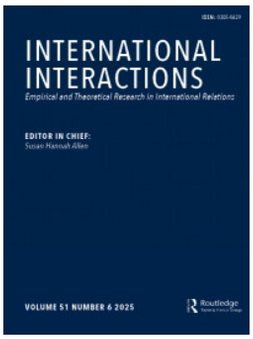

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ABSTRACT

Existing quantitative studies of conflict rarely move beyond treating military capacity as a control variable to explicitly examine its relationship with civilian victimization. Addressing this gap, this paper argues that military capacity is a double-edged sword. On the one hand, lack of military capacity leads to agency loss, makes soldiers more "desperate", and inhibits selective applications of violence. However, should physically harming civilians serve a strategic or ideological purpose for the government, military capacity would only facilitate the government's implementation of this policy. Which side of the sword prevails, this paper argues, depends on the political costs of civilian victimization: military capacity aggravates one-sided violence when the government faces low costs. The paper evaluates this theoretical argument using dyadic data on one-sided violence from 1990 to 2011. The dataset includes all intrastate conflicts during this period that resulted in 25 or more battle deaths, encompassing 60 governments and 195 rebel groups. Empirically, military capacity increases one-sided violence in contexts where the government experiences limited political costs from victimization: in ethnic outgroups and autocracies. These associations are robust to alternative measures of the variables and different model specifications.

KEYWORDS

Democracy; ethnic conflict; internal conflict; military capacity; one-sided violence

RESUMEN

Los estudios cuantitativos existentes sobre conflictos rara vez van más allá de tratar la capacidad militar como una variable de control que tiene por objetivo estudiar de forma explícita su relación con la victimización civil. Este artículo aborda esta brecha de forma que nos permite argumentar que la capacidad militar es un arma de doble filo. Por un lado, la falta de capacidad militar provoca pérdida de agencia, hace que los soldados estén más «desesperados» e inhibe las aplicaciones selectivas de la violencia. Sin embargo, si el hecho de provocar daños físicos contra los civiles sirviera a un propósito estratégico o ideológico para el Gobierno, la capacidad militar solo facilitaría la implementación de esta política por parte del Gobierno. El artículo argumenta que el que prevalezca uno de estos «filos» o el otro depende de los costes políticos de la victimización civil: la capacidad militar agrava la violencia unilateral cuando el Gobierno se enfrenta a bajos costes. El artículo estudia este

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argumento teórico utilizando datos diádicos sobre violencia unilateral desde 1990 hasta 2011. El conjunto de datos incluye todos los conflictos intraestatales durante este período que provocaron 25 o más muertes en combate, abarcando 60 Gobiernos y 195 grupos rebeldes. De forma empírica, la capacidad militar aumenta la violencia unilateral en contextos donde el Gobierno soporta costes políticos limitados por la victimización, como, por ejemplo, en grupos étnicos externos y autocracias. Estas asociaciones son sólidas frente a medidas alternativas de las variables y a diferentes especificaciones del modelo. Palabras clave: violencia unilateral, capacidad militar, democracia, conflicto étnico, conflicto interno Capacité militaire et meurtres perpétrés par l'État lors de conflits internes

RÉSUMÉ

Les études quantitatives existantes se contentent souvent de traiter la capacité militaire comme une variable de contrôle pour examiner de façon explicite sa relation avec la victimization civile. Pour combler cette lacune, cet article affirme que la capacité militaire est une arme à double tranchant. D'une part, le manque de capacité militaire conduit à une perte d'agence, rend les soldats plus « désespérés » et inhibe l'application sélective de la violence. Cependant, si la violence physique sur des civils sert un objectif stratégique ou idéologique pour le gouvernement, la capacité militaire ne ferait que faciliter la mise en œuvre de cette politique par le gouvernement. Cet article affirme que les coûts politiques de la victimization civile déterminent la mécanique qui l'emporte : la capacité militaire aggrave la violence unilatérale quand le gouvernement risque de faibles coûts. L'article évalue cet argument théorique à l'aide de données dyadiques sur la violence unilatérale entre 1990 et 2011. L'ensemble de données inclut tous les conflits interétatiques au cours de cette période qui ont débouché sur au moins 25 morts au combat, couvrant ainsi 60 gouvernements et 195 groupes rebelles. Sur le plan empirique, la capacité militaire accentue la violence unilatérale dans des contextes où le gouvernement subit des coûts politiques limités à cause de la victimization : dans les groupes ethniques extérieurs et les autocraties. Ces associations résistent à d'autres mesures des variables et différentes caractéristiques de modèles.

Introduction

From early 2008, Somalia's capital, Mogadishu, was heavily contested between rebel and TFG (Transitional Federal Government of Somalia) forces. By November that year, as Islamist rebels took gradual control of Mogadishu, many government and pro-government forces abandoned their posts (Stepanova 2009). During that period, TFG and pro-government troops resorted to indiscriminate and excessive shelling of residential and commercial areas (Stepanova 2009). According to reports from non-governmental organizations in the area at the time, TFG forces also engaged in high levels of one-sided violence and human rights abuse.

"Among the most common violations reported were gang rape, and a type of killing locally referred to as 'slaughtering,' or 'killing like goats'" (Amnesty International 2008). Observers on the ground blamed these atrocities on the "weak transitional Somali government" (Amnesty International 2008, 7). Did Somalia's lack of relevant capacities beget these outcomes? Just as terrorism tends to be a "weapon of the weak" (Crenshaw 1981), could it be that civilian victimization is a weapon of weak militaries?

Improving a government's fighting capacity, however, may not necessarily rectify the problem. Consider Sri Lanka's fight against the Liberation Tigers of Tamil Eelam (LTTE). In 2009, Sri Lanka's military was significantly more capable than that of Somalia's transitional government. Its total military spending was 1,485 million USD, which was more than 30 times that of Somalia (Singer 1988; Singer, Bremer, and Stuckey 1972). That year, Sri Lanka invested approximately 151,000 USD in each soldier, whereas Somalia spent a meager 2,000 USD per military personnel (Singer 1988; Singer, Bremer, and Stuckey 1972). According to reports on the ground, however, Sri Lankan government forces repeatedly attacked civilian areas, killing countless lives. As an example, between May 9 and 10, government forces conducted heavy shelling near an NFZ (No Fire Zone), killing or wounding 430 ethnic Tamil civilians (U.S. Department of State 2009, 42). It is alleged that the government had urged civilians to gather in the NFZ in question, before conducting an overnight artillery barrage on the area (U.S. Department of State 2009, 42). A lot of this violence was targeted at the Tamil minority. The Sri Lankan government forces were also accused of kidnapping and disappearing young male Tamil civilians; as a result, many Tamils were "afraid to move to government-controlled areas" (U.S. Department of State 2009, 45). In Sri Lanka's case, military capacity seems to have aggravated government violence against civilians, especially for the minority Tamils.

