

Contagious Rebellion and Preemptive Repression

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Abstract

Civil conflict appears to be contagious—scholars have shown that civil wars in a state's neighborhood make citizens more likely to rebel at home. However, war occurs when both rebels and the state engage in conflict. How do state authorities respond to the potential for civil conflict to spread? We argue that elites will anticipate the incentive-altering effects of civil wars abroad and increase repression at home to preempt potential rebellion. Using a Bayesian hierarchical model and spatially weighted conflict measures, we find robust evidence that a state will engage in higher levels of human rights violations as civil war becomes more prevalent in its geographic proximity. We thus find evidence that states violate rights as a function of the internal politics of other states. Further, we argue authorities will act not to mimic their neighbors but rather to avoid their fate.

Keywords

civil war, diffusion, repression, dissent, rebellion, human rights violations, threat

Violent rebellions erupted across North Africa and the Middle East during the Arab Spring in 2011, with rebellions in Tunisia followed soon after by challenges in Egypt, Syria, and Libya, suggesting the contagious nature of civil conflict. Empirical patterns in a variety of contexts demonstrate that actors follow one another when

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making political choices (see, e.g., Gleditsch 2002a; Simmons and Elkins 2004; Vol-den 2006), and social scientists consistently find that civil conflict in a neighboring state leads to an increased likelihood of domestic conflict at home. Theoretical explanations posit that externalities such as refugee flows, ethnic ties, and resource movements alter citizens' grievances and/or expectations and make them more likely to dissent or rebel against the state (see, e.g., Kuran 1998; Moore and Davis 1998; Salehyan and Gleditsch 2006; Salehyan 2007), even leading to civil war onset (Hegre 2006; Gleditsch 2007; Buhaug and Gleditsch 2008). However, these studies leave us puzzled as to the role of the state when civil war erupts nearby. Conflict, after all, is an interaction between at least two players. To determine the conditions under which civil war in one state impacts political violence in another, we must look beyond how citizens respond to proximal conflict and assess how state authorities respond to the potential for domestic conflict to diffuse.

We argue that forward-looking elites will anticipate the diffusion of neighboring domestic conflict and crack down on potential rebels before violence begins at home. Proximal civil war is observable, and state authorities can anticipate its incentive-altering and/or informational influences on their own citizens. Authorities have a brief window of opportunity during which they can prevent rebellion, and the threat conflict represents to their hold on power provides the incentive to do so. Knowing a group is likely to mobilize in response to proximal civil war, a state can preempt the conflict—repressing the group to counterbalance its inflated expectations of success. We predict *a state will repress more severely when neighboring states are involved in civil conflict than when the neighborhood is peaceful*. We also derive hypotheses as to how cultural and regime-type similarity might condition the spatial impact of civil war to better understand the process by which state authorities come to anticipate the spread of domestic conflict and increase repression at home.

We use spatial weighting techniques and a multilevel model to estimate the effect of proximal civil conflict on the degree to which a state will repress its population. We construct spatial weight matrices to account for geographic and similarity connections facilitating the diffusion of political violence, considering the theoretical implications of a variety of weighting schemes. Using Bayesian estimation of a hierarchical model to account for the presence of both intranational and transnational independent variables, we find that as civil war becomes more prevalent within a state's geographic proximity, authorities engage in more severe repression. In fact, a neighborhood rife with civil conflict has nearly one-third of the effect that a civil war on a state's own territory has on its level of repression. That is to say that civil conflict abroad has similar effects on a state's rights practices as a civil war on its own soil. However, we find little support for our hypotheses regarding similarity. This analysis lends strong support to our theory that leaders repress citizens to preempt the spread of civil conflict, as well as clarifying that leaders do not seem to take cues in this instance from states that share similar characteristics.

This article contributes to the study of spatial influences on human rights, civil conflict, and policy diffusion generally. To our knowledge, we offer the first account

of how rights violations reflect domestic conflict occurring outside of the state. Though human rights practices are regarded to be largely a domestic concern, we find that the causes of violations can traverse borders. One state can affect the rights practices of another state even with no intention of doing so. We find that civil conflict not only affects the treatment of citizens in the state engaged in conflict but also the treatment of citizens in nearby, even peaceful, states. This suggests civil wars have externalities beyond the more commonly identified effects of lasting infrastructure damage, migration, and poor public health. We add to the growing literature on civil war diffusion, complementing the current focus on rebellion with our findings on state reactions to civil war in the neighborhood. Finally, most accounts of policy diffusion contend that states adopt policies that emulate their neighbors. In contrast, we argue that authorities will increase repression not to mimic their neighbors, but to prevent rebellion from catching on at home.

Conflict Diffusion and Preemptive Repression

Policies and political outcomes commonly move in space, in that one state adopts a policy because another state's choice has indirectly affected its own. In particular, scholars have suggested that civil war in one state makes civil war more likely in other nearby states. In this section, we present insights from scholarship concerning the diffusion of civil conflict and repressive responses to civil unrest that inform a theory of how state authorities adopt policies in highly conflicted neighborhoods.

According to Strang (1991, 325), diffusion is a process by which "the prior adoption of a trait or practice in a population alters the probability of adoption for remaining non-adopters." Elkins and Simmons (2005, 39) identify two generalized mechanisms by which policies diffuse from one state to another: "those for which another's adoption alters the value of the practice [*adaptation to altered conditions*] and those for which another's adoption imparts information [*learning*]." ¹ For either of these types of diffusion to occur, the actors must be related in some way that allows one's action to transfer indirect effects to the other, such as familiarity, proximity, communication, or institutional connections (e.g., Mooney 2001; Simmons and Elkins 2004; Volden 2006). Finally, Elkins and Simmons (2005, 38) stress that "the interdependence in diffusion is uncoordinated. Thus, the actions and choices of one country affect another, but not through any collaboration, imposition, or otherwise programmed effort on the part of any of the actors." In other words, a state may look to a proximal or similar state as a guide, but its decision does not result from coercion or manipulation.

Both types of diffusion mechanisms arise in the context of intrastate war. Civil wars generate externalities such as refugees, arms markets, economic outcomes, disease, and so on which often flow across borders. These externalities may (1) alter the incentives for groups considering rebellion in neighboring states and/or (2) send information that causes similar actors to follow suit. We discuss studies of the two types of civil war diffusion in turn.

Proximal civil wars yield externalities that alter grievances and provide resources that incentivize rebellion in an otherwise peaceful state. Refugee movements can shift the ethnic distribution of the host state, intensifying popular resentment, and strain finite resources, sparking subversion from groups who suffer from sudden redistribution (Salehyan and Gleditsch 2006). Generating new or heightening extant grievances about group status can lead a dissatisfied group to rebel (Gurr 1993). Refugee camps are commonly havens for rebels from the state of the initial civil conflict, who can bring expertise and arms and threaten the host state (Salehyan and Gleditsch 2006). “Transnational rebels” who straddle borders or otherwise exist in multiple states bring their expertise from one state to another, and they may lead a rebellion in the state serving as their base of operations (Salehyan 2007). A civil war in a nearby state can thus provide resources that improve a group’s probability of successful rebellion, making them more prone to rebel.

