Poor Prospects—Not Inequality—Motivate Political Violence

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Abstract

Despite extensive scholarly interest in the association between economic inequality and political violence, the micro-level mechanisms through which the former influences the latter are not well understood. Drawing on pioneering theories of political violence, social psychological research on relative deprivation, and prospect theory from behavioral economics, we examine individual-level processes that underpin the relationship between inequality and political violence. We present two arguments: despite being a key explanatory variable in existing research, perceived lower economic status vis-à-vis other individuals (an indicator of relative deprivation) is unlikely to motivate people to participate in violence; by contrast, although virtually unexplored, a projected decrease in one's own economic status (prospective decremental deprivation) is likely to motivate violence. Multilevel analyses of probability samples from many African countries provide evidence to support these claims. Based on this, we posit that focusing on changes in living conditions, rather than the status quo, is key for understanding political violence.

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Introduction

What explains political violence[1]? Scholars have often emphasized grievance-based explanations of political violence (Cederman, Gleditsch, and Buhaug 2013; Østby 2013). A large portion of this research has analyzed grievances associated with economic inequality. Studies have shown that economic inequality—when appropriately operationalized—relates to various forms of political violence, including civil war (ibid.). While state- or group-level analyses have extensively analyzed the inequality-violence link, we know less about the individual-level processes through which one influences the other.

In this article, we examine the role of inequality in people's motivations to participate in political violence. Since the publication of *Why Men Rebel* (Gurr 1970/ 2011), many scholars have assumed that disadvantaged individuals (vis-à-vis other individuals or groups in the same countries or regions) are more motivated to engage in violence. To date, however, this remains a theoretically and empirically underexplored claim. Furthermore, extant empirical research has mainly focused on static inequalities. However, pioneering theoretical work considered changes in living conditions, not the status quo, as key for understanding violence (Davies 1962; Gurr 1970/2011). Indeed, Gurr claimed that persistent relative deprivation (i.e., a potential consequence of inequality) should lead to psychological adjustment, not violence, stressing the need to account for dynamic forms of deprivation caused by changes in living conditions or people's expectations (1970/2011, 46–56).

This emphasis on dynamics is consistent with individual-level studies in other disciplines. Social psychologists report only a weak association between individual relative deprivation and participation in collective actions (Smith et al. 2012), and behavioral economists demonstrate that risk-seeking is related to prospective losses rather than prospective gains (e.g., resources acquired from the advantaged) (Kahneman and Tversky 1984). Although psychologists have extensively analyzed relative deprivation, they have rarely linked it to participation in violence, focusing instead on non-violent collective actions (van Zomeren et al. 2008). Furthermore, psychological research has typically relied on samples from Western democracies (Henrich, Heine, and Norenzayan 2010). By contrast, political scientists have extensively analyzed the inequality-violence link, across various countries; however, they have rarely tested individual-level theories against individual-level data (for critiques, see Bartusevičius 2019; Dyrstad and Hillesund 2020; Hillesund 2015; Koos 2018; Miodownik and Nir 2015; Pettigrew 2015; 2016; Rustad 2016).

Here, we present an analysis of large multinational survey data from African countries. Consistent with the theoretical focus on temporal dynamics, we find no empirical support for the claim that perceived inequality vis-à-vis other individuals motivates violence. By contrast, we find that a projected decrease in one's own economic status motivates violence. We use measures of behavioral intentions to alleviate reverse-causality concerns, and a large set of questions to assess alternative explanations. We also explicitly estimate the risk of omitted-variable bias (Altonji,

Elder, and Taber 2005; Oster 2019), finding that unobserved confounders are unlikely to drive our results. Our results also withstand various sensitivity tests, including those addressing reporting concerns and using alternative data.

This article makes the following contributions. Theoretically, we renovate a five-decade-old theory of relative deprivation and political violence—routinely invoked in contemporary conflict research—with insights from recent psychological work and behavioral economics. Our study aimed both to test existing theory and to build new propositions. Specifically, we aimed to assess the general argument by Gurr (1970/2011) that dynamic forms of deprivation, compared to static, are more important for violence. Concurrently, we aimed to further build Gurr's theory by drawing on individual-level research in other disciplines.

Empirically, we show that perceptions of disadvantageous economic changes, but not individual-based inequalities, relate to motivations to participate in violence. Macro-level studies have shown that inequalities between groups, compared to those between individuals, better predict civil conflict (Cederman, Gleditsch, and Buhaug 2013; Østby 2013); consequently, some scholars have called for a shift in focus from individual- to group-based grievances. Our micro-level findings suggest that group-based grievances are indeed associated with violent motivations. However, we find that certain forms of individual-based grievances also significantly relate to such motivations. Instead of shifting focus away from individual-based variables, we suggest to center on temporal economic dynamics, whether pertaining to individuals or groups. This suggestion has implications for conflict research writ large, pointing to the key role of temporal dynamics, as contrasted to static features, in the processes leading to violence (see also Buhaug et al. 2021).

More generally, our study contributes to an emerging effort to integrate psychological perspectives into the study of macro-level political phenomena (Kertzer and Tingley 2018), and the scrutiny of individual-level theories with individual-level data. Although commonly invoking theories of individual motivations, research on civil conflict has traditionally focused on states or groups as units of analysis. Directly analyzing individual motivations, we find that one key theoretical account—underlying a substantial portion of conflict research—needs to be revised, opening a range of new research avenues.

Knowledge Gaps

Early studies of political violence argued that inequalities in income or land can motivate individuals to participate in violence challenging the status quo distribution of resources (e.g., Nagel 1974; Russet 1964; Sigelman and Simpson 1977). Later studies added that inequalities overlapping with ethnic groups (horizontal inequalities) may also aid the mobilization of the disadvantaged for collective action (e.g., Cederman et al. 2011; Gurr 2000; Østby 2008; Stewart 2008). Ethnic groups share a common social identity and horizontal inequalities can strengthen the salience of these identities (Gurr

2000, 66–69; Østby 2008, 147). Salient identities, in turn, can facilitate the mobilization of solitary individuals for collective action (Gurr 2000, 75).

While emphasizing mobilization, studies of horizontal inequalities did not challenge the claim that inequalities motivate violence. These studies accentuated conditions that *facilitate* violence (e.g., inequalities aligned with identity groups), while still assuming that inequalities, whether between individuals or groups, *motivate* violence. As Stewart (2008, 12) underlined, "Large scale group mobilization—particularly for violent actions—is unlikely to occur in the absence of serious grievances", referring to grievances generated by inequalities between religion-, ethnicity-, or social class-based groups.

However, the elemental question of whether inequality-related grievances indeed motivate violence remains underexplored. State- or group-level research reports that individual-based (vertical) inequalities predict non-ethnic conflict (Bartusevičius 2014; Buhaug, Cederman, and Gleditsch 2014) and that horizontal inequalities predict ethnic conflict (Buhaug, Cederman, and Gleditsch 2014; Cederman, Gleditsch, and Buhaug 2013). However, macro- or meso-level relationships say little about individual motivations. Inequality can cause violence via multiple other pathways, unrelated to the motivations of the disadvantaged. For example, vertical or horizontal inequalities may reflect a resource concentration in the hands of the ruling elite, incentivizing intra-elite fighting over power (Boix 2008, 400–1). To assess whether inequality generates violence via grievances or other pathways, we need to scale down.