States have varying capacities to "get things done" (Mann, 1984: 189). Subject to the research question at hand, researchers have focused on different aspects of this capacity, such as the coercive (Fearon and Laitin 2003; Geddes 1996; Skocpol 1985; Tilly 1975), fiscal (Besley and Persson 2009; Geddes 1996; Levi 1988; Tilly 1975), infrastructural (Mann 1986; Soifer and vom Hau 2008), intelligence (Winward 2021), and information (Brambor et al. 2020; Lee and Zhang 2017) capacity of the state. As this paper seeks to analyze how the state's fighting capacity impacts the use of violence against civilians during conflict, it will focus explicitly on the *military* capacity of each state, defined as "the ability to project conventional military force" (Hendrix and Young 2014, 329). Empirically, the variable is operationalized with Hendrix and Young's (2014) latent measure (updated to 2010).[1]

According to UCDP's (Uppsala Conflict Data Program) data, governments have been responsible for more civilian killings than rebels: in intrastate conflicts between 1946 and 2022, a total of 528,995 civilians are known to have been killed by rebel forces and 976,843 have been killed by state forces (Davies, Pettersson, and Öberg 2022; Eck and Hultman 2007). More recently, civilian deaths directly attributable to government forces have been diminishing. Yet, as the Stockholm International Peace Research Institute (SIPRI) noted in 2009, this ostensible downward trend may merely indicate an "outsourcing" of one-sided violence, as more and more governments delegate "dirty tasks" to loosely affiliated pro-government forces (Stepanova 2009, 44). So, when does internal conflict prompt governments to kill their own citizens? This paper seeks to contribute to the scholarship on conflict (Krcmaric 2018; Schubiger 2021; Ulfelder and Valentino 2008; Valentino 2004; Valentino, Huth, and Balch-Lindsay 2004) by bringing the state's military capacity to the center of the analysis. Indeed, a key attribute of the modern state is its successful claim over "the monopoly of the legitimate use of physical force within a given territory" (Weber 1946, 78). Whereas the modern state carries out a number of other functions, its ability to apply physical force takes center-stage during an internal conflict, so that the state could combat rebel organizations that challenge the state's monopoly on violence.

According to literature, military capacity can have two contravening effects on civilian victimization. As noted by human rights scholars, state capacity can be a "double edged sword" (Chae 2021; Cingranelli, Mark, and Sadykova-DuMond 2023). On the one hand, capable states have fewer short-term motives to kill noncombatants. With enhanced intelligence, logistics, and fighting-power, state forces have little tactical gains to reap from sacrificing civilian lives. At the same time, militarily capable states have more deadly means – such as heavy weaponry and artillery – at their disposal (Stepanova 2009, 43). Hence, if a state is intentionally seeking to physically harm civilians – or at least a specific group of civilians – it would have better means to do so. The primary goal of this paper is to compare the empirical merit of these two competing arguments using dyadic quantitative data on one-sided violence and human rights abuse.

The political costs of victimization, this paper will argue, determine which of these two sides prevail. Specifically, the paper will examine two factors that may affect the cost of victimization (Davenport 2007a). First, an important factor to consider is regime type. In democracies, killing citizens can undermine an incumbent's electoral prospects. Although civilian deaths could be passed off as an unavoidable "collateral damage" for the security and prosperity of a nation, a democratic incumbent must be conscious about the effects such killings can have on his political career. Moreover, owing to intricate checks and balances, democratic executives

must convince a number of veto players if they are to wield violence against their own citizens. By contrast, autocratic leaders frequently pursue their own interests at the expense of human rights (Davenport 1999, 2004; Escribá-Folch 2013; Poe et al. 1999). Without free press or fair elections, a dictatorship's office is relatively unaffected by state-perpetrated violence against civilians. With relatively few veto players, mass atrocities generate few hurdles to the leadership (Valentino 2004). Second, military capacity could be used against civilians if the target is an ethnic outgroup. In ethnic civil wars, targeted violence against a politically relevant ethnic minority could serve the (perceived) short-term and long-term interests of an incumbent government (Klein and Tokdemir 2019). In reality, attempts to repress particular ethnic groups can backfire (Cederman et al. 2020; Goodwin 2001; Kalyvas 2012b; Kalyvas and Kocher 2007). For parties embroiled in decades of ethnic conflict, however, these considerations are unlikely to affect the apparent utility of engaging in targeted victimization. Importantly, ethnic outgroups have limited means to incur direct political costs – often through disenfranchisement (Valentino 2004) – and few veto players (Daxecker 2014; Müller-Crepon 2022) to render victimization costly on their behalf.

Empirically, the unit of analysis will be conflict dyad-year, and the sample includes all intrastate conflicts that incurred at least 25 battle deaths from 1990 to 2011, as recorded in the UCDP Dyadic Dataset (version 23.1) (Davies et al. 2024; Harbom, Melander, and Wallensteen 2008). Each dyad consists of a government actor and a rebel group; for conflicts that involve multiple rebel groups at once, each rebel group comprises one separate dyad with the government. In total, there are 60 governments and 195 rebel groups in the dataset. I calculate the amount of one-sided violence incurred in each government-rebel dyad by matching on the actor IDs in the UCDP One-sided Violence Dataset (version 23.1) with those in the UCDP Dyadic Dataset (version 23.1). One-sided violence is defined as "the use of armed force by the government of a state or by a formally organized group against civilians which results in at least 25 deaths" (Pettersson 2022, 3). To limit the influence of large outliers in the right-skewed sample, the empirical analysis will primarily analyze government one-sided violence as a binary variable, using logistic regressions. The paper will also use linear regressions to analyze two human rights indices – the Political Terror Scale (PTS) scores and Cingranelli and Richard's Human Rights Data (CIRI) – as alternative dependent variables (Cingranelli, Richards, and Clay 2014a; Gibney and Dalton 1996). On average, the data shows that the relationship between military capacity and violence against civilians is conditional on two factors: regime type and ethnic exclusion. Military capacity tends to increase civilian victimization by autocracies but decreases violence by democracies. Furthermore,

military capacity has a strong influence on ethnically excluded groups, regardless of regime type.

The paper's contribution to literature is threefold. First and foremost, the paper furthers our understanding of why governments use violence against civilians. If previous studies have examined subnational variations in territorial control (Kalyvas 2012b), and the "varieties of civil war" (Kalyvas and Balcells 2010; Krcmaric 2018; Staniland 2012, 2021; Valentino, Huth, and Balch-Lindsay 2004), this paper brings the state's underlying fighting capacity to the center stage. Holding rebel-state relations and the intensity of conflict constant, how appealing an option is civilian victimization to states with different military capabilities? Second, the paper seeks to bridge the gap between two distinct yet closely related fields: human rights and conflict. Following recent developments in the human rights scholarship, the paper explores the duality of the state's military capacity and considers the political cost of victimization as a possible moderator. Finally, the study underscores the importance of ethnic dimensions in conflicts. For killings with no ethnic exclusion, military capacity has no meaningful influence on the government's propensity to use violence. Only when targeted at politically excluded ethnic group does military capacity affect government violence toward civilians.

