Is Collective Repression an Effective Counterinsurgency Technique? Unpacking the Cyclical Relationship Between Repression and Civil Conflict

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Abstract. Research on the relationship between civil conflict and repression has led to one conclusion—the law-like finding that states respond to internal challengers with repression—and one puzzle with competing hypotheses—whether state repression escalates civil conflict or not. Studies of repression’s effect on conflict have been limited to case studies and subnational designs, which limits the external validity of the arguments. Studies of conflict’s effect on repression have treated conflict as a control variable without taking into account the inherent endogeneity between internal conflict and state repression. This article contributes by providing a general, cross-national study of repression’s effect on conflict, and vice versa, for external validity. Results of simultaneous equation models demonstrate that both directions of the relationship between state repression and conflict are positive and significant—suggesting a cyclical relationship—while single equation models with a lag structure establish that the effect of repression on conflict is greater than the reverse.

Keywords: repression, civil war, state violence, rebel mobilization, counterinsurgency

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Introduction

The relationship between repression and armed conflict remains one of the most enduring puzzles in conflict studies. Is state repression a cause of armed conflict, an effect, or both? An examination of scholarship addressing these questions would lead us to one conclusion and one remaining puzzle. Research on the causes of state repression has concluded that state repression is a consequence of threats to the state (Davenport, 2007a; Davenport, 2007b; McCormick and Mitchell, 1997; Poe and Tate, 1994; Regan and Henderson, 2002; Tilly, 1978), where armed threats are clearly more threatening than unarmed threats (Gurr, 1986). This finding is so consistent that it has been termed the law of coercive responsiveness (Davenport, 2007b).

The reverse side of the relationship—whether or how state repression affects conflict—remains an unresolved puzzle. State repression is thought to be both the mechanism through which conflicts escalate (Goodwin, 2001; Kalyvas, 2006; Mason and Krane, 1989) and the means by which opposition movements are defeated, thwarted, or deterred (Downes, 2008; Hegre and Sambanis, 2006, 522; Lyall, 2009). State repression has also been found to have no effect on conflict (Gurr and Moore, 1997), competing effects (Rasler, 1996), and varied effects based on whether dissent is rising or declining (Sullivan, Loyle, and Davenport, 2012).

The purpose of this article is to examine further both directions of the relationship between state repression and armed conflict. Despite its consistency, the law of coercive responsiveness requires further investigation because studies of state repression tend to treat the presence of armed conflict as a control variable without taking into account the endogeneity between the two variables. It is necessary to model this relationship simultaneously.

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1 A supplementary online appendix, the replication dataset, and corresponding do-files are available at phultquist.wordpress.com/research/. All statistical analyses were performed with STATA 12.
Studies of state repression’s impact on dissent, for their part, have considered both sides of the equation theoretically (DeNardo, 1985; Lichbach, 1987; Pierskalla 2010; Ritter, 2014) and empirically using both case study and cross-national methods (Carey, 2006; 2009; Francisco, 1996; Moore, 1998; Rasler, 1996; Ritter, 2014; Shellman, 2006a; 2006b). Still, most of these studies have focused on protest dynamics, which differ from conflict dynamics where the opposition is armed and civilians may be targeted without ever participating or cooperating with the opposition.\(^2\) Thus, we cannot apply the lessons of the protest-repression nexus directly to the study of repression under the condition of armed conflict.

Another subset of the literature on repression and dissent, to which this article seeks to directly contribute, has focused on repression’s impact on armed conflict dynamics. This subset has largely relied on case study research, which has provided rich, detailed, and disaggregated evidence for their arguments yet still vary on their conclusions. Both arguments for repression’s effectiveness and its counterproductivity have logical mechanisms that have been supported by in-depth qualitative interviews, process-tracing research, and sophisticated subnational quantitative designs.

While it is possible that both arguments are accurate for their individual cases, a broader investigation is necessary to identify whether there is a general pattern or whether the finding has external validity. Here, an aggregated cross-national study that explicitly examines armed conflict can contribute additional insights, despite the otherwise laudable direction of the research program towards disaggregated research designs.

\(^2\) It is important to note that some of this work conceptualized dissent on a continuum that viewed the differences between protest and armed conflict as a matter of degrees rather than a difference in kind (Moore, 1995; Moore, 1998). Thus, not all are concerned only with protest.
This article contributes to the study of repression and conflict by providing a general, cross-national study of repression’s effect on conflict and vice versa. In doing so, this study gives up measurement specificity in favor of generalizability and contributes to literature by testing its claims across a large number of cases. I model the endogeneity between state repression and conflict using a simultaneous equation estimation technique – bivariate ordered probit (Sajaia, 2008). The simultaneous equation findings demonstrate that both directions of the relationship between state repression and conflict are positive and significant, which is consistent with the theoretical expectation of a cyclical process. I then use a lag structure with single equation models to establish that the effect of repression on conflict is greater than the reverse. Taken together, this study finds that greater levels of repression lead to greater levels of conflict and, by implication, that the cases of repression providing effective deterrence are too few to be statistically significant. Further, the law of coercive responsiveness is supported but should be qualified because conflict’s effect on repression is smaller than repression’s escalation effect on conflict.

