my university's Institutional Review Board (IRB) was necessary but not sufficient, in part because the IRB review did not explicitly consider the safety of the research staff. There is no Zimbabwean research ethics review board that governs social science research.

My university's IRB process provided an assessment of whether the research met the Belmont Report principles of beneficence, respect for persons, and justice. The protocol was first approved in April 2015, and modifications were approved in May, June, and September 2015 as the methodology was finalized.

I also sought the advice of local researchers on any additional ethical considerations specific to the Zimbabwean context. This was partly informed by an IRB requirement that I submit a "cultural appropriateness letter" from a Zimbabwe expert. Dr. Masunungure reviewed my IRB protocol and submitted a letter stating that the research met local ethical standards and did not violate local practices or customs. In practice, the local customs that were most relevant were standards around asking Zimbabweans about political beliefs and past trauma. Dr. Masunungure's feedback, the adoption of questions from surveys that had already been successfully fielded in Zimbabwe,

consultation with the survey team, and piloting helped ensure that the survey did not violate any local norms.

Finally, I found that the IRB framework provided little guidance on how to ethically engage with a local research team. A small methods literature around research in violence-affected or authoritarian contexts has noted the ethical imperative to consider the impact of research on local partners (Paluck, 2009; Lü, 2016). I ultimately ended up applying a similar standard of beneficence and respect for persons in my dealings with the surveyors, although this was not explicitly required by my IRB.

D.2 Processes for protecting participants

This section describes protocols developed to minimize the risks of re-traumatization and retribution, as well as general practices taken to enable informed consent and ensure that the research team followed safety procedures.

The first risk to participants that I identified and worked to minimize and monitor was the risk of re-traumatization. Because the study asked respondents to answer questions about political violence, and in some cases to describe political or non-political events that made them afraid, there was a risk that they could be re-traumatized during the interview. For the closed questions, I tried to minimize the risk of re-traumatization by using a shortened and modified version of the Harvard Trauma Questionnaire that had been used in surveys with Zimbabweans, including in a national poll (ActionAid International, 2005; ZTVP, 2008; Bratton, 2011). This section asked whether the respondent had experienced or heard about seven types of violence using yes or no questions.

The open-ended questions that made up the fear induction had not been used before in Zimbabwe. However, I assessed that they were unlikely to pose a significant risk of re-traumatization for several reasons. First, this type of emotion induction had been used with other violence-affected populations in Colombia and Afghanistan without adverse effects (Callen et al., 2014; Bogliacino et al., 2017), and in numerous studies with student populations (Westermann et al., 1996; Lench, Flores and Bench, 2011; Myers and Tingley, 2016). This suggested that it could be safely used in a wide range of populations, including some similar to the study participants in terms of past trauma.

Second, asking individuals to describe an emotional stimulus in detail when prompted and encouraged by an interviewer is actually a well-established form of therapy for patients with anxiety or post-traumatic stress disorder (PTSD). The protocol in what some practitioners call "imaginal" exposure therapy are remarkably similar to the Affective Emotional Memory Task (AEMT) that I used in this study. One exposure therapy protocol asks participants to "relive, in their imagination, the traumatic experiences, describing it aloud 'as if it were happening now'" (Rothbaum and Schwartz, 2002, 63). Therapists also commonly probe for details of the experience. A 2002

review article on exposure therapy for PTSD concludes that "in the last 15-20 years, exposure has been applied and adapted for treatment of PTSD. In fact, exposure therapy has more empirical evidence for its efficacy than any other treatment developed for the treatment of trauma-related symptoms" (Rothbaum and Schwartz, 2002, 61). According to the 2016 Encyclopedia of Mental Health, "exposure-based treatments are the current gold-standard treatment for anxiety disorders" (Steinman, Wootton and Tolin, 2016, 186). The purpose of the AEMT in this study was not to help participants recover from trauma, and the literature on exposure therapy does not suggest that a short, one-off session would have any positive effects. However, the fact that reliving emotional memories is part of a well-documented therapeutic protocol for patients with anxiety or PTSD also suggested to me that it could be used with this participant population in Zimbabwe without an unjustifiable risk of re-traumatization.

Before using the AEMT on a large participant population, I also took steps to assess whether these findings from the general literatures on measuring trauma, inducing emotions, and exposure therapy applied in Zimbabwe. I got feedback on the emotion induction protocol from Dr. Masunungure and from the members of the survey team during a participatory training. I also ran several pilots with smaller groups of participants in which the surveyors were instructed to assess the risk of re-traumatization. These consultations and pilots did not expose any signs that talking about a frightening past event was more traumatizing in Zimbabwe than in other contexts. The survey team did however provide the feedback during the pilots that the initial debriefing process that I had outlined was not sufficiently strong. They were concerned that some participants might leave the study feeling slightly more disempowered and pessimistic. Based on this feedback, I worked with the surveyors to develop a more prolonged and positive debriefing at the end of the interview, as described below.

Although this process convinced me that the risk of re-traumatization or other significant negative psychological effects was small, I took steps to monitor and mitigate it. First, I worked with the survey team to develop a protocol to monitor re-traumatization during and after the interview that was culturally specific. The interviewers watched participants for crying, an unusual lack of affect, jumpiness, or irritability after the emotion induction. At mild levels of these behaviors the surveyors were instructed to pause the interview and ask the participant if she wanted to continue. At more severe levels they were instructed to stop the interview. In general, the surveyors were instructed to continue the emotion induction discussion until they believed the participant felt a mild state of fear. During training we acted out scenarios in which participants (played by myself and the surveyors) exhibited these symptoms and discussed at what point the surveyor should pause or stop the interview. These practices were reinforced during the field training and piloting, when surveyors would discuss with each other how participants had reacted during the emotion inductions. Starting in the pilots, at the end of each survey the surveyor answered a final question asking whether the

participant had been re-traumatized during the interview. Putting this question on the survey was a more structured way of ensuring that the surveyors didn't forget to consider the emotional state of the participant, and enabled me and the survey manager to monitor in real time whether participants were being re-traumatized. Finally, around one week after the survey team had finished interviews in an area, they asked the community organizer who had helped them mobilize people in each community whether they had gotten any reports of re-traumatization from the people who had participated.

Second, we set up a number of practices to mitigate the risk of re-traumatization, and general negative affect after the fear induction. As mentioned above, the first step was to pause and then stop the interview for participants who showed signs of emotional distress. I did not track how frequently the surveyors paused the interview to let a participant calm down. However, the interview did not have to be stopped due to distress for any participants. The post-interview debrief was the second way that I tried to mitigate the potential negative effects of the interview. In the debrief, which was largely designed by the surveyors themselves, the surveyors talked casually with respondents about their experiences and the general situation in Zimbabwe. In most of these debriefs they discussed how the respondent had shown herself to be capable of coping with a difficult situation, and that the respondent was not alone. Although I do not think that the debrief techniques was informed by exposure therapy, emphasizing coping ability is part of many exposure therapy protocols and is in line with the theory that a belief in one's self-efficacy has a positive effect on actual ability. Emphasizing "power in numbers" was a technique that VfD developed during its own programs as a way of empowering communities to resist election violence. Finally, the survey manager had a plan to refer people who were re-traumatized as a result of the interview for counseling services with a Harare-based provider of psychosocial support to survivors of political violence. The financial cost of the referrals (transportation and lodging in Harare for rural participants) was to be covered by the researcher and not the research firm to reduce the risk that they would under-report trauma to avoid financial costs. Referral to counseling was ultimately not judged to be necessary for any of the participants.

In addition to these risk-specific procedures, I also used more general management practices to ensure that the safety procotols were followed. Through my interactions with the team and their manager, I tried to create a strong safety-first culture. First, I stressed to the team and their manager that we would modify the research rather than canceling the contract if something was too risky. I also demonstrated this to them during the preparatory period as we developed protocols and data collection tools together by cutting questions and modifying procedures to make them less risky. Second, I tried to create an open and risk-averse culture with lots of opportunities for communication. During piloting and surveying, we debriefed at the end of each day on risk-related information, among other things. These sessions allowed me to concretely set expectations about

the level of risk that I wanted them to take on, and provided a forum for continual improvement of safety practices. To give one example, at one point during piloting one of the surveyors realized that he was interviewing a supporter of the ruling party. The protocol we developed for this scenario was that the surveyor would skip the sensitive questions on the survey, but the surveyor in this case doubted that the interviewee was truly a ruling party supporter and so continued on with the full survey. I sensed that he also wanted to show that he was committed to the research by taking a small risk. I discussed the event with the whole team, asking them to reflect on other possible outcomes to reinforce the idea that this behavior was risky even if it had gone well in this case. We also discussed why this risk was not necessary from a research perspective given the focus of the project. In this case and others, I tried to use open discussion with the surveyors to set a strong "safety-first" culture in which they unequivocally saw that it was in their interest to minimize risks to themselves and the respondents.

D.3   Processes for protecting surveyors

The surveyors faced higher risks than survey participants, and I tried to minimize these while maximizing potential benefits and providing surveyors with the information that they needed to make a decision of whether or not to participate, in the entire project and in specific steps. Ultimately, I found it harder to balance the principles of beneficence and respect for persons with the survey team than with the participants. Although the surveyors were paid for their time and received no "hazard pay" or other compensation for risk, the scarcity of paid work in Zimbabwe in 2015 raised concerns that almost any job offer was hard to refuse. One way that I dealt with this conflict was by stressing to the surveyors that their jobs on the survey were secure and that they should provide input into the level of sensitivity of the survey questions so that they would face a reasonably low personal risk while doing them. I believe that by showing them that I was happy to modify questions based on their feedback on issues ranging from comprehension to safety made this credible.

I also determined that it was important to "micro-manage" the research in this context. This involved recruiting a relatively small team, forming individual relationships and channels of communication with the surveyors, and communicating frequently during data collection. Although the team had a local management structure, I wanted to make sure that communication did not have to pass through their manager. WhatsApp was a useful tool as it enabled me to communicate with all members of the team even when I was outside of Zimbabwe. I believe that forming personal relationships with the surveyors over lunches and more casual interactions during training, which was possible since the team included just eight people, also helped set up direct communication channels.

Finally, I sought to minimize risks to the surveyors after data collection ended. Although to

my knowledge this has not happened in the past, I was concerned that at some point the government might decide to crack down on the local employees of firms involved in the collection of sensitive data. I therefore encoded the surveyors' names in the replication dataset, and have avoided thanking them by name or including pictures of them in any write-ups or presentations of the results. I also checked in with them on subsequent visits to Zimbabwe and via WhatsApp to monitor whether they might have faced any repercussions for their participation in the research. So far they have not.

E   Substantive mediation analysis

The theory that I propose in Section 2 specifies a chain of causal relationships: first, that fear increases pessimism and risk aversion, and second, that pessimism and risk aversion reduce dissent. Although the lab-in-the-field experiment does not allow a design-based test of this causal process, I conduct a mediation analysis using the methods developed by Imai, Keele and Yamamoto (2010); Imai et al. (2011) and Imai and Yamamoto (2013). In this section I discuss the identifying assumptions of this method and present the full results of the substantive mediation analysis discussed in Section 5.3. This analysis tests whether the changes in pessimism and risk aversion seem to be driving the observed changes in dissent.

Imai et al. (2011) provide a framework for estimating causal mediation effects with a single mediator. They use potential outcomes notation to describe the causal model. M_i and Y_i represent the observed value of the mediator and the outcome, respectively, for unit i. $M_i(t)$ represents the potential mediator values under treatment status $t = 0, 1$, and $Y_i(t, m)$ represents the potential outcome values under treatment status t and mediator value m. As a result, the causal mediation effect, or the causal effect of the treatment on the outcome caused by the change in the mediator induced by the treatment, is $\delta_i(t) \equiv Y_i(t, M_i(1)) - Y_i(t, M_i(0))$. The direct effect is $\zeta_i(t) \equiv Y_i(1, M_i(t)) - Y_i(0, M_i(t))$. Imai, Keele and Yamamoto (2010) show that the mediation and direct effects can be identified under the assumption of sequential ignorability.