Pioneering work on relative deprivation and political violence suggested interviewing individuals (Davies 1962, 18; Gurr 1970/2011, 29–30). As Weede noted: "It is hard to imagine how relative deprivation can be adequately assessed without recourse to survey data" (1981, 652). Several studies, following Gurr (1970/2011), collected surveys on people's deprivations, attitudes toward political violence, and self-reported participation (reviewed in Muller 1977). However, this early research was limited in geographic scope—focused on single or several, and mainly Western, samples—and relied on now superseded analysis techniques (Dyrstad and Hillesund 2020, 1730).

Within contemporary conflict research, only a handful of studies collected systematic data on individual participation in violence (e.g., Humphreys and Weinstein 2008) or perceptions of inequality (e.g., Langer and Mikami 2013; Langer and Smedts 2013). These latter studies challenged the common assumption that subjective and objective inequalities correspond. Using surveys from five African countries, Langer and Mikami (2013) found that members of some ethnic groups that were not the most disadvantaged perceived themselves as the most disadvantaged, whereas members of other ethnic groups perceived their status as better than it was. Using Afrobarometer data, Langer and Smedts (2013) found a negative correlation between objectively measured and perceived horizontal inequalities. Actual and perceived inequalities might not correspond for multiple reasons (e.g., poor quality of data or public opinion manipulation). The key point is that—contrary to the assumptions of prior research— objective measures of inequalities may not reflect perceived inequalities. Hence, macro-level analyses with objective measures of inequality may not tap into the

individual-level relationship between perceived inequalities and motivations for violence. We need to focus either on the factors that affect perceived inequalities or directly measure perceived inequalities (Pettigrew 2015, 13–4; Pettigrew 2016, 9–10).

Accordingly, more recent research started using surveys to measure perceived inequalities and attitudes toward political violence. Rustad (2016) conducted a survey in the Niger Delta measuring both objective and perceived inequalities and assessing their effects on people's attitudes toward violence. Rustad found little evidence for a correlation between objectively measured and perceived inequalities. Furthermore, perceived inequalities, compared to objective, better predicted attitudes toward violence, and this relationship held for both individual- and group-based inequalities. In another study in the Niger Delta, Koos (2018) found that a perception that one's group receives an unfair share of oil revenues also predicted attitudinal support for violence.

In addition to economic inequalities, several studies analyzed political inequalities. Hillesund (2015) found that a perception of higher status of civil and political rights among Palestinians in the Gaza Strip and the West Bank predicted lower support for violent resistance tactics. Dyrstad and Hillesund (2020) found that a perception of unfair distribution of political power among groups predicted support for violence in some countries but not others, and that this association was moderated by perceptions of political efficacy. Using Afrobarometer data, Miodownik and Nir (2015) found another nuanced pattern: the association between perceived group-based inequalities, both economic and political, and attitudes toward violence was moderated by objectively measured inequalities.

We contribute to this emerging micro-level literature in several ways. First, extant studies have focused on attitudes toward political violence. Attitudes are linked to motivations and behavior: individuals supportive of violence, ceteris paribus, should be more motivated engage in it (Hillesund 2015, 79–80). Furthermore, attitudes toward violence are of interest in themselves: armed groups often rely on popular support for their survival; hence, people's support for violence is a key risk factor for civil conflict (Dyrstad and Hillesund 2020). However, if the primary goal is to assess people's motivations to engage in violence or actual participation, then attitudes may not be the most suitable proxies (van Zomeren et al. 2008, 510). Here, we explore more direct measures: behavioral intentions to engage in political violence and self-reported participation. Second, in addition to perceptions of static inequalities, we analyze perceived changes in living conditions. As we theorize below, the latter, compared to the former, is more likely to motivate violence. Finally, most existing micro-level studies report results from only one or several countries, which can be confounded or moderated by country-specific factors (Dyrstad and Hillesund 2020). Here, we utilize large multinational samples, enabling broader generalization.

Theory

Drawing on pioneering theories of political violence, social psychological research on relative deprivation, and prospect theory from behavioral economics, we develop the

following overarching argument: political violence relates more strongly to deprivations arising out of temporal changes in economic conditions than to deprivations arising from individuals comparing their own conditions with those of others. The argument is developed as follows.

We first present a general theoretical framework, which largely draws on Gurr (1970/2011). Here, we stress that this pioneering work emphasized the role of dynamic, rather than static, deprivations. This discussion places our more specific, subsequent arguments in a familiar theoretical framework. The specific arguments pertain to psychological variables. First, temporal comparisons, compared to static, produce stronger negative emotions, which in turn more strongly motivate participation in collective actions. Second, temporal comparisons promote risk-seeking, whereas static comparisons promote risk-aversion, which in turn relate to the probability of using violence or joining a violent collective action.

Our theory centers on motivational factors. We do not claim that opportunities for violence are unimportant. We merely assume that—holding opportunity factors constant—stronger motivations for violence constitute a higher probability of actual violence.

Types of Deprivation and Political Violence

The key element of Gurr's (1970/2011) theoretical model is the concept of relative deprivation, defined as "actors' perception of discrepancy between their value expectations and their value capabilities". Value expectations are "the goods and conditions of life to which people believe they are rightfully entitled" and value capabilities "the goods and conditions they think they are capable of getting and keeping" (ibid. 24). In short, relative deprivation is a perception of a discrepancy between what one has and what one believes one ought to have. This definition implies that relative deprivation occurs over values to which people think they are rightfully entitled. The standards of such an entitlement are determined by some reference point, commonly other individuals or groups;[2] however, they may also include own status in the past or abstract ideals (ibid. 24–25).

Objective living conditions do not directly map onto subjective perceptions; therefore, inequalities per se may not necessarily produce relative deprivation. However, the presence of inequalities implies the presence of advantaged individuals or groups that constitute potential reference points. Therefore, some scholars suggested using the income Gini coefficient not just as an indicator of income inequality but also as a measure of aggregate levels of relative deprivation (Yitzhaki 1979).

The actual goods or conditions over which deprivation may occur vary across cultures and include welfare (e.g., material goods), power (e.g., to vote), and interpersonal (e.g., ability to participate in associations) values (Gurr 1970/2011, 25–26). However, for operational reasons, subsequent empirical research has largely focused on material goods, such as income or land (e.g., Nagel 1974; Sigelman and Simpson 1977).

When relative deprivation occurs, individuals experience anger—commonly referred to as "grievance"—which motivates violence or predisposes people to being

mobilized for violence. This claim assumes the existence of a psychological mechanism that generates violence-motivating anger in response to relative deprivation. For Gurr, the link between relative deprivation and violence-motivating anger was provided by the frustration-aggression mechanism (Berkowitz 1989). Eventually, research on aggression developed more complex models of the mechanisms that produce aggression in response to obstructed goals (Anderson and Bushman 2002). Whether relative deprivation produces anger via the frustration-aggression mechanism, or some other psychological mechanism, is beyond our study's scope. What matters here is the more general assumption that some psychological mechanism generates motivation for violence in response to relative deprivation. The presence of such a mechanism has been implicitly assumed in much of conflict research, including recent studies on horizontal inequalities.