Substantively, the findings in this article suggest that states facing insurgent threats should avoid high levels of repression in times of armed conflict, not only because it is morally reprehensible, but also it is most likely a strategic blunder, with the perverse effect of strengthening the insurgency it is trying to quell. Presumably, if states adhered to this advice, the most consistent finding in the repression literature—the law of coercive responsiveness—may cease to exist in the future.

This article proceeds in five sections. First, I outline the state of the literature on state repression as a dependent variable. Second, I outline the state of the literature on the reverse side of the relationship, discussing the mixed findings regarding repression’s effect on conflict. Third,
I discuss data and the methods employed followed by a discussion of measurement issues and the endogeneity problem. I then present results for statistical tests followed by a discussion of the contribution of the study and address the limitations of the research design.

The Law of Coercive Responsiveness and the Punishment Puzzle

The question of how states respond to internal threats is a fundamental question of political order. Philosophers, classic sociologists, and contemporary social scientists alike have weighed in on the state’s role in quelling challenges to the existing order. Both Thomas Hobbes and Max Weber, for instance, viewed the state as an instrument to provide security for civilians by monopolizing violence, thereby crowding out internal armed challengers to the political order. Contemporary observers are not surprised, then, when states respond to armed challengers with violence. Later theorists, however, have questioned the utility of the repressive approach for dealing with armed challengers. They note that repression may motivate the opposition and even convince civilians to join the rebellion, which has the perverse effect of leaving the state worse off (Goodwin, 2001; Mason and Krane, 1989). The question of state repression and armed conflict, then, presents a classic puzzle. Do states respond to internal challenges with repression that effectively reduces dissent or does state repression lead to an increase in dissident behavior?

Challenge to the State and the Law of Coercive Responsiveness

Why do states use repression against their citizens? The state repression literature—a large and progressive research program—has provided several answers. Among the more prominent explanations for state repression concern the role of democracy (Bueno de Mesquita, et al. 2005; 3

3 These terms come directly from Christian Davenport’s (2007b) review article in the Annual Review of Political Science.
Davenport, 2007a; Davenport, 1999; Davenport and Armstrong, 2004), variations in autocratic regimes (Davenport 2007c), economic development (Mitchell and McCormick, 1988), state capacity (Englehart, 2009), and international factors (Hafner-Burton, 2005; Hafner-Burton and Tsutsui, 2005; Keith, 1999).

While these factors rightfully gain significant attention in the literature, the proximate cause of state repression is usually, if not explicitly, some perceived challenge to the political order. According to Gurr (1986, 51), “the necessary condition for state terrorism is the existence of a group, class, or party that is regarded by ruling elites as an active threat to their continued rule.” Indeed, the variables of interest listed above are often theorized to influence state violence indirectly. They constrain or facilitate the use of state repression given the presence of a challenge—whether latent, nascent, or actualized.

Empirically, this relationship is so overwhelmingly consistent that it has been termed the “law of coercive responsiveness” (Davenport, 2007b). While the hypothesis is specified in continuous terms, the findings are dependably strong across a variety of measures, including dichotomously at both 1,000 and 25 annual battle-deaths thresholds (Davenport, 2007c; Kathman and Wood, 2011; Krain, 1997; Mitchell, Carey, and Butler, 2014; Poe and Tate, 1994; Poe, Tate, and Keith, 1999). The consistency holds when more nuanced versions of dissident behavior are introduced to include strikes, protests, guerrilla warfare, and protests (e.g., Davenport, 1995; 1999; Valentino, Huth, and Balch-Lindsay, 2004).

The Punishment Puzzle

While states respond coercively to armed challenges with law-like consistency, the direction of the relationship is contested. State repression can backlash and increase the threat of armed
challengers by deepening grievances against the government and, in some cases, solving the rebels’ recruitment problem (DeNardo, 1985; Lichbach, 1995; Machain, Morgan, Regan, 2011; Mason and Krane, 1989). Alternatively, state repression can deter civilians from supporting or joining rebels, thereby de-escalating the conflict (Downes, 2008; Lyall, 2009).

The logic of the escalation effect of state repression is based on the rebel recruitment problem—a classic example of a collective action problem. The rational citizen, as much as he prefers the cause, chooses to not participate (i.e., free-ride) in the rebellion given the risks of supporting or joining a challenge to the state.

Whether rebel groups actually face this recruitment problem, however, depends on government’s use of violence/repression (Kalyvas and Kocher, 2007). If states are able to target rebels and their supporters selectively (i.e., individually), this rebel recruitment problem is reinforced, civilians are deterred from joining the rebel movement, and the conflict de-escalates because rebels can no longer recruit at high enough levels to sustain the conflict (DeNardo, 1985; Kalyvas, 2006; Mason and Krane, 1989).

