Appendix

A Measurement

Table A.1: Survey measures used to operationalize independent variables

Concept	Survey Measures
Dissent events	Now I'd like to find out what's been happening in your area since we last spoke. For each of these, I'd like you to tell me if you have either seen or heard about people in your community doing that thing since we last spoke to you. Since we last spoke, have people in your community: 1. Been talking freely about their political beliefs or opinions? 2. Been engaging in acts of protest, like participating in a demonstration or a stayaway? 3. Been openly wearing regalia for parties other than the ruling party? Yes, I have seen it happening; Yes, I have heard about it happening; No, I don't think it's happening
Dissent media	Since we last spoke, have you seen a video or photograph of people doing one of these things (talking freely about their beliefs, engaging in acts of protest, or wearing regalia for parties other than the ruling party)? Yes, No, DK/Ref
Repression events	And since we last spoke, have people in your community: 1. Been threatened or intimidated because of their political beliefs? 2. Experienced physical violence like assault because of their political beliefs? 3. Lost some business or had property destroyed because of their political beliefs? Yes, I have seen it happening; Yes, I have heard about it happening; No, I don't think it's happening
Repression media	Since we last spoke, have you seen a video or photograph of people experiencing one of these things (intimidation, violence, or the loss of business or property)? Yes, No, DK/Ref

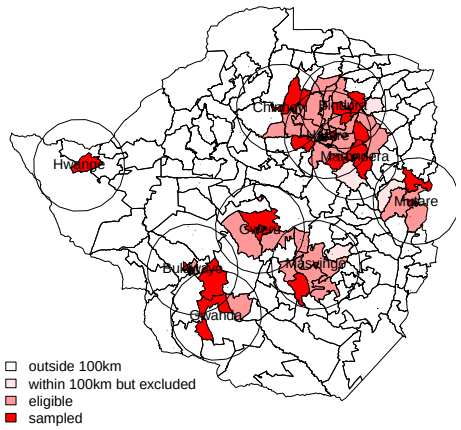
Table A.2: Survey measures used to operationalize outcome variables and mechanisms

Concept	**Predictions**	**Survey Measures**
Dissent Propensity Index	1A, 1B, 3A, 3B	Now I want to ask you about some things that you might think about doing. How likely is it that you would do each of these things in the next week? 1. Participate in a protest or a stayaway if one were organized for a service that you care about? 2. Wear the t-shirt / regalia of your preferred political party outside if you were given one? 3. Join the Facebook group of your preferred political party if you were invited? Not at all likely, A little bit likely, Somewhat likely, Very likely, DK/Ref
Perceived probability of repression	2A, 4A	If you go to a rally or meeting for your favorite political party in the next week, how likely is it that: 1. You will be threatened? 1. You will be assaulted?
Others Dissent Likelihood		Next, I want to ask about those same things, but I want you to tell me how many other people in your community would do them. 1. How many others who care about the same service as you would participate in a protest or stay-away for that service if one were organized? 2. How many other supporters of your preferred party would wear the party tshirts / regalia outside if given regalia? 3. How many other supporters of your preferred party would join the party's Facebook group if invited? None, A few, Some, Many, All, DK/Ref
Perceived quality of the regime	2B, 4B	And at this moment, would you say Zimbabwe is a democracy? Yes, it is a democracy; Yes, but with minor problems; Yes, but with major problems; No, not a democracy; DK/Ref
Positive emotions	2C	I'd like to start by asking about how you've been feeling since we last spoke. Since we last spoke, how happy have you felt?
Negative demobilizing emotions	4C	Since we last spoke, how sad have you felt? Since we last spoke, how afraid have you felt?
Negative mobilizing emotions	4D	Since we last spoke, how angry have you felt? Not at all; A little bit; Somewhat; A lot; DK/Ref
Strength of group identity	2D, 4E	When we last spoke, we asked you to tell us about your feelings about different groups and parties using a scale slider (PROMPT: "you or the interviewer moved it with your finger"?). Do you remember this? Ok. So first I'm asking you about ZANU-PF. One week ago, you said you gave them a [fill from baseline]. If you had to give them a number now, what would it be, or would you say it hasn't changed? MDC? 0-100 feeling thermometer

B Sampling

Figure B.1 presents a map of Zimbabwe with constituencies that were eligible for our sample in red and selected constituencies in dark red.

Figure B.1: Map of sampled constituencies



We next analyze two types of potential selection effects. We begin by analyzing selection into the WhatsApp panel in general. Our sampling strategy introduced two types of mechanical selection: first, participants had to reside in one of the ten selected cities or surrounding rural areas, and second, they had to possess a smartphone that could access the internet. As of 2018, mobile phone ownership trends suggested that 84% of the national population had a cellphone, and 37% had a cellphone that could access the internet (smartphone).[16] WhatsApp is one of the most commonly used smartphone applications in Zimbabwe with carriers offering cheap data bundles that offer unlimited use. 50.2 percent of our baseline participants – or 467 individuals –

[16] 87% of Zimbabwean households had mobile phones as of 2015 based on the Demographic and Health Survey data. The nationally representative Afrobarometer surveys in 2015 and 2018 corroborate this figure: in the 2018 data, 84% of the weighted sample own their own personal mobile phone and an additional 7% have access to a phone owned by someone else in their household. 36% of Afrobarometer respondents in 2018 reported that they have access to the internet on their mobile phone.

indicated that they owned smartphones. Of these 467 individuals, 330 consented at the end of the face-to-face baseline interview to participate in the WhatsApp panel survey (panel uptake rate of approximately 71 percent of those eligible). 93% of those who consented participated in at least one wave of the WhatsApp panel. Appendix Table B.1 compares the baseline characteristics of people who participated in at least one panel wave to (a) our full baseline sample population and (b) the nationally representative Afrobarometer sample in February 2018, conducted approximately five months before our baseline survey.