The path from motivation for political violence to actual violence involves numerous mediating and moderating variables, such as justification and likely efficacy of violence, as well as political opportunity structures. These factors were discussed in detail by Gurr (1970/2011; see Figure 15 in Gurr (1970/2011) for a summary). Given our focus on motivations, we omit these variables. Gurr's theory holds that the intensity of anger and the population share of angry individuals, ceteris paribus, positively vary with the incidence and magnitude of collective political violence (1970/2011, 319–322).

As mentioned, value capabilities are determined by some reference point. These points characterize the types of deprivation. Static relative deprivation reflects an instantaneous discrepancy between one's value expectations and value capabilities, which typically arises out of comparison of oneself or one's group to others. Other types of deprivation concern changes in value expectations or capabilities over time (ibid. 46–56). Decremental deprivation arises when value expectations remain unchanged, but value capabilities decline, which typically occurs due to losses. Aspirational deprivation arises when value capabilities remain unchanged, but expectations increase. Progressive deprivation occurs when steady and simultaneous improvement in value expectations and capabilities is followed by stabilization or decline of the latter. The latter is analogous to Davies' J-curve, which typically reflects "…a prolonged period of objective economic and social development…followed by a short period of sharp reversal" (1962, 6).

Concurring with Davies, Gurr (1970/2011, 46) hypothesized that dynamic types of deprivation were more likely to generate violence:

> …because RD [relative deprivation] is a psychically uncomfortable condition… [people] tend over the long run to adjust their value expectations to their value capabilities. Societal conditions in which sought and attainable value positions are in approximate equilibrium consequently can be regarded as "normal," however uncommon they may be in the contemporary world, and provide a base-line from which to evaluate patterns of change. Three distinct patterns of disequilibrium can be specified: *decremental deprivation…aspirational deprivation…and progressive deprivation*…All three patterns have been cited as causal or predisposing factors for political violence.

This discussion (see also 46–58) has attracted less attention in recent conflict research, which has primarily focused on static inequalities.

Some research analyzed economic downturns as causes of civil conflict, with several studies reporting a positive association (e.g., Miguel, Satyanath, and Sergenti 2004) and others no association (e.g., Ciccone 2011). At least one study explicitly linked downturns to decremental deprivation and found an association with revolutions (Knutsen 2014). Some research also examined the association between *political* status loss and civil conflict. According to Petersen (2002), members of ethnic groups that lose government control after a pro-longed period of political dominance, are likely to experience particularly strong anger, and, consequently, that such "status reversals" produce "the highest likelihood of violent conflict" (52). Cederman, Wimmer, and Min (2010) found that recently (politically) downgraded ethnic groups were over-represented in civil conflicts, more so than ethnic groups that generally lacked power. Combining the two literatures, Buhaug et al. (2021) found that local income shocks were particularly likely to promote conflict among recently (politically) downgraded groups. Altogether, research on economic downturns and political status reversals suggests that changes, compared to the status quo, are more important for violence. However, akin to research on the inequality-conflict nexus, these studies have focused on states or groups as units of analysis; hence, they did not directly account for individual motivations.

In sum, the above discussion suggests that violence should relate more strongly to deprivations caused by temporal changes in economic conditions than to deprivations resulting from comparisons between individuals. We now turn to work in psychology, which supports this general proposition.

Emotions and Political Violence

A perception that one is worse off than others potentially generates negative affect. But is such affect sufficient to motivate individuals to use violence or predispose them to being mobilized for violence? Appropriately addressing this question involves individual-level research, focused specifically on human perceptions of, and responses to, economic conditions (Pettigrew 2015, 13–4; Pettigrew 2016, 9–10). Such research has been extensively conducted within social psychology, the field that pioneered the concept of relative deprivation (Stouffer et al. 1949). Psychological research has rarely linked individual-level variables to large-scale political violence; however, this work has analyzed a range of other outcomes that likely constitute precursors of political violence, such as participation in collective actions (e.g., strikes) and intergroup attitudes (e.g., intergroup hostility) (Smith et al. 2012).

In a review of the psychological literature, Smith et al. (2012, 204) suggest that "feelings of GRD [group relative deprivation] should be associated with ingroup-serving attitudes and behavior such as collective action and outgroup prejudice, whereas IRD [individual relative deprivation] should be associated with individual-serving attitudes and behavior such as academic achievement and property crime".

Group relative deprivation refers to the perception that one's group is disadvantaged relative to other groups (corresponding to perceived horizontal inequality), and individual relative deprivation to the perception that you as an individual are disadvantaged relative to other individuals (corresponding to perceived vertical inequality). Subsequently, in a meta-analysis, encompassing 210 studies ($N = 186,073$), Smith et al. (2012) found evidence to support this general claim. This suggests that group relative deprivation, compared to individual, should be a stronger predictor of support for and participation in collective actions. But does this imply that individual-based deprivations are irrelevant for political violence?

While Smith et al.'s (2012) meta-analysis found that group relative deprivation is a stronger predictor of group-level outcomes (e.g., participation in strikes and outgroup hostility), it did not show that individual relative deprivation was unrelated to them. Indeed, many studies show that individual relative deprivation is a significant (although weaker) predictor of such group-level outcomes (e.g., Pettigrew et al. 2008, 390). There are at least three plausible processes that account for this.

First, there may be spillover effects, such that a perception of a disadvantage compared to other individuals may lead to a perception that one's whole group is disadvantaged compared to other groups. Pettigrew et al. (2008) found that group relative deprivation partly mediated the association between individual relative deprivation and intergroup prejudice. Second, the direct association between individual-based deprivations and group-level outcomes potentially suggests that people compare themselves to individual *outgroup* members. Such comparisons can lead to evaluations of whole outgroups and can therefore lead to group-level outcomes (Smith et al. 2012, 205). The correlation between individual relative deprivation—resulting from comparisons to outgroup members—and collective behaviors is comparable to the correlation between group relative deprivation and collective behaviors (2012, 216). Finally, and particularly relevant for political violence, there is virtually no difference in the associations of individual and group relative deprivations with state-focused outcomes. In Smith et al. (2012), the two types of deprivations were equally strong predictors of negative attitudes toward the "larger system", such as trust and legitimacy of political institutions, which are most relevant for violence targeting the state. The group-focused attitudes and behaviors, best predicted by group relative deprivation, may be more relevant for communal conflicts than for conflicts with governments (see also Hillesund 2019). The three processes suggest that individual-based deprivations may affect participation in political violence, but also that this effect is unlikely to be strong. Unlike participating in a strike, political violence entails the risk of injury and death. Difficulties in recruitment to violent movements due to higher risks are well documented (Chenoweth and Stephan 2011). Thus, for individual-based deprivation to motivate participation in violence, it would have to generate particularly intense affect.

This is where temporal relative deprivations come into play. In particular, "when respondents compare their present with their own past—rather than comparing with other individuals or groups", the correlation between individual-based deprivation and

individual-based outcomes nearly doubles (Pettigrew 2015, 18). This implies that inward temporal comparisons may be especially relevant for political violence.

The general notion that temporal comparisons strongly relate to anger concurs with a basic psychological insight that, rather than focusing on stasis, humans are attentive to changes, especially those that "threaten to make things worse" (McDermott, Fowler, and Smirnov 2008, 337). This basic tendency was documented in research on hedonic adaptation, showing that subjective well-being is strongly influenced by changes (e.g., becoming unemployed), but then gradually adjusts, eventually returning to pre-event baseline levels (Diener 2000). That people are particularly sensitive to changes for the worse also aligns with Gurr's original prediction that dynamic forms of deprivation are key for understanding political violence. While grievances generated by interpersonal comparisons are arguably too weak to motivate violence, anger caused by temporal comparisons might be of greater magnitude and thus have sufficient motivational force to predispose individuals to political violence.