Military capacity and civilian victimization

While states wield violence against civilians for a myriad of reasons, conflict – and particularly civil conflict – is one of the most consistent predictors of repression. Not surprisingly, therefore, a large body of research seeks to fathom the dynamics of violence against civilians that occur during wars. However, existing studies rarely consider the impact of the government's military capacity. Where capacity is examined, it tends to be with respect to a specific subset of violence: sexual violence. This paper proposes to address this gap by directly assessing military capacity's influence on the government's one-sided violence against civilians.

Conflict has one of the strongest associations with human rights abuse. A large body of research finds that ongoing conflicts increase the propensity of state-perpetrated violence (Beber and Blattman 2013; Cohen and Norda˚s 2015; Kalyvas 2012b). This relationship is so widely acknowledged that civil conflict is included as a standard variable in most statistical models of human rights (Chae 2021; Englehart 2009, 2017; Keith, Tate, and Poe 2009; Poe and Tate 1994). Comparing different explanatory models, Hill and Jones (2014) finds that, relative to the other common variables, civil war improves model fit and predictive power by the greatest amount. As underlined by Kalyvas (2012a), there is a burgeoning field of studies that explore a variety of mechanisms

through which conflict could affect the state's use of violence against citizens.

This conflict literature closely examines how violence against civilians is shaped by the strategic environments of a conflict. The military may believe that violence against the civilian population would generate advantages in the battlefield (Arreguín-Toft 2001; Downes 2017; Lyall 2009), minimize its losses (Downes 2017), allow it to overcome resource scarcity (Costalli, Moro, and Ruggeri 2020; Hultman 2007; Metelits 2009; Wood 2014a) and remove grass-root support for the enemy (Bhavnani, Miodownik, and Choi 2011; Kalyvas 2012b; Vargas 2009). State agents may also use violence along the lines of political (Balcells 2012, 2017; Valentino 2004) or ethnic (Costalli, Moro, and Ruggeri 2020; Salvatore 2016; Valentino 2004; Weidmann 2011) identities. Meanwhile, the utility of civilian victimization may depend on the characteristics of war. Staniland (2012, 2021) and Kalyvas and Balcells (2010) showcase a great deal of heterogeneity behind the context of civil wars. These analyses, however, do not examine the variations' consequences for civilian victimization. Although Valentino, Huth and Balch-Lindsay (2004) and Krcmaric (2018) debate whether guerrilla warfare is more conducive to mass violence than conventional wars, there is no consensus on this matter.

Civilian victimization is often the product of principal-agent issues. An agent's use of violence against civilians may not serve the principal's interests. Notably, violence could occur out of revenge, even when there are no clear strategic benefits to occur from it (Balcells 2017). Similarly, the organizational structure and recruitment methods of the military – rather than the principal's strategic considerations – may shape its propensity to victimize civilians (Cohen 2013; Hoover Green 2016; Manekin 2013; Weinstein 2006; Wood 2009). Despite such acknowledgement of principal-agent issues, however, scholars of civilian victimization rarely examine how a state's military capacity affects the propensity of violence. Ulfelder and Valentino (2008) mention the possibility that state capacity may be associated with variations in government use of violence against civilians, but the paper does not expand the argument much further. And while the impact of capacity is explicitly examined in the context of sexual violence (Butler et al. 2007; Lee and Tomashevskiy, 2023) and terrorism (Byman and Kreps, 2010; Chae and Kim, 2024), state capacity – and in particular the state's *military* capacity – deserves further attention in studies of government one-sided violence.[2]

Beyond conflict settings, state capacity – though not specifically military capacity – has received explicit attention from human rights scholars. Englehart (2009, 2017) notes that, despite the growing number of democratic countries and the development of international legal norms over

time, there has been no commensurate historical improvement to human rights indicators. State capacity has been proposed as a potential explanation for this lack of variation. Englehart's (2009, 2017) work, in particular, argues that weak states are most prone to human rights violations, since the center cannot prevent its rogue agents from abusing citizens for their selfish gains. Even in studies where state capacity is not the main explanatory variable, the state's ability to control its agents has been found to critically affect the level of repression (Acemoglu, Ticchi, and Vindigni 2010; Butler, Gluch, and Mitchell 2007; Fearon and Laitin 2003; Sullivan 2012). Capacity, however, concurrently has the potential to aggravate repression, depending on who wields this power. As Chae (2021) finds, a stronger state is not necessarily more humane if it falls under the hands of a dictatorship. Cingranelli, Skip Mark and Sadykova-DuMond's (2023) recent paper similarly argues that regime type and state capacity can jointly affect compliance with rights protection promises. This paper will incorporate these insights into conflict settings and explore how a state's *military* capacity can affect a government's propensity to inflict violence against civilians during civil wars.

In short, conflict prompts governments to use violence against citizens. There is little consensus, however, about why some governments engage in more abuse than others. Literature explores a number of factors that drive this variation, including – in some cases – the state's capacity to fight and govern. Building on these existing works, the paper will take a close look at how a state's military capacity affects the government's propensity to use violence against civilians.

Military capacity as a double-edged sword

How does military capacity affect a government's use of violence against civilians during internal conflict? Military capacity is a double-edged sword. Which side prevails depends on the government's accountability to the victims. Where the political costs of victimization are low, military capacity empowers states to pursue its goals at the expense of the civilian population.

On the one hand, civilian victimization, like terrorism (Crenshaw 1981), could be a "weapon of the weak". First, incumbents with poor military capability face significant uncertainty over their prospects of remaining in power and, as a result, face shorter time horizons. Violence against civilians can offer (perceived) short-term advantages to a "desperate" army (Downes, 2006). When the supply line is compromised, for instance, government forces may need to source supplies from civilians in their vicinity (Hultman 2007; Metelits 2009; Wood 2014a). Relatedly, lack of military capacity may inhibit state agents from selectively employing violence to target groups, resulting in indiscriminate forms of violence (Kalyvas 2012b).

Second, weaker militaries suffer from greater principal-agent problems. Soldiers could wield violence as part of personal vendettas, even when they are against the higher command's interests (Balcells 2017). Troops may similarly practice various acts of violence to facilitate recruitment and solidarity (Cohen 2013; Hoover Green 2016; Manekin 2013; Weinstein 2006; Wood 2009). According to this logic, stronger military capacity would reduce the state's victimization of civilians.