Imai and Yamamoto (2013) expand the mediation framework to accommodate the existence of a second mediator $W_i(t)$ that is not assumed to be independent from $M_i(t)$. Under this framework, the sequential ignorability assumption is relaxed to allow M_i to be exogenous conditional on post-treatment confounders W_i. Formally, the sequential ignorability assumption is:

$$\{Y_i(t, m, w), M_i(t, w), W_i(t)\} \perp\!\!\!\perp T_i | X_i = x$$
$$\{Y_i(t, m, w), M_i(t, w)\} \perp\!\!\!\perp W_i | T_i = t, X_i = x$$
$$\{Y_i(t, m, w)\} \perp\!\!\!\perp W_i(t) = w, T_i = t, X_i = x$$

This assumption requires that the treatment, mediator of interest, and alternative mediators are conditionally exogenous. However, the mediator of interest M is only assumed to be exogenous after conditioning on the alternative mediators, treatment, and pretreatment confounders. In addition, in order to identify the mediation effect, we must either assume no interaction between the treatment and mediator, or set two parameters by assumption. The first parameter ρ_t is the correlation between the mediator $M_i(t)$ and the interaction effect of the mediator and the treatment. The second is the standard deviation of the coefficient for the treatment-mediator interaction, σ. Imai and Yamamoto

(2013) suggest presenting the results assuming that $\sigma = 0$ and assessing the sensitivity of the analysis to a range of values of ρ_t and σ.

Next, I test whether increases in pessimism and risk aversion mediate the decreases in the propensity to dissent. In this analysis, it is somewhat more plausible that some of the potential mediators are independent of each other given that my measurement strategy for risk aversion shuts down variation in risk perceptions. Nevertheless, given that risk aversion and pessimism are thought to be closely related (Weber and Milliman, 1997), and given the clear relationship between the perceived risk of repression and pessimism about others' actions, I continue to use the framework for multiple mediators rather than the framework that relies on unconfoundedness of the mediator on post-treatment factors. Therefore, in this analysis, in addition to conditioning on the same vector of pre-treatment characteristics, I condition on the alternative post-treatment psychological outcomes in order to estimate the mediation effect of each mechanism. For example, to estimate the ACME of pessimism about the risk of repression, I condition on the post-treatment measures of pessimism about others' participation and risk aversion. Table E.1 presents the results, showing that in general, there is support for the hypothesis that the cognitive changes induced by the treatments are mediating the changes in dissent, if we accept the assumption of conditional unconfoundedness. This table presents a more complete version of the results in Table 5.

Table 5 shows that all of the observed effects are mediated by changes in risk aversion or pessimism about the risk of repression or others' participation in dissent. The first set of results in Columns 1-2 show the ACME and ADE based on a pooled version of the treatment. It shows that the reductions in hypothetical measure of dissent, Propensity to Act, are mediated by changes in both the perceived risk of repression, the perceived likelihood that others will act, and risk aversion. These effects are mostly driven by the effects of the Political Fear treatment, but the 90% confidence intervals on the tests for whether the effects of the General Fear treatment on the hypothetical measure of dissent is mediated by risk aversion and pessimism both exclude zero. The effects on the wristband, however, are only mediated by changes in risk aversion. One explanation for this pattern might be that the wristband is such a low-risk action that there is little ambiguity about the risk of punishment.

Table E.1: Pessimism and risk aversion mediate the effects on dissent

M	Y	Fear (Pooled)		Political Fear		General Fear	
		ACME	ADE	ACME	ADE	ACME	ADE
		(1)	(2)	(3)	(4)	(5)	(6)
Perceived Repression Risk	Propensity to Act	-0.04**	-0.46**	-0.04	-0.53**	-0.01	-0.37**
		(-0.06, -0.01)	(-0.6, -0.33)	(-0.09, 0)	(-0.7, -0.36)	(-0.03, 0.02)	(-0.52, -0.23)
		[0.08]		[0.07]		[0.02]	
	Wristband	0.01	-0.13**	0	-0.14**	0.01	-0.11**
		(-0.01, 0.03)	(-0.21, -0.05)	(-0.01, 0.02)	(-0.23, -0.06)	(-0.01, 0.03)	(-0.21, 0)
		[-0.08]		[-0.03]		[-0.09]	
Risk Aversion	Propensity to Act	-0.04**	-0.46**	-0.04**	-0.55**	-0.02	-0.37**
		(-0.06, -0.01)	(-0.6, -0.32)	(-0.08, 0)	(-0.7, -0.4)	(-0.05, 0.01)	(-0.54, -0.2)
		[0.07]		[0.07]		[0.05]	
	Wristband	-0.02**	-0.14**	-0.03**	-0.15**	-0.01	-0.12**
		(-0.04, -0.01)	(-0.22, -0.07)	(-0.05, 0)	(-0.25, -0.06)	(-0.02, 0.01)	(-0.21, -0.03)
		[0.13]		[0.14]		[0.06]	
Others' Participation	Propensity to Act	-0.13**	-0.37**	-0.15**	-0.46**	-0.11**	-0.29**
		(-0.2, -0.06)	(-0.49, -0.24)	(-0.25, -0.05)	(-0.6, -0.32)	(-0.22, -0.01)	(-0.41, -0.17)
		[0.26]		[0.25]		[0.29]	
	Wristband	-0.03**	-0.1**	-0.03**	-0.11**	-0.02	-0.08
		(-0.05, 0)	(-0.19, 0)	(-0.06, 0)	(-0.2, -0.01)	(-0.04, 0)	(-0.16, 0)
		[0.21]		[0.24]		[0.21]	

95% confidence intervals in parentheses.

Estimated proportion of the effect mediated in square brackets.

The first two columns present the results from a mediation analysis where the treatment variable is a pooled version indicating that the participant received either fear treatment. The middle columns present the results of a mediation analysis on the General Fear treatment compared to control, and the last two columns present the same for the Political Fear treatment. The ACME (Columns 1, 3, and 5) represents the estimated Average Causal Mediation Effect, while the ADE (Columns 2, 4, and 6) represents the Average Direct Effect.

F Manipulation check

F.1 Robustness to a binary coding of emotions

First, I test whether the manipulation check is robust to a binary coding of the emotion outcomes. I run this test in large part because respondents tended to bunch at the top and bottom of the emotion scales rather than using the entire four-point spectrum from "Not at all" to "Very". In Table F.1, the emotion outcomes are coded as either zero for "Not at all" or "A little bit" or one for "Somewhat" or "Very". The results of the manipulation check are similar with this binary coding.

Table F.1: The fear treatments increase a binary version of fear by more than a binary coding of other negative emotions

	Fear		Anger		Sadness	
	General Fear	Political Fear	General Fear	Political Fear	General Fear	Political Fear
	(1)	(2)	(3)	(4)	(5)	(6)
ATE[1]	0.507	0.544	0.243	0.332	0.225	0.308
SE[2]	(0.035)	(0.032)	(0.046)	(0.043)	(0.045)	(0.043)
RI p-value[3]	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001
N	496	502	496	501	496	502

	Disgust		Surprise		Happiness	
	General Fear	Political Fear	General Fear	Political Fear	General Fear	Political Fear
	(7)	(8)	(9)	(10)	(11)	(12)
ATE[1]	0.268	0.35	0.046	0.098	-0.268	-0.455
SE[2]	(0.045)	(0.042)	(0.045)	(0.044)	(0.044)	(0.043)
RI p-value[3]	< 0.001	< 0.001	0.330	0.024	< 0.001	< 0.001
N	496	502	496	502	496	502

[1] The odd columns present the estimated Average Treatment Effects (ATEs) of the general fear treatment, and the even columns of the political fear treatment. All emotion outcomes are binary variables that take a value of one if the respondent reports being somewhat or very high on each emotion. Columns 1-2 present the results for the outcome of Fear, 3-4 for Anger, 5-6 for Sadness, 7-8 for Disgust, 9-10 for Surprise, and 11-12 for Happiness.
[2] Robust standard errors (SEs) from linear regression analysis.
[3] The p-values are based on a two-tailed test using randomization inference.

F.2 Mediation analysis

One challenge in interpreting the results of the experiment is that the fear induction not only increased levels of fear, but also increased other negative emotions and decreased happiness. This experimental design does not allow me to make a design-based inference about whether the

treatments are causing the measured changes in dissent and the psychological outcomes. However, the Imai and Yamamoto (2013) mediation method has been suggested as a way to isolate the portion of the outcome in an AEMT experiment that is mediated by the targeted emotion (Myers and Tingley, 2016; Albertson and Gadarian, 2016). Because the Imai and Yamamoto (2013) method rests on some stringent assumptions described in Appendix E, I prefer to present the identified effects of the treatment in my main analysis. However, as a robustness check in this section I also provide an analysis using this mediation framework.

In this analysis, fear is the mediator of interest M and anger, disgust, surprise, sadness, and happiness are a vector of alternative mechanisms W. In this case, rather than assuming that M is unconfounded by any post-treatment variables, I use the Imai and Yamamoto (2013) method to condition on other emotions that the treatment may have induced. In this case, it is plausible that other emotions would be related to the amount of fear induced as well as the outcomes of interest. In order to make W and M conditionally unconfounded, I also control for a number of pre-treatment variables X that might be related to emotions and the outcomes of interest, including gender, age, education, household assets, farm assets, the number of earners in the household, two subjective measures of economic insecurity, the number of rooms in the household, and a community fixed effect. Table F.2 presents the results of an analysis of whether fear mediates the relationship between the treatment and the five outcomes of interest, conditional on the other five post-treatment emotions. If we accept the assumptions of conditional unconfoundedness of the mediator and no interaction between the treatment and mediator, these results suggest that fear mediates the observed effects on the outcomes of interest.

Conditional on the pre-treatment characteristics X listed above and post-treatment levels of five other emotions W, I find that the Average Causal Mediation Effect (ACME) of the pooled fear treatment is substantively large and statistically significant for all of the outcomes of interest. The tests of whether fear mediates the relationship between the General Fear and Political Fear treatments on their own and the outcomes of interest are also significant at the 95% level in six out of ten cases. This analysis suggests that changes in fear explain between 52 and 76% of the variation in the substantive outcomes.

As a placebo, I use the same methodology to test whether the other five emotions that I measured mediate the observed relationships between the treatments and the outcomes of interest. Table F.3 presents the estimated proportion of the effect mediated for anger, sadness, surprise, disgust, and happiness. The AEMT treatment increased all of the negative emotions (although not by as much as it increased fear) and decreased happiness.

This analysis finds that there is little reason to believe that other emotions are mediating the relationship between the treatments and the five substantive outcomes. The ACME is statistically distinguishable from zero in the case of three of the 25 mediation tests using alternative emotions as

Table F.2: Fear mediates the effect of the treatment conditional on other emotions

	Fear (Pooled)		Political Fear		General Fear	
	ACME	ADE	ACME	ADE	ACME	ADE
	(1)	(2)	(3)	(4)	(5)	(6)
Propensity to Act	-0.48**	-0.16	-0.63**	-0.11	-0.36**	-0.18
	(-0.61, -0.34)	(-0.33, 0.01)	(-0.83, -0.43)	(-0.37, 0.15)	(-0.49, -0.23)	(-0.37, 0)
	[0.75]		[0.81]		[0.71]	
Wristband	-0.1**	-0.05	-0.12**	-0.07	-0.06	-0.06
	(-0.17, -0.03)	(-0.17, 0.06)	(-0.22, -0.02)	(-0.25, 0.11)	(-0.12, 0.01)	(-0.19, 0.08)
	[0.65]		[0.6]		[0.51]	
Perceived Repression Risk	0.19**	0.13	0.24**	0.17	0.09	0.04
	(0.04, 0.34)	(-0.06, 0.32)	(0.02, 0.46)	(-0.1, 0.43)	(-0.05, 0.23)	(-0.17, 0.26)
	[0.6]		[0.48]		[0.69]	
Others Actions	-0.19**	-0.17	-0.36**	-0.07	-0.12	-0.24
	(-0.33, -0.05)	(-0.37, 0.03)	(-0.57, -0.15)	(-0.38, 0.23)	(-0.26, 0.02)	(-0.49, 0.01)
	[0.52]		[0.83]		[0.43]	
Risk of Lottery	-0.21**	-0.07	-0.16	-0.14	-0.19**	0.08
	(-0.37, -0.05)	(-0.29, 0.15)	(-0.38, 0.07)	(-0.46, 0.17)	(-0.35, -0.04)	(-0.16, 0.32)
	[0.76]		[0.46]		[0.82]	

95% confidence intervals in parentheses.

