Repression and Dissent in Moments of Uncertainty: Panel Data Evidence from Zimbabwe

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*S*tate repression and protest are common in modern authoritarian and hybrid regimes, yet individual responses to these events are not well understood. This article draws on unique panel data from the months spanning Zimbabwe's 2018 election, which we view as a moment of uncertainty for most Zimbabwean citizens. Using a difference-in-difference estimator, we estimate change in individual protest intentions following exposure to repression and dissent and we assess three individual-level mechanisms hypothesized to drive responses. We find evidence that exposure to local repression and dissent are mobilizing among opposition supporters and nonpartisans. Analysis of potential mechanisms suggests that the effects of exposure to dissent may be driven by information updating, whereas relational and emotional mechanisms seem to drive backlash against repression, despite increased perceptions of risk. We find no evidence of counter-mobilization by ruling party supporters, and little effect of exposure to contentious events over social media.*

INTRODUCTION

Despite significant gains in overall levels of democracy, the modal political regime today is characterized by popular mobilization, some degree of electoral contestation, *and* substantial restrictions on political rights and civil liberties (Alizada et al. 2021; Levitsky and Way 2010). In this set of regimes, repression is common, particularly when governments have doubts about the depth of their support and fear that protest might affect political change (Hafner-Burton, Hyde, and Jablonski 2014; Hendrix and Salehyan 2019). Events like elections, leadership transitions, wars, or economic crises can lead to "moments of uncertainty" for citizens, when there is higher than usual uncertainty about the balance of power between incumbents and their challengers (Hafner-Burton, Hyde, and Jablonski 2018; Howard and Roessler 2006; McAdam, Tarrow, and Tilly 2003; Przeworski et al. 2000).

In these contexts, citizens are often exposed to instances of protest and repression, and the ultimate outcome of competition between incumbents and opposition actors may remain uncertain for some time. Both regime signals and other events can lead citizens to dynamically update their assessments of the degree of political opening, the likelihood of regime change, and

personal beliefs and motivations (Kuran 1991; Pearlman 2016a; Weyland 2012). During these "moments of uncertainty," how do ordinary citizens make decisions about participating in protest, voting against the ruling party, or otherwise displaying preferences counter to those of the ruling coalition? Are individuals sensitive to new information about protests or instances of state repression? Does the risk of repression dissuade individuals from participating in protest, or might repressive action spur more resistance? And how might individual calculations be influenced by emotions and feelings of collective solidarity?

This article leverages panel data collected in Zimbabwe to examine individual decision-making in environments where both repression and potentially consequential dissent occur. Our sample focuses on urban respondents, who are generally more critical of entrenched incumbent parties and serve as a natural constituency for large-scale protest (Branch and Mampilly 2015; Harding 2020). Since 2000, Zimbabwe has experienced protracted political crisis coupled with high levels of partisan polarization. During this time, Zimbabweans have witnessed close elections, routine state-sponsored violence, and periodic large-scale street protests. Opposition politicians sit in parliament and call on their supporters to protest government actions, but these politicians and their supporters are also subject to arrest, torture, and disappearance. Our data were collected during a period spanning Zimbabwe's July 2018 presidential and parliamentary election, a speculated leadership struggle within the ruling coalition, and economic protests. The 2018 election was the first held in Zimbabwe without the participation of President Robert Mugabe, who held executive power in Zimbabwe from independence in 1980 to his removal by the military in November 2017. The degree of

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political opportunity varied during our data collection period. Mugabe's removal, his successor's expressed commitments, and the relative freedom of expression and opposition activity during the 2018 campaign period resulted in changes to Zimbabweans' views about the extent of political opening and the likelihood of state repression (IRI/NDI 2018; MPOI 2019). Political space opened in the run-up to the 2018 election, when we conducted our first rounds of surveying, but the election was then followed by a significant campaign of targeted state repression against post-election and economic protests. The context of our study thus captures many common characteristics of moments of uncertainty: a ruling party leadership transition, a contentious election, and deepening economic crisis.

We collected data on individuals' exposure to dissent and repression events, willingness to engage in protest and other relevant dissent behaviors, and beliefs and opinions via a face-to-face household survey and five rounds of follow-up surveys conducted via WhatsApp calls. This data collection method enabled us to collect frequent observations during a period of state repression and protest without putting our participants at unjustifiable risk. Many of our respondents witnessed dissent in their own communities and via social media both before and after the July 31st election. To different degrees, they also observed a largely unanticipated government crackdown in the post-election period. Our survey data capture exposure to large-scale violent events, which may be reported in the media, but also to the kinds of personalized threats and more subtle intimidation that can generate fear and influence dissent decisions (Bratton and Masunungure 2012; Frye, Reuter, and Szakonyi 2019). Our methodology allows us to track citizens' reactions to these events in real time, reducing the risk of measurement bias, and it enables us to test potential mechanisms that could drive the relationship between exposure to repression and dissent events and subsequent protest intentions. We divide mechanisms into three families that may jointly or individually link exposure to contentious events to changes in behavior: information on costs and benefits, affective polarization, and emotions.

We advance the empirical literature on repression and dissent in four ways. First, we study exposure to real, as opposed to hypothetical, repression and dissent events. Emotional or social-psychological mechanisms might be especially hard for participants to accurately predict and report in a hypothetical situation, so our approach enables a more reliable test than recent literature based on survey experiments. Our empirical strategy also enables us to assess mechanisms in real time, an advantage over observational studies based on recall of events months or years prior. Using new difference-in-difference methods, we can identify the immediate effects of exposure to contentious events during a period of uncertainty and test for their persistence some weeks later (de Chaisemartin and d'Haultfoeuille 2020; 2022). Third, while most observational research has only been able to observe one way that repression events are transmitted throughout the population—usually via social media networks or through

proximal events—we are able to measure and test for the effects of repression and dissent events transmitted over social media networks and through local exposure. This allows us to compare the relative effectiveness of these two forms of transmission. Finally, by measuring individual exposure to both repression and dissent events, we can do a better job of parsing out the differential effects of each.

Our analysis contributes to our understanding of dissent in four distinct ways. First, we provide evidence that individual exposure to both dissent and repression events increases willingness to protest for respondents who do not support the incumbent regime. Our evidence is consistent with both diffusion models of dissent and theories that emphasize the likelihood of backlash to repression. Our findings, therefore, add new empirical evidence in support of theories of protest cascades (Kuran 1991) and cut against recent evidence that potential dissidents are more likely to dissent when they learn that protests will be smaller than expected (Cantoni et al. 2019). Our evidence on the effects of repression is also consistent with recent findings that exposure to the levels of repression commonly used against nonviolent opposition in contemporary regimes is mobilizing for those who are indirectly exposed (Aytaç and Stokes 2019; Bautista et al. 2021; Pan and Siegel 2021; Steinert-Threlkeld, Chan, and Joo 2022). We do not find evidence of an "inverse-U" relationship between repression and subsequent dissent in our data. However, our results are not inconsistent with the idea that the effects of repression depend on its severity and the strength or resolve of the regime. Repression events that we observe in this case cluster at the lower end of the historical spectrum of state repression severity (Rozenas and Zhukov 2019; Zhukov 2023). But intimidation, arrests, and beatings have become the most common forms of state repression in hybrid regimes and electoral autocracies. Our finding of repression-mobilizing dissent is, therefore, likely to apply in a broad set of cases where more severe forms of repression, such as mass killing, are less common. To the extent that repression and dissent differ systematically during electoral periods (Bhasin and Gandhi 2013; Rød 2019), our findings may be most likely to generalize to contentious election periods, since much but not all of the repression and dissent we observe is related to Zimbabwe's 2018 election.

Second, our findings suggest that fundamentally different mechanisms underlie the diffusion of dissent and backlash against repression. We find that individuals update their beliefs in logical ways after observing others' dissent, but their emotional and relational states are largely unaffected. Exposure to repression, on the other hand, triggers emotional and relational responses that seem to outweigh individuals' beliefs about the increased risks of expressing dissent. The evidence that affective polarization increases for some respondents in response to repression events is particularly compelling given the pre-existing high levels of partisan and attitudinal polarization in Zimbabwe (Bratton and Masunungure 2018). Our findings are part of a recent literature emphasizing the effects of repression on

emotional reactions and on identities (Aytaç and Stokes 2019; LeBas 2011; Nugent 2020; Pearlman 2016b; Young 2019).

Third, our findings speak to a growing literature on supporters of ruling parties in nondemocratic regimes (Chenoweth and Stephan 2011; Weyland 2019). Our analysis of ruling party supporters highlights their overall stability in the face of contentious events: although ruling party supporters report exposure to repression and dissent events, we find limited evidence of behavioral or emotional responses. This is surprising, as ruling party supporters express strong sentiments against political opponents and might, therefore, be expected to counter-mobilize when witnessing dissent or to endorse their party's use of repression (Hellmeier and Weidmann 2020; Robertson 2010; Williamson and Malik 2021).