Civil wars can also affect outside actors’ behavior by informing them of potential choices and their attendant consequences. A group rebelling against the government in one state may have members in another. Their kinship ties and lines of communication allow otherwise peaceful groups to learn about the potential for rebellion, the success or failure of particular tactics, and so on (Gurr 1993; Moore and Davis 1998; Saideman 2001; Buhaug and Gleditsch 2008). A potential rebel group may mimic the actions of similar groups abroad, suggesting demonstration effects (Kuran 1998; Woodwell 2004). When diaspora members bring new choices to light, the kin group is more likely to rebel against the state than a group with no knowledge of the potential for successful rebellion. Likewise, citizens are more likely to rebel when they believe others are likely to do so. Though many individuals will free ride on others’ dissent (Lichbach 1995), some will join the movement when they observe others taking to the streets (Kuran 1991). In other words, potential rebels take cues as to tactics or success from other, similar actors abroad, and make their own decisions accordingly.

The scholarly focus on civil war diffusion is limited primarily to how dissatisfied citizens learn from and/or adapt to changes in the neighborhood—predicting rebellion—but this obscures the role of state authorities in neighboring, peaceful countries. Since conflict is costly, states have an incentive to avoid it by generating countermeasures to prevent contagious challenges (Braithwaite 2010). Domestic conflict is observed when the state has not been able to avoid it in one of these ways. How do state authorities respond to the fact that civil war in the neighborhood makes rebellion more likely at home?

One of the most consistent findings in the study of political violence is that state authorities repress to counter or avert threats arising from challenges to the status quo (Davenport 2007a, 7). Repression and dissent are intrinsically interrelated, almost by definition.² The consistently interconnected relationship indicates groups and authorities are likely to be aware of their connection and act strategically, knowingly anticipating the effects of their decisions on one another’s behavior. If authorities expect a group to take an action that could undermine their power, they may try

to preempt that action, repressing to undercut the group's will or capacity to challenge them (Davenport 2007b, 47).

Authorities are particularly likely to repress potential challenges when their position is vulnerable or their power is explicitly threatened. Even a legitimate challenge can instigate repression; state authorities increase repression prior to elections when the opposition represents a potential threat to the incumbent's hold on power (Hafner-Burton, Hyde, and Jablonski 2010; Levitsky and Way 2010). In 2005, Azerbaijani authorities used intimidation tactics against voters, arrested political opponents, and restricted voting capacity for fear that the opposition would gain steam after recent peaceful revolutions in the Ukraine and Georgia (Osborn 2005). The government, fearing the loss of power that had recently occurred nearby, repressed to keep opponents and voters from following the example of their neighbors.

A state is likely to view a nonelectoral demand for regime change as significantly more threatening than a demand for policy change (Poe et al. 2000). Scholars have consistently found that institutions that would otherwise constrain rights violations fail to do so when the state faces violent dissent that can threaten the regime (Keith and Poe 2004; Davenport, Moore, and Armstrong 2007; Conrad and Moore 2010). Civil war is specifically dangerous to political survival, as it represents a purposeful attempt to remove ruling authorities from power. As the most threatening of challenging actions, leaders have an incentive to preempt dissent and rebellion that could lead to civil war before they ever occur. The spreading rebellions in the Middle East frightened the Azerbaijani government once again in 2011; Amnesty International (2011) reports that "Inspired by their counterparts in Egypt and Tunisia, youth and opposition activists in Azerbaijan . . . [called] for organized cycles of protests. The authorities of Azerbaijan have suppressed these nascent signs of popular protest at their roots with a new wave of repression and intimidation." This example illustrates the desire of authorities to nip potential revolutions in the bud, using repression early when civil war might spread from other states. They have an incentive to act early because dissatisfied populations distort the perception of the true likelihood of rebellion, making rebellion difficult to predict (Kuran 1995). Rather than waiting for externalities to reach them and cause unexpected rebellion, leaders will repress to undercut the rebellion before it can begin.

We argue that authorities will anticipate that civil conflict in the neighborhood increases the likelihood that citizens will rebel within their own borders and so will repress the population to preempt likely challenges. Forward-looking state authorities can anticipate that civil wars abroad will alter the incentives of their own citizens to rebel. To prevent a rebellion, authorities will crack down on potential subversives before refugees, arms transfers, learning effects, and so on arrive in their state, undermining citizens' will and/or capacity to challenge the state. Preemptive repression makes it more difficult for the group to act and alters its expected utility calculations, so a state may be able to stave off the diffusion of violence from proximal states. We predict that greater civil war incidence in the neighborhood will

lead to an increase in repression, whether or not that state eventually experiences civil conflict.

Hypothesis 1: A state will repress at higher levels as civil conflict becomes more proximal and/or prevalent in its neighborhood.

We also posit more specific hypotheses in the attempt to understand how actions in nearby states affect authorities' decision to repress. Do states respond to geography alone, or do shared characteristics and interconnections lead a state to expect civil conflict abroad to affect the probability they will face rebellion at home?

Authorities may anticipate that neighboring civil conflict will spread to their peaceful state when groups within their borders are similar to groups in the states experiencing civil war. Groups may learn or receive support from others who are similar to them. Lines of communication are common between like groups, either because families maintain connections (Bryceson and Vuorela 2002) or religious or cultural institutions facilitate communication (Simmons and Elkins 2004). People are more likely to believe the lessons from a like group will be applicable to them, finding the experiences of similar groups abroad more salient than those of different cultural groups. Groups are also more likely to transfer resources to groups similar to themselves rather than groups who may not have the same goals (Leblang 2010). Importantly, groups share these connections regardless of distance. If a state represses where rebellion is most probable, it will be most concerned about linkages between like groups.³

Hypothesis 2: (Cultural similarity and global learning) A state will repress at higher levels when civil conflict abroad involves groups who have similar cultures to groups in its own state.

If leaders look to referents as to how a rebellion would affect their ability to maintain power, they should observe the consequences of conflict in states with similar political structures. Actors can determine whether authorities under similar regime types fail to predict whether rebellion is likely to occur (and succeed) at home. If a rebellion in one state succeeds in bringing down the leader (as occurred in Tunisia in 2011), groups and authorities in a similarly structured state may assume that the consequences of rebellion would be similar in their state (as they seem to have correctly thought in Egypt soon after). When the French Revolution occurred in the late eighteenth century, it was similarly structured monarchies who became wary of similar rebellions and repressed their own populations (Goldstein 1983). Looking to similar types of political regimes can help a state determine its own vulnerability. We predict that when states of similar regime type are involved in civil wars, a state will increase repression in anticipation of its diffusion. Again, this prediction is global, as learning from like states does not necessarily follow geographic constraints.⁴

Hypothesis 3: (Regime similarity and global learning) A state will repress at higher levels as more states of the same regime type experience civil conflict.