Estimated proportion mediated in square brackets.

** 95% confidence intervals do not include zero.

The first two columns present the results from a mediation analysis where the treatment variable is a pooled version indicating that the participant received either fear treatment. The middle columns present the results of a mediation analysis on the General Fear treatment compared to control, and the last two columns present the same for the Political Fear treatment. The ACME (Columns 1, 3, and 5) represents the estimated Average Causal Mediation Effect, while the ADE (Columns 2, 4, and 6) represents the Average Direct Effect.

Table F.3: Other negative emotions generally do not mediate the relationships between the treatments and outcomes

Outcome	Mediator					
	Fear	Anger	Sadness	Disgust	Surprise	Happiness (Reversed)
	(1)	(2)	(3)	(4)	(5)	(6)
Propensity to Act	0.75**	-0.13	0.15**	-0.07	0.03	-0.14
Wristband	0.65**	-0.18	0.27	-0.04	0.03	-0.94**
Perceived Repression Risk	0.6**	-0.22	0.15	0.21	0.05	0.27
Others' Actions	0.52**	-0.12	0.21	-0.33**	0.09	0.14
Risk of Lottery	0.76**	0.07	0.05	-0.12	0.02	-0.42

** indicates 95% confidence intervals for the ACME do not include zero.
The estimated proportion of the effect mediated is presented in the table.
Column 1 presents the estimated proportion of the effect of the pooled treatment on the outcome of interest mediated by fear (as in Table F.2. Column 2 presents the analysis with anger as the mediator, Column 3 with anger, Column 4 with disgust, Column 5 with surprise, and Column 6 with a reverse-coded measure of happiness.

mediators. Interestingly, sadness is found to be a statistically significant mediator of the relationship between the treatment and one of the measures of political participation, explaining an estimated 15% of the total relationship. Sadness is not found to mediate the effect of the treatment on the psychological outcomes of pessimism and risk aversion or the behavioral measure of dissent. This result may be in line with other research suggesting that depression may decrease political participation in violent environments (Shaver, 2017). On the other hand, the estimated proportion of the effect on dissent mediated by anger, disgust, and happiness is often negative, implying that these mediators are working in the opposite direction of the main effect. This is also in line with theory suggesting that anger and happiness should increase political participation. In general, aside from the effect via sadness, which is found to explain a much smaller proportion of the total effect on dissent than fear, there are no statistically significant effects in the direction of the total effect of the treatment.

Overall, this analysis suggests that there is little evidence that other emotions induced by the treatments might explain the observed treatment effects. The ACMEs of the other potential mediators are small in magnitude if they are in the same direction as the overall effect, and 22 out of 25 of the coefficients are statistically insignificant. By contrast, the ACMEs of fear are substantively large and consistently statistically significant.

G   Disaggregated results

G.1   The effect of fear on propensity to dissent

In this section I analyze the results of the fear treatments on each sub-component of the hypothetical measure of dissent. The index presented in Section 5.1 was based on the sum of the standardized measures of twelve questions that asked about six different acts of dissent in two periods (now and shortly before the next election).

- Shirt - wearing an opposition t-shirt outside
- Joke - sharing a funny joke about the president
- Rally - attending a public rally by an opposition party near your community
- Reveal - telling a war veteran [a group known as perpetrators of violence] that you do not support ZANU-PF
- Refuse - refusing to attend a ZANU-PF pungwe [rally] when requested by a community leader
- Testify - testifying publicly in the trial of someone who had killed for ZANU-PF

In Figure G.1 the six acts of dissent are analyzed individually, and in Figure G.2 sub-indices are created for each period to test whether the effects are different for hypothetical actions that the person would take now (during a period of low repression risk) vs. those that they would take around the time of the next election when the risk of repression is higher. Treatment effects are calculated using linear regression with no controls and confidence intervals are based on robust standard errors.

Figure G.1 shows that the effects of the fear treatment are consistent across sub-indices. There is some evidence that they may be larger for higher-risk actions such as sharing a joke about the president or testifying against a perpetrator of violence, and smaller for lower-risk actions such as wearing an opposition t-shirt, but these differences are not statistically significant and substantively small.

Similarly, Figure G.2 breaks out the effect by time period. Again, treatment effects are calculated using linear regression with robust standard errors.

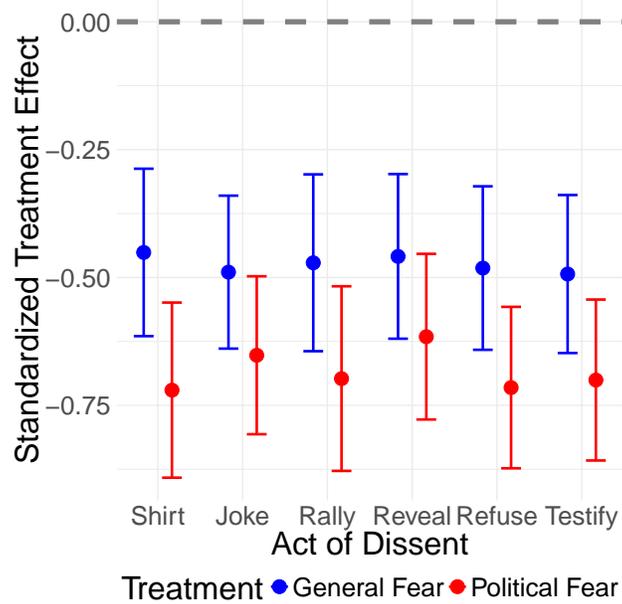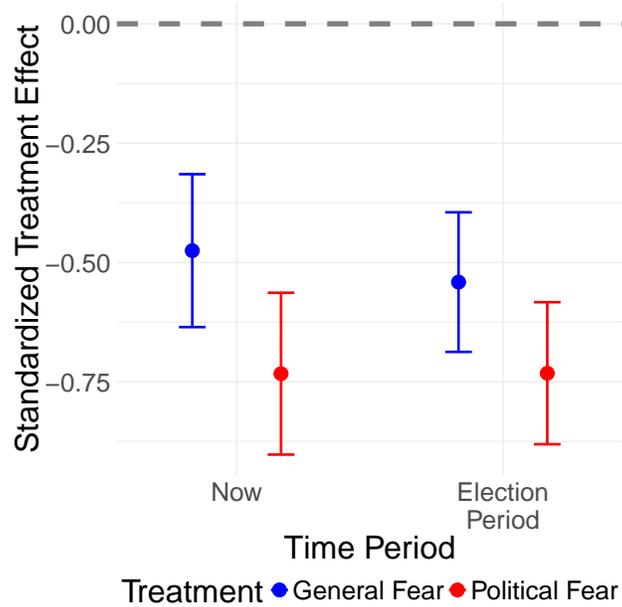Figure G.1: The effect of fear on propensity to dissent by act of dissent

Figure G.2: The effect of fear on propensity to dissent by time period

G.2 The effect of fear on the perceived cost of dissent and risk attitudes

This section presents the results for the psychological outcomes using data disaggregated by individual measure and by time period, when relevant. Figure G.3 presents the analysis of the effect of the treatments on beliefs about the proportion of other opposition supporters in the respondent's community who would take each of the six acts of dissent. Figure G.4 presents the results of this analysis by time period.

Figure G.3: The effect of fear on beliefs about others' propensity to dissent by act of dissent
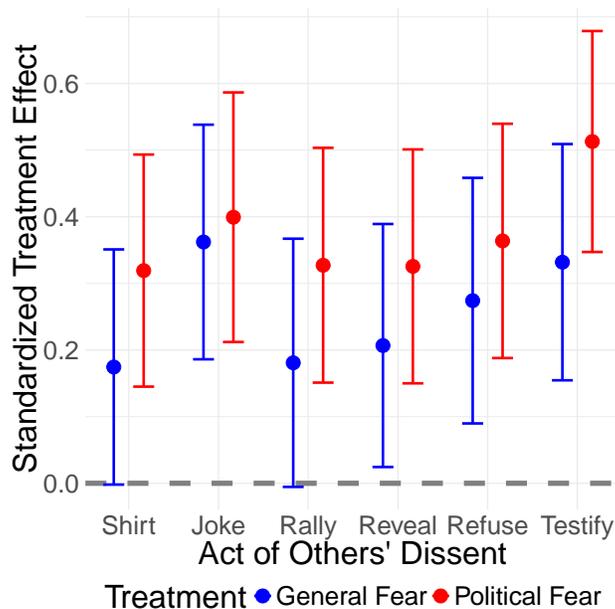
Figure G.4: The effect of fear on beliefs about others' propensity to dissent by time period
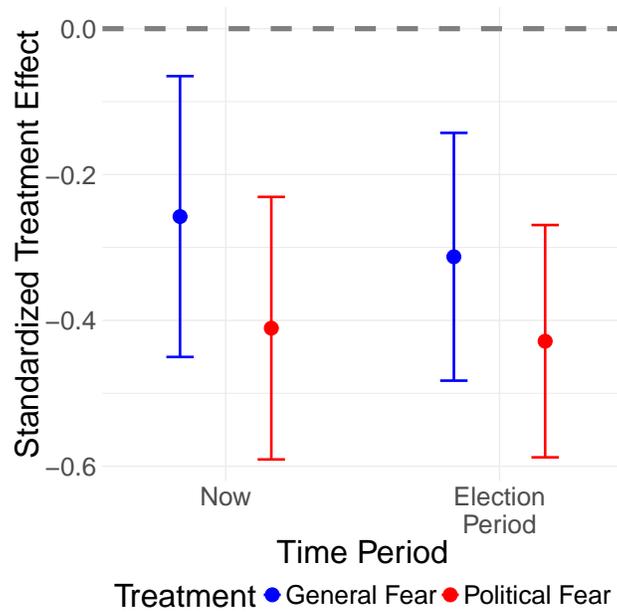
Figure G.5 presents the analysis of the effect of the treatments on beliefs about the probability that the respondent would face six acts of repression, averaged over two time periods. Figure G.6 presents the results of this analysis by time period, averaged over type of repression. The six acts of repression are:

- Threats
- Assault
- Abduction and torture
- Destruction of property
- Sexual abuse (you or someone in your family)
- Murder

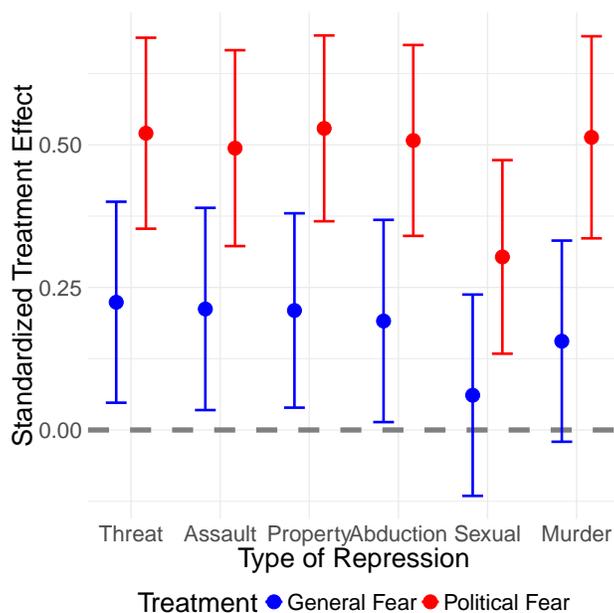Figure G.5: The effect of fear on beliefs about repression risk by act of repression