Finally, we find that the channel of exposure to repression and dissent matters. While individuals consistently react to local repression and dissent events, exposure to images or videos of events via social media is not associated with changes in beliefs, emotions, or identities. Without denying that social media can diffuse dissent or spur backlash against repression (Tufekci and Wilson 2012), this suggests that even individuals with access to social media respond more strongly to local events.

EXPECTATIONS FROM THEORY

The institutional characteristics of modern nondemocratic regimes create regular moments of uncertainty, when citizens are less certain about the character and relative strength of incumbents and opposition. The literature on social movements and democratization have identified both structural and strategic drivers of regime crisis, during which the choices of individual and collective actors become more consequential (Collier and Collier 1991; Tarrow 1994; Tilly 1978; Treisman 2020). These moments of contingency are often termed "critical junctures" or shifts in "political opportunity structure." We use "moments of uncertainty" to focus attention on how shifts in the broader political environment affect *individual citizens' expectations* of state strength and behavior, the resources or support of opposition actors, and the behavior of fellow citizens. As we discuss, changes in political context can trigger emotional and relational responses as well.

Elections, economic crises, and leadership transitions are common moments of uncertainty in hybrid regimes. Elections are regularly used by incumbents to legitimate their rule and to manage distributive conflict, thereby stabilizing authoritarian rule (Blaydes 2009; Gandhi and Lust-Okar 2009). But elections can be mishandled by incumbents or may occur in tandem with other developments that favor opposition, thereby providing greater opportunity for regime opponents (Greene 2007; Howard and Roessler 2006; Treisman 2020).

In this section, we generate theoretical expectations from the literature on two separate questions that are central to understanding political behavior during moments of uncertainty. First, how are individual decisions to protest affected by the dissent behavior of other citizens or by repressive actions by the state? Second, what mechanisms underlie individual decision-making? The literature addressing these questions includes research on social movements, the repression–dissent nexus, and the effects of violence on political attitudes. For exposure to repression and dissent, the expected effects depend on assumptions about which mechanisms are likely to dominate.

Effects of Dissent and Repression Events on Opposition Mobilization

Debate about individual protest participation has centered on differing views of the severity of the collective action problem. This results in contradictory expectations about the effect of exposure to protest on individual willingness to dissent. For those who see collective dissent as a public good, it is assumed that each individual prefers to benefit from the gains that collective protest might provide without bearing the costs of participation (Olson 1965). Especially where incentives to free ride are significant, as they are for larger groups or for individuals with weaker preferences for change, exposure to others' protest behavior might lessen one's own willingness to participate because others' participation substitutes for one's own effort. Repressive governments and politicians supportive of the status quo also devise strategies to exacerbate collective action problems (Lichbach 1995) or trigger the counter-mobilization of other groups of citizens (Hellmeier and Weidmann 2020; Weyland 2019).

A rival literature argues that dissent is contagious, even at the early stages before strong mobilizing structures and personal ties develop. In threshold models, individuals join collective action once the number of participants crosses a critical threshold defined by their beliefs about the number of others who will also participate (Granovetter 1978). In these theories, those who are exposed to dissent update their beliefs about the level of popular support for the regime (Kuran 1995; Lohmann 1994), the value they themselves attach to the status quo (Shadmehr and Bernhardt 2011), or the possibility of change (Weyland 2012). Witnessing others' dissent, in these accounts, makes individuals more likely to themselves express dissent.

The literature produces similarly conflicting expectations about exposure to repression. Repression can clearly reduce individuals' willingness to express dissent by increasing its perceived risks (Lohmann 1994; Shadmehr and Boleslavsky 2022). Even if one is not particularly risk-sensitive, repression may reduce participation by negatively affecting expectations about others' participation or the success of collective action. However, this view has been challenged by scholars emphasizing that dissent frequently increases in response to repression. Repression may generate backlash by deepening grievances (Gurr 1970; Opp and Roehl 1990), spurring outrage (Aytaç and Stokes

2019), or activating a group-based moral identity that compels participation (LeBas 2011; Pearlman 2016a). As with the literature on protest, there are strong theoretical explanations for effects in either direction.

Mechanisms Linking Contentious Events and Dissent

Much of the disagreement in the literature on whether repression should increase or decrease dissent has been driven by different assumptions about the mechanisms underlying responses to these contentious events. The classic literature on protest focuses on the strategic logic of collective action, in which beliefs about the likelihood of repression, the protest behavior of others, and the presence of political opportunities feed into calculations of the costs and benefits of participating in dissent (Kuran 1991; Lichbach 1995; Lohmann 1994; McAdam 1982; Tarrow 1994; Tilly 1978). These explanations rely heavily on expectations-based or *informational mechanisms*, in which assessments of risk and potential for success change dynamically in response to events. Informational updating has been particularly central in theories about exposure to dissent. For instance, the literature on protest cascades has argued that individuals who witness protest will expect more participation by others, more successful protest, or a lower likelihood of personally experiencing repression (Kuran 1991; Weyland 2012). Response to repression has also been theorized as working primarily through informational signals. In the wake of repression events, individuals might adjust downward their expectations about others' dissent and adjust upward the riskiness of their own dissent (Opp 1994; Shadmehr and Boleslavsky 2022).

Other explanations are, therefore, necessary to explain backlash protests. One approach, which can be termed relational, focuses on the impact of collective identities and network structures on the expression of dissent. Relational mechanisms emphasize that individuals' connections and perceived ties to one another can be impacted by exposure to protest and repression. Exposure to protest appeals and framing can generate new identities, reinforce existing collective solidarities, or make participation in activism a more central component of one's identity (Polletta 2009; Polletta and Jasper 2001; Simon and Klandermans 2001; Van Zomeren, Postmes, and Spears 2008). Others suggest that exposure to dissent can activate group loyalties or a group-based moral reasoning that feeds into or may even override cost–benefit analysis (LeBas 2011; Pearlman 2016a). These theories imply that in-group affinity should increase after protest. The relational literature on the effects of repression also largely predicts that exposure to repression should boost dissent by reinforcing group identities. In a previous cycle of repression and opposition mobilization in Zimbabwe, state repression reinforced opposition activist commitment, generated stronger partisan identity among opposition supporters, and made it difficult for others to remain neutral (LeBas 2006; 2011). Deeply embedded affective ties can lead individuals to discount risk, sustaining

mobilization even when the expected cost is high (Gould 1991), but there is evidence that similar effects exist even with minimal groups in the short term (Nugent 2020). For both repression and dissent exposure, we expect that increases in affective polarization would be driven more by increases in in-group affinity, but it is also plausible that out-group animus would increase (Iyengar et al. 2019).

A final approach stresses emotional mechanisms as a driver of dissent. Like identities and networks, emotions may have direct effects on dissent or they may change the way that individuals perceive and process information or the strength and salience of their social ties. Seeing others dissent might generate positive emotional reactions, such as "joy in agency," pride, or satisfaction (Jasper 1998; Wood 2003). Scott (1990) argues that it is precisely these emotions of "personal release, satisfaction, pride, and elation," and not the "novelty as information" communicated through dissent, that leads to cascades of participation and political breakthroughs (207–8). Repression may also produce strong emotional responses, though different emotions may have countervailing effects on behavior. Anger or moral outrage against repression can spur new dissent (Aytaç and Stokes 2019; Gurr 1970; Jasper 2014; Young 2020); however, when the emotional reaction to repression is dominated by fear rather than anger, it can dampen future dissent by making citizens pessimistic and risk averse (Young 2019). The existence of a moment of uncertainty, during which priors may be weaker, is a scope condition for our theory.

Our article suggests that the effects of events should be conditional on citizens' prior beliefs and identities. Whether or not protest exposure is associated with joy and heightened expectations of others' dissent, for instance, depends on the individual's prior expectations of the likelihood of protest or expected protest size. During moments of unambiguous regime strength, citizens' prior beliefs about the costs and futility of protest are likely stronger, and these appraisals should also shape emotional and relational reactions to protest and repression events. We expect that individuals' established beliefs weaken during moments of uncertainty, making them more likely to respond to new information about others' protest behavior or about the character of the regime.

We view these three families of mechanisms as operating together to produce changes in willingness to dissent rather than as alternative drivers of behavior. An individual's willingness to update beliefs in light of new information or exposure to events may be mediated by their identities or affected by emotions. We expect that the updating of prior beliefs in light of events is tightly interwoven with emotional responses and affective loyalties.