In contrast to Hypotheses 2 and 3, distance may play a factor even for learning-based mechanisms of diffusion. At issue here is the salience an act of rebellion abroad has for potential rebel groups at home. States look to similar states when making decisions, but they are even more likely to be interdependent within the same region (Gleditsch 2002a). For example, an African democracy should be more concerned about a civil war in an African democracy than a civil war in a Latin American democracy. Hypotheses 4 and 5 propose that learning is in play, but that its effects are conditioned by distance. If this is the case, we should expect that leaders will preemptively repress potential dissenters in response to proximate civil war in similar states more so than in response to proximate civil war in dissimilar states.

Hypothesis 4: (Cultural similarity and proximate learning) A state will repress at higher levels in response to civil war in its neighborhood that involves culturally similar states than it does to civil war in its neighborhood that involves states with different cultures.

Hypothesis 5: (Regime similarity and proximate learning) A state will repress at higher levels in response to civil war in its neighborhood that involves similar regime types than it does to civil war in its neighborhood that involves states with different regime types.

Empirical Analysis

We assess the accuracy of our hypotheses with empirical evidence. We first describe the data sources and coding, including the construction of our key independent variables and a descriptive analysis to demonstrate the plausibility of our claims. We follow this with the results of multivariate analyses of data with global coverage from 1981 to 2004.

Operationalization

Predicting repression. We operationalize state repression using the Cingranelli and Richards (CIRI; 2010) human rights index. CIRI indicators represent the prevalence of rights abuse in four categories: torture, political imprisonment, extrajudicial killings, and disappearances. Amnesty International and US State Department reports describe the preponderance of rights violations, and coders use these reports to approximate the overall level of abuse in each category. Each indicator is coded as zero (more than fifty reported abuses), one (between one and fifty reported abuses), or two (no reported abuses). These categorizations can be thought of as systematic, occasional, or no torture, political imprisonment, and so on. CIRI sums the individual scores to create a nine-point scale, which we invert such that higher

scores indicate higher levels of rights violations. This measure represents a picture of state abuse in a given year, suggesting the prevalence of abuse and the severity of government actions against civilians.⁵

Neighborhood Civil War. To approximate the influence of civil conflict in a state's geographic neighborhood, we carefully consider how space and interconnectivity influence a state's expectation of rebellion and decision to repress.⁶ Rather than using a dichotomous indicator of "region," we generate a spatially weighted measure that captures the concepts of both distance and prevalence more precisely. In this way, we can model the spatial relationships with a clearer idea as to the mechanisms through which nearby states affect one another.

Spatially weighted variables incorporate characteristics of neighboring units (in our case, civil war) and the degree of connection between those units into a single measure. The general strategy is to create an $N \times N$ matrix matching each state i with each of the N states, j , in the analysis. We populate the cells of this matrix with a measure of the degree of connection between the units—distance (or a function thereof, discussed in the following)—and multiply that matrix by a vector of the characteristic of interest (civil war occurrence utilizing data from the Uppsala Conflict Data Program/Peace Research Institute Oslo (UCDP/PRIO) Armed Conflict Data set⁷). The result is an $N \times 1$ vector of distance-weighted civil conflict occurrence, which we sum across N states in the neighborhood. We perform this operation for each year in our data to yield *Neighborhood Civil War*, the spatially weighted prevalence of proximate civil war for every state-year.

Spatial lag variables differ on two important dimensions. The first considers whether the measure captures the *total number* of neighbors experiencing conflict, or the *proportion* of neighbors experiencing conflict. This distinction is conceptually meaningful; if conflict spreads due to refugee inflows, then the absolute number of states experiencing civil conflict would cause a state to worry—two states in conflict is just as dangerous whether the neighborhood includes three or twelve states. If civil war in the neighborhood depresses economic growth, this may be because there are fewer trading partners in proportion to the total.

The second dimension captures *proximity* in spatial relationships; should the spatial weight treat all neighbors equally or weight proximate neighbors more heavily? International relations studies commonly choose the former, treating a state as a "neighbor" if it is within 950 km of the target state (cf. Gleditsch and Ward 2001). However, some linkages are weaker when states are farther apart. Again, the microtheories underlying the spread of civil war point in both directions. Groups who learn from coethnics in other states are likely to do so regardless of distance, while transnational rebel groups are likely to only affect contiguous states.

Rather than choosing among these options a priori, we create four spatial lag matrices to represent the possible combinations of these dimensions. This allows us to be agnostic as to which anticipated mechanism of civil conflict diffusion

leaders most fear when testing Hypothesis 1, the most general of our claims. In the first, a matrix that treats states as additive and equal within the neighborhood, we populate the matrix with ones whenever two countries are within 950 km of each other, using Weidman, Kuse, and Gleditsch's (2010) C-Shapes data on the distance between the two closest geographical points between states. In the second, we divide the number of conflict-ridden neighbors by one plus the total number of countries within 950 km to capture conflict-ridden states as a proportion of the total in a neighborhood of equally weighted states.⁸

Two other matrices use a function of distance between two states in each cell of the weighting matrix. We define degraded distance as $1 - \left(\frac{\text{Distance}}{950}\right)^{\frac{1}{4}}$. Substituting the minimum distance between each pair of states into this equation yields a value of one for contiguous states and nearly zero for states 950 km apart. The functional form penalizes increasing distance early, then allows its effect to diminish more slowly, heavily weighing contiguous states while down-weighting states further from the target state.⁹ We create two matrices using this distance function—one additive and one proportional as earlier. Multiplying each matrix by a vector of civil conflict in state j generates four different variables measuring nearby civil conflict.

Each of these measures captures slightly different concepts of “neighborhood conflict.” Proportional measures give less weight to conflicts in areas with many small nations than additive ones, though they more aptly capture how conflicts dominate a region than additive measures. Distance-depreciated measures allow for a subtler substantive importance than the blunt cutoff of 950 km, yet they downplay conflicts further from the target nation. We included each variant in our empirical models and found our results to be statistically robust to this choice. That said, measures of model fit, as well as visual inspection, pointed toward the distance-weighted, proportional variant as providing better fit than the alternatives. Accordingly, following results all employ this variant as our preferred indicator of Neighborhood Civil War.

Cultural Similarity and Regime Type. Authorities may expect civil war abroad to be more threatening when groups in the affected state are similar to those at home. Simmons and Elkins (2004, 180) identify states as having similar and connected groups when they share a dominant language or religious tradition. Following their lead, we created an $N \times N$ similarity matrix populated by ones whenever two states share a dominant language or religion in a given year. We employ data from Ellingsen (2000), who codes 9 distinct religions and 132 languages. We multiplied this cultural-similarity matrix with a vector indicating whether state j was embroiled in civil war, yielding a sum of the number of culturally similar states that experienced civil wars.