Figure G.6: The effect of fear on beliefs about others' propensity to dissent by time period
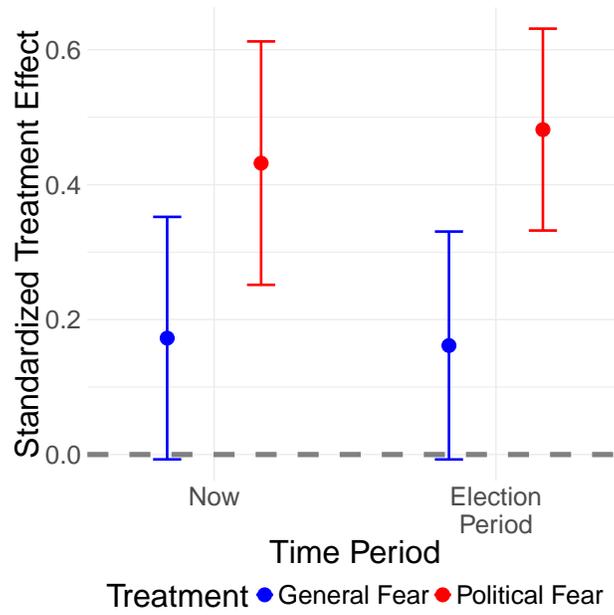
Table G.1: The effect of fear on risk attitudes

| | *Dependent variable:* | | | | | |
| | Risk Acceptance | | Uncertainty Acceptance | | Loss Acceptance | |
	(1)	(2)	(3)	(4)	(5)	(6)
Fear Treatment - General	−0.34***	−0.34***	−0.09	−0.09	0.15	0.15
	(0.11)	(0.11)	(0.12)	(0.12)	(0.16)	(0.16)
Fear Treatment - Political	−0.45***	−0.44***	0.04	0.04	0.19	0.18
	(0.11)	(0.11)	(0.12)	(0.12)	(0.16)	(0.16)
Female	−0.09	−0.09	0.12	0.10	0.35**	0.35**
	(0.09)	(0.10)	(0.10)	(0.10)	(0.14)	(0.14)
Age		0.02		−0.02		0.004
		(0.02)		(0.02)		(0.03)
Age^2		−0.0002		0.0002		−0.0000
		(0.0002)		(0.0002)		(0.0003)
Education		−0.04		−0.03		0.07
		(0.07)		(0.07)		(0.10)
Assets Index		0.13**		−0.06		−0.14
		(0.06)		(0.07)		(0.09)
Constant	3.13***	2.52***	0.03	0.47	−0.10	−0.12
	(0.21)	(0.45)	(0.23)	(0.48)	(0.31)	(0.66)
Community FE	✓	✓	✓	✓	✓	✓
Observations	669	666	669	666	669	666
R^2	0.05	0.07	0.01	0.02	0.02	0.02

Standard errors in parentheses.
*p<0.1; **p<0.05; ***p<0.01
The dependent variable in Columns 1-2 is a standardized measure of the spread of the lottery that the participant chose to play in a draw with 50-50 chances of winning. In Columns 3-4, it is the difference between the participants' choice of lottery in the standard draw and her choice in the draw in which 60% of the balls were of an unknown color. In Columns 5-6, the outcome is the difference between the participants' choice in the standard draw and on a draw where the choices were framed as losses rather than gains. The independent variables include dummies for the two fear treatments, a dummy for being a female, the age of the respondent at the time of the experiment. Education is a five-point measure of the highest level of education completed by the participant. The assets index is the first principal component of a list of assets based on the Demographic and Health Survey's assets module from Zimbabwe. Finally, I include fixed effects for each of the constituencies where the experiment was conducted. Models are estimated using ordinary least squares (OLS).

Figure G.7 provides the substantive interpretation of the reductions in the hypothetical measures of dissent for the current (non-electoral) period. This figure shows the rest of the measures presented in Figure 2, which displays the reductions in hypothetical dissent during an electoral period. The reductions are quite similar in size and consistent across the two time periods.

Figure G.7: The fear treatments cause substantively large increases in the proportion of respondents who are very likely or sure to dissent now
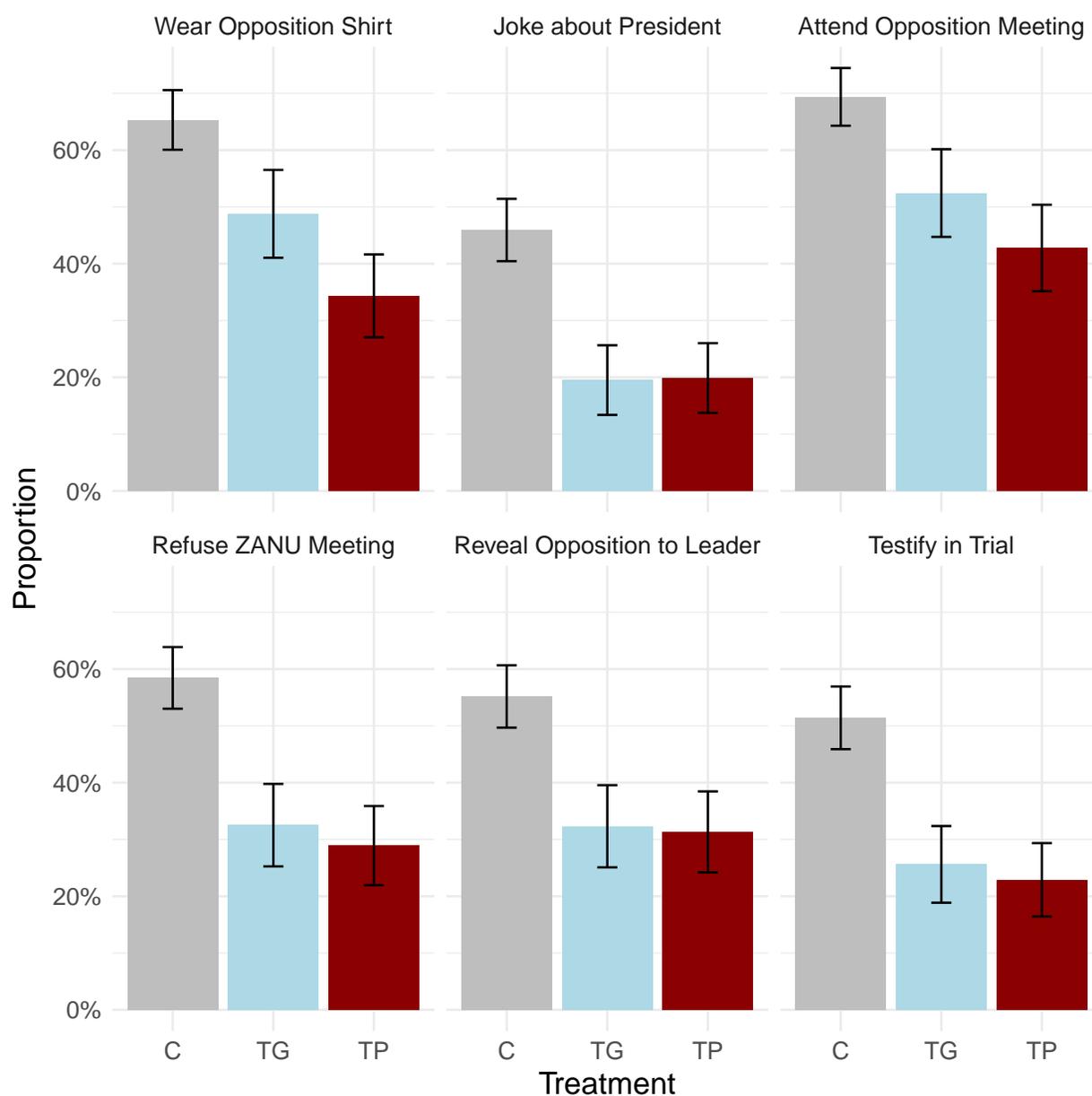
H   Sensitivity to enumerator effects

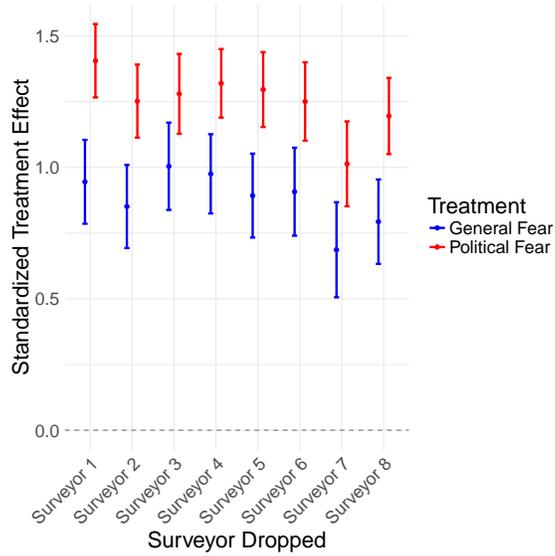Figure H.1: Treatment effects on fear leaving out each surveyor
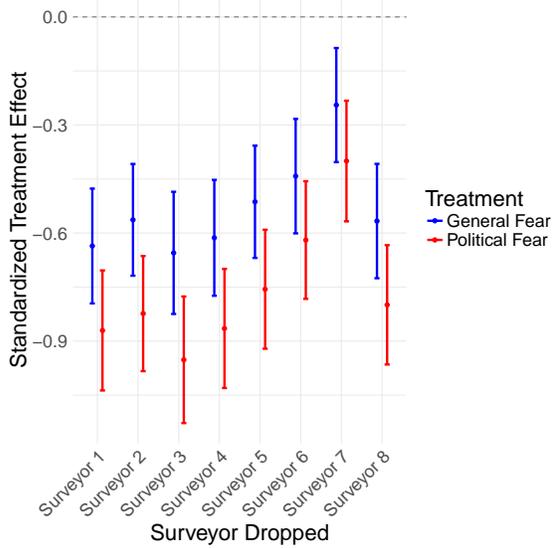

Figure H.2: Treatment effects on dissent leaving out each surveyor
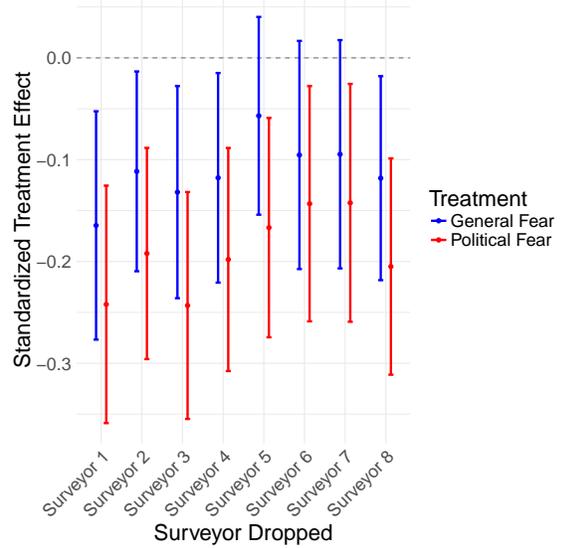
(a) Propensity to Act

(b) Wristband

Figure H.3: Treatment effects on beliefs and risk attitudes leaving out each surveyor