Effects on Ruling Party Supporters

Protest research has traditionally focused on citizens who are critical of the incumbent regime and must decide whether or not to express their negative views (Kuran 1991; Scott 1990). We follow in this tradition

and our primary analyses will be based on a sample of opposition and unaffiliated voters. However, the actions of regime supporters are also important during moments of uncertainty (Chenoweth and Stephan 2011). Ruling parties in hybrid regimes need large numbers of supporters to turn out during elections and to counter-mobilize during mass protests (Hellmeier and Weidmann 2020; Robertson 2010). The reactions of incumbent supporters to repression and dissent events have generally been understudied, and we, therefore, present results for this group of respondents as well. To the extent that we see the same patterns in the sample of regime supporters as we do among others, it could be interpreted as evidence against our theory.

How might ruling party supporters react to repression and dissent events? Existing research finds that opposition mobilization often hardens attitudes toward the opposition, causes counter-mobilization, and increases support for a crackdown (Weyland 2019). While it is theoretically plausible that ruling party supporters could be persuaded by expressions of dissent, this seems unlikely in contexts with high levels of identity-based or partisan polarization, where opposition protest can be framed as illegitimate (Edwards and Arnon 2021; Manekin and Mitts 2022).

The expected responses of ruling party supporters to repression are less clear. Much of the literature on repression and electoral violence presumes that these tactics are relatively costless when it comes to supporters (Collier and Vicente 2012; Hendrix and Salehyan 2019), particularly when government narratives present the targets of repression as threats or highlight the benefits of repression to the in-group (Lynch 2014; Williamson and Malik 2021). Yet survey-experimental work has found that politicians' use of violence has audience costs, even among supporters (Gutiérrez-Romero and LeBas 2020; Rosenzweig 2021). The combination of these effects may leave ruling party supporters feeling cross-pressured after state repression (Curtice 2021; Curtice and Behlendorf 2021).

REPRESSION AND DISSENT IN ZIMBABWE

Zimbabwe gained independence in 1980 after a 15-year armed liberation struggle against a white minority government. Since independence, the ruling Zimbabwe African National Union—Patriotic Front (ZANU-PF) has dominated politics, partly through the consistent use of violence against political opposition (Kriger 2005). From 1983 to 1987, as many as 20,000 civilians were killed in western Zimbabwe in a military operation against what the ruling party described as insurrection by its main opposition party rival (CCJPZ 1997). In the early 2000s, a new opposition party, the Movement for Democratic Change (MDC), built a sizeable following, and ZANU-PF responded by deploying state security forces and party-linked militia to attack MDC activists and presumed supporters (LeBas 2006; 2011; Sachikonye 2011). Violent land invasions were part of this strategy. Violence again

peaked in the run-up to a presidential election run-off in 2008, from which the opposition withdrew. Since 2008, the opposition MDC has lost public support and has been generally out-campaigned by ZANU-PF (Beardsworth, Cheeseman, and Tinhu 2019; LeBas 2014; Tendi 2013). Yet the expression of dissent has remained costly for much of this period: protesters were beaten and jailed during 2016–17 mass demonstrations against the rising cost of living, and opposition politicians and activists face legal harassment, arbitrary detention, and physical violence.

In November 2017, President Robert Mugabe was removed in a military coup. Though Mugabe had held executive power since independence, both opposition and ruling party supporters cheered the coup. In the days after the coup, ZANU-PF formally impeached Mugabe, expelled 20 of his most prominent supporters from the party, and named former Vice President Emmerson Mnangagwa, who had been recently dismissed from office by Mugabe, as head of state. Mnangagwa appointed many long-standing ZANU-PF military leaders to key positions of power, including selecting Commander of the Zimbabwe Defence Forces Constantino Chiwenga as Vice President. Yet in his first address, Mnangagwa promised a break from the Mugabe period and announced that Zimbabweans were "witnessing the beginning of a new unfolding democracy." Mnangagwa also invited the European Union and the Commonwealth to observe the July elections, stating that he wanted "free, fair, and credible elections."[1] In subsequent months, Mnangagwa reassured both international and domestic audiences that economic and political reforms were underway and, after years of international sanctions under Mugabe, that Zimbabwe was "open for business" (Beardsworth, Cheeseman, and Tinhu 2019).

The run-up to the 2018 harmonized elections was characterized by political opening. The MDC-A and other opposition parties were able to campaign openly, even in rural areas that had previously been dangerous. Election observers determined that there was "only sporadic interparty violence from November 2017 to July 2018... the once-prevalent targeting and arrest of activists was curtailed sharply" in comparison with past elections (The Carter Center 2020, 30). Consistent with his messaging in the immediate postcoup period, Mnangagwa ran a campaign that stressed discontinuity with the Mugabe period and marketed himself as a candidate of change (Beardsworth, Cheeseman, and Tinhu 2019). Between November 2017 and July 2018, many Zimbabweans recognized this political opening. For instance, Beatrice Mtetwa, a human rights lawyer who had been repeatedly detained during the Mugabe period, said in 2020 that "everybody thought [in the first half of 2018] that no one would take us down the

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[1] "Zimbabwe's president seeks to build ties with the West," *Financial Times*, January 18, 2018. The EU, the Carter Center, and the International Republican Institute (IRI) would all ultimately send delegations.

Mugabe road again."[2] Public opinion data from this period also show that fear of political violence dropped significantly through mid-2018. A large majority of Zimbabweans (63% in May and 68% in July) said that the government was performing well in preventing violence (MPOI 2019).

The results of the July 31st election were the closest since the 2008 election, in which the opposition won a parliamentary majority and (in all likelihood) the presidency in the first round. In 2018, Mnangagwa won 51.4% of the presidential vote to 45.1% for the MDC-A's Nelson Chamisa; the MDC-A gained an additional 16 seats in the National Assembly and 2 in the Senate, giving them 33% of the seats in the assembly and 42% in the senate. This was a respectable result for the MDC, which was riven by factionalism and had struggled to rebuild its grassroots structures after the 2008 repression (LeBas 2014). The party also made gains in parts of Zimbabwe where campaigning had previously been difficult, with Chamisa outpolling Mnangagwa in several rural districts that had previously voted ZANU-PF by significant margins.

Two days after the election, this political opening sharply closed. The MDC-A gathered supporters in Harare to protest alleged irregularities in the presidential vote count and a premature announcement of ZANU-PF victory. The military opened fire on unarmed protesters and bystanders, killing seven, and videos of the events circulated on social media. Though ZANU-PF announced that the shooting would be investigated, it also launched a broader campaign of targeted repression. In the weeks following the election, local NGOs documented a wave of arrests, beatings, and abductions of MDC activists, perceived opposition supporters, and civil society figures.[3]

By the fall of 2018, the economic effects of the flawed election were beginning to materialize. Without a clean election to legitimate the Mnangagwa government, donors and investors continued to hold back significant international financing and debt relief. Food prices began increasing and currency shortages created fuel and medicine shortages.[4] Economic protests, mostly organized by civil society groups, students, and worker organizations, began popping up around the country in October and November.

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[2] "Nobody thought Mnangagwa would be this ruthless—Mtetwa," *The Zimbabwean*, August 20, 2020. https://allafrica.com/stories/202008260242.html.

[3] See, for example, report by the Zimbabwe Human Rights NGO Forum, *The Guardian*, and the Zimbabwe Association of Doctors for Human Rights (Zimbabwe Human Rights NGO Forum 2018; Zimbabwe Association of Doctors for Human Rights 2018). "Zimbabwe opposition face wave of detentions, beatings after election loss," *The Guardian*, August 5, 2018. https://www.theguardian.com/world/2018/aug/05/zimbabwean-opposition-reports-human-rights-abuses.

[4] "Zimbabwe's economic crisis will deepen without aid, ruling party warns," *The Guardian*, October 19, 2018. https://www.theguardian.com/world/2018/oct/19/zimbabwe-needs-aid-to-prevent-further-crisis-warns-ruling-party.

METHODOLOGY

We use panel data collected through frequent surveys with a sample of Zimbabwean citizens between July and November 2018 to test for the relationships between exposure to repression and dissent events and subsequent intentions to protest. We recruited participants into the panel using a face-to-face representative survey of mostly urban Zimbabweans. We then conducted follow-up surveys in the weeks spanning the election and subsequent repression. These interviews were conducted on voice calls via WhatsApp, a widely used cellphone application that enables encrypted communication. This strategy enabled us to measure over time individual-level exposure to repression and dissent, intentions to engage in dissent, and emotions, identities, and beliefs. In this section, we describe each of these elements and discuss two types of selection, as well as ethical considerations.

We recruited participants into the panel from a face-to-face household survey with a sample of 928 Zimbabweans. Our sampling frame included all 10 provinces of Zimbabwe, but it overrepresented urban areas and excluded rural areas far from the urban center in each province to manage costs and security and increase the proportion of our sample that would have access to WhatsApp. We then drew a multistage, stratified random sample. Our baseline survey is thus representative of each province's largest urban area and the rural constituencies within 100 km.