We also argue states may consider the consequences of civil conflict in similarly structured regimes when choosing a level of repression (Hypothesis 3). We use the

Unified Democracy Scores (UDS) to operationalize regime type. Pemstein, Meserve, and Melton (2010) employ a Bayesian latent variable approach using the information from ten different existing indicators of democracy (such as Polity IV; Freedom House; Przeworski, Alvarez, and Cheibub [2000]; etc.) to create one continuous scale that ranges from around -2 (*least democratic*) to around 2 (*most democratic*). Because it incorporates information from ten different measures based on a variety of concepts, the scale accounts for differences in electoral competition, checks and balances, rule of law, institutional structure, and anything else operationalized in the different component indicators. Pemstein, Meserve, and Melton (2010) note that the measure is “at least as reliable as the most reliable component measure” (p. 426). Using this measure allows us to avoid some of the well-known difficulties with using the Polity scales when studying civil war (cf. Vreeland 2008), though these scales are among those captured in the UDS estimates.¹⁰ The UDS also have greater geographic and temporal coverage than any one of its component scales. Using this data, we create an $N \times N$ connectivity matrix populated by ones whenever a pair of states are rated within 0.2 UDS points each other.¹¹ Multiplying this matrix by a vector of civil conflict occurrence generates a sum, for each state-year, of the number of civil wars ongoing in similar regimes. We predict that the more civil conflict there is among like regimes, the more wary leaders will be about their own fates, so they will repress accordingly.

Hypotheses 4 and 5 contend that a state is more likely to increase repression in response to civil wars in proximal states when those proximal states are similar to the home state in culture or regime type. In other words, we need to be able to compare the effect of civil war in geographically proximal and similar states to geographically proximal and dissimilar states. We first scalar multiplied our preferred spatial matrix by the matrix indicating cultural similarity between each pair of states. We then multiplied this new matrix by the vector of civil conflict occurrence, generating a measure that accounts for the prevalence of civil conflict in proximal and culturally similar states. Finally, we subtract this new indicator from the unadulterated Neighborhood Civil War variable to yield a measure of the prevalence of civil conflict in proximal and culturally dissimilar states. In short, we divide our neighborhood conflict variable into two components; the portion driven by civil war in culturally similar places, and the remaining portion driven by civil war in culturally dissimilar places. We repeated this process for proximal states with similar and different regime types.

These four measures differ importantly. Under Hypotheses 2 and 3, the target state makes its decisions based on similar states regardless of distance. Thus, these measures of cultural and regime similarity are simply sums of civil conflict occurrence in similar states globally. Under Hypotheses 4 and 5, the target state fears outcomes in proximal, similar states more than those in proximal, different states, so these indicators are spatially weighted and only capture conflict within 950 km. The latter, spatially weighted measures of similarity thus eliminate strange cases, such as Mexico repressing out of fear of civil conflict spreading from the Philippines due to their shared dominant religion.

Finally, our theory is one of preemption, such that a state will try to avert civil war by increasing repression when states with some form of connection experience civil conflict. Our predictions are thus conditioned on civil war occurrence abroad *and the absence of civil war at home*. These conditions imply the use of an interaction term, multiplying our measures of civil war in connected states j by the occurrence of civil war in state i . We interpret the coefficient on the constituent Neighborhood Civil War as the effect of connected civil conflict on repression in the absence of civil war at home (Brambor, Clark, and Golder 2006), or the effect of civil war abroad on preemptive repression. We also estimate our models interacting neighborhood civil conflict with a measure of dissent at home, using the Cross-National Time-Series (Banks 2010) measures, which account for the number of riots, strikes, and demonstrations in a state in a given year. Doing so allows us to determine whether states are responding to civil war abroad rather than dissent or low-level conflict short of civil war at home.

Control Variables. We control for state-level characteristics likely to influence both the dependent and key independent variables. We capture the target state's regime type with the previously mentioned UDS, since regime type influences respect for human rights (e.g., Bueno de Mesquita et al. 2005),¹² and also clusters in space (Gleditsch and Ward 2006; Ward and Gleditsch 2008; Elkink 2011). Duration dependence is likely in our data, since choices about the level of human rights violations likely depend on prior values, so we include one-year lags of the state's CIRI scores.¹³ We account for a state's wealth and population (Gleditsch 2002b), as these have commonly been shown to affect respect for human rights (e.g., Poe, Tate, and Keith 1999) and generally cluster in space. We model these slow-moving variates using the average for each country over the time it comes under study, as we discuss in more depth in the Model Specification section.

Descriptive Evidence

If civil conflict in a state's geographic proximity affects the human rights violations perpetrated in that state, we would expect the indicators of these two concepts to covary. As an initial probe into the plausibility of this argument, we present descriptive evidence of this relationship. Figure 1 shows our preferred (i.e., proportional, distance-degraded) measure of Neighborhood Civil War plotted alongside CIRI physical integrity scores over time for four states in different regions. The solid lines represent the prevalence of civil conflict within 950 km of each state, with higher levels indicating a higher proportion of states experiencing civil wars and/or closer civil wars in the neighborhood; the dashed lines illustrate the level of rights violations, with higher values indicating higher levels of repression. The patterns are smoothed, with the points themselves suppressed for visual clarity.