The literature on state repression juxtaposes selective violence with indiscriminate or collective violence and argues that it accomplishes the opposite of selective violence. In contrast to selective violence that targets only insurgents and their collaborators, collective violence targets at a higher level of aggregation and thus cannot sufficiently distinguish between the guilty

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4 While rationalist accounts are perhaps most prevalent, other explanations have been proposed. State repression creates and/or reinforces political grievances against the state that may motivate civilians to join rebel movements through anger and desire for revenge (e.g., Hashim, 2006). The data presented below cannot differentiate between these motivations.

5 I use the term “collective” rather than “indiscriminate” to more accurately convey the level of aggregation at which states target, since the latter implies random violence, which is not accurate.
and innocent at the individual level. Two types of collective violence are most common: those that target collectively by location (e.g., village) and those that target collectively by identity (e.g., ethnicity). In each case, innocent villagers or co-ethnics are killed alongside those who support the insurgency. As problematic as this is for humanitarian and moral reasons, it is also a strategic blunder. Rather than reinforcing the logic of the insurgent collective action problem like the use of selective violence, collective state violence turns it on its head (Kalyvas and Kocher, 2007). Under conditions of collective state violence, rational individuals determine that nonparticipation is perhaps more costly than or just as risky as participation. In effect, collective violence fosters material and intelligence support for rebels from noncombatants and bolsters the ranks of the rebels (Kalyvas, 2006; Mason and Krane, 1989). Stronger rebels, then, can mount a more significant challenge to the state and eventually shift from low-intensity tactics, like sabotage and guerrilla warfare, to high-intensity tactics that challenge the government directly for control of territory (Butler and Gates, 2009; Hultquist, 2013).

That indiscriminate or collective violence is counterproductive is supported by numerous studies employing a variety of methods: including theoretical rational choice models (DeNardo 1985; Machain, Morgan, Regan, 2011; Mason and Krane, 1989), qualitative case studies (e.g., Goodwin, 2001; Peceny and Stanley, 2010; Stanley, 1996), and quantitative subnational designs (Balcells, 2011; LaFree, Dugan, and Korte, 2009; Kocher, Pepinsky, and Kalyvas, 2011).

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6 No use of violence discriminates perfectly either. As Kalyvas (2006, 174) argues, the perception of selectivity is what matters and, in reality, there will always be some level of uncertainty.

7 These categories overlap, in many cases.

8 The evidence that some locations/villages are targeted over others does not make the use of this violence selective. Since civilians in contested locations and those who are co-ethnics with insurgents represent the rebel recruitment pool, when they are targeted while innocent, it fits the logic of collective or indiscriminate targeting. We should still expect it to strengthen the insurgents.
and Ron (2007) provide cross-national evidence for the relationship but acknowledge that it suffers from endogeneity problems (697).

This one-time consensus has been challenged by several studies arguing that collective violence is militarily productive—that it weakens rebels or deters rebel violence. State collective violence against civilians can present the rebels with several logistical problems, such as re-settlement strategies that deny rebels of strongholds of civilian support (Downes, 2008), or “making it difficult for insurgents to maintain supply lines, protect safe refuges, and concentrate their forces” (Lyall, 2009, 336).

When rebels are weak and cannot protect their civilian supporters, targeting civilians may lead the civilians to abandon the rebels and even provide support for the state (Lyall, 2009; Kalyvas, 2006, chapter 6). If rebels offer no protection, civilians who support them get no benefit of security when the state uses collective violence and may turn on rebels for inciting the attacks (Kalyvas, 2006, chapter 6; Wood, 2010, 604).

Strong rebels may too provide the condition for collective violence. Several studies exploring why states target civilians find that they do so under conditions—across time and space—where a desperate state faces a formidable rebellion, such as those with substantial civilian support (Valentino, Huth, and Lindsay, 2004), during wars of attrition or taking back territory (Downes, 2006), or, subnationally, where states may choose massacre sites based on insurgent threat (Sullivan, 2012).

Despite the fact that collective violence can be effective under some circumstances, in general, collective violence is counterproductive in that it creates avoidable risks for states and is no more likely to be effective than selective repression.
Data and Methods

By reviewing the law of coercive responsiveness and the punishment puzzle, this article has identified three empirical questions, though only two of which are addressed empirically in this article. First, the discussion of the punishment puzzle raises the question of whether collective violence is effective, counterproductive, or neither. Second, that discussion also raises the question of whether selective state violence is effective or not. This question will not be addressed in the empirical section due to lack of adequate data. Third, given the dual expectations that greater rebel threat causes greater repression and that greater repression causes greater rebel threat, the question of which direction, if any, is strongest remains open.

To provide evidence that sheds light on these questions, I employ a cross-national, time-series research design aimed at identifying generalities of the relationship between repression and armed insurgency. Since previous research on the effectiveness of repression has tended to be in the form of qualitative case studies or quantitative subnational designs, where multiple studies have demonstrated the internal validity of the argument, this paper seeks to test this argument for generalizability or external validity. In doing so, I recognize the tradeoff of losing measurement specificity for generalizability.