Risk-Seeking and Political Violence

Gurr's original claim that comparison reference points are key for understanding political violence is compatible with the emphasis on reference points in prospect theory (Kahneman and Tversky 1979; 1984). People evaluate the outcomes of their decisions relative to some reference point: when outcomes top the reference point, they constitute a gain; and when outcomes come below the reference point, they constitute a loss. Research has shown that "changes that make things worse (losses) loom larger than improvements or gains" (Kahneman, Knetsch, and Thaler 1991, 199), so that $100 loss has a greater impact on one's subjective well-being than a $100 gain. Furthermore, people faced with potential "gains tend to be risk averse, while those confronting losses become much more risk seeking" (McDermott, Fowler, and Smirnov 2008, 335). The preference for risky alternatives in the domain of losses is an empirically robust phenomenon (Kahneman and Tversky 1984).

Tezcür (2016) invoked prospect theory to explain why people prefer participation in violence to less risky alternatives. Rebel leaders often emphasize threats and frame the status quo as a loss. In such framing, not rebelling involves continued and certain losses and rebelling has some prospect for gains, although at an increased risk of higher losses. Framed in such a way, and expressed in "life or death" language, leaders' messages can lead followers to "take desperate gambles, accepting a high probability of making things worse in exchange for a small hope of avoiding a large loss" (Kahneman 2011, 318–9).

The deprivation-violence link can be explained similarly. Whereas Tezcür's argument focuses on leaders or events as inducing loss frames, this function can also be performed by certain types of deprivation, corresponding to Gurr's typology. Specifically, (persisting) static relative deprivation corresponds to a status quo situation, where the disadvantaged perceive likely outcomes, that is acquiring resources from the advantaged, as gains. For such individuals, prospect theory predicts risk avoidance and preferences for

more certain, less risky options, such as continued status quo or non-violent activism. By contrast, decremental deprivation corresponds to a situation where the status quo has changed, and where the disadvantaged are likely to perceive their outcomes as a loss. Mercer (2005, 5) notes: "situational factors, such as an economic collapse, throw leaders and citizens into a domain of loss where the pre-crisis status quo becomes the standard reference point". For such individuals, prospect theory predicts risk-seeking with the aim of averting the loss, and, by extension, higher susceptibility to being mobilized for violence.

Gurr's theory does not explicitly address the difference between experienced and anticipated losses. However, prospect theory suggests that the risky choice of participating in violence should be more appealing to those who anticipate loss than those who have already experienced it. If a loss has already occurred, then people are more likely to take their disadvantaged situation as the reference point and perceive the possible outcome of participating in violence as gains. By contrast, if a loss has not yet occurred—but is prospective—then people are more likely to perceive the possible outcome of participating in violence as averting the loss. Thus, while experience of loss influences one's subjective well-being, prospect theory predicts that anticipation of loss or *prospective* decremental deprivation should relate more strongly to violence. The other two types of deprivation, aspirational and progressive, are both characterized by expectations that top the actual value capabilities. Hence, both should lead people to perceive the possible outcome of participating in violence (to meet the expectations) as gains, thereby reducing risk-seeking.

Altogether, our theoretical analysis suggests that only one type of Gurr's original deprivation will promote risk-seeking: decremental deprivation or, more specifically, prospective decremental deprivation. Such risk-seeking, combined with strong anger, as suggested by psychological research, should predispose individuals to partake in violence. Thus, more formally:

> H1: Prospective decremental deprivation is positively associated with motivations to participate in political violence.

Research Design

Data

Instead of surveying one or several counties, we attempted to identify existing multinational surveys that asked questions pertaining to deprivations and political violence. We identified two large datasets collected by Afrobarometer in 2002–2003 (Round 2) and 2011–2013 (Round 5). Afrobarometer surveys nationally representative samples of citizens of voting age on topics related to governance, public services, and living standards. We first analyzed Round 5, spanning 34 countries with $n = 1200$ or 2400 per country (total $N = 51,587$). The surveys used clustered, stratified, multi-stage probability sampling, where random selection with a probability proportionate to the population size was applied at every stage.

Aside from relevant predictor and outcome measures, these datasets confer several advantages. First, they enable analysis of nationally representative data that would otherwise be hardly possible. Political violence is an extreme event. The number of those willing to participate in violence or actual participants, in most countries, is very small (see descriptive statistics below); thus, samples from single countries would likely contain an insufficient number of people motivated to participate in violence, or self-reported participants, unless the sample size was considerably increased or the sampling non-randomly targeted such individuals. Second, deprivation-related grievances may differently affect motivations for violence across different countries (Dyrstad and Hillesun 2020). Multilevel analysis of Afrobarometer data allows accounting for such country-level characteristics.

Outcomes

The first outcome measure is a behavioral self-report of participation in political violence (PVPARTICIPATION): "Here is a list of actions that people sometimes take as citizens. For each of these, please tell me whether you, personally, have done any of these things during the past year. If not, would you do this if you had the chance: Used force or violence for a political cause?" ("No, would never do this", "No, but would do if had the chance", "Yes, once or twice", "Yes, several times", "Yes, often", and "Don't know"). Our interest was in whether people participated in violence—not whether higher levels of deprivation lead to higher incidence of violence. Thus, we coded this variable as binary, aggregating the first two responses into 0 = "no" and the last three into 1 = "yes".

We do not know whether all interviewees understood what "violence for a political cause" entails, nor we can identify the exact form of violence the interviewees had in mind. However, this item followed four other items on political activism in the same battery ("Attended a community meeting"; "Got together with others to raise an issue"; "Refused to pay a tax or fee to government"; and "Attended a demonstration or a protest march"). The question about violence thus refers to participation in a protest-like, violent antigovernment action.

"Violence for a political cause" is a broader term that subsumes various more specific types of violence. We assume that factors that increase people's general motivations to use violence for a political cause, ceteris paribus, also increase people's willingness to engage in the more specific forms of violence (e.g., civil conflicts). To rule out engagement in violence in support of governments (e.g., pro-government rallies), we analyzed sub-samples of government supporters and non-supporters; we also controlled for the comprehension of questions.

People may not report participation in violence for many reasons, for example, fears of repression. Thus, behavioral self-reports may underestimate the extent to which people participate in violence. Therefore, we controlled for whether interviewees were deemed suspicious or dishonest by the interviewers, and whether interviewees were concerned about repression.

We also assessed potential nonresponse bias. The average response rate to the survey was high, 78.50%, and only 1.83% of interviewees refused to answer (or replied "Don't know" to) the question about violence. For comparison: 1.94% refused to answer (or replied "Don't know" to) an adjacent less sensitive question about participation in protests. Furthermore, non-zero nonresponse to a survey or particular questions does not necessarily imply bias (Grooves 2006). Nonresponse would bias results if non-responders were systematically different with respect to the variables of interest. As shown in Table S7 in Supplemental Material (SM), means of deprivation measures (operationalized below) were larger in the sub-samples of non-responders, compared to responders. This suggests that our analyses potentially underestimate the effects of deprivations on violence.