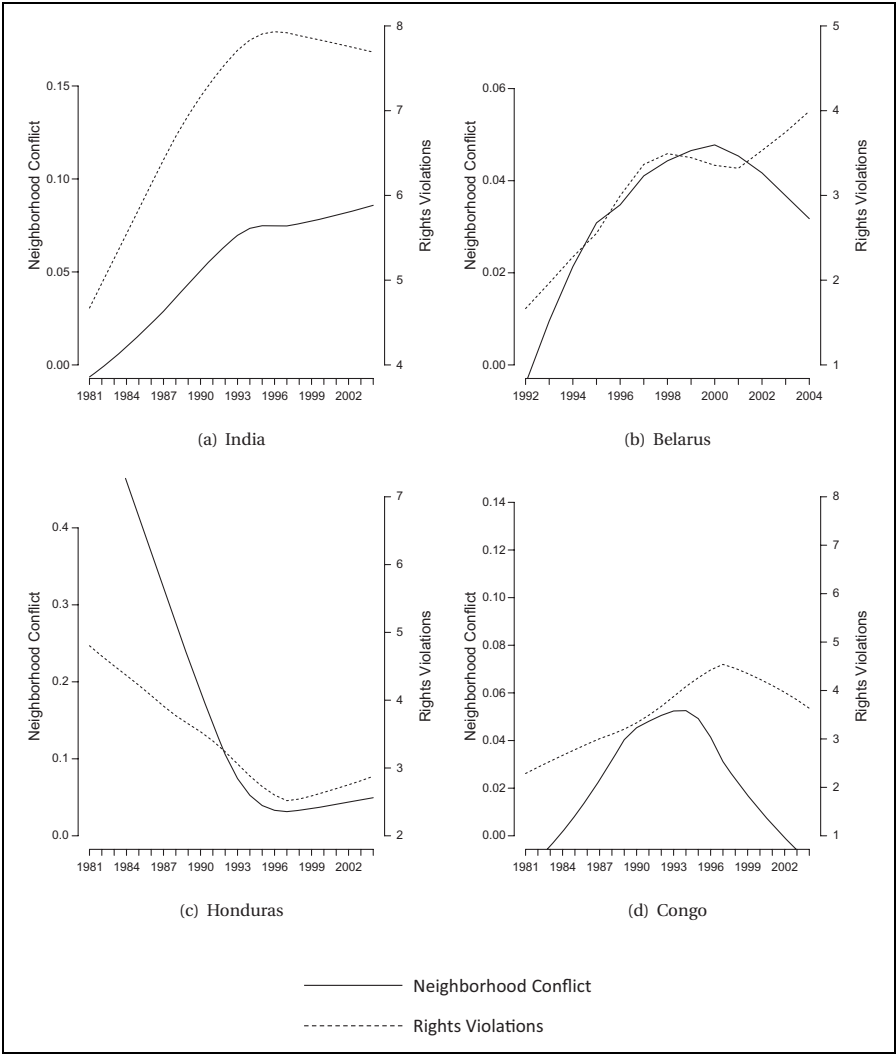


Figure 1. Neighborhood conflict and human rights violations. Smoothed measures of human rights violations and the prevalence of nearby civil conflict plotted against time. The points are suppressed for visual clarity.

All four graphs in Figure 1 demonstrate the plausibility of our most general claim—that nearby civil conflict leads to increased repression. In Belarus (Figure 1b), human rights practices closely mimic the prevalence of civil conflict in the region. As a greater proportion of states proximal to Belarus experienced civil war, its citizenry experienced higher levels of human rights violations. The

magnitude of the impact is substantial: variation over one-tenth of the range of the proximate conflict measure generates movement across about one-fourth of the range of the CIRI human rights scale. Similar correlations in India, Honduras, and Tunisia suggest that higher levels of (and, thus, more threatening) civil war in a state's neighborhood coincides with increasing levels of human rights violations across different regions and regime types. Further, the timing of changes supports our causal account of the relationship and weighs against concerns of endogeneity. For example, neighborhood conflict peaked and fell in the Congo before the level of rights violations subsided.

Of course, covariance does not indicate that proximate civil conflict causes increased rights violations. Though illustrative, Figure 1 cannot speak to the generalizability of our theory, nor can it aid us in measuring the size of the effect of neighborhood conflict on repression. To that end, we turn to a set of multivariate analyses.

Model Specification

We specify a general model to test each of our claims. We estimate the process by which a state chooses a level of repression using a Bayesian multilevel model with a linear link.¹⁴ Mathematically, for each country i and year t , we estimate the following¹⁵:

$$\begin{aligned}\text{State Repression}_{it} &= \alpha_i + \beta X_{it} + \gamma Z_{it} \\ \alpha_i &= \alpha_{00} + \phi Z_i.\end{aligned}$$

The vector β includes coefficients representing the effects of key independent variables, X_i , on the dependent variable, State Repression, and γ are coefficients for control variables Z_i . The intercepts at the lower level (α_i) are modeled for each unit (i.e., country) as a function of unit-level regressions of averages of slow-moving variates, ϕ , and unit-level intercepts, α_{00} .¹⁶

The multilevel structure parallels the time-series, cross-sectional structure of the data (state-years nested within states), and allows us to account for unit heterogeneity (Gelman and Hill 2007). We model the unit-level intercepts as a function of averages. This choice is warranted substantively because the levels of these variables have been found to inform the levels of rights violations, while changes in these variables are not robust predictors of rights violations. For instance, though population is a common right-hand side variable with a statistically significant effect on repression, changes in population have no consistent theoretical or empirical effect on rights violations (Poe, Tate, and Keith 1999). As a technical matter, modeling these slow-moving variates at the state-year level induces substantial collinearity between them and the unit intercepts. Modeling them as unit-level averages allows them to inform each country's baseline level of violations, while avoiding issues of collinearity.

The use of a Bayesian model allows us to estimate this multilevel structure simply. An additional reason to employ Bayesian estimation is its graceful handling of

Table 1. Predicting the Level of Repression in State *i*.

Variable	Refugee model	Model 1	Model 1b
Neighborhood CW		0.85 (0.25, 1.45)	0.75 (0.10, 1.38)
Refugee inflow	0.02 (0.01, 0.03)		
Neighborhood CW \times CW _{<i>i</i>}		-1.57 (-3.21, 0.13)	
Neighborhood CW \times dissent _{<i>i</i>}			0.50 (-0.67, 1.69)
Refugees \times CW _{<i>i</i>}	-0.01 (-0.04, 0.02)		
Civil war _{<i>i</i>}		1.11 (0.86, 1.37)	
Low-level dissent _{<i>i</i>}			0.26 (0.13, 0.39)
Democracy	-0.42 (-0.50, -0.33)	-0.40 (-0.49, -0.32)	-0.41 (-0.50, -0.33)
Lagged CIRI	0.47 (0.44, 0.50)	0.48 (0.45, 0.51)	0.50 (0.47, 0.54)
Log population _{unit}	0.27 (0.21, 0.33)	0.27 (0.21, 0.33)	0.26 (0.20, 0.32)
Log GDP per cap _{unit}	-0.27 (-0.36, -0.17)	-0.28 (-0.37, -0.19)	-0.29 (-0.38, -0.19)
Mean α_j	-0.52 (-1.78, 0.80)	-0.39 (-1.63, 0.86)	-0.30 (-1.57, 1.0)

Note. CIRI = Cingranelli and Richards Human Rights data set; CW = civil war; GDP = gross domestic product. Point estimates presented beside 95 percent credible intervals. $N = 3,860$. Priors are $N(0, 10)$. 2,500 iterations, the first 1,000 of which were omitted as “burn-in.”

missing data. Like most International Relations (IR) data sets, ours includes a modest number of missing cells.¹⁷ Using a Bayesian model allows for in-line “imputation” of missing values (Jackman 2009), thus allowing us to use all of the available data, rather than resorting to listwise deletion.

Results and Analysis

We contend that civil war in the neighborhood can increase the risk of civil war at home for several reasons, including refugee flows, transnational rebels, demonstration effects, and so on. Our theory posits that the fear of any or all of these vectors of contagion incentivizes leaders to repress their citizens preemptively and thereby raise the costs of rebellion or its expected probability of success, preventing its occurrence. In the following, we test this theory of preemption. However, if elites preemptively repress their citizens due to the expectation or fear of any or all of these externalities, then we should certainly expect them to repress in response to their actual occurrence. As a plausibility check for our preemption argument, we first test whether the most likely and common externality of proximal civil war—refugee inflows¹⁸—sparks repression.

We specify the hierarchical model described above, with the full suite of control variables, but we replace the Neighborhood Civil War variable with spatially weighted *Refugees*. This indicator captures the natural log of the number of incoming refugees from states within 100 km, employing refugee flow data from Salehyan (2007). The results of this estimation are reported in the first column of Table 1.