The data I have compiled consists of a country-year dataset from various sources that includes 2,862 observations from a sample of 140 countries covering 28 years (1976-2004). To construct the dataset, I begin with Uppsala’s Armed Conflict Dataset (Gleditsch et al, 2002), which uses the country-year as the unit of analysis. The sample is limited spatially to 140 countries and between the years 1976 and 2004 due to available data for the measures presented below.

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9 The number of observations, countries, and years varies by the availability of data; hence, several models below have fewer observations.
Since we should expect rebel strength and collective repression to be interrelated, I employ a simultaneous equation model to control for the endogeneity of these two dependent variables. For jointly determined ordinal dependent variables, I use a seemingly unrelated bivariate ordered probit estimation technique (Sajaia, 2008) (discussed further below).

**Dependent Variable #1: Two Measures of Conflict Escalation**

The primary dependent variable is conflict intensity or the battle death rate, which proxies rebel strength. “As insurgent ranks swell, so too does its coercive capacity” (Lyall, 2009, 335). New recruits increase how many attacks a rebel organization can execute as well as how much battlefield experience it can sustain (Lyall, 2009, 336). Furthermore, the intensity of conflict is an indicator of rebel strength due to the tactics employed. It takes a strong rebel group to fight the government conventionally (that is, directly), which produces the most battle deaths. When very weak and moderately weak, rebels rely on terror tactics that target civilians (but do not count as battle deaths) and guerrilla warfare-style hit and run tactics, respectively (Butler and Gates, 2009; Wood, 2010).

I follow the literature in assuming that states are rational and prefer the absence of an armed threat. Therefore, an increase in the intensity of an armed conflict represents a greater threat to the state. The reverse is true as well. State strategies attempt to reduce the threat posed by an armed rebellion. The intensity, or battle-death rate, is one indication of whether or not these strategies are working.

For the primary models reported below, I measure the intensity of armed conflict by constructing an ordinal version of annual battle deaths from a continuous measure of the annual number of battle deaths (Lacina and Gleditsch, 2005). I use an ordinal version for two reasons.
First, while all measures are imperfect, a continuous, count measure of battle deaths has problems of false specificity. Battle death data are best estimates. An ordinal version suffers less from this problem because the order of values is much more likely to be accurate. Second, the measures for repression (discussed below) are ordinal and there is no available simultaneous equation method to my knowledge that is designed for one ordinal dependent variable and one that is continuous.

I use Coarsened Exact Matching (CEM) from Iacus, King, and Porro (2012) to construct ordinal versions of the battle death measure. CEM can be used to create cutpoints from continuous variables, among other uses. To make comparing models easier below, I created a 5-value ordinal version of conflict intensity that matches the number of values for the repression variable (see below). As the basis for these measures, I use the low estimate of battle deaths from the Lacina and Gleditsch (2005) dataset, which is the most conservative estimation and should bias against the argument of this article.

Due to limitations with the conflict intensity measure, I also report several models using an estimate of the number of fighters from the Political Instability Task Force (PITF) datasets on Ethnic and Revolutionary Wars (Marshall, Gurr, and Harff, 2014). The main benefit to doing so is that using conflict intensity as a proxy for rebel strength is imperfect—

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10 However, an ordinal version does suffer from the opposite problem of discarding potentially useful information.
11 The appendix reports several variations on this construction for robustness purposes, including using the best estimate when available, then low estimate if best estimate is mission. Some of the appendix models use different, non-ordinal, versions of the battle death rate (e.g., raw count of battle deaths and per capita battle deaths.
12 Some conflicts count as both ethnic and revolutionary wars leading to overlapping observations. Since there are coding differences between the two datasets, I use the more intense conflict when there is an overlap.
higher fatalities, in particular, could be caused because the government is winning, thus not accurately reflecting rebel strength. The estimated number of rebel combatants overcomes this shortcoming by attempting to measure the size of rebel forces directly. The estimated number of rebel combatants falls within a 5-point ordinal scale. The breakdown of estimates of rebel combatants for PITF-Fighters variables is as follows: 1 (less than 100 combatants); 2 (100-1000); 3 (1000-5000); 4 (5,000-15,000); 5 (more than 15,000 combatants). Country-years that do not experience conflict are included as value 1. To be included as a conflict in the PITF dataset, there are three requirements: 1) rebel political groups must mobilize at least 1,000 people, there must be at least 1,000 fatalities over the course of the conflict, and 3) there must be at least one year that results in 100 fatalities (Marshall, Gurr, and Harff, 2014). For comparison purposes, this is a higher threshold for inclusion than the battle-death rate (Lacina and Gleditsch, 2005) used above, which requires a minimum of 25 battle-deaths. Still, it is a lower threshold than the Correlates of War (COW) project, which requires 1,000 battle-deaths each year.