(a) Proportion of Others Acting   (b) Perceived Repression Risk   (c) Risk Seeking
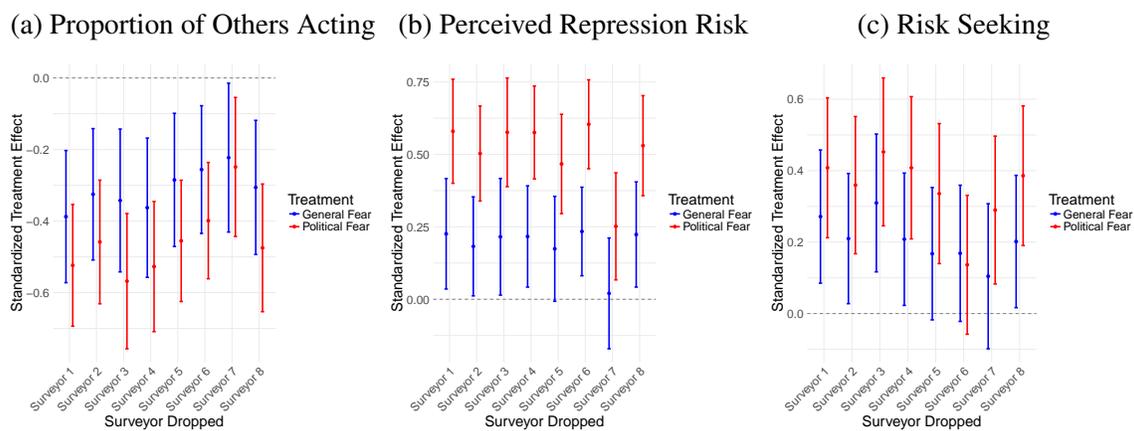
I Generalizability

I.1 Comparison of sample to nationally representative data

One concern with the experimental results presented in Section 5 is that they were run on a non-random sample of Zimbabwean opposition supporters. While this is quite standard for a lab experiment, it raises questions about the generalizability of the results to other populations. In particular, given the very high levels of past exposure to political violence observed in my sample, we may be worried that if other Zimbabweans are less exposed to political violence and past exposure moderates the effects, then the effects observed in this sample may overstate the effect size in the population. In this section, I compare the experiment participants to the pool of respondents on two nationally representative samples of Zimbabweans. I find that the demographic breakdown as well as the past exposure to political violence observed in my sample is quite similar to that observed in nationally representative samples in Zimbabwe. The only measures where I find substantively large differences between the nationally representative sample and my participant pool are measures of subjective poverty.

For a demographic comparison, I use the most recent round of the Afrobarometer survey for which the data is publicly available. I exclude people from the comparison group who reveal that they support the regime on the survey because this was an eligibility criteria for my study. On demographic measures, the experimental sample is quite similar to the opposition supporters surveyed in the fifth round of the Afrobarometer survey in 2011. The median age in my sample is 35, while the Afrobarometer opposition supporters' median age is 34. The median level of education in both samples is a high school degree, with means of 1.72 in my sample and 1.55 in the Afrobarometer.[3] My sample is 52% female, while the Afrobarometer opposition supporters are 51% female. However, my sample seems to be poorer than the Afrobarometer opposition supporters. In my sample, the median respondent reports that they "often" go without enough food and without a cash income, while the median Afrobarometer opposition supporter reports that she "rarely" goes without enough food and "sometimes" goes without a cash income. The mean values of these variables in my sample are 3.59 and 4.12 for food and a cash income on a five-point frequency scale, while in the Afrobarometer they are 2.13 and 3.15. This is not a function of the constituencies that we selected, which are quite similar to the national averages in the Afrobarometer on the subjective poverty measures. It is possibly because the potential to win up to $1.10 in the lottery was more attractive to the poorest residents, whereas the Afrobarometer did not offer any incentivized games.

Finally, I turn to another dataset to assess whether my sample is similar to the national average in terms of its past exposure to state repression. Past exposure to repression is not covered in the

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[3]0=no formal education, 1=finished primary school, 2=finished high school, 3=finished vocational school or university, and 4=post-graduate studies.

Afrobarometer surveys, but it was included in a 1,200 person, nationally representative survey conducted by the Mass Public Opinion Institute (MPOI) in 2009. In the MPOI 2009 survey, respondents were asked: "Thinking of the period since independence in 1980, please tell me if you personally/members of your family were ever affected in any of the following ways. Important: please refer only to events that were politically motivated." In the experiment survey, respondents were asked: "I'm going to read you a list of things that many people consider political violence. For each of these things, please tell me whether you have personally experienced it and whether you have heard about it happening here since the year 2000" and enumerators clarified (1) that "experience" referred to something that happened to you or to a member of your household, and (2) that respondents should report things that they consider to be political violence.

A few things must be kept in mind while comparing the observed victimization in the two populations. First, many things about the two samples are similar. Both samples are restricted to only opposition supporters.[4] Second, in both cases I analyze the reported incidence for either the respondent or someone in her family, although this was more explicit in the main text of the MPOI 2009 survey.

However, there are several differences that are important to keep in mind. First, the questions in the MPOI 2009 survey and on my survey measuring past exposure to repression ask about different specific items. When possible, I have combined them in this analysis to make the comparison as similar as possible. Second, the two surveys ask about different time periods: the MPOI 2009 survey asks respondents to report repression that has occurred between 1980 and 2009, while my survey asks about repression exposure between 2000 and 2015. This is unlikely to lead to large differences both because the two measures both include the period from 2000-2008 when most state repression occurred, and because recall bias and age might lead respondents to under-report violence between 1980 and 2000, when the median respondent was a child or adolescent. Finally, the MPOI survey stressed that reported experiences should be "politically motivated," while my survey only mentioned "political violence." However, given that "political violence" is frequently discussed in Zimbabwe as politically motivated abuse, it is unlikely that this changes the measure in important ways.

Table I.1 presents a comparison of the incidence of different types of political violence in the MPOI sample and in my experiment participant pool. Because this measure was asked post-treatment to avoid inducing negative emotions in the control group, I present the results only for the control group that did not go through a fear induction.

Despite the differences across these measures, they present broadly similar pictures of the overall level and distribution of victimization in Zimbabwe. Whereas in my sample 83% of the respondents or their family members have experienced verbal abuse, 70% of respondents or their

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[4]This eliminates just 11% of the sample in the 2009 survey.

Table I.1: Comparison of past exposure to political violence in the experiment participant pool and a nationally representative sample

Experiment Control Group		Bratton (2011)	
Verbal abuse or threats	83%	Intimidation, threat or harassment	70%
Withholding of benefits (food, goods, etc)	67%	Denial of food or starvation The closure of a business The loss of a job	43%
Torture	43%	Personal injury (including physical assault, sexual assault, or torture)	39%
Physical harm (beating, assault, etc)	40%		
Sexual abuse or rape	2%		
Destruction of property	41%	Theft of (or damage to) your personal property	44%
		Forced removal from your home or confiscation of land	
Kidnapping or abduction	21%	Arrest, kidnap or abduction	22%
Arbitrary detention or arrest	19%		
Murder	0%	Death	14%

family members have experienced verbal abuse in the MPOI sample. The measures of the incidence of arrest or abduction are very similar at 19-22% in both samples. One major difference between the surveys is that in the MPOI survey a huge number of people reported that a family member had been killed for political reasons since 1980.

Overall, this analysis suggests that my participants, who have been exposed to extremely high levels of political violence in the past, are not far from the average exposure in Zimbabwe. Assuming that groups tracking political violence in Zimbabwe are correct that the vast majority of politically-motivated violence is perpetrated by the ruling party, the MPOI survey underscores the widespread use of repression. It also suggests that we have little reason to believe that the effects of the treatment observed with this non-random participant population are unlikely to generalize to the rest of the population of Zimbabwe because the participants are quite similar on key measures to representative samples taken around the same time.

I.2   Estimates with re-weighted data

One consideration in the interpretation of these results is whether the experimental estimates from this sample can be generalized to the population of opposition supporters in Zimbabwe. To minimize the risk of harm to participants and surveyors, I did not randomly select Zimbabwean citizens for the study. To assess the extent to which the effects estimated here might depend on the characteristics of the participant population, I re-estimate them in a sample that is weighted on observable characteristics to match a general population of interest using a sample weighted to match the general population of opposition supporters on gender, age, education, and subjective measures of poverty in the fifth round of the Afrobarometer. All estimated treatment effects are

robust to this population-weighted sample.

Table I.2 re-estimates the effects of the general and political fear treatments with weights based on the distribution of age, gender, education, and two subjective poverty measures in the fifth round of the Afrobarometer survey. The results are very similar after re-weighting to make the sample similar to the national population of Zimbabwe on these observable demographic measures.

Table I.2: ATEs calculated using sampling weights based on demographic distribution in the Afrobarometer

	Dependent variable:				
	Hypothetical Dissent	Behavioral Dissent	Perceived Risk of Repression	Propensity of Others to Dissent	Risk Aversion
	(1)	(2)	(3)	(4)	(5)
General Fear	−0.536***	−0.100**	0.237***	−0.322***	0.183**
	(0.077)	(0.050)	(0.087)	(0.092)	(0.091)
Political Fear	−0.759***	−0.141***	0.563***	−0.475***	0.313***
	(0.080)	(0.053)	(0.083)	(0.085)	(0.095)
Constant	0.023	0.820***	−0.021	0.014	−0.115**
	(0.056)	(0.026)	(0.056)	(0.056)	(0.056)
Observations	650	439	649	652	669
R^2	0.132	0.021	0.062	0.048	0.018

*Note:* *p<0.1; **p<0.05; ***p<0.01

In addition to differences on demographic measures, the participants in my study are probably more ideologically aligned with the opposition and more likely to dissent than the typical opposition supporter because they were recruited on a voluntary basis by a pro-democracy organization. I also conduct a sensitivity analysis to assess the extent to which the results might change if the sample included fewer people who strongly identify with the opposition. The results are robust to cutting the number of core opposition supporters at least in half.

Figures I.1 and I.2 present the results of an assessment of the sensitivity of the results to the distribution of pro-opposition preferences in the sample. They plot ATEs and 95% confidence intervals from samples in which the proportion of people who identify as core supporters of the opposition is reduced from 60% to 0%, and the number of individuals in the sample who are only weakly identified with the opposition is increased proportionally.

The estimate of the treatments on the hypothetical measure of propensity to act gets smaller in magnitude but remains statistically significant as the number of weakly affiliated opposition supporters in the sample increases. Even if the sample includes no strong supporters of the opposition, the effect of the treatments on the hypothetical measure of dissent remains negative and statistically significant.

The estimate of the effect on the behavioral wristband measure is more sensitive to this

Figure I.1: Sensitivity of results on dissent to reducing the number of core opposition supporters in the sample

(a) Propensity to Act

(b) Wristband
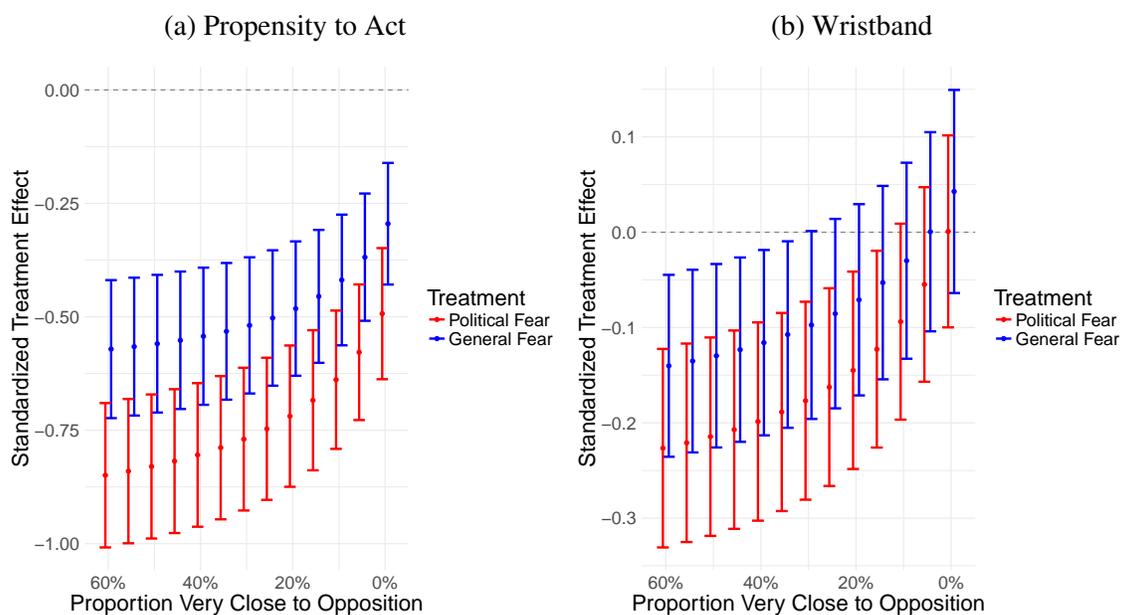
dimension of the sample composition. In this case, the simulations show that when 30% of the sample who report being strong supporters of the opposition are replaced with individuals who are only weakly affiliated, the effect of the general fear treatment is indistinguishable from zero. The effect of the political fear treatment is robust until just 10% of the sample is made up of strong opposition supporters.