The estimates support the contention that refugee inflows lead to increased repression. The variable Refugees has a positive and statistically significant relationship with human rights abuses. As the number of refugees coming into the state increases, the state increases its levels of repression. The substantive impact of refugee inflows on human rights abuses is meaningful: a shift across the range of refugee inflows, from none to more than three million, generates an increase in CIRI scores of 0.3. Thus, refugee flows lead to a statistically significant and noteworthy increase in repression even in the absence of materialized rebellion.

The preceding test demonstrates that refugee inflows—a realized externality of civil wars abroad—lead states to increase repression. The results give credence to our argument that leaders are likely to be concerned about externalities from wars abroad. However, leaders are unlikely to wait for the refugees or lessons to arrive: they anticipate that civil war could spread by any (or all) of the possible mechanisms and will attempt to thwart it before rebellion can begin. To restate our proposition, we argue that the fear of contagion—regardless of the mechanism by which it spreads—will be sufficient to cause leaders to increase repression.

We test the central claims of our theory by analyzing the spatially weighted neighborhood civil conflict variable (and the variants accounting for similarities between states) in the multilevel model specified earlier. The results, including the estimated coefficients and their associated 95 percent credible intervals, are presented in columns 2 and 3 of Table 1. In model 1, we include state i 's involvement in a civil war and interact it with the neighborhood conflict variable, while in model 1b, we replace *Civil War* in state i with the lower level conflict variable *Dissent* and interact this with the neighborhood conflict variable. To highlight and compare the effects of neighborhood conflict on state repression, Figure 2 illustrates the estimated effects of our key independent variables on a state's level of repression for each variant of our model.

The top section of Figure 2 displays the point estimate and associated uncertainty for the effect of geographically proximate civil war on a state's level of human rights violations. Recall that the inclusion of the interaction term Neighborhood Civil War \times Civil War means we should interpret the estimated coefficient of the constituent Neighborhood Civil War variable as the effect of the prevalence of civil war in the neighborhood on state repression when there is no civil war in the target state. This allows us to account for preemptive, rather than reactive, repression. As predicted in Hypothesis 1, the effect of civil war in a state's neighborhood on its chosen level of repression is strongly positive. As civil war becomes more prevalent in the state's proximity, threatening the state with externalities that could make civil conflict more likely, the state increases its level of repression. Substantively, we find that moving from a conflict-free neighborhood (e.g., Switzerland in 2000) to one riddled with civil conflict (e.g., Swaziland in 1982) generates a change of four-tenths of a CIRI unit. Recall that each unit on the CIRI scale corresponds to a shift from absent to occasional torture, extrajudicial killings, imprisonment, or disappearances—or from occasional to widespread and systematic rights violations. Thus,

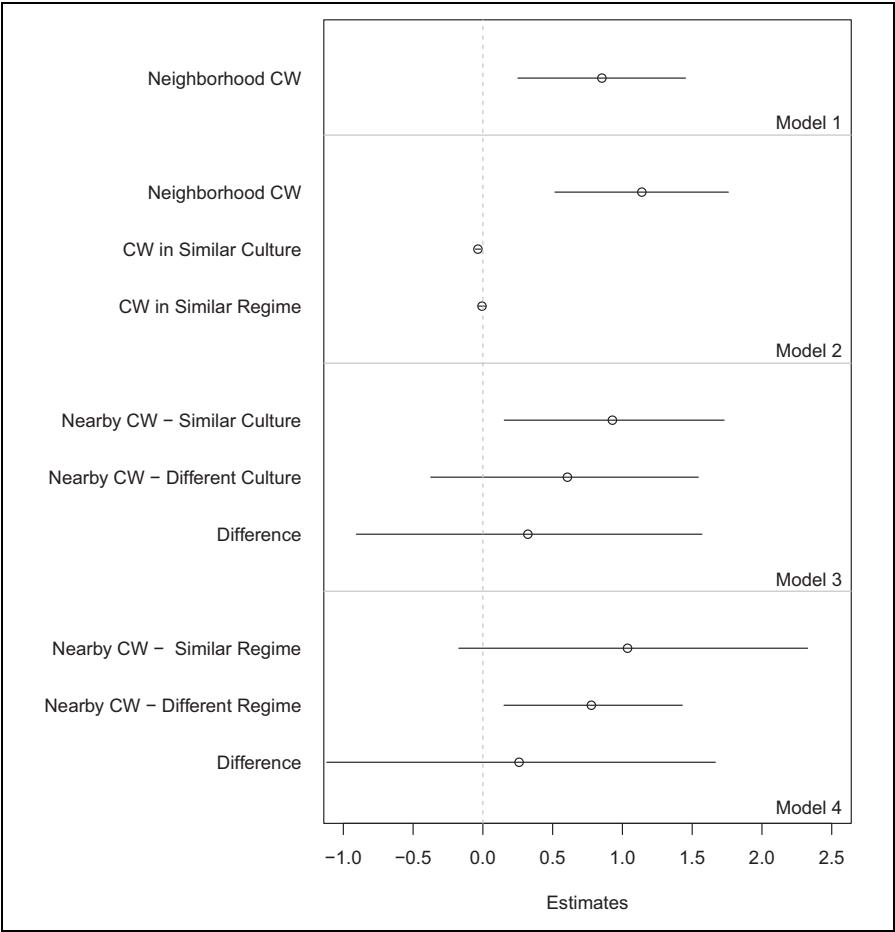


Figure 2. Point estimates of the effects of independent variables (listed on the y = axis) on the level of state repression and 95 percent credible intervals across model specifications. The vertical dotted line represents zero/no effect.

substantively, this change is very meaningful, particularly when considering the significant underreporting in measures of human rights violations.

Another way to understand the magnitude of the effect of Neighborhood Civil War on human rights violations is to compare the size of the effect mentioned above to the size of the effect of other covariates. Consider the impact of civil wars within a state's own territory, which is a very consistent predictor of human rights violations (Poe, Tate, and Keith 1999; Neumayer 2005). We estimate that an ongoing civil war in state i when there is no civil war in the neighborhood leads to a 1.1-unit increase on the CIRI scale. This is the equivalent of a shift from, say, occasional torture to

systematic and widespread torture—a very large increase. A highly conflicted area *outside* of the state's borders thus has almost a third of the impact that a civil war within a state's own territory has on its predicted level of repression. As another illustration of substantive meaning, as a state becomes one point more autocratic across the four-point Unified Democracy Score scale, we predict the same effect on rights violations as moving from a peaceful to a highly civil conflict-ridden neighborhood.

It could be that we are observing states repressing as a response to dissent rather than preempting civil war. If civil wars in the neighborhood lead to the roots of rebellion—dissent preceding outright rebellion—we may be observing state authorities responding to that low level but actualized dissent rather than acting on their fears about conflict abroad.