Our WhatsApp panel differs from a nationally representative sample in two ways by design. First, it is almost entirely (89%) urban, while about 37% of the re-weighted nationally representative sample is urban. Second, participants in our sample are much more likely to have cell phones and smartphones.[17] These differences were necessitated by our methodology. Our WhatsApp sample also differs in non-mechanical ways that are probably consequences of this sampling strategy: participants in our WhatsApp panel are slightly younger, more likely to have completed high school, and somewhat more likely to have most assets. Many of these differences disappear when comparing our sample to the urban participants in the last Afrobarometer, suggesting that they are largely a function of the mostly urban eligibility criteria. Importantly, there is not much evidence that our sample is particularly politically active: they are slightly more likely to be affiliated with a political party than the Afrobarometer sample, although half of our sample still reports that they are not close to any party. Our WhatsApp group was not more likely to participate in 2018 electioneering at the time of the baseline compared to our full sample, and it was not substantially more likely to have engaged in non-electoral forms of citizen engagement than our baseline or the Afrobarometer sample. They express less fear of election violence than the Afrobarometer sample, but this may be a function of time as our baseline survey took place towards the end of a largely peaceful 2018

[17] Interestingly, only 71% of the WhatsApp panelists said at baseline that they personally own a phone that can access the internet. This is most likely a reflection of the question wording, which asked participants if they have a phone that can access the internet instead of one that can access more commonly known apps like WhatsApp. We used the same question wording as the Afrobarometer for this measure.

election campaign. There is no evidence that level of fear or past exposure to political violence is associated with selection into the WhatsApp panel overall.

Table B.1: Comparison of WhatsApp participants to our baseline and the last nationally representative Afrobarometer survey

	WhatsApp	Baseline	Afrobarometer	Afrobarometer - Urban
Age	34.67	37.98	38.92	35.25
	(13.54)	(15.53)	(16)	(13.5)
Female	0.56	0.56	0.5 (0.5)	0.5 (0.5)
	(0.5)	(0.5)		
Education: High school	0.69	0.61	0.48	0.66
	(0.46)	(0.49)	(0.5)	(0.47)
Mother tongue: Shona	0.51	0.51	0.63	0.64
	(0.5)	(0.5)	(0.48)	(0.48)
Mother tongue: Ndebele	0.07	0.08	0.12	0.14
	(0.25)	(0.28)	(0.33)	(0.35)
Urban	0.89	0.78	0.37	1 (0)
	(0.32)	(0.41)	(0.48)	
Assets: Radio	0.57	0.57	0.73	0.8
	(0.5)	(0.49)	(0.45)	(0.41)
Assets: TV	0.78	0.69	0.48	0.89
	(0.42)	(0.46)	(0.5)	(0.32)
Assets: Computer	0.24	0.24	0.21	0.44
	(0.43)	(0.43)	(0.41)	(0.5)
Assets: Cell	0.94	0.84	0.84	0.95
	(0.24)	(0.36)	(0.36)	(0.21)
Assets: Smartphone	0.71	0.5	0.37	0.61
	(0.45)	(0.5)	(0.48)	(0.49)
Party ID: Any	0.57	0.56	0.62	0.43
	(0.5)	(0.5)	(0.49)	(0.5)
Party ID: ZANU-PF	0.26	0.28	0.38	0.19
	(0.44)	(0.45)	(0.48)	(0.4)
Party ID: MDC-A	0.22	0.18	0.13	0.14
	(0.41)	(0.38)	(0.34)	(0.35)
Fear election violence	1.23	1.38	1.57	1.89
	(1.28)	(1.29)	(1.24)	(1.24)
Violence experienced index	0.2	0.18		
	(0.29)	(0.27)		
Election activism	0.24	0.21		
	(0.32)	(0.29)		
Non-election activism	0.13	0.1	0.1	0.09
	(0.19)	(0.18)	(0.18)	(0.2)
N	301	932	1200	448

Sample means are reported and standard deviations are in parentheses. The sample WhatsApp participants is based on the baseline responses of anyone who participated in at least one WhatsApp wave. The sample baseline participants is based on the full baseline sample for our study. The sample Afrobarometer is based on the Feb 2018 round of the Afrobarometer survey, using the provided sampling weights. The sample Afrobarometer - Urban is based on the Feb 2018 round of the Afrobarometer survey, using the provided sampling weights, subset to only urban participants.

The second and more problematic type of selection that we test for is selection into attrition from specific waves of the panel. In each wave, the participation rate was between 71 and 79% of participants.[18] The total number of participants remained stable over time from the August to November WhatsApp waves, with each participant responding to an average of 3.7 out of five waves. Appendix Table B.2 shows that attrition is only weakly predicted by baseline characteristics and is not predicted by lagged values of exposure to repression and dissent or baseline measures like fear of election violence or past exposure to violence that might indicate reticence. A key test of non-random attrition comes by looking at predictors of attrition during rounds where the risk of repression was highest. In this case, the risk of retribution was highest in the week of August 9-12, right after the post-election crackdown. There is no evidence that people who were particularly fearful, who had perceived violence as particularly likely, or who had been exposed to repression or dissent events in the previous round were more likely to attrit overall, or in the second most violent wave. A few demographic predictors come up as statistically significant predictions of attrition but not more than you would expect just through random chance. Thus, it may be reasonable to assume that missingness due to attrition is as-if random in this panel. One explanation for the attrition may be that the Zimbabwean cellular and electrical networks are unreliable, so participants may have dropped out while out of cellular range or when unable to regularly charge their phones.

[18] We calculate attrition out of the participants who ever participated because many of those who never participated appear to have misreported that they actually owned a smartphone.