We then used WhatsApp voice calls to conduct the subsequent waves of our panel survey. Because WhatsApp is encrypted, it enabled us to protect the confidentiality and safety of our respondents with a reasonable degree of certainty.[5] At the end of the face-to-face survey, participants who reported that they possessed a phone that could access the Internet were invited to opt into the WhatsApp panel.

Participants in the WhatsApp panel were called five times after the face-to-face survey. Each WhatsApp wave was carried out over a period of 4–6 days by five surveyors managed by the Mass Public Opinion Institute (MPOI), which had also conducted our baseline survey. Participants were able to schedule times for the calls and participants who could not be reached were attempted multiple times. The first WhatsApp wave ($N = 234$) occurred from July 25–29, in the week before the election. The second occurred from August 9–12 ($N = 229$), just over a week after the election. The final three rounds occurred at monthly intervals from September to November 2018 ($N = 242$, 223, and

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[5] In Zimbabwe, the government has broad authority to surveil domestic communications, and cellular calls by some critics of the ruling party have been tapped. Due to WhatsApp's encryption, WhatsApp voice calls cannot be tapped using standard methods. The Zimbabwean government has tried to shut down Internet access during sensitive periods and has threatened to prohibit the distribution of encrypted communication channels (Zimbabwe Human Rights NGO Forum, et al. 2016). While there have been reports that the Israeli firm NSO Group sold spyware to Zimbabwe, the Mnangagwa regime is not known to be using the NSO product that can capture and transmit communications before they are encrypted.

219, respectively). As in the baseline survey, interviews were carried out in English, Shona, or Ndebele. Participants were provided with monthly data credit worth $3 to enable them to participate in the call and compensate them for their time.

Analysis of Two Types of Selection Effects

There are two types of selection effects that could affect the validity of our analysis. First, the baseline survey is not representative of the overall Zimbabwean population and only respondents with Internet-accessible phones were invited to join the WhatsApp panel. To what extent might this affect the external validity of our results? Supplementary Table B.1 shows that our panel sample is similar demographically to the urban participants of the nationally representative 2018 Afrobarometer survey. Importantly, our sample is not more politically active or less afraid or less exposed to political violence than the average urban citizen.

Second, differential selection into particular WhatsApp waves could threaten the internal validity of our results. This selection might occur if, for instance, an individual became less willing to participate in a round after witnessing repression. If selection into specific waves were correlated with exposure to repression and dissent events, it could bias our estimates. We test for selection by analyzing the correlates of attrition from specific WhatsApp waves. In each wave, the participation rate was between 71% and 79% of participants, representing between 219 and 242 people. Supplementary Table B.2 shows that the strongest predictors of attrition are being young and having fewer assets. Baseline measures of fear of election violence and political participation do not predict attrition, nor do lagged exposure to repression and dissent or dissent intentions. To assess sensitivity of our results to both types of selection, in section D.3 of the appendix, we show that our results are substantively similar when we reweight the data based on observable characteristics, including age, urban–rural status, and nonelectoral activism, to resemble the full 2018 urban Afrobarometer sample.

Practices to Adhere to Ethical Principles

Adhering to the principles of ethical research, particularly protection of participants and research staff, was a first-order methodological concern. Even though the 2018 pre-election period was less violent and more open than previous elections, there is a history of targeted repression in Zimbabwe. In designing our study, we assessed the risks that participants and staff might face, designed a methodology to minimize or avoid those risks, and established procedures for monitoring risks during study implementation. We briefly describe how ethical considerations shaped our research design here and provide a more comprehensive discussion in section C of the Supplementary Material.

We were most concerned that a breach of confidentiality could lead to retribution by the government or its supporters. We developed a methodology to conduct follow-up surveys using encrypted WhatsApp voice calls to minimize risks to surveyors and respondents. This method meant that our surveyors could work from the safety of their office or homes, even during violent periods. It also greatly increased our confidence that our surveys would be confidential, as we assessed that it was very unlikely that the government could tap WhatsApp voice calls.[6]

We also designed our questionnaire to avoid putting respondents or surveyors at risk if a breach of confidentiality occurred. We determined that asking about actual protest participation presented risks for both respondents and enumerators, especially if repression escalated during the baseline survey. We therefore measured our outcomes of interest by asking respondents to estimate how likely they are to participate in acts of dissent instead of asking them whether or not they actually did so.

Finally, we monitored the incidence of realized risks 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.[7] The fact that our participation rate remains stable across the five WhatsApp waves suggests that respondents continued to feel comfortable answering questions.

Analysis

These data enable a difference-in-difference analysis where we compare the change in outcomes of people who were just exposed to repression or dissent to people who were not. In other words, we estimate the effects of exposure to repression and dissent by exploiting variation across individuals in exposure to events during a particular panel period.

The standard estimator for a difference-in-difference design is the coefficient on exposure from a two-way fixed effects specification. However, recent advances in research methods have shown that the two-way, multiperiod fixed effects estimator β_{FE} is biased in the presence of heterogeneous treatment effects, as in our study where exposure to events, individual-level characteristics, and time periods probably interact. β_{FE} is a weighted average of various pairs of groups that overweights units with more variance in treatment status. In the presence of treatment effect heterogeneity, some units get negative weights (Goodman-Bacon 2021). Much of the discussion on correcting for this has focused on an absorbing treatment design, but de

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[6] See further discussion in section C of the Supplementary Material.
[7] 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.

Chaisemartin and d'Haultfoeuille (2020; 2022) have developed an estimator that is robust to heterogeneous treatment effects and controls. This is the estimator we adopt:

$$DID_M = \sum_{t=2}^{T} \sum_{d_{-1} \in \{0,1\}^{K-1}} \left(\frac{N_{1,0,d_{-1},t}}{N_S} DID_{+,d_{-1},t} + \frac{N_{0,1,d_{-1},t}}{N_S} DID_{-,d_{-1},t} \right).$$

This estimator captures the average treatment effect across groups whose treatment status changes (either by receiving or losing one of the treatments) between periods t and $t-1$. Groups are defined as having the same treatment status on other treatments d_{-1}.[8] de Chaisemartin and d'Haultfoeuille (2020) also propose diagnostics to assess the risk of bias in β_{FE}, which involve identifying comparisons that receive negative weights and assessing the amount of heterogeneity that would be necessary to flip the sign of the β_{FE}. The proportion of groups that receive negative weights in the standard fixed effect estimator across our four different treatments varies from 1% to 33%. Across all four treatments, there is evidence that treatment effects vary systematically across groups and over time. Based on the results of these diagnostics, we conclude that β_{FE} may be biased and, therefore, use the DID_M estimator.

Importantly, both β_{FE} and DID_M rely on the common trends assumption, meaning that treatment and control units must not be on different pretreatment trajectories and must not be subject to additional time-variant shocks at the moment of treatment. We, therefore, test for pretreatment differences between units that do and do not receive treatment. Across our nine substantive outcomes (one measure of dissent intentions and eight measures of mechanisms), we find some evidence of pretrends for exposure to repression via social media. There is no evidence of pretrends for our other three forms of exposure to repression and dissent (local repression, local dissent, and media dissent), suggesting no selection into exposure to these events. Because the diagnostic suggests that there is selection into exposure to repression events via social media, we present this analysis in section D.1 of the Supplementary Material rather than in the main paper.

EMPIRICAL EVIDENCE

Exposure to Repression and Dissent and Subsequent Dissent Intentions

Descriptive Analysis

Before presenting estimates of the relationship between exposure to repression or dissent events and subsequent willingness to dissent, we describe our

strategy for measuring outcomes and present some descriptive evidence. We conceptualize dissent as visible expressions of opinions that are at variance with those sanctioned by authorities or, in this case, the ruling party (Paluck and Green 2009). We are interested in both high- and low-risk forms of dissent, as even small expressions of opposition can be meaningful signals (Scott 1990). We measure two types of exposure to repression and dissent: first, events that a respondent saw or heard about in their own community, which we term *local exposure*; and second, events that they found out about via social media, which we call *media exposure*. Our survey questions about local dissent asked whether the respondent had seen or heard about people in their community wearing opposition party regalia, speaking freely about their political beliefs, and engaging in acts of protest like a demonstration or stay-away. The questions about media exposure ask whether respondents had seen a video or photograph of people engaging in the same three types of dissent. Figure 1 plots the percentage of respondents reporting any exposure to each type of event with 95% confidence intervals.