Meanwhile, the state could purposefully victimize civilians as part of a grand strategy. According to Arreguín-Toft (2001), "barbarism" is a strategy of the relatively strong, used for breaking a weaker insurgent's will. Scholars raise doubt about whether such barbaric abuse of the civilian population accomplishes its intended goals (Cederman et al. 2020; Goodwin 2001; Kalyvas 2012b; Kalyvas and Kocher 2007). Actual effects of victimization aside, nevertheless, governments around the world are frequently convinced that killing civilians in enemy territory – especially during irregular warfare – would deteriorate the insurgent's ability to continue the war (Downes 2006; Kalyvas 2012b). In addition to the perceived tactical benefits of civilian victimization, there may be ideological reasons for killing non-combatants during a civil war. Dubbed by Mitchell (2004) as "Grand Inquisitor" style abuses, violence against civilians could fulfill a government's political ideals. Comparable to the Grand Inquisition avidly seeking heretics to destroy, a government could employ violent means to accomplish its vision. Indeed, some of the world's most horrible atrocities have been carried out by what were arguably highly capable states (Easterly, Gatti, and Kurlat 2006; Valentino 2004).

The political cost of victimization determines which of these two relationships prevail. When killing civilians is politically costly, military capacity safeguards civilians from harm; for civilians whose victimization inflicts low political costs on the incumbent, military capacity can exacerbate the use of violence against them. Among a myriad of reasons why victimization may not incur costs, this paper will focus on two factors: authoritarianism and ethnic exclusion. First, authoritarian leaders face relatively few restrictions from abusing their citizens (Davenport 1999, 2004; Escribá-Folch 2013; Poe, Tate, and Keith 1999). By contrast, democracies face higher costs from abuse, because governments are more accountable to its people through popular votes (*voice*) and elite competition (*veto*) (Davenport 2007a, 57). Indeed, a wide range of empirical studies find that democracies protect human rights better than dictatorships (Davenport 1999, 2004; Englehart 2009, 2017; Poe, Tate, and Keith 1999; Poe and Tate 1994; Valentino, Huth, and Balch-Lindsay 2004). Importantly, even dictatorships could restrain their use of violence against civilians if institutional mechanisms are in place to raise the costs of victimization. For example, there is evidence that dictatorships with legislatures (Rivera 2017) and

single-party regimes (Davenport 2007b) are relatively less inclined to use violence against their population. Thus, democracy, in our context, represents a spectrum of political costs for the incumbent, rather than a dichotomous indicator of a political ideal.

Given the importance of political costs, ethnic outgroups face significant threats from their own governments. Indeed, a large scholarship underlines the salience of ethnic dimensions during internal conflict. In conflicts that mobilize forces along ethnic lines, ethnic identity becomes a key determinant of civilian victimization (Cederman et al. 2020; Fjelde et al. 2021; Klein and Tokdemir 2019; Salvatore 2016; Weidmann 2011). In such contexts, ethnically targeted killings hardly inflicts costs on the incumbent government, because mass killing of ethnic groups tend to go hand in hand with mass disenfranchisement, which curtails the group's voice (Valentino 2004, 156). And if not total disenfranchisement, the violence aims to reduce the electoral prospects of politicians from the target ethnic group, proscribing the ethnic group's representatives from incurring indirect political costs on the government (Daxecker 2014; Müller-Crepon 2022).

In short, military capacity is a double-edged sword. When there are low costs to civilian victimization, a strong military could point their guns toward noncombatants. Whereas citizens in democracies could check their elected officials through popular voice and elite vetoes, citizens in autocracies or members of ethnic outgroups are limited in their ability to do so.

Empirical analysis

Since variations in the military capacity of a state are readily observable at the macro level, this paper will compare government one-sided violence in 198 conflict dyads from 1990 to 2011, each of which consists of one government and one rebel group. In terms of model specification, a series of logistic regressions will be conducted. To examine the moderating effects of regime type, a set of models will interact military capacity with a measure of democracy. All models use robust standard errors clustered by country to adjust for groupwise heteroskedasticity. The main models also include year and region dummies.

Dependent variables

The study analyses two outcomes: government one-sided violence and ethnically targeted one-sided violence. The first dependent variable is the presence of any government one-sided violence in each dyad-year (Davies, Pettersson, and Öberg 2022; Eck and Hultman 2007). The variable takes the value of "1" if there was any one-sided violence by government agents

in a dyad-year and "0" if otherwise.[3] UCDP defines one-sided violence as "the use of armed force by the government of a state or by a formally organized group against civilians which results in at least 25 deaths" (Pettersson 2022, 3). Following UCDP's definition, any party controlling a state's capital is considered to be the government side. Another set of models will examine ethnically targeted one-sided violence to analyze whether the effect of military capacity depends on ethnic outgroup status. These models will use Fjelde et al.'s (2021) data on Ethnic One-Sided Violence to compare ethnically targeted killings with those that were not ethnically targeted.

Three indices on physical integrity rights will be used as alternative dependent variables. All three variables code the human rights practices of state and government agents. The first set of robustness checks will employ Cingranelli and Richard's Human Rights Data (CIRI) as alternative measures of state-perpetrated violence against civilians (Cingranelli, Richards, and Clay, 2014a,b). CIRI's physical integrity rights score ranges from "0" to "8". It is constructed by adding the scores across four categories of physical integrity rights abuse: disappearances, extrajudicial killings, torture, and political imprisonment. Another set of robustness checks will use Political Terror Scale (PTS) scores (Gibney and Dalton 1996) as alternative dependent variables. Gibney and Dalton (1996) created two versions of PTS scores: one is created based on US State Department reports, while the other is compiled through Amnesty International's sources. I have inverted the original PTS scores so that higher scores represent better human rights conditions. The PTS scores range from "0" to "5". As final robustness checks, as set of models will also use Fariss's (2014) dynamic ordinal item-response estimates of physical integrity as an alternative measure of repression. Fariss (2014) refines PTS data by introducing a theoretically-motivated modification to the estimation procedure. Schnakenberg and Fariss (2014) argue that reports on different aspects of a country's human rights protection provide varying levels of information concerning the nation's latent human rights environment. Fariss's latent measure also accounts for the possibility that the human rights regime in one period may be affected by that of the previous period and that the standards of human rights accountability may change over time (Fariss 2014).

Independent variable

The core explanatory variables will be military capacity, democracy, and ethnic exclusion. All explanatory variables except ethnic exclusion are lagged by 1 year to address reverse causality. Military capacity will be primarily operationalized using Hendrix and Young's latent measure

(Hendrix and Young 2014). The variable is created through a factor-analysis of three measures from the National Military Capabilities Dataset (Singer 1988; Singer, Bremer, and Stuckey 1972): military personnel, military expenditures, and military expenditures per soldier (Hendrix and Young 2014). Using the authors' original methods, I have extended the data up to 2010. Since the models will lag this variable by 1 year, the study period will range from 1990 to 2011. The latent military capacity variable is min-max normalized to range between "0" and "1".