Table B.2: Predictors of attrition

	Dependent variable:		
	Attrition		
	(1)	(2)	(3)
Female	−0.217	−0.217	−0.149
	(0.171)	(0.171)	(0.203)
Age	−0.030***	−0.030***	−0.024***
	(0.007)	(0.007)	(0.009)
Urban	−0.311	−0.311	0.107
	(0.244)	(0.244)	(0.328)
Assets Index	−0.999**	−0.999**	−0.568
	(0.457)	(0.457)	(0.561)
Language: Ndebele	−0.240	−0.240	−0.767
	(0.373)	(0.373)	(0.492)
Language: Shona	0.033	0.033	0.015
	(0.176)	(0.176)	(0.209)
Education	−0.151	−0.151	−0.139
	(0.106)	(0.106)	(0.112)
Party ID: Opposition	−0.069	−0.069	0.141
	(0.278)	(0.278)	(0.352)
Party ID: Ruling Party	0.247	0.247	0.400
	(0.266)	(0.266)	(0.336)
Fear of Election Violence	0.086	0.090	0.071
	(0.066)	(0.068)	(0.078)
Past Violence Index	0.299	0.299	0.377
	(0.276)	(0.276)	(0.363)
Electoral Activism	−0.300	−0.300	0.011
	(0.286)	(0.286)	(0.320)
Non-Electoral Activism	0.464	0.463	−0.480
	(0.491)	(0.491)	(0.587)
Fear of Election Violence X Round 2		−0.021	
		(0.088)	
Local Repression (Lag)			−0.167
			(0.252)
Local Dissent (Lag)			−0.224
			(0.247)
Media Repression (Lag)			−0.120
			(0.268)
Media Dissent (Lag)			0.155
			(0.224)
Dissent Likelihod (Lag)			−0.164
			(0.111)
Constant	1.119	1.119	0.102
	(0.751)	(0.751)	(0.854)
Observations	1,365	1,365	844
Log Likelihood	−711.796	−711.769	−386.055
Akaike Inf. Crit.	1,451.593	1,453.538	810.109

*p<0.1; **p<0.05; ***p<0.01
Estimated using a logit model with a binary measure of attrition in each wave as the dependent variable.

C Ethical Considerations

This section provides additional details related to the ethics of the research. This study was approved by the IRBs at both of the authors' universities under protocols 1253790 (UC Davis) and 201935 (American University). There is no Zimbabwean IRB that reviews this type of social science research. In addition to the protections for human subjects considered by the IRBs, we also considered the ethical imperative of protecting the domestic research assistants who worked on the project.

C.1 Risk assessment

We took a number of steps to make sure that we were accurately assessing and minimizing risks to research participants and staff. The main risk associated with participation in our study was the threat of state repression, which could have been targeted on respondents in the case of a loss of confidentiality, or on research staff. We also designed the study with the knowledge that we were in a moment of uncertainty in which political conditions could change quickly.

Before beginning the study we assessed the risk of retribution for participating in research during different periods in Zimbabwe's history. One important data point was the fact that in-person survey research, even on politics, is common in Zimbabwe. Afrobarometer surveys have been carried out on a regular basis since 1999, and the World Bank, Freedom House, and other organizations have also conducted in-person surveys on political attitudes without adverse events. Some of this research was conducted during periods of state-sponsored violence that was significantly more prevalent and more severe than during the 2018 election period. During the 2018 pre-election period, there were a number of surveys with questions similar to ours, including two Afrobarometer surveys.

We also consulted data on violent events, particularly from ACLED and domestic human rights monitoring groups like the Zimbabwe Peace Project, media reports from aggregators of local news

sources like zimbabwesituation.com, and members of the policy community monitoring the election such as our funder the International Republican Institute. These data sources gave us an up-to-date view of the state of violence before launching our study, as well as shedding light on geographic variation in the potential risk of violence.

One of the difficulties of assessing the risk of this particular project was that we anticipated that the risk might change dramatically during the panel. We thus based our design on the expectation that the level of repression could conceivably return to some of the highest levels that it had been at in Zimbabwe, not the lower level of repression risk that had been in place since 2009/2010.

C.2 Research design

Because we anticipated the the risk of repression might increase during the panel, it was essential that we survey respondents in a way that minimized the exposure of surveyors to potentially dangerous situations and the exposure of respondents should a breach of confidentiality occur. To protect surveyors during the baseline, we closely monitored field conditions and also removed the most sensitive questions from the baseline questionnaire. No adverse events occurred. For the period immediately preceding the election and for the post-election period, we decided that the lowest risk would be if research were conducted via phone-based surveys that would allow enumerators to survey respondents remotely. We then had to assess the risk of a breach of confidentiality to respondents in different phone survey modalities.

We used a mix of desk research and consultations to assess the risk of surveillance in phone and WhatsApp voice surveys. Ultimately, we judged that phone tapping through Zimbabwe's cellular providers was too widespread for us to interview people over regular voice calls. However, as of 2018, the evidence that we found suggested that the government did not have the capacity to monitor encrypted ICT at the scale of the average citizen. The government's reliance on blunt cybersecurity tools like mass internet shutdowns and seizure of devices suggested that they were not yet able to monitor and selectively punish based on encrypted communications. Finally, all

arrests for "cybercrimes" in recent years had been based on public communications on Twitter or Facebook, again suggesting that their capacity to monitor WhatsApp was limited.

Nevertheless, we were still concerned that a motivated government agent might breach confidentiality through two possible means: 1) by seizing respondents' physical devices, or 2) by confiscating the data from our survey partner MPOI. To ensure that responses could not be seized after our interviews from respondents' devices, we did our surveys via voice instead of written chat exchanges. To minimize the risk that data could be confiscated from MPOI, we set up a system to keep identifying information separate from the survey responses. Perhaps most importantly, we decided not to ask questions about behaviors that we thought the government might want to punish. To this end, we asked respondents about protest intentions rather than actual protest behavior.

After we had designed this methodology, we got feedback from a number of organizations and individuals with experts in cybersecurity, repression, and data collection in Zimbabwe. The project funder (a policy organization with extensive experience in many different cultural contexts) reviewed and signed off on the process and instrument. A tenured Zimbabwean professor in the social sciences reviewed our IRB protocol and wrote a letter attesting to its contextual appropriateness. Our research partner MPOI, which has two decades of running similar surveys in Zimbabwe under a range of conditions, contributed to and reviewed the protocol. Based on their advice, we removed some questions that they felt would be too sensitive during an election period, including one that asked about individual experience of violence in the 1980s (during Gukurahundi). Following this senior review, we also work-shopped and discussed the instrument with the field enumerators during a two-day training prior to piloting. Initial feedback from enumerators, which largely focused on wording and politeness, was incorporated, and amendments to the instrument were made, particularly to the Shona translation. Finally, we used pilots of both instruments to assess whether there were any questions that seemed to make respondents uncomfortable.