The top panel of Figure 1 shows that our survey data on dissent events show a large spike in exposure to dissent before the election, followed by a drop in the post-election period when the ruling party began its repression campaign. Following this period of repression, the measures of local exposure to dissent largely bounce back to near their preelection high, especially speaking freely about politics and wearing opposition party regalia. Exposure to dissent via videos and images steadily drops from a preelection peak. In this and subsequent analyses, we use binary versions of these variables that take a value of 1 if a respondent was exposed to any of the three types of dissent. We split the sample based on respondents who at baseline told us that they supported the opposition (overwhelmingly the main opposition party MDC Alliance), the ruling party or presidential candidate, or no party and no presidential candidate.[9]

We conceptualize repression as violence or the threat of violence that is intended to punish or disincentivize dissent. The survey questions about local repression exposure asked whether people in the respondent's community had been threatened or intimidated, experienced physical violence like assault, or lost business or had property destroyed because of their political beliefs.[10] The questions about media exposure

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[8] Controls for other time-varying confounders can be considered as other treatments.

[9] We use three questions to assign people to party ID: a question measuring closeness to party, and two questions measuring the presidential candidate for whom the respondent intended to vote.

[10] We did not ask respondents to identify the perpetrator of the violence or intimidation that they reported. However, data from the domestic human rights monitor the Zimbabwe Peace Project show that 83% of identified perpetrators of violent events during our panel were ZANU-PF affiliates, and 85% of identified victims were opposition affiliates. Most violence perpetrated by the opposition during this period was associated with an intraparty conflict over the leadership of the party.

FIGURE 1. Percentage of Respondents Reporting Any Exposure to Contentious Politics Events and Media by Baseline Partisanship
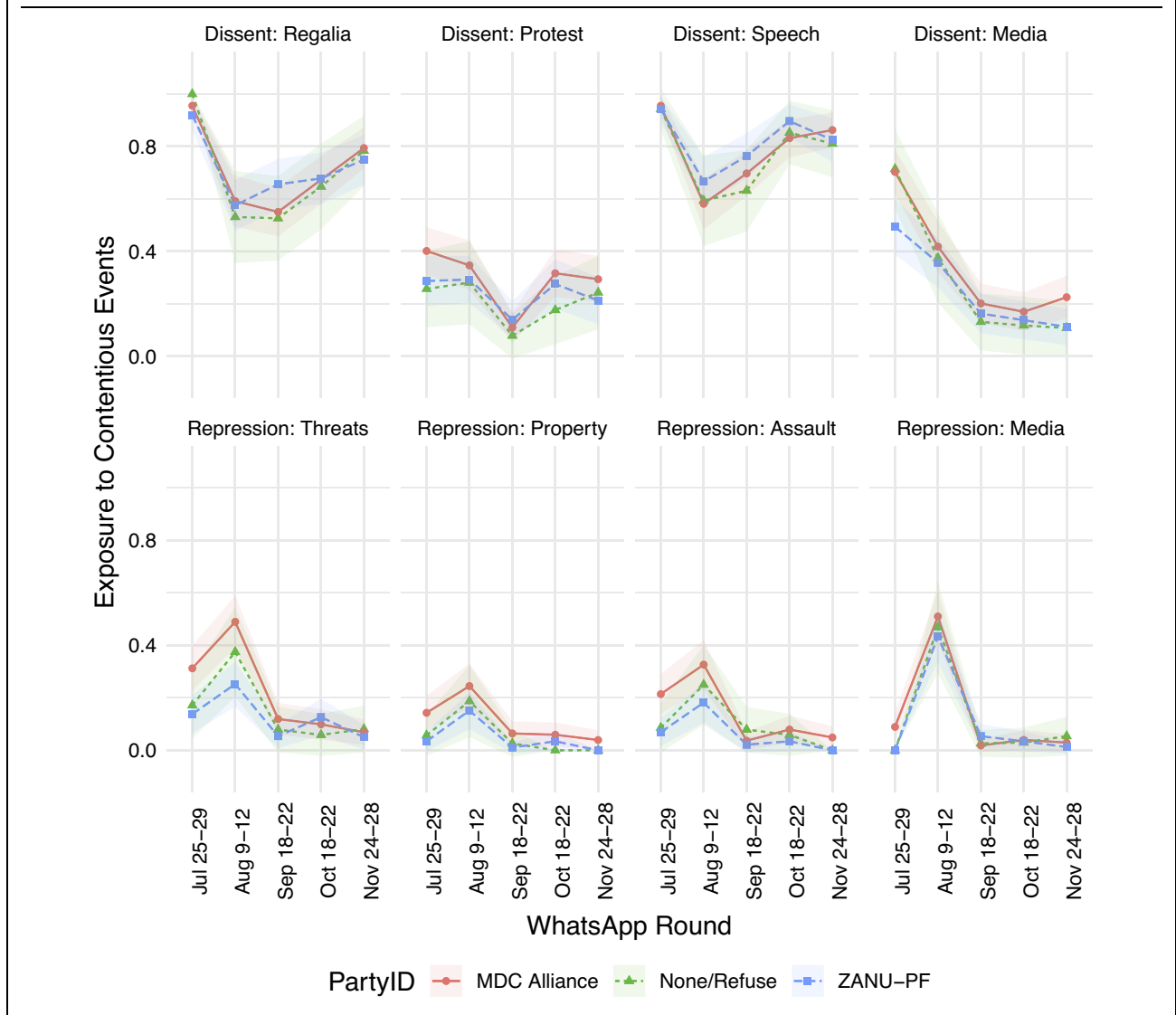
ask whether respondents had seen a video or photograph of people experiencing the same three types of repression.[11]

The bottom panel of Figure 1 shows that there is variation in exposure to repression within all partisan categories. Repression exposure is highest in the immediate post-election period, as expected. In the week before the election, approximately 23% of the total sample reported hearing or seeing acts of intimidation, 9% reported hearing or seeing property loss, and 14% reported hearing or seeing assault in their community. Exposure to repression spikes immediately after the election, when a full 38% report intimidation, 26% report property loss or damage, and 20% assault in

their community. Again, we see considerable variation in exposure to dissent events within all partisan groups, and differences across groups are often quite small.

We map the geographic distribution of repression and dissent in Figure 2. Constituencies in our sample are shaded to reflect the proportion of respondents in a constituency who say that they have been exposed to at least one form of local dissent (Figure 2a) or repression (Figure 2). The cities of Harare and Bulawayo are displayed as insets. Figure 2a shows that exposure to dissent was universally high before the election (top left map), and then dropped during the two post-election waves across many constituencies. Figure 2b shows that exposure to local repression was most common in areas that have historically been more violent, such as the Mashonaland provinces in the northeast, Manicaland province in the east, and in Harare.

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[11] Section A of the Supplementary Material includes the full text of the questions that we used to measure these different forms of exposure to repression and dissent.