In addition to this main variable, I will use two alternative indicators of military capacity. One set of robustness checks will use the logged value of each country's annual military expenditure (in millions of dollars) from the National Military Capabilities Dataset (Singer 1988; Singer, Bremer, and Stuckey 1972). A second set of analysis will operationalize military capacity with the log of military spending per personnel[4] (in millions of dollars), which represents the amount of military investment made to each serviceman or woman (Singer 1988; Singer, Bremer, and Stuckey 1972). Both estimators will be lagged by 1 year. Finally, one of the robustness tests will analyze the data using a different type of state capacity: bureaucratic capacity. Unlike military capacity, bureaucratic capacity has little relevance for the outcomes of a conflict. As a result, we should expect to see that bureaucratic capacity has no direct or mediating effect on one-sided violence. This capacity will be operationalized using Hendrix and Young's (2014) latent measure of bureaucratic capacity. Again, the variable is lagged by 1 year and min-max normalized to range between "0" and "1".

Democracy will be operationalized using Polity IV's composite score, where countries are placed on a scale between "-10 (strongly autocratic)" and "10 (strongly democratic)" (Marshall, Gurr, and Jaggers 2014). The measure is min-max normalized to vary between "0" and "1". As originally intended by the creators of the variable, we do not use the Polity score to dichotomously categorize regimes into democracies or autocracies. Instead, the Polity score is left as a continuous variable that acknowledges a spectrum of different institutional arrangements in between ideal types.

Ethnic exclusion indicates whether the non-government side in each conflict dyad represents an excluded politically relevant ethnic group. This measure is constructed in two steps. First, the involvement of ethnic groups in each dyad-year is identified based on the Conflicts Between Ethnic Groups Dataset (ACD2EPR 2021) (Vogt et al. 2015). Then, the status of each ethnic group associated with the conflict dyads is derived from the Ethnic Power Relations (EPR) Core Dataset 2021 (Vogt et al. 2015). Any ethnic group that is "discriminated", "powerless", or "self-excluded" is considered to suffer from ethnic exclusion ('1'). Conflict dyads that involve ethnic groups that are "dominant", "junior partners", "senior partners" or "monopolies" are considered not to be ethnically excluded

('0'). Dyads that do not involve ethnic groups at all or ethnic groups that the EPR Core Dataset considers to be politically "irrelevant" receive no values for the ethnic exclusion variable. I will operationalize ethnic exclusion as both continuous and binary variables. The continuous measure is the number of excluded ethnic groups involved in each dyad-year; for the binary measure, any dyad-year that involves even one ethnically excluded group is assigned "1" and those without any excluded group are assigned "0".

Control variables

The models will include a battery of control variables. To account for the heterogeneity of conflict (Kalyvas and Balcells 2010; Krcmaric 2018; Staniland 2021, 2012; Valentino 2004), the main models consider the strength of the rebel group in each dyad, according to the Non-state Actors Dataset (Cunningham, Gleditsch, and Salehyan 2013). The models also consider the intensity of conflict (Davies et al. 2022; Eck and Hultman 2007) by including the natural log of the number of battle deaths (lagged) in each dyad-year and account for the presence of any pro-government militia (PGM) (lagged) (Carey, Mitchell, and Paula 2022). Moreover, all main models include two variables that measure a country's commitment to international human rights laws. Existing studies underscore the importance of legal devices for the protection of human rights (Bobbio 1996; Henkin 1990). The first variable, "IGO involvement", is created using the International Governmental Organizations (IGOs) Data Set (version 3.0) (Pevehouse et al. 2020). Each country is given a different score depending on its engagement with IGOs: an observer is given a score of "1", an associate a score of "2", and a full member a score of "3". These scores are subsequently summed up by country-year and then divided by that year's global average. The second variable is a dummy variable that records if a country has ratified the first optional protocol of the International Covenant on Civil and Political Rights (ICCPR) in a given year. Signing this protocol allows citizens of that country to file complaints to the UN Human Rights Council, strongly binding a state to international human rights expectations. Finally, every main model controls for the log of GDP per capita and the log of each country's population size.

Some of the models will introduce additional controls. These models will also consider the type of conflict (Cunningham, Gleditsch, and Salehyan 2013) as yet another means to address the heterogeneity of conflict. Each government-rebel dyad is categorized into one of eight types of conflict (Table 2). Furthermore, these models will also consider diplomatic pressure from foreign donor states. Using AidData (AidData 2017),

the models will control for the sum of all foreign aid commitments a government received in a given year (in constant 2011 USD). Finally, models with additional controls will also take ethnic fractionalization into account. The ethnic fractionalization index estimates the probability that two randomly selected people in a country in a given year are from different ethnic groups (Drazanova 2020).

Table 1 lists the summary statistics for all variables considered in the empirical models (Table 2).

Main results

Military capacity's relationship with government-perpetrated violence is complicated. Confirming theoretical expectations, victims whose deaths generate low political costs for the government are negatively impacted by military capacity. For one, military capacity reduces the probability government on

Table 1. Summary statistics.

	Obs.	Mean	SD	Min	Max
Government OSV dummy	848	0.349	0.477	0	1
PTS (Amnesty)	847	1.930	0.813	1	5
PTS (State Department)	837	1.881	0.745	1	5
Fariss human rights score	848	−1.267	0.659	−3	1
Ethnically targeted OSV (intentional)	848	0.271	0.445	0	1
Ethnically targeted OSV	848	0.186	0.390	0	1
Ethnically untargeted OSV (intentional)	812	0.140	0.348	0	1
Ethnically untargeted OSV	826	0.206	0.405	0	1
Ethnic exclusion	663	0.781	0.414	0	1
Military capacity (latent)	848	0.688	0.109	0	1
Bureaucratic capacity (latent)	691	0.500	0.205	0	1
Ln (military expenditure)	834	14.051	2.251	6.908	20.357
Ln (military spending by personnel)	848	4.976	1.516	1.099	7.366
Polity scores	848	0.590	0.300	0	1
Rebel strength	848	1.594	0.645	1	4
Ln (Battle deaths)	848	4.807	2.313	0	9.397
PGM presence	848	0.862	0.345	0	1
IGO involvement	848	1.106	0.277	0.366	2.038
ICCPR	848	0.408	0.492	0	1
Ln (GDP per capita)	848	6.765	1.430	3.127	10.817
Ln (population)	848	17.510	1.708	13.316	20.947
Ln (total foreign aid)	848	20.290	3.352	0	24.334
Ethnic fractionalization index	723	0.593	0.216	0.016	0.889

Table 2. Conflict type.