C.3 Monitoring incidence of potential harms

Finally, we monitored the incidence of realized harms during implementation. In order to assess the adequacy of our consent process, we asked participants whether they were happy that they had participated at the close of interviews: just 4% reported that they were unhappy that they had participated at the end of the baseline survey, and no respondents said they were unhappy after the first WhatsApp round.[19] We worked with our small team of WhatsApp surveyors to assess any change in study risks during the violent post-election period, and we gave them the option of pushing back or modifying the survey if they had concerns. Our team decided that this option was not necessary, and the fact that our participation rate remains stable across the five WhatsApp waves suggests that respondents continued to feel comfortable answering questions.

[19] Baron and Young (2022), following research in trauma psychology, recommend empirically assessing the adequacy of the consent process for studies that involve sensitive questions with survivors of violence.

D Additional Results

D.1 Media Repression

We excluded analysis of the effects of Media Repression from our main analyses because tests for pre-trends indicated that the treatment and control respondents were not comparable. We present these tests for pre- and post-treatment differences on Media Repression here.

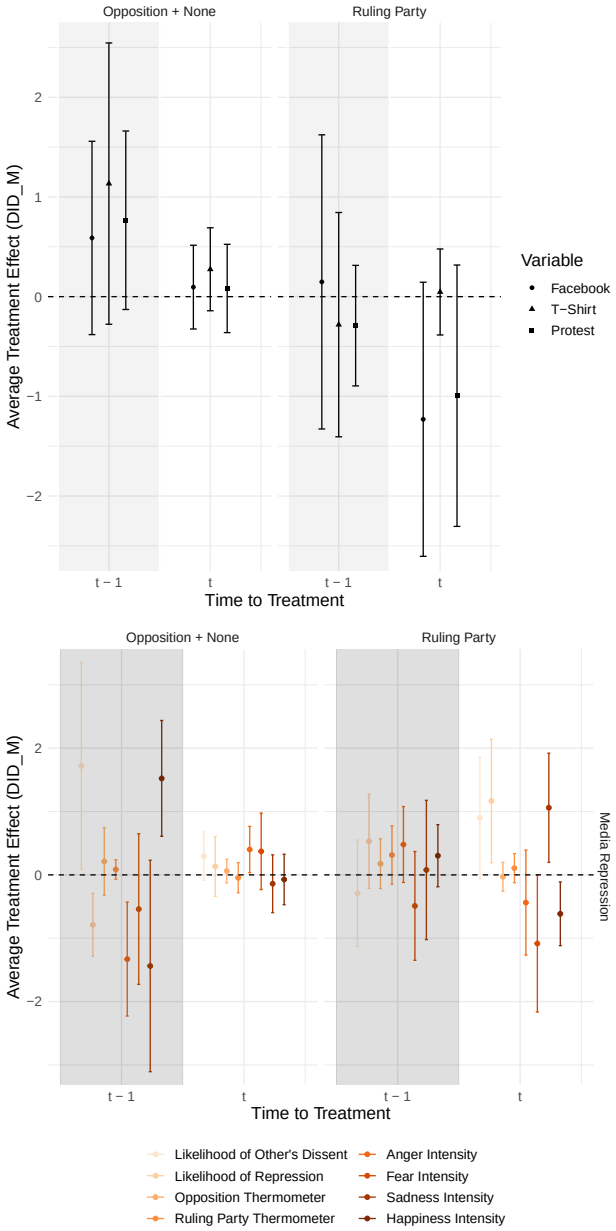


Figure D.1: Evidence of pre-treatment differences for those exposed to media repression

D.2 Dissent intentions

D.2.1 Comparison to two-way fixed effects estimates

Table D.1 presents estimates of both the classic two-way fixed effects estimator β_{FE} in Columns 1 and 3 and the DID_M estimator developed by de Chaisemartin and d'Haultfoeuille (2020) in Columns 2 and 4. Columns 1-2 present estimates of the ATE for participants who at baseline said they supported the opposition or no party and Columns 3-4 for participants who at baseline said they supported the ruling party ZANU-PF.

Table D.1: Local repression and dissent events increase propensity to dissent

	(1)	(2)	(3)	(4)
	FE	DiD$_M$	FE	DiD$_M$
Local Dissent	0.410	0.644	0.292	0.023
	(0.131)	(0.204)	(0.163)	(0.261)
Local Repression	0.486	0.608	0.051	0.252
	(0.122)	(0.223)	(0.152)	(0.322)
Media Dissent	0.132	0.580	0.347	0.157
	(0.111)	(0.251)	(0.133)	(0.253)
Media Repression	0.055	0.267	0.187	-1.607
	(0.161)	(0.235)	(0.186)	(0.955)
N_{FE}	696		444	
R-squared$_{FE}$	0.458		0.422	
N_{DiD_M}(switchers)		455(126)		285(54)
N_{DiD_M}(switchers)		457(127)		267(53)
N_{DiD_M}(switchers)		457(148)		277(85)
N_{DiD_M}(switchers)		398(70)		282(75)
	Opposition + None		ZANU-PF	

Columns 1 and 3 present results estimated using a fixed effects model with fixed effects for each respondent and survey wave. Columns 2 and 4 present results estimated using the de Chaisemartin and d'Haultfoeuille (2020) DiD_M estimator. Standard errors at the 95% confidence level are in parentheses.

D.2.2 Disaggregated results

We collected multiple measures of our key variables of interest. In this section we first assess whether the results are similar across different ways of measuring dissent intentions. Figure D.2 shows that the main interpretation is consistent across the three different types of dissent intentions that we measured: intentions to protest (presented in Figure 3) as well as intentions to join the Facebook group or wear the regalia of the party you support. Across all three dissent intentions, there are null effects for regime supporters. Effects in the opposition and unaffiliated sample are all positive and generally similar in magnitude but vary in statistical significance. This suggests that the effects of exposure to contentious events do not have particular effects on protest intentions but a more general relationship with intentions to dissent.