FIGURE 2.   Geographic Distribution of Exposure to Local Contentious Events by Event Type
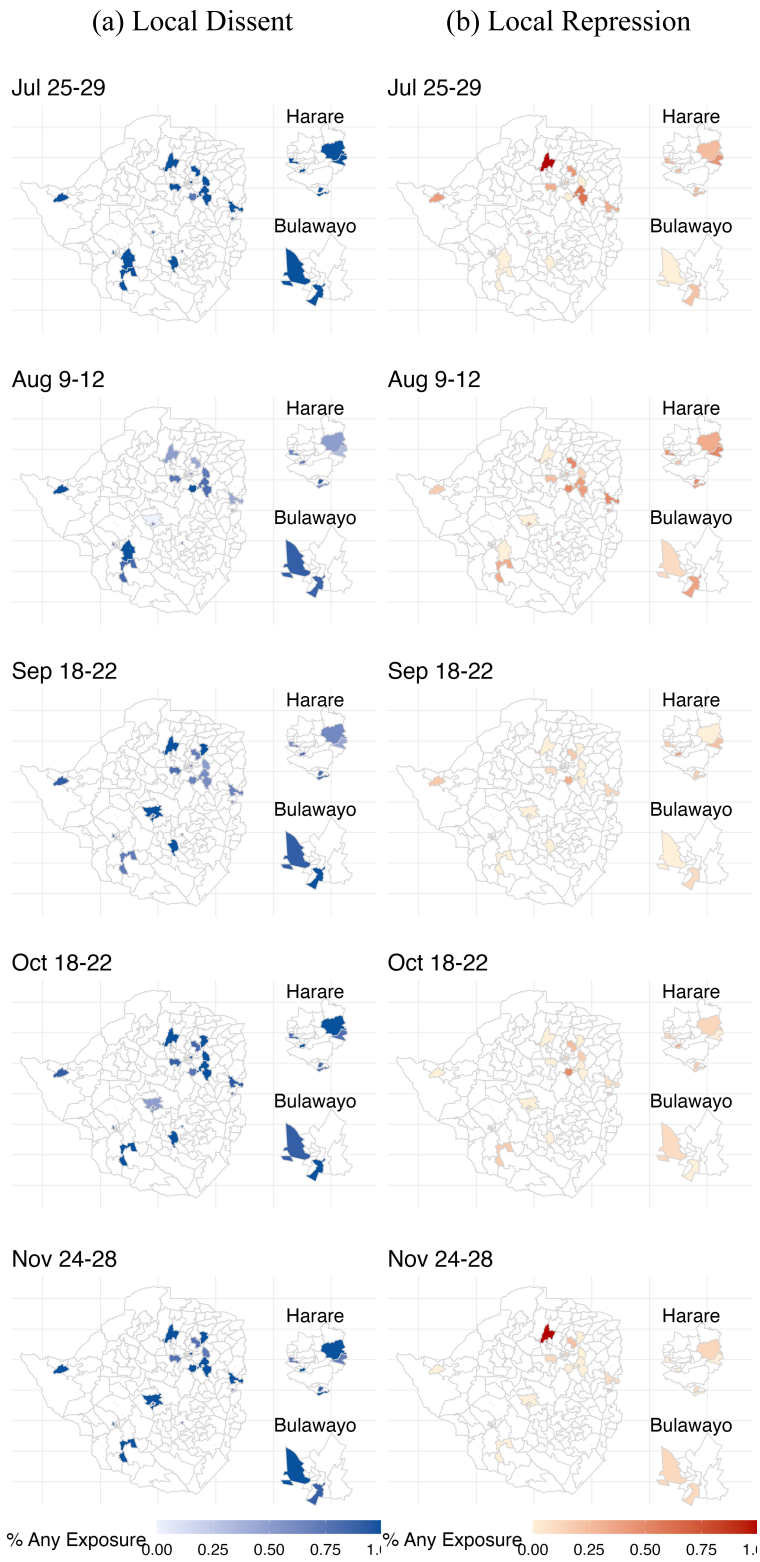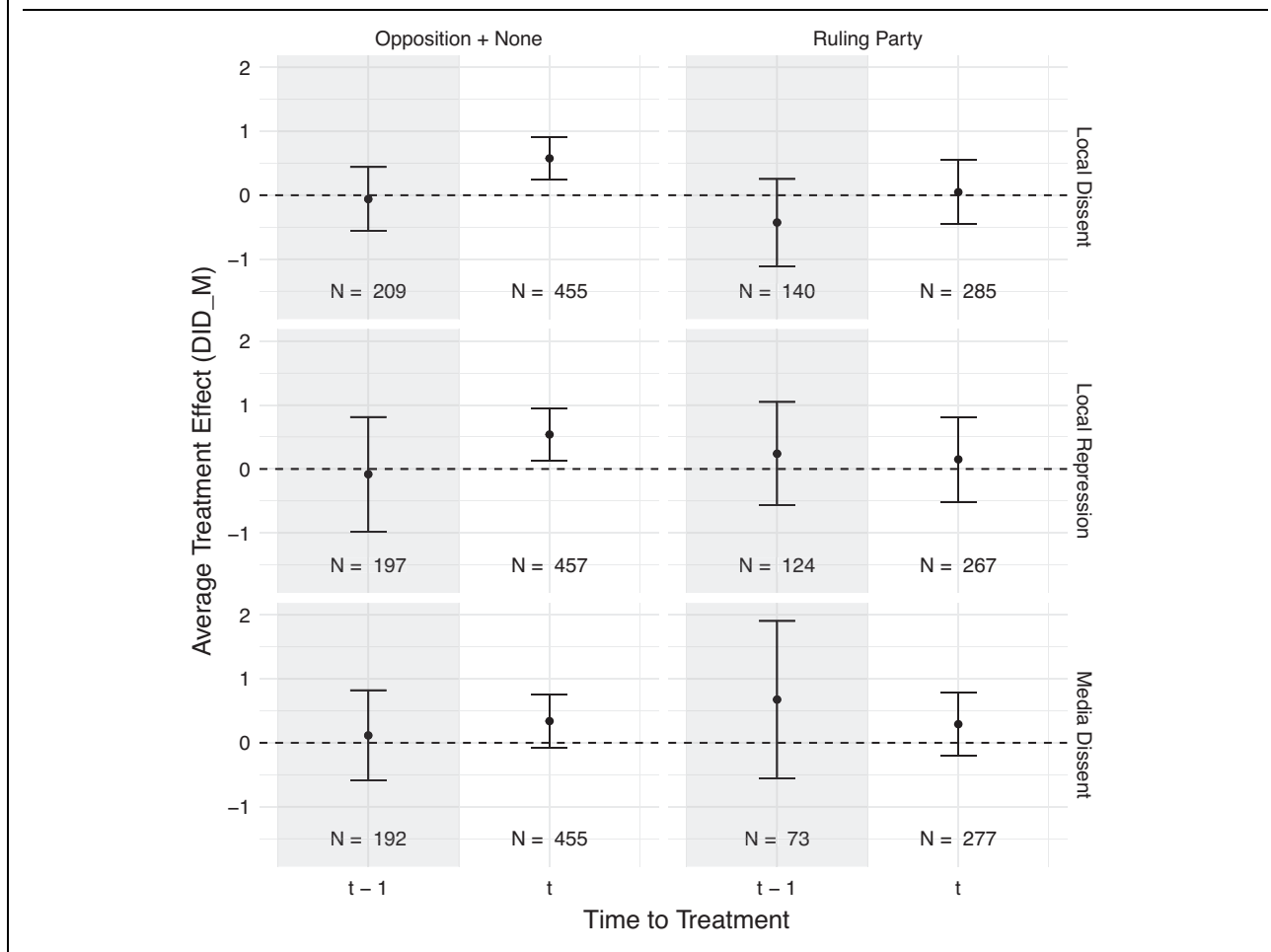
FIGURE 3. Repression and Dissent Events Increase Intentions to Protest for Opposition and Unaffiliated Voters

Difference-in-Difference Analysis of Willingness to Dissent

What is the effect of exposure to repression and dissent on subsequent willingness to engage in protest? Our outcome is measured using a question that asks respondents to report how likely they are to participate in a protest or stay-away about a service that they care about on a four-point likelihood scale.[12] Asking about willingness at the moment of the survey provides us with temporal precision, as we know that exposure reported in each round occurs prior to our measure of protest intentions. Our independent variables of interest are binary indicators of local exposure to any of the three repression or dissent events and binary indicators of exposure to images or video of any of the three dissent events shared over social or traditional media.

Figure 3 presents the difference in the protest intentions index for people who support the opposition or no party (left panel) and people who support the ruling party ZANU-PF at baseline (right panel). The first coefficient for each panel, shaded in gray, shows the effect of the treatment in $t-1$, the period immediately before exposure, as a test of the common trends assumption. The second coefficient shows the DID_M estimate of the effect of exposure, starting with Local Dissent events in the top row down to Media Dissent in the bottom. Estimates are presented with 95% confidence intervals. Because of evidence of pre-trends on exposure to Media Repression (see section D.1 of the Supplementary Material), we do not present estimates of the effects of Media Repression on our outcomes of interest. However, it is included in other specifications as a control variable.

Figure 3 shows first that there is little evidence of pretreatment differences between individuals who were and were not exposed to repression and dissent for Local Dissent, Local Repression, and Media Dissent. Estimates of the effect of treatment in $t-1$ are all close to zero, although imprecisely estimated, in the opposition and unaffiliated sample. In the smaller

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[12] We also measured two other outcomes that are clear measures of dissent for opposition supporters: intentions to join the Facebook group and intentions to wear the regalia of the political party that you support. Results on these additional outcomes are largely consistent with the results on protest intentions and are presented in section D of the Supplementary Material.

ruling party sample, we also fail to reject the null hypothesis that there are no pre-trends.

Substantively, Figure 3 shows that exposure to local dissent is associated with increased intent to protest in the sample of opposition supporters and nonpartisans. Exposure to at least one local dissent event is associated with a statistically significant 0.58 point increase on a four-point scale in protest intentions among opposition supporters and unaffiliated voters. Exposure to media dissent is associated with an increase in protest intentions, but it is not statistically distinguishable from zero. Exposure to local repression is also associated with a significant 0.54 point increase in protest intentions in the sample of opposition supporters and nonpartisans. For context, the mean reported likelihood of attending a protest among opposition and unaffiliated voters who were not exposed to any repression or dissent is 0.75 on this scale, or just below "a little bit likely." These effects are largely consistent across the different types of dissent intentions that we measured (Supplementary Figure D.2).

How do these effects change over time? With only five rounds of data collection, our ability to test for long-term effects is limited. However, we do have sufficient variation in exposure to test whether the effects continue or dissipate for one additional wave, 2–4 weeks after exposure. Supplementary Figure D.5 shows that the patterns of persistence for repression and dissent exposure are somewhat different. Exposure to Local Dissent persists in significance and magnitude into the next wave several weeks later. By contrast, there is no detectable effect of exposure to Local Repression in the following wave.