Instead of nonresponding, interviewees may provide dishonest replies. Fearing repression, people are unlikely to report participation in violence if they had not participated in it. By contrast, participants in violence may be tempted to report nonparticipation instead of providing nonresponse. Such replies would also bias our results if the interviewees were systematically different with respect to the variables of interest. Unfortunately, the data at hand do not allow empirically assessing this possibility. Theoretically, one could argue that deprived individuals are also more likely to be state-repressed; if this is true, then deprived individuals would also likely tend to report nonparticipation due to fears of reprisals. In such a scenario, our results would also be underestimates of the true deprivation effects. These concerns must be considered while evaluating our results.

The question about violence asks about participation over the last year. Since some of our predictors reflect interviewees' experience over the last year, at the time of interview, or future perceptions, this raises reverse-causality concerns. Individuals who engaged in violence in the past may be motivated to express their deprivations. Our analysis, therefore, may capture the effects of past participation in violence on the voicing of deprivations. However, the question about violence is formulated in a particular way that allows alleviating this concern. Interviewees who have not reported participation in violence could also indicate whether they would if they had a chance. Thus, we introduced PVINTENTIONS, which constitutes a constrained version of PVPARTICIPATION: 0 = "No, would never do this", 1 = "No, but would do if had the chance". The question explicitly states "would you do" and—as a reply—"would do", as contrasted to "would you have done"/"would have done". This question thus refers to motivations to engage in violence at the time of interview and prospectively. If individuals participated in violence in the past, this could influence their expressions of deprivations and, simultaneously, their current motivations to engage in violence. Note, however, that PVINTENTIONS excludes those who reported participation in violence over the last year. Still, it is possible that interviewees participated in violence more than one year ago; hence, PVINTENTIONS does not fully address reverse-causality concerns.

PVINTENTIONS does not indicate the actual conflict behavior; however, it reflects behavioral intentions to take part in violence. Individuals who report intentions to participate in violence if they had a chance will more likely take part in such violence

once the chance arises, ceteris paribus. The use of measures of behavioral intentions as proxies of behavior is standard in psychology (Sheeran 2002). Studies show that expressed intentions to engage in collective actions predict actual participation (van Zomeren et al. 2008). Gomez et al. (2017) report a convergence between stated and actual sacrifices on the frontline among fighters against the Islamic State.

It is most useful to consider PVINTENTIONS as reflecting *motivations* to engage in violence, corresponding to our key theoretical focus:

> Intentions can be inferred from participants' responses that have the form, "I intend to do X", "I plan to do X", or "I will do X". In psychological terms, a behavioral intention indexes a person's motivation to perform a behavior. That is, behavioral intentions encompass both the direction (to do X vs. not to do X) and the intensity (e.g., how much time and effort the person is prepared to expend in order to do X) of a decision (Sheeran 2002, 2).

As such, although intentions do not directly tap behavior, they index the probability of it: a motivation to perform an action should relate to the probability of performing it.

Given that intentions to participate in violence do not constitute actual violence, PVINTENTIONS may also suffer less from nonresponse bias. Furthermore, this measure possibly allows assessing people's motivations to engage in violence independent of opportunities. People who have not participated in violence could have taken part in it if they had an opportunity to do so, and some opportunities for violence may be limited for reasons beyond our study's scope (e.g., sickness). As mentioned, our theoretical interest is in motivations, not opportunities that enable behavior based on motivations.

In Round 5, 2.86% ($N = 1,473$) reported participation in violence, with the highest figures in Uganda (9.46%) and the lowest in Botswana (0.67%); and 6.63% ($N = 3,420$) reported intentions to participate in violence, with the highest figures in Mozambique (15.83%) and the lowest in Tunisia (1.08%). Table S2 reports between-country variation in the outcomes.

Predictors

Round 5 allows measuring three types of deprivation. Static relative deprivation was measured with perceived (individual-based) economic inequality (hereafter INEQUALITY): "Let's discuss economic conditions. In general, how do you rate your living conditions compared to those of other [name of nationals]?" (0 = "Much better", 4 = "Much worse"). Decremental deprivation was measured in two ways, reflecting perceived retrospective and prospective losses: "Looking back, how do you rate the following compared to twelve months ago: your living conditions" (0 = "Much better"; 4 = "Much worse") (RETROSPECTIVE LOSS); "Looking ahead, do you expect the following to be better or worse: your living conditions in twelve months time" (0 = "Much better"; 4 = "Much worse") (PROSPECTIVE LOSS). Note that these questions were part of same battery asking questions about economic conditions.

Our measures reflect perceived economic conditions as such, not those that are deemed unfair, whereas Gurr stressed that relative deprivation occurs over values to which people believe they are rightfully entitled. Furthermore, our measures do not directly measure anger. The affective aspect of relative deprivation is typically embedded in definitions within social psychology: "(a) People first make cognitive comparisons, (b) they next make cognitive appraisals that they or their ingroup are disadvantaged, and finally (c) these disadvantages are seen as unfair and arouse angry resentment" (Pettigrew 2016, 9). Research suggests that relative deprivation measures that disregard unfairness or do not tap anger, are weaker, but still significant, predictors of individual- and group-based outcomes (Smith et al. 2012). These measurement issues must be considered while evaluating our results. Tables S3–S5 present descriptive statistics for the predictors.

Modeling

Given the hierarchical structure of our data, with individuals nested within countries, and binary outcomes, we used hierarchical generalized linear models with the logit link function. We implemented several pre-analysis steps, following Hox, Moerbeek, and van de Schoot (2018, 42–46). First, we estimated an intercept-only model, which revealed significant intraclass correlation in PVPARTICIPATION (ρ = .14) and PVINTENTIONS (ρ = .10). This substantiated the modeling of random intercepts. We then added the predictors with between-country variance of the corresponding slopes fixed at 0. PROSPECTIVE LOSS was a significant predictor, whereas INEQUALITY and RETROSPECTIVE LOSS were not. Finally, we tested whether any of the slopes of the predictors significantly varied across countries. Likelihood ratio tests indicated that modeling random slopes for each predictor, except gender and INEQUALITY in the model of PVINTENTIONS, significantly improved the model fit. Therefore, below we include random intercepts and randomly varying slopes for all predictors that have significant between-country variance.[3]

To aid interpretation, we normalized all variables to [0, 1]. Normalizing variables to the same narrow range also improves convergence in numerical integration-based MLE.[4] In the initial stage, we did not center the data. Analysis of non-centered predictors produces coefficients that are a mix of the within- and between-country effect (Raudenbush and Bryk 2002, 139). If the between-country effect is non-zero, such blended coefficients will be biased estimates of the within-country effects. In additional analyses, we used several techniques to account for this, including specifications with country-level fixed effects.

Results

We first report results of parsimonious models examining associations between the key variables. We then present analyses addressing endogeneity concerns, first reverse causality, and then unobserved confounding. Finally, we present robustness tests

assessing the sensitivity of the main results to alternative model specifications, measurements, estimators, and data.

Establishing Associations

Post-treatment bias is a key concern in observational data analysis. For example, the perception of worsening economic conditions may influence the perception of one's current economic status, which in turn may influence violence motivations. A model controlling for individual economic status may thus underestimate the total effect of PROSPECTIVE LOSS on violence. We were less interested in mediators through which the three measures of deprivation influence violence. Hence, we started with basic models that only included controls that were unlikely to be outcomes of deprivations: gender (0 = man, 1 = woman) and age.