Type of conflict	Proportion (%)	Observations
Autonomy conflict	3.36	28
Civil war	37.41	312
Communist rebellion	7.91	66
Coup d'etat	1.32	11
Ethnic conflict	7.67	64
Islamist rebellion	1.56	13
Secessionist conflict	33.69	281
Terrorist attacks	7.07	59
Total =	100.00	834

one-sided violence in democracies but not in autocracies. In addition, military capacity is more likely against groups that are ethnically excluded.

The models in Table 3 analyze military capacity's effect on government one-sided violence, by regime type. Model 1 conducts a naïve analysis without taking accountability into account. According to this model, military capacity appears to have no association with one-sided violence. However, Models 2 reveals that military capacity's relationship with one-sided violence is conditional on regime type. Compared with the naïve model, Model 2 has a lower AIC (Akaike Information Criterion), which indicates superior model fit. Model 3 analyses the count of government one-sided violence, using a negative binomial regression; the results do not substantively differ from those of Model 2. Model 4 is again a logistic regression upon a binary dependent variable, introducing additional control variables: foreign aid, type of conflict, and ethnic fractionalization. There are fewer observations here, due to missing values in the latter two variables. In both models, interactions between military capacity and the polity score are negative and significant, while the coefficient on military capacity is significantly positive. But the results do not change substantially from those of Model 2. For lower values of the polity score, military capacity has a positive influence on the likelihood of one-sided violence; for countries that are closer to full democracies, on the other hand, military

Table 3. Military capacity and one-sided violence.

	Model 1	Model 2	Model 3	Model 4
	Gov OSV	Gov OSV	Gov OSV	Gov OSV
Military capacity $_{t-1}$	1.300	16.183**	18.404*	18.076**
	(5.719)	(7.768)	(10.079)	(7.876)
Polity $_{t-1}$	−1.289	14.049***	15.697**	14.907**
	(0.821)	(5.204)	(7.515)	(5.877)
Polity $_{t-1}$* Military capacity $_{t-1}$		−21.776***	−27.600**	−23.309***
		(7.882)	(12.038)	(8.921)
Rebel strength $_{t-1}$	−0.075	0.020	1.468***	0.310
	(0.274)	(0.274)	(0.328)	(0.281)
Ln (Battle death) $_{t-1}$	−0.008	0.017	0.046	−0.009
	(0.072)	(0.072)	(0.095)	(0.066)
PGM presence $_{t-1}$	1.176*	0.585	−0.007	0.565
	(0.668)	(0.646)	(0.645)	(0.654)
IGO score	1.508	1.538	−1.798	1.733
	(1.374)	(1.513)	(2.091)	(1.402)
ICCPR	−0.926*	−1.534**	−1.986***	−1.529**
	(0.521)	(0.628)	(0.672)	(0.700)
Ln (GDP per capita)	−0.649**	−0.769**	−0.504	−0.790**
	(0.252)	(0.314)	(0.556)	(0.322)
Ln (population size)	−0.552	−0.587	−0.093	−0.349
	(0.343)	(0.403)	(0.370)	(0.457)
Ln (alpha)			2.365***	
			(0.229)	
Year and region dummies?	Yes	Yes	Yes	Yes
Additional controls?	No	No	No	Yes
AIC	899.618	856.819	5187.094	734.556
Observations	848	821	848	687

*indicates $p < 0.1$, **indicates $p < 0.05$ and ***indicates $p < 0.01$.
Cluster robust standard errors are in parentheses.

capacity has a negative effect on one-sided violence. In terms of marginal effects, Model 2 predicts that raising military capacity a standard deviation above its mean value increases the probability of government one-sided violence by 0.278 for full dictatorships (Polity score = 0) and by 0.096 for anocracies (Polity score = 0.5). For full democracies (Polity score = 1), by contrast, the model expects the probability of government one-sided violence to *decrease* by 0.087.

Figure 1 visually illustrates the findings from Model 2. In terms of predicted probabilities (Figure 1a), military capacity increases the likelihood of government one-sided violence in full dictatorships (Polity = 0); in full democracies (Polity = 1), however, military capacity reduces the likelihood of civilian victimization by the government. In terms of marginal effects (Figure 1b), the polity score reduces the positive impact of military capacity, such that each marginal increase in military capacity has a negative effect on government one-sided violence from polity scores of 0.76 and above.

Are ethnic outgroups more likely to suffer from the military capacity of a government undergoing internal conflict? Two models in Table 4 examine whether conflict dyads involving ethnically excluded groups are more prone to government one-sided violence. Model 5 uses a continuous measure of ethnic exclusion, whereas Model 6 uses a binary indicator. As expected, government one-sided violence is more likely in dyads where politically relevant ethnic groups are excluded. According to Model 6, increasing the government's military capacity by one standard deviation lifts the probability of government one-sided violence by 0.156 when a conflict dyad-year involves at least one ethnically excluded group but reduces it by 0.061 if a dyad-year has no ethnically excluded group.

As a further test of this relationship, the models in Table 5 distinguish between ethnically targeted (Models 7 and 8) and non-ethnic (Models 9 and 10) killings based on Fjelde et al.'s (2021) data. The dependent variable in Model 7 is whether a dyad-year experienced any government one-sided violence against a politically relevant ethnic group. In Model 8, the dependent variable is whether a dyad-year records any government one-sided violence that *intentionally* targeted a politically relevant ethnic group.

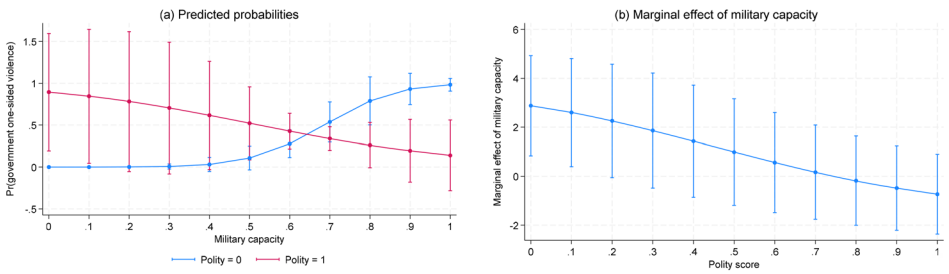

Figure 1. Military capacity, democracy and one-sided violence (Model 2).

Table 4. Military capacity and one-sided violence.