For ruling party supporters, there is little evidence that exposure to repression or dissent events is associated with changes in dissent intentions, as shown in Figure 3 and Supplementary Figure D.2. In this context, ruling party supporters may have strong partisan preferences that limit defections even in the face of repression events. However, our null results also show no evidence of counter-mobilization on the part of ruling party supporters. Exposure does not increase willingness to wear a ruling party T-shirt or join a ruling party Facebook group, potentially suggesting greater resistance to updating among ruling party supporters.

Overall, these results are in line with the expectation that opposition participation in dissent is characterized by complementarities, meaning that opposition supporters and unaffiliated voters prefer to participate in protest and other forms of dissent the more that they see others doing so. For this group, repression does not seem to dissuade dissent but instead increases individual willingness to protest, at least in the short term. This result is consistent with a backlash model of citizen reactions to repression.

Might these results be biased by exposure to other individual-specific shocks, such as variable exposure to information or party mobilization efforts? The difference-in-difference design relies on the common trends assumption. Figures 3 and 4 show that we generally do not find pre-exposure differences between people exposed to Local Dissent, Local Repression, and Media Dissent, which provides a first test of the plausibility of the common trends assumption. However, to the extent that people who are exposed to repression and dissent events are also exposed to other things that cause changes in their protest intentions, our results could be biased.

To assess the plausibility of such bias, we test whether our results are robust to the inclusion of additional controls. First, all of our main estimates control for exposure to other types of contentious events. In other words, we can identify the effect of exposure to local repression conditional on exposure to media repression and local dissent. Second, in Supplementary Figure D.4, we show that our results are robust to controlling for knowledge of other national political events, mostly episodes of repression that received national attention. Third, the results in Supplementary Figure D.5 show that our estimates of the immediate effects of exposure are not driven by persistent effects from the previous period. Fourth, in Supplementary Figure D.6, we show that the results are robust to adding controls in the immediate post-election wave for whether the ruling party candidate won the parliamentary seat in the respondent's constituency and whether the parliamentary race in the constituency was close. Finally, in Supplementary Figure D.7, we show that the effects are not systematically stronger among the people who at baseline are: (1) more exposed to news, (2) discussing politics more, (3) members in more community groups, (4) participating in more electoral activism, or (5) participating in more nonelectoral activism. Nevertheless, because exposure to contentious events is not random, the results could still be biased by another time-variant, individual-specific treatment correlated with contentious events that we do not observe. These checks, however, increase our confidence in our findings.

Another possible source of bias in our data is measurement error, particularly misreporting of exposure to contentious events. We think that measurement error is unlikely to be driving our results for several reasons. First, the difference-in-difference estimator partials out time-invariant differences across participants in the propensity to misreport. Nevertheless, bias could be introduced if a respondent's propensity to misreport varies over time in a way that is related to exposure—as it would, for instance, if those who engage in dissent are more likely to report subsequent repression or dissent events. We tried to minimize the risk of this bias by carefully designing our measures. In addition to asking respondents if they had been exposed to any repression and dissent events, we asked them to briefly describe the events. Two percent of the participants who told us that they had been exposed to a dissent event and 3% who reported exposure to a repression event gave a vague or seemingly contradictory description of the event.[13] Supplementary Figure D.8 shows that our estimated effects are slightly

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[13] These measures were free response and we coded vague descriptions as people saying things like "I don't remember," "Some time ago," "Last year," or "Haven't seen anything" after having said yes to the closed questions.

FIGURE 4. **Exposure to Dissent Events and Media Leads to Informational Updating**
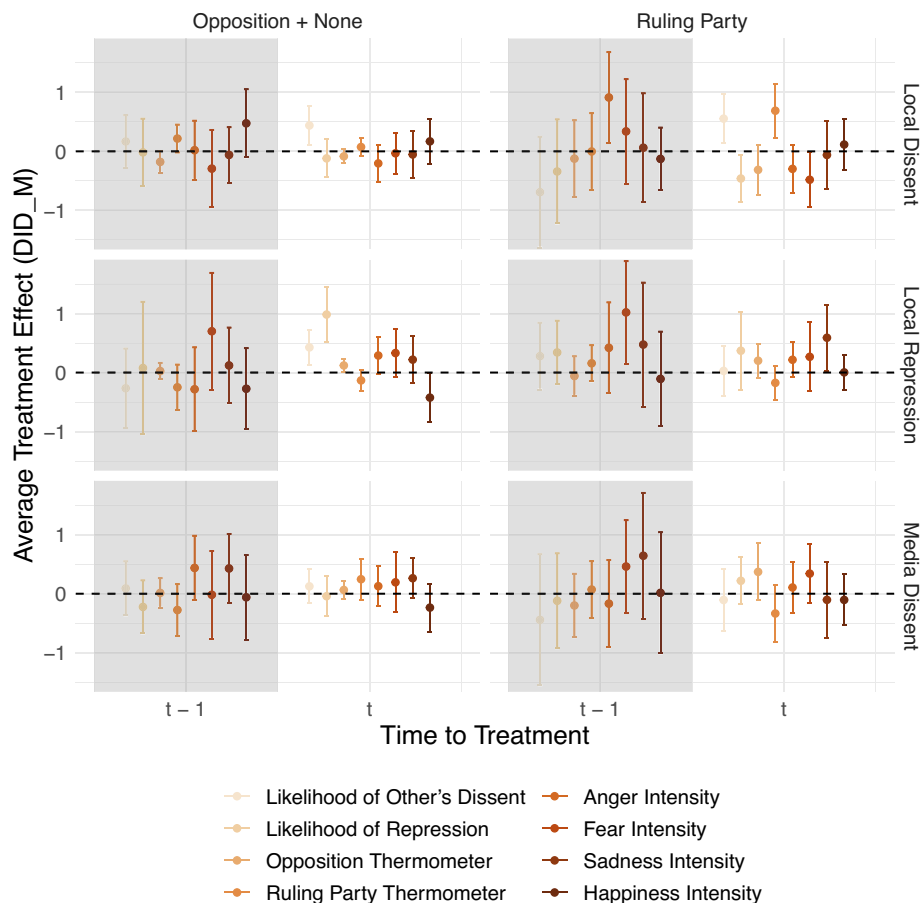
stronger if we recode participants who gave these vague or seemingly inaccurate descriptions as unexposed to repression or dissent. Another possible source of measurement error could be introduced by the fact that we did not ask respondents to specify the perpetrator of the violence that they reported.[14] To the extent that respondents are reporting violence perpetrated by the opposition against the ruling party, it should bias our coefficients toward zero.

To what extent might differences in protest intentions translate into differences in behavior? Ultimately, this question must be addressed by additional research. However, based on past research, we do not believe that self-reported intentions to protest are meaningless signals in this context. For one, Young (2019) shows that similar behavioral intentions questions are strongly correlated with a behavioral measure of dissent in Zimbabwe. In addition, our results are in the opposite direction of past estimates of the effect of repression on preference falsification in Zimbabwe (García-Ponce and Pasquale 2015), suggesting that if

anything we may be underestimating the effect of exposure. Additional research is needed to establish the conditions in which protest intentions like those that we have measured translate into behavioral participation.

Finally, to what extent might these results generalize to a representative sample of Zimbabweans? By design, our sample is more urban and online than the average Zimbabwean, and certain demographic groups (those with more assets, older people) were less likely to drop out of the sample. Section D.3 of the Supplementary Material shows that our results are similar when we reweight our observations to look like the 2018 Afrobarometer on demographic characteristics, including age, gender, education, and participation in nonelectoral forms of activism.[15] In particular, the effect of exposure to Local Repression is stronger after reweighting, and the effect of Media Dissent increases in magnitude and becomes statistically significant at $p < 0.05$.

Overall, there is robust evidence that during this moment of uncertainty, exposure to local repression and dissent events increased protest intentions for

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[14] We did not ask because perpetrator identities are generally sensitive and because past data on political violence show that ruling party agents are responsible for the vast majority of election-related violence.

[15] We weight using the fixed dimensions on which our sample differed from the Afrobarometer, and on our predictors of attrition in section B of the Supplementary Material.

opposition supporters and nonpartisans. This effect is not driven by obvious confounders like information about national political events, is stronger if we correct for a proxy of measurement error on exposure, and is robust to reweighting the sample to resemble the population. Ruling party supporters report exposure, but they are no more or less likely to intend to engage in pro- or anti-ruling party behavior when they are exposed to contentious events. In the next section, we explore the mechanisms that might underlie these relationships.

Evidence of Mechanisms

In this section, we test three interlinked families of mechanisms that might shape responses to repression or dissent. First, we look at whether individuals update their beliefs in response to the informational content in repression and dissent events. Second, we test for indications of increased affective polarization. Third, we measure emotional reactions. Given that in the previous section we found evidence that exposure to protest and dissent was mobilizing for opposition supporters and nonpartisans, we would expect to see mechanisms that favor mobilization, such as increases in affective polarization and action-oriented emotions like anger, as well as greater expectations that others will participate in dissent. We found that exposure to both types of events had no effect on ruling party supporters' mobilization, so we expect to see smaller or countervailing effects in mechanisms for ruling party supporters.