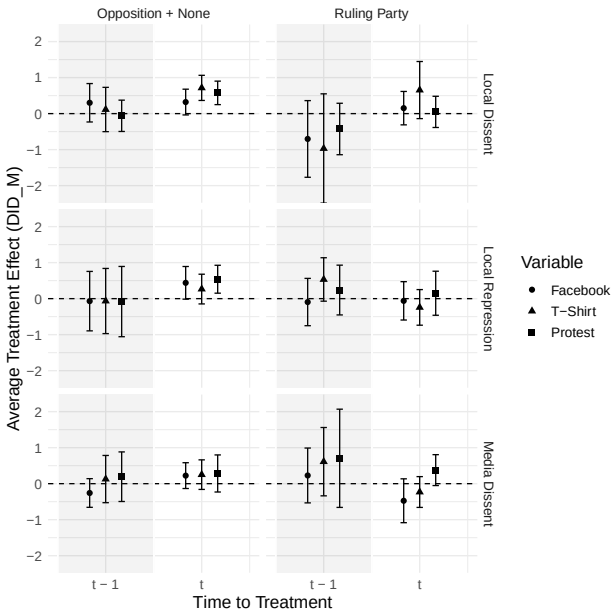


Figure D.2: Repression and dissent events increase propensity to engage in multiple forms of dissent for opposition and unaffiliated voters

Figure D.3 looks separately at the three different types of local repression exposure: intimidation, destruction of property, and assault. It is plausible that opposition and unaffiliated voters would have different reactions to low-level forms of repression like intimidation as opposed to

higher-level repression like assault, as some past research has found an “inverse-u” or n-shaped relationship between repression and dissent (Muller and Weede, 1990; Opp, 1994; Zhukov, 2018). However, this analysis is limited by two important constraints. First, the repression that occurred during this period in Zimbabwe, while deeply troubling, ultimately all falls at low-to-medium levels on the spectrum of state repression. The events that we capture in the panel range from low-level intimidation to physical violence such as beatings, temporary abductions, torture, and six deaths at a post-election protest. Second, exposure to different types of repression events is correlated, and we do not have the sample size necessary to estimate the effect of each type of local repression event conditional on the other types. With these caveats in mind, Figure D.3 shows that the estimated coefficients are similar in magnitude across the three types of local repression that we measure.

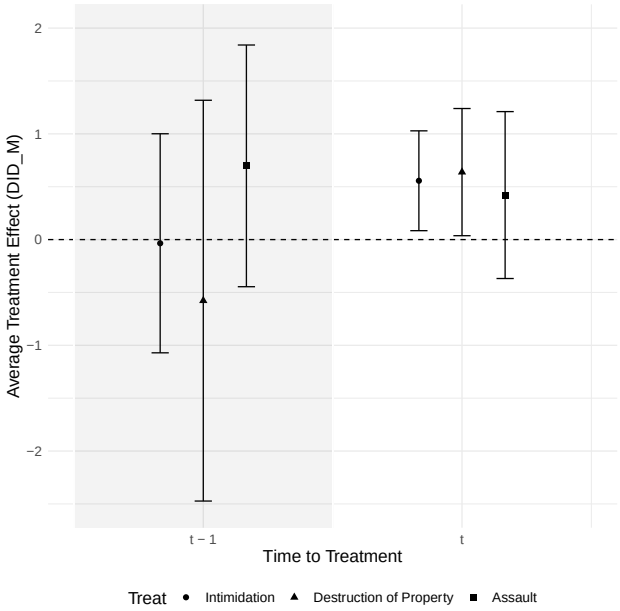


Figure D.3: The effects of exposure to repression are similar across types of local repression

D.2.3 Additional tests of the plausibility of the common trends assumption

In this section we present additional tests of the plausibility of the common trends assumption. In Figures 3 and 4 we show that there is no systematic evidence of pre-exposure differences across people who are exposed and not exposed to contentious events. This is the first piece of evidence suggesting that the common trends assumption is reasonable in this case. Furthermore, our estimates in Figures 3 and 4 are all conditioning on exposure to other forms of repression or dissent that we measure. In this section, we test for the robustness of our results to two additional potential confounders and then assess whether the results are driven by people who have fixed characteristics that might make them more likely to be exposed to other political information or mobilization efforts during specific waves of our panel.

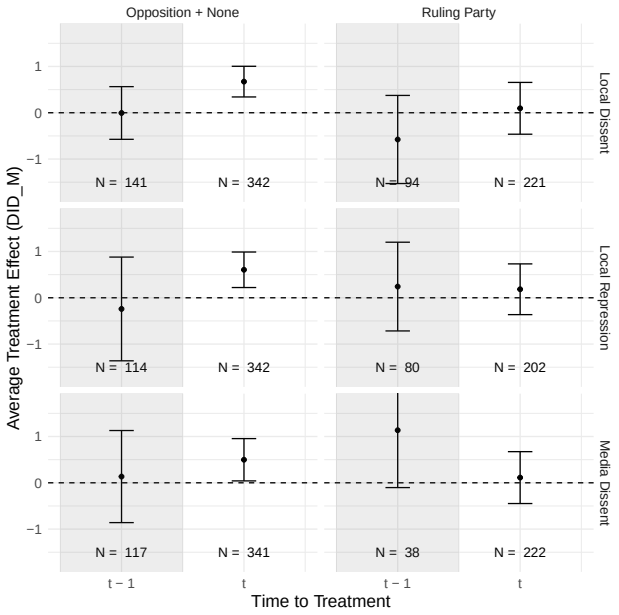
One potential confounding factor would be exposure to other forms of information during the panel waves, which could be correlated with exposure to our measures of repression and dissent and affect dissent propensity. We assess the risk of such bias by adding a control for whether the respondent received information about national political events during our survey waves. In waves 1-4, we asked participants whether they had heard of a number of national events that had occurred since we last spoke with them. These events included an episode where citizens were allegedly beaten by police for leaving a ruling party rally, a police raid of the opposition party headquarters, a court petition filed by the opposition challenging the election outcome, and a mass arrest of striking trade unionists.[20] In most rounds, 50-58% of respondents had heard of the events; opposition supporters were more likely to have heard of them but not exclusively so. The one event that virtually everyone (99%) had heard of was the opposition petition challenging the election results.