Table 1 reports logit estimates of participation in violence as a function of the three predictors. Models 1 and 2 indicate that the coefficients of INEQUALITY and RETRO-SPECTIVE LOSS are insignificant. By contrast, Model 3 indicates that PROSPECTIVE LOSS has a positive and significant coefficient. Models 1–3 analyzed the three measures separately. Model 4 included all three simultaneously. The results remain similar, with INEQUALITY and RETROSPECTIVE LOSS having insignificant coefficients and PROSPECTIVE LOSS having a significant coefficient. Substantively, an individual who believes that economic conditions will be "Much worse" is more than twice as likely to report participation in violence than an individual who believes that the conditions will be "Much better" (average predicted probabilities = 5.70% and 2.42%, respectively) (see also Figure 1). For comparison: analogous probabilities for men and women are 3.69% and 2.94%, respectively.

Addressing Endogeneity Concerns

The association between PROSPECTIVE LOSS and PVPARTICIPATION may be driven by past participation in violence causing expressions of deprivations. Hence, we analyzed PVINTENTIONS, which reflects interviewees' motivations to participate in violence at the time of the interview. The coefficient of RETROSPECTIVE LOSS was insignificant when regressed separately, whereas that of INEQUALITY was significant and positive (Table 2, Models 5–6). PROSPECTIVE LOSS coefficient was also positive and significant (Model 7). The three variables correlate (rs from .3 to .35); when all three were analyzed simultaneously, only PROSPECTIVE LOSS remained significant (Model 8). Substantively, an individual who believes that economic conditions will be "Much worse" is nearly twice as likely to report intentions to participate in violence than an individual who believes that the conditions will be "Much better" (10.29% and 6.03%, respectively) (see also Figure 1). For comparison: analogous probabilities for men and women are 8.07% and 6.50%.

We now turn to potential confounding, first addressing level-2 characteristics. The variation in our outcomes has two components: within-country variation (due to level-1

Table 1. Self-Reported Participation in Political Violence: A Multilevel Analysis.

	(1)	(2)	(3)	(4)
Fixed Effects				
Inequality	0.07 [−0.25, 0.38]			−0.14 [−0.49, 0.22]
Retrospective loss		−0.04 [−0.36, 0.27]		−0.34 [−0.70, 0.01]
Prospective loss			0.59** [0.23, 0.96]	0.73**** [0.37, 1.10]
Gender	−0.28**** [−0.38, −0.17]	−0.27**** [−0.38, −0.17]	−0.26**** [−0.37, −0.15]	−0.25**** [−0.36, −0.13]
Age	−0.42 [−0.94, 0.10]	−0.43 [−0.95, 0.09]	−0.41 [−0.96, 0.13]	−0.41 [−0.96, 0.14]
Constant	−3.63**** [−3.93, −3.33]	−3.59**** [−3.87, −3.30]	−3.80**** [−4.07, −3.54]	−3.64**** [−3.96, −3.32]
Random Effects				
Var(inequality)	0.31 [0.11, 0.86]			0.40 [0.15, 1.06]
Var(retrospective loss)		0.28 [0.10, 0.81]		0.35 [0.12, 1.04]
Var(prospective loss)			0.56 [0.23, 1.35]	0.49 [0.19, 1.29]
Var(gender)	†	†	†	†
Var(age)	0.96 [0.39, 2.39]	1.00 [0.41, 2.49]	1.11 [0.45, 2.71]	1.12 [0.46, 2.74]
Var(constant)	0.54 [0.30, 0.99]	0.46 [0.25, 0.85]	0.42 [0.22, 0.80]	0.52 [0.27, 1.00]
Observations	48686	49841	45572	44205
AIC	12351.67	12501.04	11659.85	11358.55
BIC	12413.22	12562.76	11720.94	11454.21
χ^2	26.90	26.93	32.16	37.81

The table reports fixed and random effects in logit with 95% CIs in brackets;

$*p < 0.05$, $**p < 0.01$, $***p < 0.001$, $****p < 0.0001$.

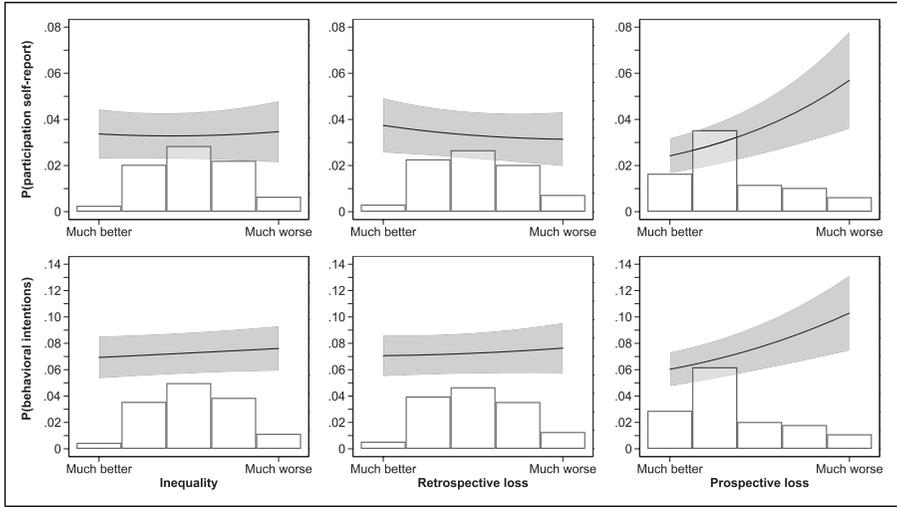† = excluded randomly varying slopes due to no significant improvement in the model fit

Figure 1. Average predicted probabilities with 95% CIs of self-reported participation in political violence and behavioral intentions to participate in political violence as a function of INEQUALITY, RETROSPECTIVE LOSS, and PROSPECTIVE LOSS. The probabilities were estimated using *margins* postestimation command in Stata following Models 4 and 8 as reported in Tables 1 and 2. The bars represent percentage frequency distributions of the predictors.

characteristics) and between-country variation (level-2 characteristics). For example, in wealthier countries, individuals may be on average less prone to violence. If in such countries individuals also feel less deprived, the covariation between country wealth and (individual-level) violence may confound the relation between deprivation and violence. We first attempted to address level-2 confounding by controlling for a standard set of level-2 observables, such as democracy, wealth, and population size (Section S2). Adding level-2 variables considerably reduced constant variance (Table S8); yet it still remained above zero, suggesting that some level-2 influences remained unaccounted for. If such unmeasured variables correlate with both our individual-level predictors and outcomes, this can lead to biased level-1 parameter estimates.

To fully partition the within-cluster effect from the between-cluster effect, scholars often use fixed-effects models. However, random-effects models can analogously account for higher-level confounding, but have better statistical properties, and allow for modeling randomly varying slopes (Bell et al. 2015; 2019; Gelman and Hill 2007; McNeish and Kelley 2019). To illustrate the issue, we plotted the coefficients for each predictor by country in Figure S1. The figure indicates considerable between-country variation in the coefficients. Likelihood ratio tests indicated that such between-country variation, for all three predictors, was unlikely due to chance. Failing to account for significant between-country heterogeneity can lead to biased estimates of level-1 coefficients and anticonservative standard errors (Bell et al. 2019, 1062–1065).