Figure I.2: Sensitivity of results on pessimism and risk attitudes to reducing the number of core opposition supporters in the sample

(a) Perceived Repression Risk   (b) Proportion of Others Acting   (c) Risk Aversion
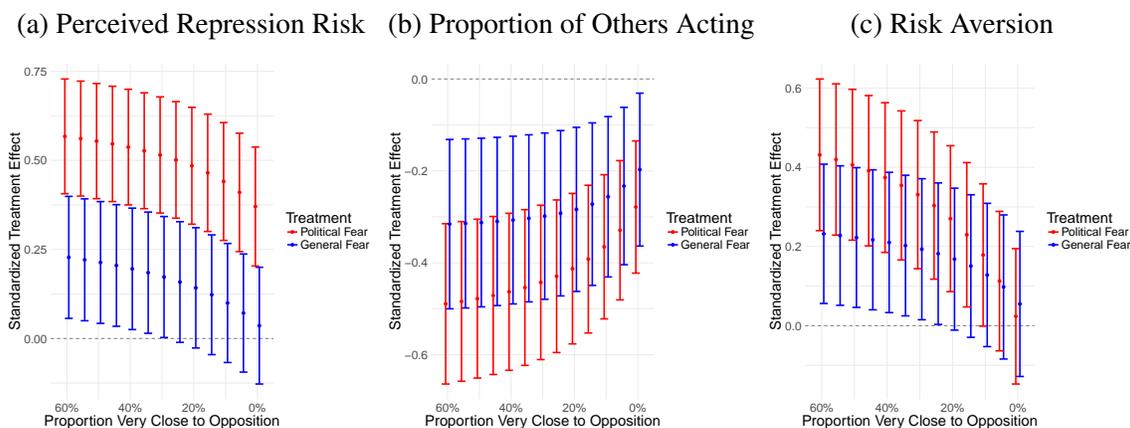
J Additional Outcomes: The effect of fear on pessimism in economic domains

Next, I test the prediction that fear, including fear of repression, should create pessimism that spills over into perceptions of economic domains. I have already shown that fear causes increases in risk aversion on a series of monetary lotteries, which has consequences for economic risk taking. To test whether fear also affects pessimism in economic domains, I measured a series of six perceived economic risks over two time periods and created an index of economic pessimism.[5] Table J.1 presents the average treatment effects calculated by treatment arm with 95% confidence intervals.

Table J.1: The fear treatments increase pessimism in economic domains

	Perceived Risk of Economic Failures	
	General Fear	Political Fear
	(1)	(2)
ATE[1]	0.091	0.181
SE[2]	(0.080)	(0.082)
RI p-value[3]	0.304	0.042
N	479	484
Sample	All	

[1] The first row presents the estimated Average Treatment Effects (ATEs) of the general and political fear treatments on perceptions of economic risks. ATEs are calculated based on assignment to treatment and weighted by inverse propensity scores by block.

[2] Robust standard errors (SEs) from linear regression analysis.

[3] The p-value is based on a two-tailed test using randomization inference.

In the case of economic risks, participants in both the general and political fear treatment arms are more pessimistic than the control participants. Only the difference between participants experiencing fear in a political context, however, is statistically significant. These results, coupled with the results showing that fear increases risk aversion on lotteries presented in Section 5.2, show that fear of repression affects beliefs and preferences in economic domains that could lead to

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[5]The measures included in the economic pessimism index include the risk that a business would not make a profit (measured as the probability of making profits and then inverted), the risk of job loss, the risk of losing or breaking an asset, the risk of your economic situation in general getting worse, the risk of theft, and the risk that a family breadwinner would have to stop working. For all six of these economic risks, I measured the perceived likelihood that it would happen in the next six months and in the next two years.

under-investment and lower economic outcomes.

Figures J.1 and J.2 break these results down by economic risk and time period.

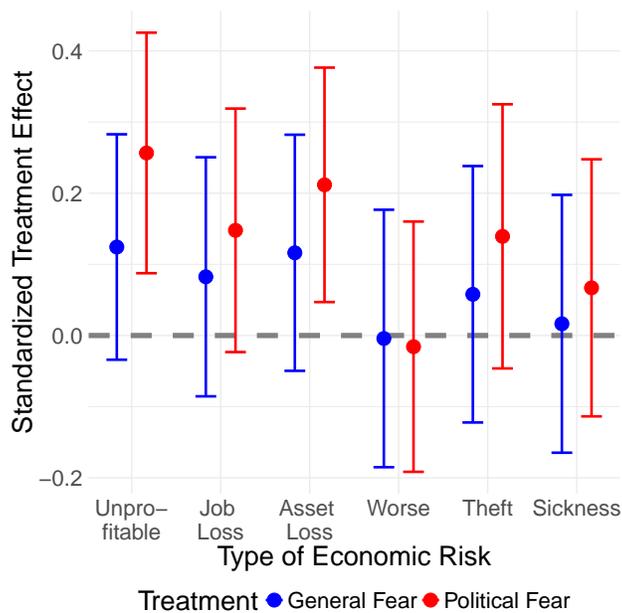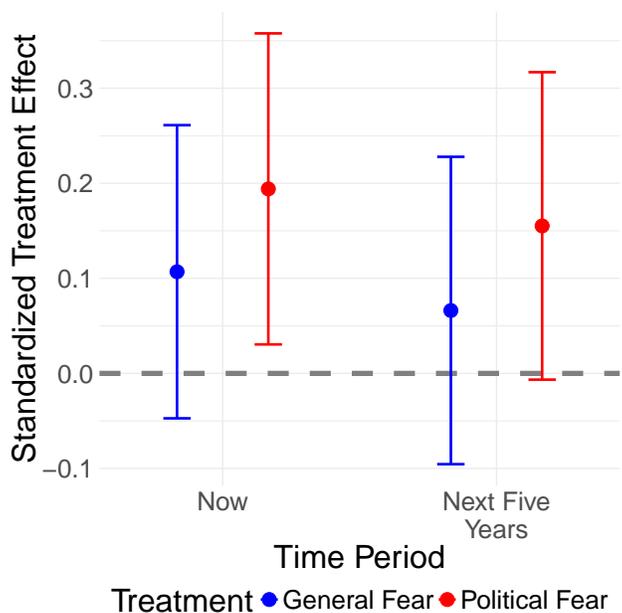Figure J.1: Fear and perceptions of economic risks

Figure J.2: Fear and perceptions of economic risks

K Results from the first experiment

The first experimental test of some of the propositions tested here was carried out in May 2015 with a similar participant population. While most of the experimental design was the same, there were several aspects that differed. First, we induced three different emotions: anger, fear, and a relaxed control. Second, all participants were asked about past exposure to political violence before they responded to the modules measuring outcomes. Third, rather than using different instructions for the political and apolitical versions of the emotion inductions, we used an encouragement design that increased the proportion of participants who reflected on political anger and fear by randomizing the order of two pre-treatment modules of questions. Specifically, some people were asked to reflect on what makes them angry or afraid immediately after answering a module of questions on past political violence, while for others that module came earlier in the study. Fourth, in the emotion inductions, in addition to describing something that makes them angry, afraid or relaxed, participants looked at a photograph of a person expressing that emotion.

These aspects of the design were changed in the second round. The anger treatment was dropped in order to increase power for the fear treatment. The questions about past political violence were moved to the end of the survey because of worries that they were priming everyone to think about traumatic negative events and therefore reducing the effect sizes. The encouragement into thinking about politics was dropped in favor of directly asking some people to think about politics and giving examples of situations involving political violence.

Table K.1: Effect of fear and anger stimuli on perceived probability of punishment and propensity to act

	Dependent variable:					
	Prob(Pun)			Prob(Act)		
	(1)	(2)	(3)	(4)	(5)	(6)
Fear	0.27**	0.25**	0.27*	−0.04	−0.05	−0.07
	(0.11)	(0.11)	(0.15)	(0.12)	(0.12)	(0.16)
Anger	0.16	0.15	0.15	−0.06	−0.07	−0.09
	(0.11)	(0.11)	(0.15)	(0.12)	(0.12)	(0.16)
Political (Z)			0.03			0.03
			(0.15)			(0.17)
Fear × Political (Z)			−0.04			0.05
			(0.22)			(0.24)
Anger × Political (Z)			−0.02			0.03
			(0.22)			(0.24)
Female	−0.08	−0.08	−0.08	−0.49***	−0.47***	−0.47***
	(0.09)	(0.09)	(0.09)	(0.10)	(0.10)	(0.10)
Age		0.03	0.03		0.04*	0.04*
		(0.02)	(0.02)		(0.02)	(0.02)
Age^2		−0.0003	−0.0003		−0.0005*	−0.0005*
		(0.0003)	(0.0003)		(0.0003)	(0.0003)
Education		0.08**	0.08**		−0.13***	−0.13***
		(0.04)	(0.04)		(0.04)	(0.04)
Assets		0.05	0.05		−0.04	−0.04
		(0.05)	(0.05)		(0.05)	(0.05)
Intercept	2.10***	1.13**	1.12**	3.28***	3.03***	3.03***
	(0.15)	(0.49)	(0.49)	(0.17)	(0.52)	(0.53)
Community FE	✓	✓	✓	✓	✓	✓
Observations	473	473	473	473	473	473
R^2	0.25	0.26	0.26	0.17	0.19	0.19
Specification	OLS	OLS	OLS	OLS	OLS	OLS

Standard errors in parentheses.
*p<0.1; **p<0.05; ***p<0.01

L   Pre-analysis plan

The following pre-analysis plan was posted to the EGAP research design registry on September 29, 2015 at `http://egap.org/registration/1353`.

L.1   Introduction

This study uses a lab-in-the-field experiment to assess how fear and anger influence the propensity of citizens to take risky political actions in a repressive environment. I experimentally induce fear using directed recall of emotional experiences. I predict that fear depresses political action and increases the perceived probabilities of punishment. Some respondents will be encouraged to experience emotions that are relevant to a political context, while others will experience apolitical emotions. Furthermore, I will test for the effects of fear of repression on pessimism and risk-taking in an unrelated economic sphere.

This project aims to provide a micro-empirical test of the potential psychological effects of political repression. The threat of political violence is often analyzed as an informational shock to the perceived probability of punishment. However, a large body of research on perceptions of risks suggest that affect has a large effect on how risks are perceived. This study applies this literature to understanding the effects of political violence, particularly violence intended to suppress opposition in an electoral autocracy.

This PAP follows up on an earlier round of this experiment (preregistered with EGAP on May 8, 2015). In the first round, the experimental design involved two different treatments – anger and fear – and one control group for a total sample of 500. I used images of expressive faces in addition to the reflection task to induce emotions, and measured assessments of political risks and self-reported propensity to take political actions as outcomes. As will be discussed below, in that experiment I found that fear significantly increased perceptions of the risk of repression, and decreased propensity to take action (although this second result was not statistically significant). Anger had similar effects. The current design builds on that first study and the results will ultimately be combined into two separate papers on the effects of fear and then anger on political and economic risks and outcomes.