Appendix Figure D.4 shows that our main estimates do not change when we control for a binary measure of exposure to information about these national-level events. The coefficients

[20]These questions were asked late in the survey after our main outcomes to avoid priming effects.

remain statistically significant and very similar in magnitude, despite the fact that the last wave of the survey is dropped from these analyses due to missing data. The fact that the coefficient on the dummy variable indicating knowledge of these national repression events is positive and significant is additional evidence that exposure to repression is associated with higher propensity to dissent.

Figure D.4: Effects of local repression and dissent events are robust to controlling for knowledge of national events



Could the effects of exposure to contentious events be confounded by exposure in the previous round? Appendix Figure D.5 shows that the effects of Local Dissent and Local Repression are robust to controlling for exposure in the previous round. In addition, they show that the effects of Local Dissent exposure persist into the subsequent round, while the effects of Local Repression do not.

Could the effects of exposure to contentious events be confounded by the constituency-specific electoral outcomes that were revealed between the first and second WhatsApp rounds? Appendix Figure D.6 shows that our main estimates do not change when we include a control in round 2 for whether or not ZANU-PF won the parliamentary seat, or whether or not the parliamentary election in that constituency was close (within 10 percentage points).

Figure D.5: The effects of exposure to local dissent persist into the following wave 2-4 weeks later

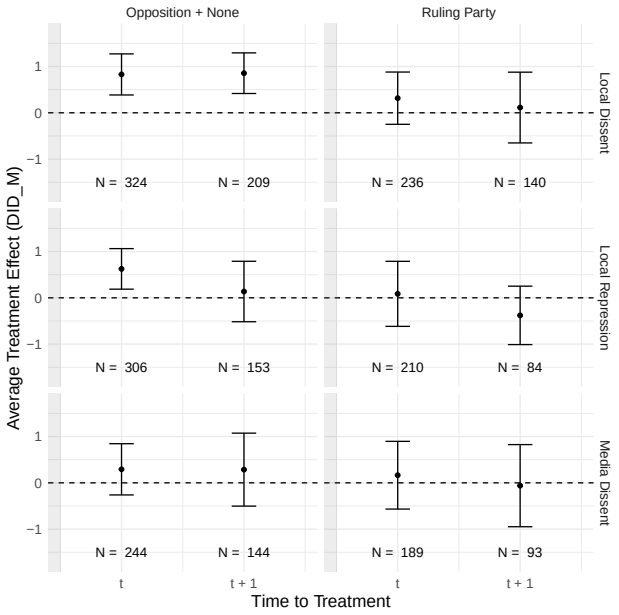
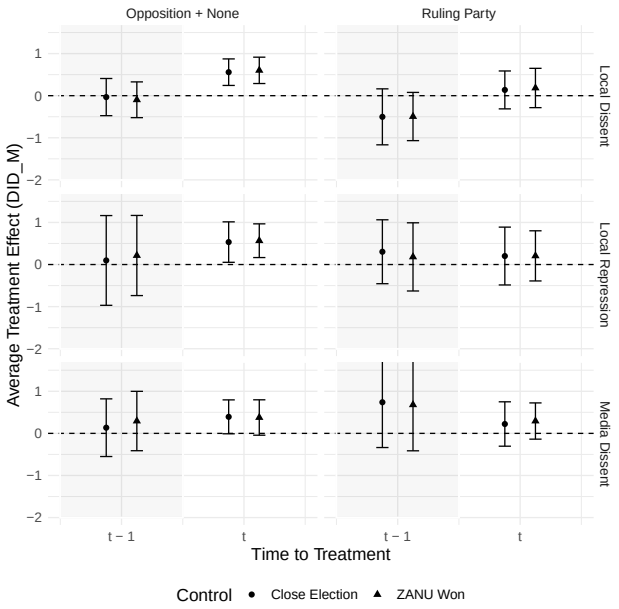


Figure D.6: Effects of local repression and dissent events are robust to controlling for constituency-level election results in round 2