Table 2. Behavioral Intentions to Participate in Political Violence: A Multilevel Analysis.

	(5)	(6)	(7)	(8)
Fixed Effects				
Inequality	0.24** [0.08, 0.39]			0.11 [−0.06, 0.27]
Retrospective loss		0.21 [−0.02, 0.43]		0.03 [−0.20, 0.25]
Prospective loss			0.49*** [0.21, 0.76]	0.47*** [0.20, 0.74]
Gender	−0.30**** [−0.42, −0.18]	−0.30**** [−0.41, −0.19]	−0.28**** [−0.39, −0.16]	−0.27**** [−0.39, −0.15]
Age	−1.18**** [−1.50, −0.85]	−1.21**** [−1.53, −0.89]	−1.21**** [−1.53, −0.89]	−1.23**** [−1.56, −0.89]
Constant	−2.50**** [−2.71, −2.29]	−2.49**** [−2.72, −2.27]	−2.56**** [−2.77, −2.35]	−2.63**** [−2.87, −2.39]
Random Effects				
Var(inequality)	†			†
Var(retrospective loss)		0.22 [0.09, 0.52]		0.16 [0.06, 0.44]
Var(prospective loss)			0.44 [0.20, 0.93]	0.38 [0.17, 0.87]
Var(gender)	0.07 [0.03, 0.18]	0.06 [0.02, 0.16]	0.06 [0.02, 0.16]	0.07 [0.02, 0.19]
Var(age)	0.27 [0.07, 1.07]	0.27 [0.06, 1.11]	0.20 [0.04, 1.11]	0.24 [0.05, 1.17]
Var(constant)	0.30 [0.17, 0.54]	0.37 [0.20, 0.69]	0.33 [0.19, 0.60]	0.36 [0.19, 0.67]
Observations	47247	48391	44199	42863
AIC	22949.98	23436.12	21492.28	20804.28
BIC	23011.32	23506.42	21561.85	20899.61
χ^2	78.58	80.11	83.64	80.20

The table reports fixed and random effects in logit with 95% CIs in brackets;
*$p < 0.05$, **$p < 0.01$, ***$p < 0.001$, ****$p < 0.0001$.
† = excluded randomly varying slopes due to no significant improvement in the model fit

Therefore, instead of specifying standard country-level fixed effects, we continued with hierarchical models with randomly varying slopes, introducing several extensions to account for level-2 confounding.

One way to account for level-2 confounding in the random-effects framework is to add level-2 means of level-1 predictors as controls, following Mundlak (1978).[5] Mundlak specification is mathematically equivalent to group de-meaning of predictors (Bell et al. 2019, 1056), another method to obtain "pure" within-group effects (Raudenbush and Bryk 2002, 139–141). We replicated analyses with country-level means of level-1 predictors as controls, country demeaned level-1 predictors, and fixed-effects models (conditional maximum likelihood) (Table S9). All models show that unobserved level-2 variables do not confound the key relationships of interest.

We now address level-1 confounding. Scholars are sometimes advised to report balance tests, indicating whether the "treatment" and "control" groups are significantly different with respect to some other covariates. A failed balance test, however, does reveal whether the imbalance confounds our key relationships of interest. Conversely, a passed balance tests does not rule out confounding, since confounders may be among the unobserved variables, and such tests are mute on unobservables. Considering prior research and theory (Deaton and Cartwright 2018), we analyzed five sets of potential confounders: demographics; variables related to political activism or mobilization; measures of perceived repression; variables pertaining to government support; and variables highlighted in psychological research on collective action (including ethnic discrimination). We report the analyses of these variables in Section S3. In short, most of these variables had significant coefficients, corroborating their relevance for explaining violence. None of them, however, notably attenuated the relationships between deprivation measures and violence.

Despite our efforts to rule out alternative explanations, our estimates remain vulnerable to unobserved confounding. While we cannot rule out this possibility, we can evaluate the influence that unobserved confounders would need to have to change our main conclusions. Specifically, drawing on the idea that the amount of selection on the observed controls in a model provides a guide to the amount of selection on unobservables (Altonji, Elder, and Taber 2005; Oster 2019), we estimated the risk of omitted-variable bias. Section S4 demonstrates that unobserved confounders are unlikely to drive our results.

A range of alternatives exist to conventional conditioning that may enable causal interpretation of observational results, including matching and instrumental variable (IV) estimation. We see these alternatives as less useful or applicable in our case. Pre-processing data with matching can be useful in many other respects; for example, to increase the overlap between "control" and "treatment" groups (Gelman and Hill 2007, 206–212) or produce less model-dependent inferences (Ho et al. 2007). However, matching, as a model-based method to generate causal inferences, does not solve the unobserved confounding problem, because units are matched on observables (Sekhon 2009). Furthermore, model dependency is unlikely to be a problem in our case, since

our estimates essentially do not change as we move from very simple to more complex models, and we have estimated nearly 100 models.

We do not see an IV-based strategy as plausible because our predictors are psychological variables. Instrumental variables typically instrument objective characteristics, which may not clearly map onto subjective perceptions; hence, instruments that correlate with, for example, objective economic downturns will not necessarily capture the perceptions of economic downturns. In our sample, (level-2) economic downturns correlated with (level-1) RETROSPECTIVE LOSS only at $r = 0.0003$, and with PROSPECTIVE LOSS at $r = 0.04$. One might argue that we should then instrument for subjective perceptions. However, we cannot envision instruments that relate to perceptions of prospective loss or inequality, and which have no direct effects on motivations for violence. Furthermore, conventional IV-based approaches produce instrument-dependent estimates of complier average causal effects, but do not allow identifying the compliers, that is, individuals among whom the instrument induced changes in the variable of interest. Since this variable is subjective perceptions in our case, such compliers (i.e., individuals whom the instrument induced to perceive prospective loss) may represent a specific sub-set of people. Among this sub-set, deprivations may or may not relate to violent motivations similarly to the population of interest (Deaton 2010; Swanson and Hernán 2014).

Additional Sensitivity Tests

We conducted multiple other sensitivity tests, (i) with alternative measures of predictors, (ii) using intentions to participate and self-reported participation in protests as outcome variables, (iii) splitting the sample into government supporters and non-supporters, (iv) controlling for dishonesty, influenced answering, and comprehension, (v) using alternative operationalizations of outcomes and alternative estimators, (vi) and using alternative data (Afrobarometer Round 2). Section S5 demonstrates that our results are not sensitive to these alternative modeling choices.

Discussion

Drawing on political science, psychology, and economics, we have argued that dynamic forms of relative deprivation, specifically prospective decremental deprivation, should predict motivations to participate in political violence, whereas static forms of relative deprivation should not, or only weakly. Analysis of two large, multinational datasets provided evidence consistent with these claims. Before turning to the implications of these results, we consider potential limitations of our analysis.

Limitations

Several macro-level studies have identified a positive association between (static) vertical inequalities and civil conflict (e.g., Bartusevičius 2014; Boix 2008; Buhaug,

Cederman, and Gleditsch 2014; see also Bartusevičius 2019). Why have we not found an association between inequality and violence? One possibility is that our outcome measures do not distinguish between different types of violence. The mentioned studies suggest that vertical inequalities should specifically relate to non-ethnic conflicts; hence, our results potentially underestimate the effects of inequality on non-ethnic violence. Another possibility is that our outcome measures do not distinguish between small- and large-scale violence, and vertical inequality may specifically relate to large-scale conflicts. However, some scholars suggest that the association between vertical inequality and civil conflict is not strong, at least when compared to the horizontal inequality-conflict link (e.g., Østby 2013). Hence, our results are not incongruent with all macro-level research. Indeed, our findings hint at a plausible explanation for the weak link between vertical inequality and civil conflict: perceived inequality does not motivate individuals to partake in violence or does so only weakly.