**Dependent Variable #2: Measures of Repression**

For the primary measure of repression, I use the Political Terror Scale (PTS). PTS reports an ordinal 5-point scale of the severity of actual state violence rather than the general level of political repression or violence from non-state actors (Gibney, Cornett, and Wood, n.d.; Wood and Gibney, 2010, 370). The PTS reports estimates based on Amnesty International reports and, separately, from the US State Department. I use the data from the Amnesty International reports for the main models below because state sources may introduce bias regarding allies and

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*This variable is correlated with the 5-value conflict intensity variable at 0.8.
Table A4 in the appendix reports an alternative measure, the physical integrity dimension of the Cingranelli and Richards’ (CIRI) project and the results are similar to those in the main article.*
adversaries, but I include the US State Department version in alternative models and the results are similar. Despite the common use of the PTS, I report the coding rules from PTS since it will be important for interpretation in the results and discussion section below. Wood and Gibney (2010, 373) report the levels as:

Level 1: Countries…under a secure rule of law, people are not imprisoned for their views, and torture is rare or exceptional…Political murders are extremely rare…
Level 2: There is a limited amount of imprisonment for nonviolent political activity. However, a few persons are affected; torture and beating are exceptional…Political murder is rare…
Level 3: There is extensive political imprisonment…. Execution or other political murders and brutality may be common. Unlimited detention, with or without trial, for political views is accepted…
Level 4: The practices of Level 3 are expanded to larger numbers. Murders, disappearances, and torture are part of life…In spite of its generality, on this level terror affects primarily those who interest themselves in politics or ideas.
Level 5: The terrors of Level 4 have been extended to the whole population…The leaders of these societies place no limits on the means or thoroughness with which they pursue personal or ideological goals.

Because the PTS is a relative score that places state repression on a continuum, it is clearly ordered from little repression (1) to extreme repression (5). However, it aggregates the severity of repression along three dimensions: scope, intensity, and range (Wood and Gibney, 2010, 373). Scope refers to the severity of the type of repression (from imprisonment to killing) while intensity refers to the frequency of state repression (ibid). Last, range refers to the selectivity of the targets, with higher values indicating less selectivity (ibid). The main models presented below generally take the PTS as ordered in its selectivity; higher values indicate less selectivity.

For our purposes, the categories of 4 and 5 connect most closely with collective or indiscriminate repression. Victims, at level 4, are targeted by the state due to their political ideas, rather than their involvement in an armed movement and are in large numbers. At level 5, state violence has “extended to the whole population,” clearly indicating indiscriminate violence.
Wood and Gibney (2010, 373, fn 20) note that while range (or selectivity) of state violence can affect scores at all levels, the distinction is most obvious between levels 4 and 5.

**Modeling Strategy**

Theoretically, there are strong reasons to expect a cyclical relationship, where increases in state repression lead to increases in rebel strength, thereby increasing the likelihood of state repression. That is, the arguments above are not mutually exclusive. Since we should expect rebel strength and collective repression to be interrelated, I employ a simultaneous equation model to control for the endogeneity of these two dependent variables. Simultaneous equation models jointly run two regressions, one for each dependent variable. The model reports how related the two equations are and whether that relationship is statistically significant. I use a seemingly unrelated bivariate ordered probit estimation technique (Sajaia, 2008), which allows the researcher to examine jointly determined ordinal dependent variables with multiple independent variables. The bivariate ordered probit estimation technique has been used in many studies, most notably for joint decisions in economics (Brecard, et al, 2009; Kaminski and Thomas, 2011) or sociology (Hyll and Schneider, 2013; Vignoli, Rinesi, and Mussino, 2013), but also recently in international relations that models the joint process of escalation in militarized interstate disputes (Huth, Croco, and Appel, 2012). It is the appropriate technique for this study because it fits the discrete ordered structure of the two dependent variables, especially the PTS.

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15 The “bivariate” in bivariate ordered probit refers to the number of dependent variables.
16 Several people have developed and implemented estimation techniques for addressing the endogeneity inherent in many social issues. The most commonly used ones are for continuous dependent variables, such as 2-stage least squares, or where one dependent variable is continuous and the other is dichotomous (Keshk, 2003). Sajaia (2008) has developed the technique for ordered and discrete dependent variables, like the ones used in this article.
The Identification Problem

Simultaneous equation modeling requires solving an identification problem: that at least one variable in each equation is thought to affect the dependent variable in that equation, but not the other (Paxton, Hipp, and Marquart-Pyatt, 2011). For the conflict intensity equation, the identification problem is solved with the percentage of mountainous terrain. Mountainous terrain has been linked to the onset or duration of civil war in several studies, though it is not consistently statistically significant (Buhaug and Lujala, 2005; Fearon and Laitin, 2003). In its most famous interpretation, mountainous terrain should facilitate civil war through an escalation mechanism by allowing would-be insurgents the space to form and grow (Fearon and Laitin, 2003). However, no study that I have found has considered its influence on repression and, theoretically, there is no reason to think that it would. I use the measure provided by Koubi and Boehmelt (2014).