Model 1b, in which we replace the indicator of civil war in state *i* with *Dissent*, or the number of riots, protests, and demonstrations in that state-year, obviates this concern. Interpreting the estimated coefficient on Neighborhood Civil War as the effect of proximal civil war on state repression when dissent is absent, we find that geographically proximal civil war has a statistically and substantively very similar effect on repression even under this more stringent test. This test goes further in demonstrating that state authorities repress to avert contagion before dissent or rebellion materializes at home.

These results support our interpretation of recent events in the Middle East. During the Arab Spring of 2011, many states across North Africa and the Middle East experienced protests and rebellion. Many states were in the position of responding once protest reached them, as was the case in Egypt, where protesters flooded the streets on January 25 and overwhelmed state efforts to contain them. As the prevalence of conflict in the region increased, however, other states readied for protests. On February 12, the Algerian government warned civilians not to protest, and riot police heavily outnumbered the thousands of protesters who defied those warnings. Syrians used social networking sites to organize protests in early February, but no protesters participated, a failure attributed to intimidation by authorities. Riot police with violent tactics were also ready to quash likely protesters in Bahrain, Iran, and Libya in mid-February.¹⁹ An activist in Zimbabwe noted a post-Arab Spring shift in the government's use of repression, saying, "Robert Mugabe's regime clearly wants to prevent a Zimbabwean spring."²⁰ By repressing, Algeria and Iran quickly preempted the development of further mobilization, while Bahrain, Libya, and Syria failed to prevent the contagious conflict.

Tests of Hypotheses 2 through 5 examine whether states look primarily to similar states for information as to what is likely to threaten them; Table 2 presents the estimates from these tests, and the effects of the key explanatory variables are illustrated in Figure 2. In model 2, we include the sum of civil wars in states with similar cultures or regime types anywhere in the world to examine whether states learn from outcomes in similar states abroad (Hypotheses 2 and 3). In the section of Figure 2 labeled "Model 2," the point estimates of both of these measures are very near the

Table 2. Predicting the Level of Repression in State *i*.

Variable	Model 2	Model 3	Model 4
Neighborhood CW	1.14 (0.52, 1.76)		
Similar Culture CW	-0.04 (-0.05, -0.02)		
Similar Regime CW	-0.01 (-0.03, 0.02)		
Nearby Similar Culture CW		0.93 (0.15, 1.73)	
Nearby Different Culture CW		0.61 (-0.37, 1.54)	
Nearby Similar Regime CW			1.04 (-0.17, 2.33)
Nearby Different Regime CW			0.78 (0.15, 1.43)
Neighborhood CW \times CW _{<i>i</i>}	-1.68 (-3.35, -0.03)		
Neighborhood CW \times dissent _{<i>i</i>}			
Nearby Sim. Cult. CW \times CW _{<i>i</i>}		0.48 (-1.61, 2.66)	
Nearby Diff. Cult. CW \times CW _{<i>i</i>}		-3.52 (-5.71, 1.29)	
Nearby Sim. Regime CW \times CW _{<i>i</i>}			-1.88 (-4.21, 0.47)
Nearby Diff. Regime CW \times CW _{<i>i</i>}			-1.40 (-3.44, 0.58)
Civil War _{<i>i</i>}	1.16 (0.90, 1.41)	1.10 (0.84, 1.35)	1.11 (0.87, 1.37)
Democracy	-0.43 (-0.52, -0.34)	-0.40 (-0.49, -0.32)	-0.41 (-0.50, -0.32)
Lagged CIRI	0.48 (0.44, 0.51)	0.47 (0.44, 0.50)	0.48 (0.44, 0.51)
Log Population _{unit}	0.27 (0.21, 0.33)	0.28 (0.22, 0.34)	0.27 (0.21, 0.33)
Log GDP per cap _{unit}	-0.27 (-0.36, -0.17)	-0.30 (-0.40, -0.20)	-0.28 (-0.38, -0.19)
Mean α_j	-0.42 (-1.65, 0.81)	-0.34 (-1.60, 0.94)	-0.38 (-1.59, 0.87)

Note. CIRI = Cingranelli and Richards Human Rights data set; CW = civil war; GDP = gross domestic product. Point estimates presented beside 95 percent credible intervals. $N = 3,860$. Priors are $N(0, 10)$. 2,500 iterations, the first 1,000 of which were omitted as "burn-in."

vertical line representing zero effect on the dependent variable; states do not increase the level of repression at home when states worldwide with similar dominant languages or religions or even regime types experience civil conflict. While neither credible interval includes zero, making these estimated effects statistically significant, the substantive significance of these effects is essentially zero.

Though states do not seem to repress in response to civil war in distant, similar places, perhaps they respond differentially to proximate civil war in similar states

than to that in dissimilar ones. Model 3 tests Hypothesis 4, replacing the general Neighborhood Civil War variable with two variables that capture the prevalence of nearby civil war in culturally similar and dissimilar places, respectively. We find that civil war in nearby, culturally similar places has an effect that is slightly larger and somewhat more precisely estimated than civil war in nearby, culturally dissimilar places. However, the difference between these coefficients, represented by the third line and point estimate in section three of Figure 2, is substantively minuscule and not statistically significant. We thus cannot conclude that nearby civil war in states with similar cultures makes leaders any more nervous about dissent than nearby civil war in culturally dissimilar states. This is additional evidence against the hypothesis that leaders respond to connections beyond geography when determining the propensity for threat at home.

Model 4 parallels model 3, except that we shift to focus on the effect of proximate civil war in states with similar versus different regime types. The bottom portion of Figure 2, labeled model 4, displays the key results of this analysis. As predicted, the coefficient for civil war in similar regimes is larger than the one for dissimilar regimes. However, as the bottom-most line indicates, there is no statistically discernible difference between these coefficients. Model 4 thus lends little support to the hypothesis that leaders repress more in response to civil wars in proximal, similar states than to civil wars in proximal, different states.

Overall, these analyses paint a consistent picture. Generally, leaders are wary of civil strife in their neighborhood spreading to their nation. To prevent diffusion, they increase repression. However, leaders do not tend to respond to the possibility of civil war spreading from culturally similar states or states with similar regime types any more than any other type of state. These findings suggest leaders are concerned primarily with the mechanical drivers of civil war diffusion that are tied to geography, such as refugee flows and transnational rebel groups, rather than cultural or political similarities that may facilitate learning. On the whole, we find consistent and substantively meaningful support for our theory that authorities, threatened by the potential for civil conflict elsewhere to foster rebellion at home by a variety of possible mechanisms, will repress more as civil war becomes more prevalent in the neighborhood.

Conclusion and Implications

Externalities from nearby civil conflicts increase potential rebels' incentives to dissent violently; we argue that state authorities repress to counterbalance these altered incentives. Using spatially weighted measures of the prevalence of civil conflict in a state's neighborhood, we find empirical support for our central claim: as civil conflict becomes more prevalent in the neighborhood, states increase levels of repression. State leaders repress not only when directly experiencing conflict but also when *other states* experience conflict.