	Model 5	Model 6
	Gov OSV	Gov OSV
Military capacity $_{t-1}$	−4.350	−4.860
	(7.099)	(7.827)
Ethnic exclusion	−8.481**	−9.323**
	(3.490)	(4.405)
Ethnic exclusion* Military capacity $_{t-1}$	14.532**	15.846**
	(5.739)	(7.479)
Polity $_{t-1}$	−1.260	−1.334
	(0.902)	(0.930)
Rebel strength $_{t-1}$	0.388	0.416
	(0.352)	(0.400)
Ln (Battle death) $_{t-1}$	−0.069	−0.049
	(0.070)	(0.070)
PGM presence $_{t-1}$	0.453	0.566
	(0.963)	(1.038)
IGO score	1.535	1.492
	(1.485)	(1.626)
ICCPR	−1.050	−1.254*
	(0.765)	(0.745)
Ln (GDP per capita)	−1.126***	−1.095***
	(0.297)	(0.300)
Ln (population size)	−0.300	−0.270
	(0.505)	(0.515)
Year and region dummies?	Yes	Yes
Additional controls?	Yes	Yes
Observations	525	525

*indicates $p < 0.1$, **indicates $p < 0.05$ and ***indicates $p < 0.01$.
Cluster robust standard errors are in parentheses.

Table 5. Ethnically targeted one-side violence.

	Model 7	Model 8	Model 9	Model 10
	Ethnic OSV	Ethnic OSV	Non-ethnic OSV	Non-ethnic OSV
Military capacity $_{t-1}$	13.558**	21.095***	−3.681	−7.220
	(6.119)	(6.317)	(4.714)	(4.762)
Polity $_{t-1}$	0.060	−1.683	−2.451**	−1.600*
	(1.075)	(1.279)	(0.968)	(0.969)
Rebel strength	−0.103	0.391	0.465*	0.307
	(0.311)	(0.337)	(0.256)	(0.257)
Ln (Battle death) $_{t-1}$	−0.035	−0.099	0.008	0.018
	(0.084)	(0.107)	(0.067)	(0.063)
PGM presence $_{t-1}$	1.344*	1.467*	−0.163	0.039
	(0.697)	(0.856)	(0.767)	(0.599)
IGO score	3.011*	7.088***	−0.955	−0.182
	(1.584)	(2.173)	(1.494)	(1.299)
ICCPR	−2.440***	−2.530***	0.649	0.119
	(0.609)	(0.700)	(0.650)	(0.525)
Ln (GDP per capita)	−1.097***	−2.457***	−0.137	0.103
	(0.321)	(0.540)	(0.296)	(0.278)
Ln (population size)	−1.144**	−1.565***	0.316	0.444
	(0.477)	(0.588)	(0.462)	(0.383)
Additional controls?	Yes	Yes	Yes	Yes
Year and region dummies?	Yes	Yes	Yes	Yes
Observations	676	518	594	665

*indicates $p < 0.1$, **indicates $p < 0.05$ and ***indicates $p < 0.01$.
Cluster robust standard errors are in parentheses.

Models 9 and 10 are counterparts to Models 11 and 12 respectively. The dependent variables in these latter two models are whether there were any incidences of government one-sided violence once ethnic (Model 11)

and ethnically targeted (Model 12) killings are removed from the count. According to these four models, military capacity increases the likelihood of ethnically targeted one-sided violence but not that of non-ethnic one-sided violence (Figure 2). The models in Table 5, therefore, buttress the argument that military capacity could harm civilians that incur low costs of victimization for the incumbent. For ethnic outgroups, who are arguably low-cost targets to governments, military capacity has a strong influence on the state's propensity to use violence. To the contrary, higher military capacity is negatively associated with killings that are not ethnically targeted.

Robustness checks

There are several predictable ways in which these analyses could be biased. In an attempt to estimate the severity of such foreseeable issues, this section will propose some alternative empirical approaches. First, as alternative dependent variables, I will refer to three human rights indices: Cingranelli and Richard's Human Rights Dataset (Cingranelli, Richards, and Clay 2014a,b), the Political Terror Scale (PTS) (Gibney and Dalton 1996), and Fariss's latent measure of human rights (Fariss 2014). To analyze these human rights scores, I will be using Ordinary Least Squares (OLS) regressions. Second, as alternative measures of military capacity, I will use two estimators from the National Military Capabilities Dataset (Singer 1988; Singer, Bremer, and Stuckey 1972). Finally, as a test of the paper's theoretical mechanism, I will use Hendrix and Young's (2014) latent measure of bureaucratic capacity in place of measures of military capacity.

In Table 6, the models use human rights scores from the CIRI dataset (Cingranelli, Richards, and Clay 2014b) as their dependent variables. If the previous section explored the implications of military capacity for state-perpetrated *killings* of civilians, these models explore whether the theoretical argument may even extend to non-lethal forms of victimization. Model 11 analyses CIRI's overall physical integrity rights score. Again, the model provides strong empirical support for the theoretical argument.

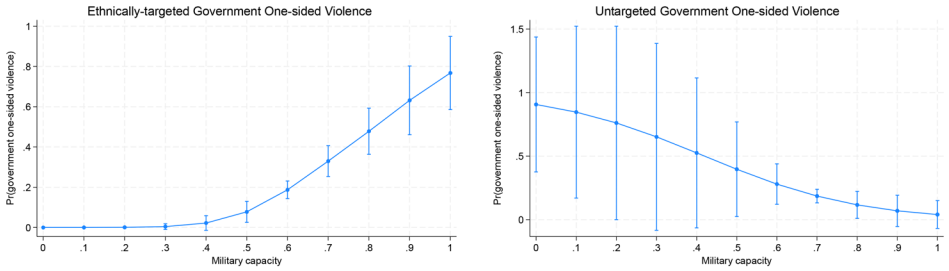

Figure 2. Military capacity, ethnic targeting and one-sided violence (Models 8 and 10).

Table 6. Military capacity and CIRI physical integrity rights.

	Model 11	Model 12	Model 13	Model 14	Model 15
	Physical integrity	Disappearance	Killings	Political prison	Torture
Military capacity $_{t-1}$	−7.299**	−4.865***	−2.944*	0.455	0.084
	(3.130)	(1.801)	(1.524)	(1.359)	(0.643)
Polity $_{t-1}$	−5.566*	−3.949**	−2.516**	0.933	−0.064
	(2.955)	(1.642)	(1.236)	(1.037)	(0.679)
Polity $_{t-1}$* Military capacity $_{t-1}$	9.503**	6.089**	3.754**	−0.782	0.445
	(4.137)	(2.311)	(1.724)	(1.553)	(0.959)
Rebel strength $_{t-1}$	−0.146	−0.049	−0.079	−0.022	0.005
	(0.130)	(0.045)	(0.056)	(0.075)	(0.034)
Ln (Battle death) $_{t-1}$	−0.196***	−0.091***	−0.049***	−0.023	−0.033***
	(0.034)	(0.016)	(0.012)	(0.015)	(0.011)
PGM presence $_{t-1}$	−0.509	−0.044	−0.088	−0.179	−0.202**
	(0.315)	(0.174)	(0.171)	(0.123)	(0.087)
IGO score	−0.583	−0.231	−0.172	−0.046	−0.133
	(0.557)	(0.337)	(0.300)	(0.336)	(0.157)
ICCPR	0.579***	0.313**	0.181**	0.109	−0.036
	(0.203)	(0.129)	(0.089)	(0.108)	(0.060)
Ln (GDP per capita)	0.024	0.044	−0.072	0.022	0.032
	(0.108)	(0.069)	(0.067)	(0.057)	(0.033)
Ln (population size)	0.344**	0.157*	0.165**	0.000	0.022
	(0.165)	(0.093)	(0.076)	(0.065)	(0.046)
Additional controls?	Yes	Yes	Yes	Yes	Yes
Year and region dummies?	Yes	Yes	Yes	Yes	Yes
R-squared (without interaction)	0.38	0.39	0.22	0.27	0.21
R-squared (with interaction)	0.40	0.41	0.24	0.27	0.21
Observations	670	672	674	672	674