The human rights equation is identified with acceptance with of international human rights treaties. Acceptance or ratification of international human rights treaties is thought to indicate a respect for human rights and should be negatively associated with the human rights violations of state repression. Acceptance of international treaties should not affect conflict intensity directly. I construct a time-varying measure by taking the number of international human rights treaties that are ratified—International Covenant of Civil and Political Rights (ICCPR), ICCPR optional protocol, Convention against Torture, and the optional ratification of either articles 21 or 22 of the Convention against Torture—divided by the number of treaties that are available for ratification in that year. These data come from Neumayer (2005) and vary between 0 and 1. The results for these simultaneous equation models are reported in Table 2.
Assessing the Impact

Finding that the two equations are related is probably not surprising to peace and conflict scholars. What is more important than showing they are related is identifying which direction is strongest. To do so, I report single equation models with multiple (i.e., one year and five year) lagged versions of the main independent variables. Since coefficients across different estimation techniques and dependent variables are not comparable, I estimate and report standardized coefficients of the effect. The standardized coefficient is calculated by subtracting the $x \hat{\beta}$ of the marginal effect when holding the independent variable (lagged) at its mean from the $x \hat{\beta}$ of the marginal effect when holding the independent variable at two standardized deviations above the mean and dividing that result by the standard deviation of the dependent variable.\(^{17}\) In equation form, the standardized coefficient equals:

$$\frac{(x \hat{\beta}_1 - x \hat{\beta}_2)}{\sigma_y}$$

where:
- $x \hat{\beta}_1 = x \beta$ of the marginal effect given $x = 2\sigma_x + \bar{x}$,
- $x \hat{\beta}_2 = x \beta$ of the marginal effect given $x = \bar{x}$,
- $\sigma_x = $ the standard deviation of the independent variable,
- $\bar{x} = $ the mean of the independent variable,
- $\sigma_y = $ the standard deviation of the dependent variable.

In other words, the standardized coefficient creates a unitless Z-score of the marginal effect of moving the independent variable from the mean to two standard deviations above the mean. I use $x \hat{\beta}$s instead of predicted values of Y (y-hat) because ordered probit produces separate values of y-hat for each outcome. The $x \hat{\beta}$s most closely approximate the linear prediction of the non-linear

\(^{17}\) I use two standard deviations above the mean instead of one to reach a level of human rights violations that would most obviously count as collective violence.
model. We then can compare the standardized coefficient for $x_{i\text{lag1}}$ (PTS) against $x_{j\text{lag1}}$ (Intensity) and so on for each lag level.

Control Variables

For the simultaneous equations models (i.e., Models 1 and 2), I nest my model in recent studies. I nest the conflict equation in Cederman, Hug, and Lutz (2010) and the repression model in Englehart (2009). Both equations use five of the same variables: tax capacity, Polity, Polity-squared (Marshall and Jaggers, 2006), the natural log of the country’s population (Gleditsch, 2002), and the natural log of the country’s Gross Domestic Product (GDP) per capita (Gleditsch, 2002). I use a version of Polity that drops the PARREG components of Polity because Vreeland (2008) found it contains measures of political violence and is thus not independent of one of my dependent variables. The conflict equation adds the natural log of the percentage of mountainous terrain (see “The Identification Problem” above). The repression equation includes the acceptance of international human rights treaties as a proportion of how many are available (Neumayer, 2005).

For tax capacity, I use Englehart’s (2009) measure of tax extraction as a proportion of GDP, which is provided by the “World Bank’s World Development Indicators, supplemented by with data from the African Development Indicators, IMF Country Reports, and the Asian Development Bank” (p. 169). While the Englehart (2009) study is interested in human rights protection, tax capacity is also found to be negatively associated with civil wars (Hendrix, 2010).

I follow the advice of Carter and Signorino (2010) and use cubic polynomial approximation to account for time dependence. This adds the $t$, $t^2$, and $t^3$ to each model.
The ordinal probit models (3 through 6) each use the same minimal control variables that are present in both simultaneous equation models. It is important that these models have the same control variables and number of observations in order for the $x\beta$s of the marginal effects to be comparable. Table 1 reports the descriptive statistics.

[Table 1 about here]

Results

The results from the simultaneous equation models (Models 1 and 2 reported in Table 2) demonstrate that both equations are interrelated. This is true regardless of whether Intensity or the PITF estimates of rebel fighters are used to measure conflict escalation. Model 1, which uses Intensity as the conflict escalation variable reports a positive rho correlation, which is a correlation of the residuals, of 0.65. The Wald test of independent equations confirms the equations are dependent by reporting a $Chi^2$ value of 64.42 that is significant at 99% level. Model 2, which uses the PITF estimate of rebel fighters as the conflict escalation variable, presents very similar results. The rho correlation is 0.66, while the Wald $Chi^2$ value is 80.71 and is significant at the 99% level.