L.2   Theoretical Implications

Opposition supporters in repressive political systems must make decisions about whether or not to express their preferences – through voting, public expression, or other actions – despite the potential that they might face negative consequences. Opposition supporters must therefore assess the probability and severity with which they may be punished for taking various actions. These

probabilities are typically assessed on the basis of little information, most of which is not credible or falsifiable. For instance, citizens may assess the likelihood of repression on the basis of past episodes of violence that are experienced first-hand or, more often, based on rumors. However, rational citizens should expect that coercive actors would lie to make violence seem more likely. There is considerable uncertainty around whether and how past violence occurs, making the probability of repression extremely difficult to estimate.

One factor that may influence how citizens perceive the risk of violence is their affective or emotional state. This should be particularly true given the uncertainty around signals of the risk of repression. A large body of research using a diversity of methods suggests that affect, particularly anger and fear, influences perceptions of risks. Indeed, although both anger and fear are negative emotions, evidence from brain imaging suggests that these emotions are associated with very different basic "approach" or "avoidance" response systems (Carver, 2004; Carver and Harmon-Jones, 2009). A number of early studies tested the extent to which individuals are likely to systematically over- or under-estimate the probability of certain risks and found that events that evoke a sense of "dread" are particularly likely to be overestimated (Fischhoff et al., 1978; Slovic, 1987). Others have induced specific emotions and asked participants to estimate a variety of risks including everyday risks like catching a cold to terrorist attacks and natural disasters (Johnson and Tversky, 1983; Lerner and Keltner, 2001; Lerner et al., 2003).

In addition to these findings that emotions influence assessments of political risks like terrorist attacks, there is also some evidence from the American context that emotions motivate participation in politics. A number of recent combined experimental and observational analyses find that anger has a larger effect on participation in electoral politics in the US than enthusiasm or fear (Valentino et al., 2011; Weber, 2013; Groenendyk and Banks, 2014). Earlier studies using less precise inductions of negative emotions or purely correlational evidence find that anxiety also has a mobilizing effect on American voters (Marcus, Neuman and MacKuen, 2000; Brader, 2005, 2006).

It is also important to note that the effect of emotions on information seeking appears to be quite different from more active participation in politics. Several studies have found that information-seeking is increased by anxiety and actually decreased by anger (Valentino et al., 2008). Brader (2005) finds weak but positive evidence that campaign ads that cue fear increase vigilance and information-seeking, although these effects are smaller than the effect of enthusiasm ads and do not translate into action. Political action in this study refers not to action to seek more information on issues or parties but rather action in support of an opposition party that an individual already has a preference for. In this study I focus on actions that require voters to take a stand for or against a party like voting, making public statements, or attending events rather than information-seeking.

This study builds on this political psychology literature from the American context to argue that fear decreases participation in politics in the context of a repressive regime. I hypothesize that these

effects should be even larger in a repressive political system where voters must worry not just that their vote is unlikely to matter, but also face high potential personal costs to participate in opposition politics. I hypothesize that this relationship between affect and action is driven by perceptions of personal risks, and that these risk attitudes and perceptions may spill over into decisions other than politics.

L.3   Research Design

This project uses a lab-in-the-field experiment to test for the effect of fear on how citizens make decisions about political behavior in a high-risk environment. The experiment will be carried out in the context of a 30-minute household survey. Enumerators will approach potential subjects, obtain consent, ask a series of questions measuring demographic characteristics, induce emotions, and then measure assessments of political risk and willingness to engage in a series of political behaviors. At the end of the substantive measures we will measure the emotional states of the respondents as a manipulation check.[6]

I induce these emotions using standard techniques from psychology. Specifically, I use a reflection task in which respondents are instructed by the enumerator to describe the last time they felt a specific emotion. The techniques that I use to stimulate emotions are most similar to Lerner et al. (2003), who stimulate anger and fear in the context of the 9/11 terrorist attacks, and Banks and Valentino (2012), who stimulate general anger and disgust and investigate their impacts on political attitudes. Exercises based on recall is one of the most effective ways to stimulate specific emotions, particularly compared to alternative treatments like viewing videos (Harmon-Jones, Amodio and Zinner, 2007).

The subject will be asked to describe a time in which they felt the assigned emotion. They will be asked the following questions:

This type of emotion-induction technique has been used in a wide range of contexts, although I have adapted it to the Zimbabwean context, including in low- or middle-income countries such as Kenya and Colombia. This method of emotion elicitation has been used in internet-based surveys where respondents are asked to describe the situations in which they felt the specified emotion in writing, lab studies where respondents either speak or write their answer, and in face-to-face interviews. Describing the situation to an enumerator is advantageous in this situation for several reasons. First, it enables us to include low-literacy subjects in our sample. Second, the enumerator can use a series of several permitted probes to direct the respondent in an interactive way to reflect on precisely the ideas or feelings that trigger the specific emotion, enabling a more potent treatment.

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[6]Emotions are also measured at the end of the questionnaire in light of research showing that asking people to report their emotions causes those emotions to dissipate (Kassam and Mendes, 2013).

After the emotion induction treatment, the subject will be asked a series of questions to measure outcomes. Assessment of political risks will be measured with a series of twelve questions on various political risks that are relevant in the Zimbabwean context. To hold constant the riskiness of the behavior that the respondent typically engages in, we ask about the probability that the respondent will face different punishments if he engages in a specific action, namely going to an opposition rally or meeting. Respondents will be asked about the probability that they will face threats, assault, destruction of property, sexual abuse, abduction and torture, and murder. They will be asked to report the risk of each if they go to a rally or meeting now (during a non-election period) and around the time of the next election. Probability will be assessed on a five-point scale that is easy to understand in the local language including not at all likely, a little bit likely, somewhat likely, very likely, and sure. The responses to these questions will be used to make an index of perceived risk of repression.

Willingness to engage in politics will be measured in a similar way. Respondents will report the likelihood that they would wear an opposition party t-shirt, share a funny joke about the president, go to an opposition rally, refuse to go to a pungwe [a mandatory rally for the ruling party] when asked by a community leader, tell a war veteran [a type of individual who is known for perpetrating political violence] that she supports the opposition, or testify in court against a perpetrator of violence. They will report the likelihood that they would take each action now (during a non-election period) and around the next election. The answers to these questions will be used to make an index of willingness to take political action.

Beliefs about others' willingness to engage in politics will be measured in a similar way. Respondents will report the number of other opposition supporters in their community that they would wear an opposition party t-shirt, share a funny joke about the president, go to an opposition rally, refuse to go to a pungwe [a mandatory rally for the ruling party] when asked by a community leader, tell a war veteran [a type of individual who is known for perpetrating political violence] that she supports the opposition, or testify in court against a perpetrator of violence. They will be asked for the number of other opposition supporters that would take each action now (during a non-election period) and around the next election. The answers to these questions will be used to make an index of beliefs about others' willingness to take political action.

There are limitations to using self-reported propensity to take political actions instead of a behavioral measure of propensity to act. There is a risk that respondents' estimates of propensity to act may be not be strongly related to their actual behavior. This would be especially problematic if there were differential bias in reporting across treatment groups. However, there is little reason to believe that the emotions treatment should differentially affect respondents' vulnerability to social desirability bias or other common factors that drive inaccuracies in self-reported political behavior. However, one concern is that if participants are not deeply considering how to respond to these

questions, the hypotheticals may under-estimate the effect of fear on propensity to act.

To measure real political action, I will offer subjects the chance to take one of two wristbands as a thank you gift for participating in the study. Respondents will be able to choose between taking a plain orange wristband and a wristband with a pro-democracy and anti-violence slogan on it. This type of pro-democracy paraphernalia does not put respondents at imminent or undue danger, but wearing it could send a signal of opposition to the ruling party that could make someone nervous. The enumerator will stress to respondents that they are not obligated to take the wristband, but if they want to take it they should commit to wearing it. Taking the pro-democracy wristband indicates that subjects want to publicly show their support for democracy.

Additionally, I will measure financial risk attitudes and pessimism about economic outcomes. Financial risk attitudes will be measured using a series of four 50-50 lotteries in which subjects choose from six types of bets with varying levels of risk. Across the four lotteries, there will be two standard conditions, one condition with ambiguity, and one with losses. From these I will construct several measures: risk aversion, ambiguity aversion, and loss aversion. This measure is designed to be effective even with low-literacy and numeracy populations and has been used in related studies in Colombia and Kenya.

Financial pessimism will be measured using a series of scenarios similar to the political risk perceptions. These questions will ask about the likelihood that investment in a small business will pay off, that someone in the family would be laid off, that an economic earner in the family would have to stop working, that a major asset would be broken or lost, or that savings would be lost or stolen in the next six months and two years. These questions will be used to test whether the fear caused by political repression has spillover effects into economic beliefs and behavior.

As a manipulation check, we will also measure the six primary emotions (anger, fear, happiness, surprise, disgust, and sadness) on a four-point scale: not at all, a little bit, somewhat, and a lot. Enumerators will also code how afraid or relaxed they think the subject is at the end of the emotion induction. Last, 10% of the emotion inductions will be recorded, transcribed and translated as a second manipulation check to test whether respondents are actually talking about things that scare them.

Randomization into the treatment categories will be blocked on community, enumerator, and gender. Each enumerator will use a survey dictionary to select the appropriate treatment based on the gender of the respondent and the number of the interview.

The order of each of the six outcome modules (1. political pessimism, 2. self-reported political action, 3. beliefs about others' political action, 4. risk aversion, 5. economic pessimism, and 6. real political action) will be fixed. Order of questions within modules 1, 2, 3, and 5 will be randomly assigned.

One concern that will be addressed through the pilot is that the effect of the emotion induction

might dissipate before the subject completes the last outcome module. To counter this, I am inducing emotions at two points during the study – once before the measures of political outcomes, and a second time before the economic outcomes.

L.4   Predictions

This study will test the following predictions:

1. Subjects who receive the fear treatments will be more pessimistic in their assessments of political risks.
2. Subjects who receive the fear treatments will believe that fewer of their fellow opposition supporters will take pro-opposition actions.
3. Subjects who receive the fear treatments will be less willing to take pro-opposition political actions.
4. Subjects who receive the fear treatments will be more pessimistic in their assessments of economic risks.
5. Subjects who receive the fear treatments will be more risk averse in financial decisions.
6. The effect of the political and general fear treatments will be equal.

The main results will come from the pooled sample of people assigned to the political and apolitical emotion conditions. I will also test for differences across the political and general treatment conditions but do not expect that these effects will differ. If they are significantly different, then I will run the analysis separately for political and general emotions and discuss the results separately.

L.5   Data

This experiment will be implemented with a sample of 700 opposition supporters in urban and rural Zimbabwe. The data is being collected by researchers connected with the Zimbabwean NGO Voice for Democracy, which carries out research on human rights abuses and organizes communities to prevent and prosecute political violence. These researchers will use Voice for Democracy's network of members to identify respondents in nine communities affected by violence around Harare and in rural areas of Mashonaland East. This sample is not representative and includes a mix of people who actively resisted and succumbed to intimidation, as well as people who were directly and indirectly affected by intimidation.

Socioeconomic status will be measured using the index of asset ownership from the last Zimbabwean Demographic and Health Survey. It covers quality of housing, land ownership, major assets like generators and cars, small assets like mobile phones and radios, and livestock. I will use the standardized first principal component in the analysis.

Self-efficacy is measured with an ten-point questionnaire developed by Jerusalem and Schwarzer (1992); Schwarzer et al. (1997). The measure will be a standardized version of the first principal component.

Past exposure to political violence is measured with a scale based on the Harvard Trauma Questionnaire. The types of traumas asked about are taken from past applications of the Harvard Trauma Questionnaire in Zimbabwe. For each item, respondents will be asked whether they experienced the trauma and whether they heard about it happening in their community.