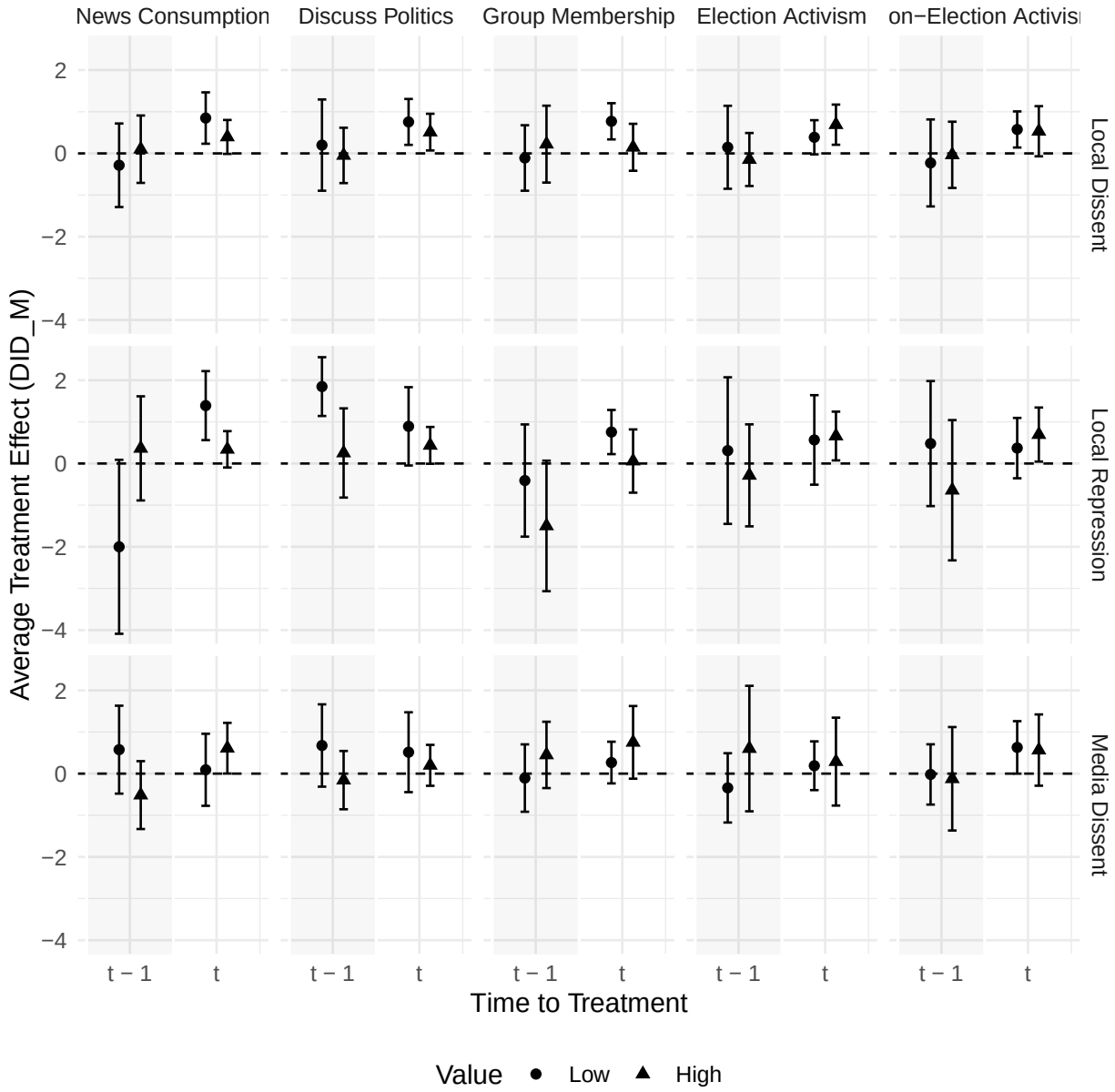
Finally, to assess the possibility that exposure to other types of individual-specific shocks that we cannot operationalize might be introducing bias into our results, we test whether the results seem to be concentrated among people who are most at risk of exposure to plausible alternative

treatments. As above, we are concerned with two main types of exposure: additional information on contentious events, and exposure to mobilization efforts that might be correlated with contentious events. We expect that people who at baseline say that they 1) get news more frequently or 2) discuss politics more frequently might be more exposed to additional information in a way that could be correlated with exposure to contentious events. If the results are driven by high-information people, we might be more concerned about omitted variable bias. We expect that people who at baseline are 1) participating in more community groups, 2) participating in more electoral activism, and 3) participating in more non-electoral activism might be more likely to be exposed to other mobilization efforts in ways that could be correlated with exposure to contentious events.

We split the opposition and unaffiliated sample at the mean levels of these variables to create similarly sized subgroups. This cutoff translates to getting news from 2.65 (out of 7 measured) news sources, membership or leadership in 1.12 (out of 7 measured) community groups, discussing politics slightly more than "a few times a month" (3.36 on a 5 point scale), participating in around 0.5 out of 5 forms of non-electoral activism in the past year, and participating in around 0.6 out of 3 forms of electoral activism around the 2018 election. On all of these variables, people coded as "high" might be more susceptible to experiencing other time-varying, individual-specific shocks that would be correlated with exposure to repression and dissent.

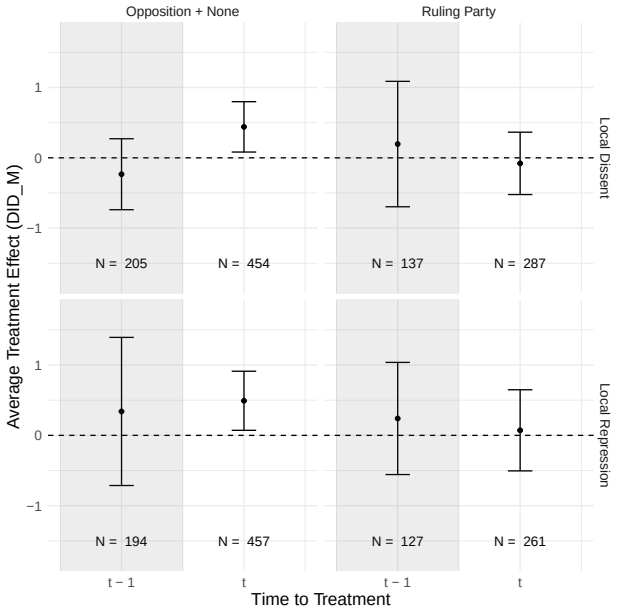
The estimates here are not precise as they require us to split our opposition and unaffiliated subgroup into even smaller samples to assess heterogeneity. In some cases the samples are too small to take all of the control variables into account. Nonetheless, they provide a test of whether there are large and systematic differences between the estimated ATEs between groups with varying sensitivity to the two types of confounding exposure that we are most concerned about.