The control variables act largely, but not completely, as the literature has found. Tax capacity is not associated with either conflict escalation equation, but is negatively associated with human rights violations at the 90% confidence level (Englehart, 2009). That tax capacity is not significant in the conflict equation is at odds with research on conflict onset (see Hendrix, 2010). This could be due to several differences in the measurement of conflict onset and conflict
The coefficients for Polity suggest a curvilinear relationship for both the conflict and repression equations. Population is positive and significant for all equations. GDP per capita is negative and significant in the intensity equations but not significant for the PTS repression equation. Neither the logged percentage of mountainous terrain nor the proportion of ratified human rights treaties are significant in any model, suggesting that the previous findings may be sensitive to modeling approaches.

[Table 2 about here]

Table 3 reports four single-equation ordered probit models with lagged independent variables that attempt to assess the strength of each direction of the relationship. As expected by the literature and the simultaneous equations, PTS is positively associated with conflict escalation and vice versa in all four models.

For this table, we are most interested in comparing the standardized coefficient of the main independent variables across comparable models (3 versus 4 and 5 versus 6). Comparing Models 3 against 4, we find that the standardized coefficient of PTS (t-1) on intensity is 1.58, which is nearly 48% higher than the standardized coefficient of conflict intensity (t-1) on PTS (at 1.07). This demonstrates that the one-year lagged effect of state repression on conflict intensity is stronger than the reverse.

Models 4 and 5, which uses the PITF estimate of rebel fighters, report the similar result that repression has a greater impact on conflict escalation than vice versa. The standardized

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18 Onset studies use conflict as a binary and ignore variations in the intensity of that conflict, either at the time of conflict onset or over the course of the conflict.
coefficient for PTS (t-1) is 1.4, which is 25% greater than the coefficient of PITF-Fighters (t-1) on PTS.\textsuperscript{19}

Figure 1 graphically shows the standardized coefficients of the linear predictions with 95% confidence intervals for Models 3 through 6. As expected from Table 3, it demonstrates that the effect of repression on conflict escalation is stronger than the reverse, even though both are statistically significant. The coefficients for the PITF-Fighters models show similar results, though the size of the effect is smaller than in the conflict intensity models. The graph further shows that not only is each standardized coefficient statistically different than zero, but that the difference between the standardized coefficients is statistically significant at the 95% level. Two-sample t-test results confirm that that the difference between both the coefficients for the one-year lag comparisons and the five-year lag comparisons are statistically significant at the 99% level.

Models 3 through 6 are specified with the same control variables to ensure the standardized coefficient is comparable, because any difference in the number of variables and the number of observations can affect the $x\beta$ of the marginal effect. I hold them constant to make sure the only difference in $x\beta$s is being produced by the difference of the independent variables. The control variables for Models 3 through 6 act as expected and very similar to the results from Models 1 and 2.

\textsuperscript{19} Table A5 in the appendix reports models with 5-year lags and shows that the relationship is long lasting, though the effect grows weaker with each lagged year.
Discussion and Conclusion

This article set out to intervene in the well-known puzzle of state repression in civil conflict. Does state repression of civilians effectively weaken rebels or strengthen them through increased participation? The statistical results reported above support the theoretical argument that large-scale state repression is counterproductive in fighting an armed insurgency. In fact, repression of civilians actually increases the threat to the state by strengthening the rebels to the point where they can escalate to more conventional warfare.

Perhaps the most important contribution of this finding is that it is a “general” finding. Previous studies on the issue have been confined—for perfectly valid reasons—to qualitative case studies or subnational quantitative designs that provide internal validity and a clearer connection to the theoretical mechanism. However, they lack the generalizability of an aggregate, cross-national study. While I applaud the recent trend in civil war literature away from cross-national studies, the research program on repression and civil war is precisely in the position of requiring an aggregate, cross-national study.

Attempts at an aggregate study have been troubled by the endogenous nature of civil war and repression. Using standard single-equation models has been insufficient to isolate the effects of repression on civil war since the state repression literature has long held civil war to be a cause of repression (Poe and Tate, 1994; McCormick and Mitchell, 1997; Regan and Henderson, 2002; Davenport, 2007a; Davenport, 2007b). The findings presented here, do not negate the state repression findings, but rather present a qualification to them.

Still, there are several weaknesses to the present study that must be discussed. The primary weaknesses of the study come from the other side of the generalizability tradeoff. While an aggregate, cross-national study contributes to external validity, it suffers from three particular
limitations. First, the aggregation to the national level combines data across large spaces and, for some countries, multiple conflicts. In small countries fighting one armed rebellion, connecting overall state repression to the dynamics of a conflict is quite feasible. In large countries with multiple conflicts, this is much more difficult. In India, for instance, Jammu and Kashmir often experiences the worst repression, but the insurgency there is not always responsible for the largest number of battle deaths.\textsuperscript{20}

Second, this study necessarily aggregates at the annual level due to data constraints. With so much going on in a country over an entire year, it difficult to isolate the effects of repression to conflict intensity precisely. This problem is difficult to overcome, as most of the literature has found, but nonetheless must be addressed. In the case of repression and conflict intensity, this may present less of a problem than many other studies. Theoretically, it should take time to witness greater intensity after indiscriminate repression. It takes time to mobilize new recruits and it may not immediately result in a change in tactics.