The fact that perceived inequality did not predict violence can also be explained by its disregard for unfairness. In contrast to the political science literature, which has typically focused on inequalities as such (exceptions include, e.g., Dyrstad and Hillesund 2020), social psychologists have stressed perceived unfairness and anger (Pettigrew 2016). Although perceived disadvantage can generate negative affect, one does not automatically follow from the other. People often accept their disadvantage as appropriate (ibid.). Thus, our measure of inequality may not tap anger, which may be necessary to motivate violence. However, this does not explain why our measure of prospective loss, which also disregards unfairness, *does* predict violence. Perhaps losing what one once possessed/attained is more often perceived as unfair.

Further, our research did not empirically account for group dynamics and political opportunity structures. Even in the presence of intense anger, people's engagement in political violence depends on contextual factors, for example, other individuals willing to engage in violence, leadership, normative and utilitarian justification of violence, and coercive capacity vis-à-vis governments (Gurr 1970/2011). Such variables may reduce the relation between motivations to engage in political violence and actual participation.

Since our analyses exclusively relied on African samples, a note on generalization is also warranted. Our theory provides no rationale to suggest that deprivations relate to violence differently among African populations, compared to other populations; hence, we believe the findings should generalize more broadly. A possibility exists that some contextual factors, specific to African countries, moderate the associations between deprivation measures and violence. Indeed, as shown in Figure S1, the associations considerably varied across countries. To further explore this, we conducted an analysis of cross-level interactions (Section S6). These analyses suggest that most standard country-level characteristics, such as wealth, economic growth, and population size, do not moderate the associations between deprivation measures and violence, and hence that our findings likely generalize to other countries varying along such characteristics.

Finally, since we used self-reports, we must reiterate the possibility of nonresponse bias. As noted, average deprivation levels, for all measures, were higher among

interviewees who refused answering questions about violence. This suggests that deprivations potentially have stronger effects on violence than our results indicate. Future research should therefore explore alternative techniques to ask sensitive questions, for example, list experiments. Note, however, that the reliability of list experiments has recently been challenged and important adjustments to the standard item-count design have been proposed (e.g., Kramon and Weghorst 2019).

Implications

Our study implies, most importantly, that temporal dynamics in economic conditions are more important than the economic status quo for understanding political violence (for concurring evidence, see Buhaug et al. 2021). A broader implication is that individual-level grievances *do* matter for violence, but that to account for this association we need to focus on particular—and theoretically informed—grievance types (see also Dyrstad and Hillesund 2020; Hillesund 2015; Koos 2018; Rustad 2016). Recent macro-level studies have suggested shifting focus from individuals to groups. This call has been motivated by both theoretical claims that "violent conflict is a group phenomenon, not situations of individuals randomly committing violence against each other", and findings that macro-level proxies of individual-based grievances weakly predict civil conflict (Østby 2013, 212–213). We do not challenge the importance of focusing on group-level attributes; but we emphasize that all groups are made of individuals, and that it is individuals, not groups, who make decisions about violence. An account of political violence that disregards individual decision-making is thus incomplete. Resentful individuals can become leaders and mobilize others or choose to become followers. Countries with larger recruitment pools are more susceptible to violence. Given the evidence above, we argue that such recruitment pools will grow particularly with the population share of individuals anticipating economic losses, thereby increasing the overall likelihood of civil conflict.

Our study potentially explains why many attempts to associate individual-based grievances and civil conflict were unsuccessful: the focus has been on the "wrong" type of deprivation, which is psychologically too weak a motivator of violence. Paradoxically, while macro research has primarily focused on static relative deprivation (or inequality), neither early theoretical work on political violence, social psychological research, nor behavioral economics suggest that it should strongly relate to political violence.

New Research Avenues

Our study also hints at several unanswered questions and fruitful research avenues. First, our theoretical analysis has suggested that (i) dynamic, compared to static, forms of deprivation should be stronger predictors of violence, and that (ii) group relative deprivation, compared to individual, should be a stronger predictor of violence. Combined, the two propositions hint at the importance of *temporal group relative*

deprivation for political violence. The temporal aspect relates to anger and risk-seeking; the group aspect should further contribute to mobilization. Thus, we expect temporal group deprivations to be among the most powerful predictors of violence (see also Cederman, Wimmer, and Min 2010; Petersen 2002).

While testing the effects of deprivations in the future, scholars should pay particular attention to unfairness. Overwhelming psychological evidence suggests that humans are more averse to unfairness than to inequality as such (Starmans, Seshkin, and Bloom 2017). Some studies even show that more unequal countries have, on average, citizens who are happier (Kelly and Evans 2017). This insight was also explicit in Gurr's original work, referring to deprivations over "the goods and conditions of life to which people believe they are *rightfully entitled* [our emphasis]" (Gurr 1970/2011, 24). While in theory macro-level research often refers to grievances generated by inequalities that are perceived unfair, few empirical analyses have attempted to directly account for this. Joining other recent micro-level studies (e.g., Dyrstad and Hillesund 2020), we thus call for the study of inequity, rather than inequality per se.

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Supplemental Material

Supplemental material for this article is available online.

Notes

1. We use "political violence" to refer to a broader construct of *coalitional violence for a political cause*, which subsumes various more specific types of violence, such as civil conflict or non-state conflict. We return to definitional issues below.
2. While this is commonly overlooked in extant research, Gurr's theory considered discontent arising out of group-based comparisons (e.g., 1970/2011, 25; see also Runciman, 1966).
3. Modeling multiple random slopes simultaneously is computationally intensive. We used Stata's *melogit*, which implements maximum likelihood estimation with numerical integration. Stata allows for choosing between three numerical integration methods, with the mode-curvature adaptive Gauss-Hermite quadrature being the fastest in our setup. Stata also allows adjusting N of integration points. A tradeoff exists between computation time and bias in parameter estimates, with every additional integration point slightly reducing bias, but

considerably increasing processing time. We selected 4 integration points, which produced estimates, for most parameters, identic to the third significant figure to those produced with 5 integration points, but decreased processing time up to five times. Analyses were conducted with parallel-processing Stata/MP on an 8-core computer.

4. Random effects in models with random slopes are not invariant to transformations of predictors. However, our primary interest was in individual-level associations, and fixed-effects are invariant to (linear) transformations. The average predicted probabilities reported below are also invariant to the transformations.

5. Mundlak specification fully accounts for confounding by level-2 characteristics in linear models; however, in generalized linear models such an approach does not reliably partition between-level-2 effects from within-level-2 effects. Simulations show that this can introduce some bias in the estimates of level-1 coefficients; however, typically, the remaining bias in level-1 coefficients due to level-2 confounding is negligible (Bell et al. 2019, 1066–1067). Given that it allows for modeling randomly varying slopes, we opted to rely on the Mundlak specification. We have crosschecked our analyses with fixed-effects estimators and found nearly identical results to those produced by the Mundlak specification (without random slopes).

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