Figure D.7: People more likely to be exposed to other mobilization efforts or information are not more likely to respond to contentious events



D.2.4 Robustness to recoding possible false positives on exposure

Figure D.8: Effects of local repression and dissent events are robust to recoding possible false positive exposures to repression and dissent

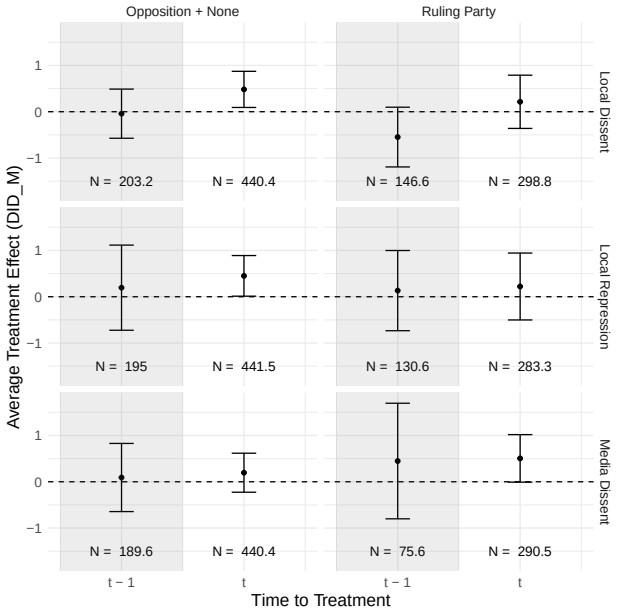


D.3 Robustness to sampling weights

To assess how our results might generalize into the Zimbabwean population more broadly, we use raking to re-weight the sample to match the 2018 Afrobarometer survey on demographic characteristics. Specifically, we re-weight the units in our panel to match the Afrobarometer in terms of urban status, gender, high school education, age (18-29, 30-44, 45+), and non-election activism (whether the respondent had done any of five types of non-electoral activism like contacting a representative). We do not reweight our sample based on things that typically change around election periods, including fear of election violence and party identification.

Figure D.9 shows that our results on local events for opposition supporters and unaffiliated voters are similar with the results presented in Figure 3 in magnitude and significance when we run the analysis on re-weighted data.

Figure D.9: Effects of repression and dissent events when the sample is reweighted to match the 2018 urban Afrobarometer sample on gender, high school education, age, and non-election activism



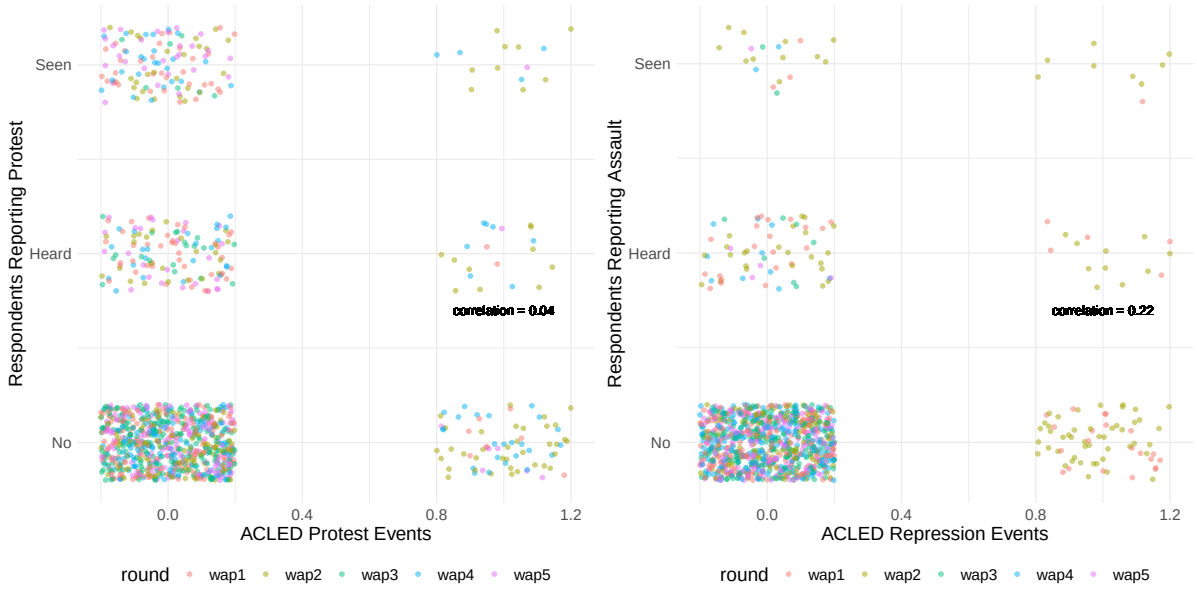
D.4 Comparison of survey exposure to ACLED data

How do our measures of self-reported exposure compare to news coverage of protest and dissent events? There is no gold standard measure of electoral repression and dissent, but some past research has used news and NGO reports coded by ACLED to identify similar concepts (Daxecker, 2012). ACLED does not try to capture all of the event types that we measure, but focuses on events that involve public violence or collective protest, making it most comparable to our measures of exposure to assault (repression) and protest (dissent). Our measure of assault correlates weakly ($\rho \in [0.1, 0.16]$) with the number of electoral repression events during the same time frame in ACLED, and our measure of protest exposure does not correlate at all with measures of the number of electoral dissent events in ACLED ($\rho \in [-0.02, 0.03]$).[21] Most of the disagreement comes from people in our data who say that were exposed but are in constituencies where there are no recorded ACLED events: 74% of disagreements in the case of repression events, and 88% of disagreements in the case of protest events. This pattern is in line with findings that ACLED under-reports electoral violence in rural areas and those that have not been violence hot spots in the past where journalists are less present (von Borzyskowski and Wahman, 2021).

Going forward, we rely on our survey measures of exposure for several reasons. First, they allow us to capture exposure to a broader spectrum of small and large-scale acts of repression and dissent (not only assault but threats, and not only protests but political speech and regalia), which we expect are meaningful as citizens make decisions about dissent (Bratton and Masunungure, 2012; Frye, Reuter and Szakonyi, 2019). Second, because we aim to separate out the effects of repression and dissent from each other, it is particularly important that we can measure each accurately. To the extent that the ACLED data has a number of false negatives, our estimates of the effects of repression and dissent would be confounded by each other.

[21] We calculate exposure to the ACLED events as being exactly within the respondent's constituency, or within 5-30 km of the respondents' constituency.

Figure D.10: Correlations between ACLED protest and violence events and survey reports



(a) Dissent Events

(b) Repression Events

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