Third, like many aggregate studies, there is a limitation of conceptual fit with measurement. The theory presented above is about the selectivity of repressive targeting and the strength of the rebel organization. The PTS and CIRI physical integrity scores measure the general level of state repression, which includes imprisonment and torture in addition to extrajudicial killing. The conceptual fit is better with PTS, but even there, selectivity of targeting is only one dimension that influences the score (scope and range are the other two) (Wood and Gibney, 2010). Likewise, conflict intensity proxies rebel strength imperfectly. Theoretically, it takes a larger rebellion to sustain larger casualties, but high levels of conflict intensity may be capturing a winning state and retreating rebels, thus misrepresenting these retreating rebels as

\textsuperscript{20} See Beer and Mitchell (2006) for subnational analysis of India’s human rights record.
The alternative model that uses a direct measure for rebel membership from the PITF overcomes much of this problem and provides remarkably similar results as the conflict intensity models.

Despite the limitations inherent in aggregate cross-national designs, the findings presented in this article should increase our confidence that large-scale state repression is counterproductive at fighting an insurgency and should be avoided when possible. The direction of the relationship is clearly and consistently positive that state repression increases the severity of conflict. This positive relationship is also consistently statistically significant. If the opposite argument—that indiscriminate repression is effective—were correct, this relationship would be negative or at least not significant. The counterargument that is presented above suggests that repression is a gamble that may be effective only under certain circumstances. If correct, these circumstances are not common enough to reduce the significance of the findings presented in this article.

This article has also addressed a secondary puzzle concerning the direction of the conflict-repression relationship. While the findings that repression follows conflict have been more consistent than those arguing that repression increases conflict, this study demonstrates that the repression-causes-conflict direction is stronger. This does not provide a major revision of existing studies, since repression clearly does follow from insurgent threat. Still, future studies of state repression should acknowledge the endogeneity and model it accordingly.


Table 1: Descriptive Statistics

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<tr>
<th>Variable</th>
<th>Mean</th>
<th>Std. Dev.</th>
<th>Minimum</th>
<th>Maximum</th>
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<td>1.01</td>
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<td>5</td>
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<td>0</td>
<td>0</td>
<td>0</td>
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<tr>
<td>Abs deaths: #2 (n=170)</td>
<td>26.54</td>
<td>4.8</td>
<td>14</td>
<td>47</td>
</tr>
<tr>
<td>Abs deaths: #3 (n=105)</td>
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<td>93.62</td>
<td>48</td>
<td>332</td>
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<tr>
<td>Abs deaths: #4 (n=190)</td>
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<td>436.03</td>
<td>336</td>
<td>2300</td>
</tr>
<tr>
<td>Abs deaths: #5 (n=32)</td>
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<td>2625.63</td>
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<td>10000</td>
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<tr>
<td>Intensity (PITF-Fighters)**</td>
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<td>1.24</td>
<td>1</td>
<td>5</td>
</tr>
<tr>
<td>PTS*</td>
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<td>1.08</td>
<td>1</td>
<td>5</td>
</tr>
<tr>
<td>CIRI PI†</td>
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<td>0</td>
<td>8</td>
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<td>14.07</td>
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<tr>
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<td>5.81</td>
<td>10.34</td>
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<td>Disaffected Groups^</td>
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<td>3.39</td>
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<td>13</td>
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<td>1</td>
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<td>t^3</td>
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</table>

Note: Descriptive statistics are from observations present in Model 3, which includes 2243 observations (country-years) over 133 countries. * From observations present in Model 1 (n=2,179). † From observations present in Model 2 (n=2,235). ^ From observations present in Model 9 (n=500). ** From observations present in Model 15 (n=2,235).
### Table 2: Simultaneous Equations – Seemingly Unrelated Bivariate Ordered Probit

<table>
<thead>
<tr>
<th></th>
<th>Model 1</th>
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<th></th>
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<td>PTS</td>
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<td>(1.06)</td>
<td>(0.91)</td>
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<td>0.31***</td>
<td>0.2**</td>
<td>0.31***</td>
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<td>(0.1)</td>
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<td>(0.003)</td>
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<td>-0.006**</td>
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<td>(0.003)</td>
<td>(0.004)</td>
<td>(0.003)</td>
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Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. Rho reports the correlation of the residuals. †Chi²(1) reports the Wald tests of independent equations for bivariate ordered probit models. Cut points omitted.
Table 3: Ordered Probit

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<th>Model 3 Intensity (5 value)</th>
<th>Model 4 PTS</th>
<th>Model 5 PITF-Fighters</th>
<th>Model 6 PTS</th>
<th>Standardized coefficient</th>
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<td>PTS(t-1)</td>
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<td>1.07***</td>
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Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. Standardized coefficient = \( \frac{(x\beta-\hat{\beta}|_{x=2SDx+x-bar}) - (x\beta-\hat{\beta}|_{x=x-bar})}{SDy} \).
Figure 1. Marginal Effects as Standardized Coefficients with Confidence Intervals