We test these hypotheses using survey questions that we tie to each of the informational, relational, and emotion-based mechanisms, provided in section A of the Supplementary Material. To test informational mechanisms, we measure beliefs about the probability of repression and the proportion of other citizens who would engage in dissent. To test relational mechanisms, we measure affective polarization using feeling thermometers toward the main opposition and ruling party. To test emotional mechanisms, we measured the average intensity that respondents reported feeling three negative emotions (anger, fear, and sadness) and the positive emotion of happiness. As in Figure 3, we present the results of tests of the assumption of no pre-treatment trends shaded in gray.

Figure 4 shows that respondents update their beliefs about the probability of repression and others' dissent actions in reaction to local exposure to dissent, regardless of their partisan status. However, ruling party and opposition supporters interpret local dissent events differently. When exposed to local dissent actions, opposition supporters and nonpartisans increase their expectations about the proportion of others who would dissent but do not lower their expectations about the propensity that they would face repression. Ruling party supporters, on the other hand, adjust their expectations of their own risk downward when exposed to local dissent. There is no evidence that respondents update their beliefs in response to media exposure. This may be because social media exposure is less

credible or seems less relevant to the respondent's own community. These effects are consistent with a process of logical informational updating that differs across partisan groups.

Turning to our relational mechanism, we find evidence that exposure to repression and dissent is associated with small changes in affective polarization that differ by partisanship. For opposition supporters and nonpartisans, there is no evidence that exposure to dissent affects feelings toward the ruling party or opposition. However, exposure to local repression is associated with a small (five points on a 100-point scale) increase in warmth of feelings toward the in-group opposition. Affective polarization among ruling party supporters, on the other hand, is responsive to dissent but not repression events. For this group, exposure to local dissent is associated with a significant increase in in-group affinity and out-group animus. Interestingly, media dissent has the opposite effect on ruling party supporters. For ruling party supporters, exposure to local dissent is associated with a 26-point increase in feelings toward the ruling party, and a 11-point decrease for media dissent.

These changes in affective polarization are small, but they are notable considering that baseline polarization in Zimbabwe is quite high (Bratton and Masunungure 2018). In our baseline data, for opposition supporters and unaffiliated voters, the mean thermometer value at baseline for the MDC was 70, and the mean value for the ruling party was 38. Ruling party supporters gave the MDC 35 out of 100, but were strongly positive toward their own party with a mean of 84. High scores leave less room for movement in either direction for most of our sample. Given this, our results suggest that individuals interpret events through the lens of group affiliation. For opposition supporters and nonpartisans, exposure to repression strengthens positive feelings toward the opposition, perhaps explaining why repression is mobilizing for these individuals. For ruling party supporters, exposure to dissent seems sensitive to event exposure and event context, even among hardened regime supporters.

We also find evidence of emotional mechanisms in both samples. If emotional mechanisms underlie the mobilizing effect of exposure to dissent for opposition or nonpartisan respondents, then we would expect to see more mobilizing emotions like anger and happiness. This is not the case: we find no evidence that local or media dissent exposure affects emotions in the opposition and nonpartisan sample. However, exposure to local repression events affects emotions in ways that might be mobilizing. Opposition supporters and nonpartisans who are exposed to local repression events are significantly more angry ($p < 0.1$) and less happy than those who do not report repression in their local communities. Turning to ruling party supporters, we find that repression events trigger sadness, a demobilizing emotion.

Overall, using data that are suited to capturing short-run responses to events, we find support for many of the mechanisms identified in the literature. Our results are consistent with informational updating driving the

positive relationship between exposure to dissent and subsequent dissent intentions in the opposition and nonpartisan sample. Evidence for informational updating in response to repression events, however, largely runs contrary to the observed increase in willingness to dissent in the wake of repression. Therefore, increases in polarization and negative emotions like anger in the wake of repression may outweigh the demobilizing effects of higher perceived costs of dissent. For ruling party supporters, we find some evidence of mechanisms that should drive mobilization and some evidence of mechanisms that should demobilize in the informational, relational, and emotional categories. These countervailing forces may explain why ruling party supporters do not show changes in willingness to mobilize in response to dissent and repression events.

Does the modality of exposure matter? As noted above, because respondents exposed to social media reports of repression are different from those who are not exposed, we cannot assess the impact of this form of media exposure. For exposure to dissent via social media, however, we find that media exposure does not generate clear evidence of mechanisms, in line with the null effect of social media dissent on protest intentions. We caution that our estimates are generally imprecise, meaning that we may be underpowered to detect smaller but still substantively meaningful coefficients on media dissent. Further, we know little about the content of information received via social media. More research on potential differences in content or relative salience of local and media exposure is needed.

CONCLUSION

Elections in nondemocratic regimes are moments of vulnerability for incumbent leaders. Elections are one of several key focal points for opposition organizing and incumbents are often uncertain about their popularity and the extent to which their supporters will actually turn out to the polls (Hafner-Burton, Hyde, and Jablonski 2014). Incumbents can unexpectedly lose elections and even if they win, post-election protests can result in significant concessions to the opposition or in loss of power (Howard and Roessler 2006; Treisman 2020). Elections, like economic shocks or changes in ruling party leadership, are periods of change when citizens' priors about the regime and other citizens' preferences are relatively weak. In these moments, mass dissent at the polls or in the streets can lead to significant political change.

How do ordinary citizens respond during these moments of uncertainty? Using a novel data collection strategy to measure mobilization intentions and three families of mechanisms around a potentially pivotal election, our research speaks to two key debates in the literature on contentious politics: whether individuals engage in dissent more or less when they see others doing so and whether repression demobilizes dissent or incites backlash. Our evidence suggests that regime opponents and the unaffiliated become more willing to protest when they see others dissent. The effects of dissent exposure persist for several weeks and do not seem driven by social-psychological mechanisms, such as affective polarization and emotional reactions. Second, we find that exposure to repression increases subsequent willingness to protest among opposition partisans and the unaffiliated, in line with theories that emphasize citizen backlash. In contrast to exposure to dissent, there is no evidence that the effects of repression persist. Individuals do update their expectations about the perceived costs of dissent in the wake of repression, but increased affective polarization and emotional reactions seem to outweigh increased perceived costs of participating in protest.

While our unit of analysis is squarely at the individual level, these results help explain protest cascades and backlash protests. The complementarities that we observe are micro-foundations for threshold models of protest (Kuran 1995) and our findings on repression help explain the puzzling outbreak of protest immediately after state repression (Aytaç and Stokes 2019). However, the durability of political closure in Zimbabwe illustrates that backlash protests are not always sufficient to generate political change. Since 2018, the regime has continued to repress civil society and opposition figures, even as subsequent protests have waxed and waned.

Our results also speak to research on social media and contentious politics and research on whether the supporters of a violent party will reward or punish the use of violence. Our findings on social media exposure to dissent suggest that while it is an important channel of exposure, it does not displace local events. Exposure to contentious events via social media may be less relevant or less credible than local exposure. We find that ruling party supporters may be cross-pressured by contentious events. There is some polarization after opposition dissent and demobilizing emotions after exposure to repression, but neither of these effects seems to be strong enough to change behavioral intentions. More generally, our results highlight the importance of disaggregating different types of voters in analyses of the effects of contentious events.

We hope that future work will explore how heterogeneity in features like personality traits, network characteristics, and past experiences shapes citizen reactions to contentious events. We are also sensitive to the question of the kinds of political settings in which we would expect to find protest complementarities and repression backlash. We view Zimbabwe as illustrative of a competitive authoritarian or hybrid regime. These regimes have become the modal form of nondemocracy in the post-Cold War period. We, therefore, think our theory and results may generalize to a broad set of authoritarian and semi-authoritarian settings. We do not, however, expect our results to travel to totalitarian regimes, countries under foreign occupation, or episodes of extreme repression like genocide and mass killing. Given our study's focus on protest, which is often nonviolent in hybrid regimes, we do not think that our results shed light on individual participation in armed resistance. Despite these scope conditions, we hope that our focus on mechanisms underlying

individual decision-making might stimulate new thinking about how emotional and relational mechanisms shape decision-making in a range of other contexts.

DATA AVAILABILITY STATEMENT

Research documentation and de-identified data that support the findings of this study are openly available in the American Political Science Review Dataverse at https://doi.org/10.7910/DVN/BLIVUP.

CONFLICT OF INTEREST

The authors declare no ethical issues or conflicts of interest in this research.

ETHICAL STANDARDS

The authors declare the human subjects research in this article was reviewed and approved by UC Davis and American University and certificate numbers are provided in the text or in the Supplementary Material. The authors affirms that this article adheres to the APSA's Principles and Guidance on Human Subject Research.

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