Interestingly, we found very little empirical support to suggest states adapt their policies in response to civil conflict in states that are either culturally or structurally similar to them. Authorities do repress to avoid civil conflict that may otherwise diffuse from another state, but we find no evidence that leaders worry about civil war diffusing from similar states around the globe. Even similarly structured or populated states experiencing civil war within the state's geographical region do not differentially affect leaders' use of repression. Geography may be even more important in influencing repressive behavior than we anticipated, surpassing likeness or explicit connections in leading a state to repress its citizens.

This project presents evidence of spatial influences on human rights practices. Though the study of policy diffusion has grown over the last decade, human rights practices have largely not entered this picture—we know very little about how or why human rights practices might spread geographically. States are more likely to ratify human rights treaties if similar states have done so for strategic reasons (Simmons 2009) or to garner a good reputation among neighbors (Goodliffe and Hawkins 2006), and states are more likely to become democratic when their neighbors do (O'Loughlin et al. 1998; Gleditsch 2002a; Gleditsch and Ward 2006; Elkins 2011). Nevertheless, we do not know whether and why states might emulate their neighbors in terms of their human rights practices. Our findings suggest that states violate rights in connection with other states' conflict occurrence; a civil conflict not only affects the treatment of citizens in the state engaged in conflict but also the treatment of citizens in nearby peaceful states.

We argue elites will increase rights violations not to mimic their neighbors, but to *differ* from them. Most diffusion studies find that policies spread because one state's action either increases the incentives for a state to follow suit or a state learns about a policy that it then echoes. In contrast, we contend states observe civil conflict in the neighborhood and desire to *prevent* the same outcome at home. Authorities will try to counterbalance the externalities that cause domestic challenges to spread geographically. States adopt repressive policies much like their neighbors who are actually engaged in civil conflict, but they do so in an attempt to avoid a similar outcome.

Beyond directly examining how neighborhood conflict affects human rights violations, we present new insights into how leaders perceive linkages between their state and others. Surprisingly, we find no evidence that leaders take stronger cues from civil wars in similarly structured states, nor do we find evidence that leaders are differentially concerned when their cultural peers experience civil war. We suspect that states rely on connections to learn from one another, so when is that the case? Learning may be a lengthy process, whereas a state may have to react quickly to preempt rebellion before learning the consequences of conflicts in its neighborhood. Our results suggest it may be useful to look further into the mechanisms by which state authorities identify spatial threats.

Finally, we offer the first account of how human rights violations reflect the domestic empirical realities of other states. Scholars tend to explain human rights practices as a function of *domestic* behaviors and institutions, with some studies

addressing direct attempts to influence another's practices, such as foreign aid, naming and shaming, or military interventions. In this study, we break this "closed state" model of rights violations and contend that one state can affect the rights practices of another state even when it has no intention of doing so.

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Notes

1. Elkins and Simmons (2005, 39) note that scholars in a variety of fields have identified nearly thirty mechanisms by which policies diffuse, but all share one of these general logics. The italicized terms are theirs.
2. Repression is defined as the use or threatened use of physical sanctions or political restrictions against persons within the territorial jurisdiction of the state with the intent to end or prevent activities perceived as threatening to the state (Goldstein 1978, xxvii). Repressive actions include both civil and political rights violations, such as torture, political killing, restrictions of speech and assembly, and the restriction of movement. We refer to repression and human rights violations interchangeably.
3. Groups may also learn and/or receive support from external actors with political likeness, such as communist groups following other communists. We focus on cultural similarity (operationalized in the following) to maximize the comparability of groups across national contexts.
4. Simmons and Elkins (2004) examine joint international organization membership, suggesting that such explicit links between states can approximate for their likelihood of communicating with one another. However, we believe that a state should not be more

afraid of rebellion at home because its trade partner (which may be a completely different type of regime) experienced one. Thus, we focus on learning by comparison with similar states.

5. Our results are robust to the use of the Political Terror Scale (Gibney and Dalton 1996) instead of CIRI. These results can be found in the supplementary appendix.
6. For a different conception of spatial relationships between states that includes societal connections, see Buzan (1983).
7. This data is available at www.prio.no/CSCW/Datasets/Armed-Conflict/UCDP-PRIO/. We use the standard operationalization for civil war: at least 1,000 battle deaths during the course of the war and evidence that both sides were able to offer some opposition. Our results are robust to using a lower threshold of twenty-five battle deaths.
8. Adding one to the denominator of this fraction reduces the effect of anomalous countries with one or few neighbors.
9. We include a figure illustrating this function in our supplementary appendix.
10. We ran our primary test (Hypothesis 1) substituting the Polity scale for the UDS scores; our results are robust to this change. See the supplementary appendix.
11. A histogram of UDS scores can be found in our online appendix, along with a table listing some state-years and their associated scores to give a sense of how “close” 0.2 UDS points is. Our results are robust to wide variations in this choice.
12. Scholars disagree as to how regime type affects repression, with some demonstrating simply that increases in democracy reduce rights violations (e.g., Henderson 1991; Poe, Tate, and Keith 1999; Hafner-Burton 2005) and others noting a nonlinear (Muller 1985; Regan and Henderson 2002) or threshold effect (Davenport and Armstrong 2004). The UDS measure includes a variety of conceptualizations of democracy, such that it does not perfectly match these prior studies, but it does allow us to control for the potential of democracy to confound the relationship between proximal civil war and state repression.
13. Davenport (2007b, 86-87, 95-97) finds evidence that repression is also spatially dependent, such that a state’s repression is related to that used by other states in its region. We estimated a model controlling for a spatially weighted indicator of repression in the region, and our findings are robust to this permutation as reported in the supplementary appendix.
14. Strictly speaking, our dependent variable is ordinal. However, we prefer a linear model for ease of interpretation. The statistical and substantive size of our findings are robust to using a model for ordered outcomes; see the supplementary appendix.
15. We employ vague priors (and hyperpriors) to generate all of the reported results. We used WinBUGS to estimate the models, as called from R. All models were run for 2,500 iterations, the first 1,000 of which served as a “burn-in” period. All models converged, as evidenced by visual examination of trace plots and R-hat statistics of essentially 1.0.
16. Our results are robust to employing a model that includes state and year fixed effects and models all covariates at the level of observation; see the supplementary appendix.
17. Of observations, 9.6 percent contain at least one missing value.
18. For more detail on civil wars as the dominant cause of refugee flows, see, for example, Davenport, Moore, and Poe (2003), Moore and Shellman (2004) and Rubin and Moore (2007).

19. Details on these events are available from the *Washington Post*'s summary, <http://www.washingtonpost.com/wp-srv/special/world/middle-east-protests/> (accessed January 19, 2012).
20. "Activist: 'Zimbabwe government increases repression following Arab Spring,'" Amnesty International News, May 25, 2012, <http://www.amnesty.org/en/news/activist-zimbabwe-government-increases-repression-following-arab-spring-2012-05-25>.

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