Last, we will measure past participation in opposition politics with a series of eight questions about whether the respondent has taken various pro-opposition actions many times, sometimes, once or twice, or never since the year 2000.

In order to avoid priming subjects on opposition politics and political violence, these last two batteries of questions will most likely come after the treatment. During piloting ($N = 200$) we will test whether putting these questions before or after the treatments influences a. the strength of the treatment or b. how respondents answer them, and this will be used to determine where in the main study they should go. Based on the first round of this study, putting the political violence questions immediately before the emotion induction does increase the likelihood that subjects will reflect on something related to political violence that makes them angry or afraid.

L.6   Analysis

Results will be analyzed using both difference-in-means tests that take blocking into account and regression analysis. The multivariate specification that will be used is:

$$Y = \alpha + \beta Z + \gamma X + \varepsilon$$

where Y is either the probability of punishment index or the propensity to act index, and Z is a dummy for assignment to treatment in the emotion induction. α is a community fixed effect. X is a vector of individual covariates including gender, an assets index, education, and age. We will also test for whether the apolitical or political context of the emotion matters using the interaction of the two treatments for both outcomes:

$$Y = \alpha + \beta Z1 + \lambda Z2 + \tau Z1 \times Z2 + \gamma X + \varepsilon$$

where Y is again probability of punishment or propensity to act for subject i, $Z1$ is still assignment to the emotion induction treatment, and $Z2$ is a dummy for whether you were assigned to the political version of the treatment.

L.7 Anticipated Results

L.7.1 Manipulation Check

First I will present the results of a manipulation check asking subjects to report their emotions at the end of the questions measuring the outcomes of interest. I expect that the fear treatment will increase fear. Other emotions should be more or less unaffected by the treatments.

In fact, it wouldn't be surprising if happiness was also negatively affected by the fear and anger treatments. It's also likely that subjects in the fear treatment will feel some anger, and those in the anger treatment some fear. People often feel multiple positive or negative emotions together. To the extent that the treatments induce both negative emotions of interest, I may underestimate the coefficients of interest.

I will also carry out the manipulation check separately for the political and apolitical conditions. I expect that the manipulation check will be less clean for the political condition given that people have strong feelings about political violence like anger that could also be introduced in this treatment.

In previous studies, similar emotion induction techniques have produced large effects on self-reported emotional states. In pretesting, Lerner and Keltner (2001) find that general reflection-based emotion inductions increased fear over anger by 2.58 points on a 10-point scale in the fear condition, while the anger condition increased anger over fear by 2.21 points on the same scales. Lerner et al. (2003) use a nine-point scale to measure emotion, and find that a political anger treatment resulted in a 0.66-unit increase in anger over fear, and a 0.50-unit increase in fear over anger in the political fear treatment. On my scale, these effects would translate into increases of about 0.33 units and 0.25 units, which are approximately the parameters that I've used in my simulations. The 2001 study effect sizes would translate into increases of approximately 0.88-1.03 units on my 4-point scale, far larger than what I simulate here.

In the first study that I ran with a similar subject group, I found very large effects on the same four-point emotions scales. Specifically, a similar emotion induction technique resulted in a control group median of "not at all" afraid versus a treatment group mean of "a lot" afraid, or a 1.6 point difference in means on the scale. Figure L.1 shows the manipulation checks that I ran from this first study.

There are several reasons why we might see such large differences in the emotions induced in the two experiments. First, the emotions inductions in the 2001 study took place in a lab setting with undergraduate students, while the 2003 study used an online panel. It's likely that the respondents in the online survey paid less attention to the task in the 2003 study. Second, it's possible that emotions are harder to influence around political events. While everyone can reflect on something that they are angry or fearful about, most people have strong feelings about important political events. In the 2003 study, Lerner et al. (2003) induced anger and fear around the 9/11 terrorist attacks, where

Figure L.1: The impact of the treatments on six major emotions
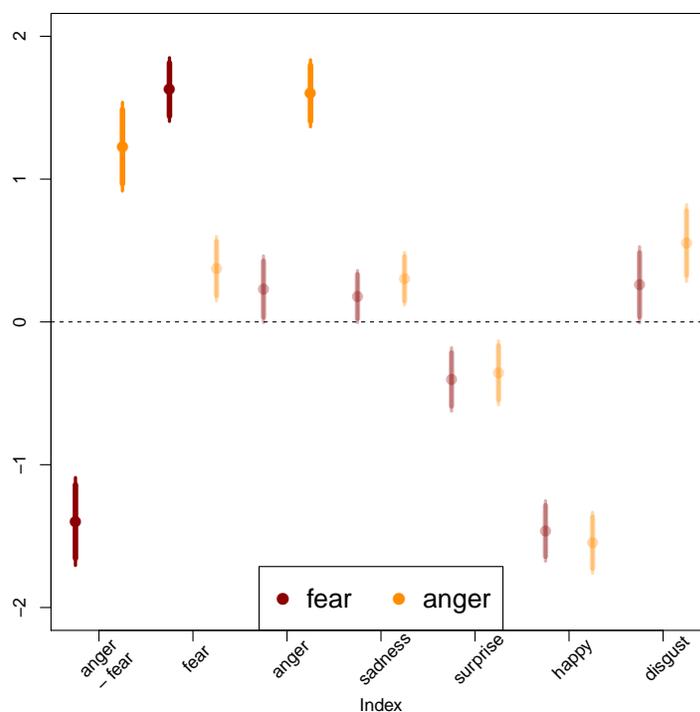
respondents presumably had strong baseline emotions about the attacks that may have been hard to sway with such a reflection task.

L.7.2 Main Effects

What effect sizes are reasonable to expect on pessimism and risk taking? There are a number of previous similar studies that can provide insight into the relationship between fear and pessimism. Unfortunately, most existing studies simply compare across negative emotions rather than comparing negative emotions to a neutral control group. Nevertheless, Lerner and Keltner (2001) find using a similar reflection technique to induce emotions that anger and fear led to an average increase in optimism (increases in the perception of the probabilities of good outcomes and decreases in perceptions of the probabilities of bad outcomes) of 2.22 on a nine-point scale of the likelihood of good or bad events, which translates into an average difference between anger and fear of 1.2 on my five-point scale. In an online survey with a much less potent emotion induction, Lerner et al. (2003) measure risk on a probability scale from 0 to 100%, and find differences in perceived probabilities between anger and fear of between 3.9 and 4.7 percentage points. This study also found that the effect of fear on pessimism extended to unrelated personal outcomes like the probability of catching the flu, with similar magnitudes of the effects.

In a previous study with a similar population in Zimbabwe, I found that fear had a significant positive effect on assessment of risks, making them ($p < 0.05$). Specifically, I found that the fear treatment increased perceived risks of repression by 0.25-0.27 points on a five-point likelihood scale over the same twelve measures that I will use in this experiment. This could mean, for instance, that respondents increased their perception of the likelihood of three repressive acts by one point on the scale. The results from that study are shown in Table L.2 in Columns 1-3.

Table L.2 also shows that fear is associated with decreases in the propensity to act, but these effects are not significant. This may be because respondents did not seriously consider the hypothetical actions, or because the increases in risk perceptions did not lead them to change their propensity to act. To test whether this might be a measurement issue, I will take a several steps. First, because it is possible that the effect of the primes had simply worn off during the questions about political risks, I will randomly assign the order of the risk assessments and political action measures. Second, because it is possible that subjects don't take the hypothetical questions seriously, I will add a real, though low-risk, political action. Although my hypotheses predict that fear should decrease political action, I have no previous estimates of this relationship to give me strong priors about what the effect size might be.

L.8 Conclusion

If I see the results that I expect, I would conclude that the fear caused by political violence – independent of the informational signal that political violence sends about the probability of punishment for various political actions – has a demobilizing effect. Specifically, fear makes citizens increase their estimation of the probability that they personally will be punished for expressing dissent. These results would suggest that political violence not only sends an informational signal to citizens that they will be punished for dissent but also influences them through an emotional channel. This phenomenon may help explain why coercive actors tend to use "extra-lethal violence" that involves mutilation or particularly counter-normative acts like rape.

Table L.1: Reflection Treatments

| Assignment | | |
Control	General	Political
$N = 350$	$N = 175$	$N = 175$
1. What are the three to five activities that you like to do to relax? Please tell me two to three sentences about each thing that you like. (Examples of things you might talk about include: playing with your children, resting, taking tea, talking to friends.)	1. What are the three to five things that make you most afraid? Please tell me in two-three sentences about each thing that makes you afraid. (Examples of things you might talk about include: being alone on a dark street, being in a traffic accident, dangerous animals like snakes or lions, etc.)	1. What are the three to five things that make you most afraid about politics and elections? Please tell me in two-three sentences about each thing that makes you afraid. (Examples of things you might talk about include: getting beaten up, being abducted, losing your home, etc.)
2. Now we'd like you to describe in more detail the way you typically like to relax. Begin by giving a description of your favorite relaxing activities. Examples of things you might describe include going to church, spending time with certain friends, watching football, eating a meal with your family, etc.	2. Now we'd like you to describe in more detail the one situation that makes you most afraid. This could be something you are presently experiencing or something from the past. Please tell me as if you're trying to make me afraid as well. What is it like to be in this situation? Why is it so scary?	2. Now we'd like you to describe in more detail the one situation around elections and politics that makes you most afraid around politics and elections. This could be something you are presently experiencing or something from the past. Please tell me as if you're trying to make me afraid as well. What is it like to be in this situation? Why is it so scary?
3. Now we'd like you to describe in more detail another way you typically like to relax. Begin by giving a description of your favorite relaxing activities.	3. Now we'd like you to describe in more detail another situation that makes you most afraid around politics and elections. This could be something you are presently experiencing or something from the past. Please tell me as if you're trying to make me afraid as well. What is it like to be in this situation? Why is it so scary?	3. Now we'd like you to describe in more detail another situation around elections and politics that makes you most afraid. This could be something you are presently experiencing or something from the past. Please tell me as if you're trying to make me afraid as well. What is it like to be in this situation? Why is it so scary?

Table L.2: Effect of fear and anger stimuli on perceived probability of punishment and propensity to act

	Dependent variable:					
	Prob(Pun)			Prob(Act)		
	(1)	(2)	(3)	(4)	(5)	(6)
Fear	0.27**	0.25**	0.27*	−0.04	−0.05	−0.07
	(0.11)	(0.11)	(0.15)	(0.12)	(0.12)	(0.16)
Anger	0.16	0.15	0.15	−0.06	−0.07	−0.09
	(0.11)	(0.11)	(0.15)	(0.12)	(0.12)	(0.16)
Political (Z)			0.03			0.03
			(0.15)			(0.17)
Fear × Political (Z)			−0.04			0.05
			(0.22)			(0.24)
Anger × Political (Z)			−0.02			0.03
			(0.22)			(0.24)
Female	−0.08	−0.08	−0.08	−0.49***	−0.47***	−0.47***
	(0.09)	(0.09)	(0.09)	(0.10)	(0.10)	(0.10)
Age		0.03	0.03		0.04*	0.04*
		(0.02)	(0.02)		(0.02)	(0.02)
Age^2		−0.0003	−0.0003		−0.0005*	−0.0005*
		(0.0003)	(0.0003)		(0.0003)	(0.0003)
Education		0.08**	0.08**		−0.13***	−0.13***
		(0.04)	(0.04)		(0.04)	(0.04)
Assets		0.05	0.05		−0.04	−0.04
		(0.05)	(0.05)		(0.05)	(0.05)
Intercept	2.10***	1.13**	1.12**	3.28***	3.03***	3.03***
	(0.15)	(0.49)	(0.49)	(0.17)	(0.52)	(0.53)
Community FE	✓	✓	✓	✓	✓	✓
Observations	473	473	473	473	473	473
R^2	0.25	0.26	0.26	0.17	0.19	0.19
Specification	OLS	OLS	OLS	OLS	OLS	OLS

Standard errors in parentheses.
*p<0.1; **p<0.05; ***p<0.01

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