*indicates $p < 0.1$, **indicates $p < 0.05$ and ***indicates $p < 0.01$.
Cluster robust standard errors are in parentheses.

Models 12 to 15 break the physical integrity score down to its four component scores. Interestingly, these models paint a particular pattern of state-perpetrated violence. Neither military capacity nor democracy has its expected effects on political imprisonment or torture. By contrast, military capacity and regime type have pronounced effects on disappearances and killings. These results hint at the purpose of the state's application of violence against civilians. Rather than keep their victims alive for some other purpose, it seems that the governments' main intention is to physically rid of undesirable segments of the population by killing or "disappearing" them.

Models in Table A1 use PTS scores and Fariss's latent human rights scores as alternative dependent variables. For both human rights indicators, military capacity has a negative effect on the state of physical integrity rights in non-democracies, which improves as countries become more democratic. In all six models (Models 16 to 21), military capacity has a significant, negative effect on the protection of physical integrity rights. And, with the exception of Model 17, a higher polity score can counteract some of military capacity's effects on human rights. In substantive terms, Model 16 predicts that a standard deviation increase in military capacity in a full dictatorship (Polity score = 0) drives the PTS (Amnesty

International) score down by "0.652". By contrast, the same increase in military capacity would only move the human score down by "0.331" if the country is an anocracy (Polity score = 0.5) and by just "0.011" if the country has the highest polity score (Polity score = 1).

Finally, Models 22 to 27 in Table A2 and Models 28 to 33 in Table A3 employ three different estimates of state capacity. Much like the main models in Table 4, models in Table A2 suggest that military capacity increases the likelihood of government one-sided violence conditional on the country's level of democracy. By contrast, Models 26 and 27 show that bureaucratic capacity has no implications for government one-sided violence. The models in Table A3 differ in some respects with those of the main models in Table 4. Models 28, 30 and 32 examine the effects of each alternative measure on ethnically targeted killings. In contrast, the dependent variable of Models 29, 31 and 33 is the presence of untargeted government killings in a dyad-year. While military expenditure (logged) affects ethnically targeted killings in the predicted direction, military expenditure per soldier (logged) has no significant effect. Finally, in line with existing works in the literature (Chae 2021; Englehart 2017, 2009), Model 33 indicates that bureaucratic capacity may significantly reduce untargeted killings.

Conclusion

Why do governments kill their own citizens during internal conflict? Military capacity, this paper argues, is a double-edged sword. Which side prevails depends on the political costs of victimization.

Based on existing works in the literature, the theoretical section hypothesized that the relationship between military capacity and government violence against civilians could go either way. On the one hand, desperate troops may resort to more desperate measures, which often involves indiscriminately killing noncombatants in a hostile area (Costalli, Moro, and Ruggeri 2020; Downes 2006). Indeed, egregious actions by state forces are often pinned on the military's "weakness" (Amnesty International 2008), characterized by a lack of discipline, disruptions in supplies, and rogue agents. As noted by recent works in human rights, however, state capacity is a "double-edged sword" (Chae 2021; Cingranelli, Mark, and Sadykova-DuMond 2023). If the government intends to kill civilians, a more capable military would only facilitate the attainment of that goal. (Arreguín-Toft 2001; Downes 2006; Kalyvas 2012b). The political cost of victimization determines which of these two sides prevails. Specifically, the theoretical section explored two factors that may influence this cost: regime type (Davenport 1999, 2004; Escribá-Folch 2013; Poe et al. 1999) and ethnic exclusion (Cederman et al. 2020; Fjelde et al. 2021; Salvatore 2016; Weidmann 2011).

In the empirical section, the paper used dyadic data on one-sided violence to evaluate the merit of the paper's argument. According to a naïve model, military capacity seemed to have no effect on government one-sided violence. However, further analysis revealed interesting heterogenous effects. First, military capacity had strong, positive effects on government one-sided violence in non-democratic contexts, but the variable had the opposite relationship with violence among democratic countries. Second, military capacity had greater influence on ethnic outgroups than it did on killings that were not targeting politically excluded ethnic groups. In terms of model fit, these models explained the data much better than naïve models without the interaction term. Altogether, the models revealed that military capacity's relationship with one-sided violence is conditional on at least the two factors studied in this paper. Null findings from the naïve model may be the consequence of these heterogenous effects canceling each other out.

The paper proposed an important moderator that affects military capacity's relationship with state-perpetrated violence during conflicts. Through a series of cross-sectional comparisons, the paper argued that the political costs of victimization affects whether a state's preparedness for war increases its propensity to physically abuse and kill civilians during conflict. There are, of course, inherent limits to cross-national comparisons. Most notably, the current empirical evaluation uses yearly aggregate measures of one-sided violence, human rights abuse and state capacity, which may not adequately reflect many of the relevant variations that occur during conflict. Nonetheless the paper's findings have important policy implications. Policymakers should account for the political incentives that shape state behavior, particularly how anticipated political costs can deter violence against civilians. Furthermore, the study implies that international and domestic actors should implement measures that enhance transparency and accountability, raising the political repercussions for states engaging in civilian victimization. Future studies could improve data collection at more granular levels to conduct further tests of this empirical association using disaggregated data.

Notes

1 The authors' original variable ends in 2007. Using Hendrix and Young's replication codes, I have extended their latent military and bureaucratic capacity variables to 2010.

2 Meanwhile, there is a tendency for literature on one-sided violence to gravitate toward studying *rebel* capacity's impact on rebel one-sided violence (Doctor and Willingham 2022; Wood 2014b).

3 As a further test of the proposed argument, one of the models will operationalize this dependent variable with the raw count of the victims of one-sided violence in each

dyad-year. Owing to overdispersion, this data will be analysed using a negative binomial regression.

4 Since the log of zero is undefined, I add "1" before taking the natural log of military spending per personnel.

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