Contagion or Confusion?

Why Conflicts Cluster in Space[•]

Halvard Buhaug

Centre for the Study of Civil War (CSCW), PRIO

Kristian Skrede Gleditsch

University of Essex and CSCW

ABSTRACT

Civil wars cluster in space as well as time. In this study, we develop and evaluate empirically alternative explanations for this observed clustering. We consider whether the spatial pattern of intrastate conflict simply stems from a similar distribution of relevant country attributes or whether conflicts indeed constitute a threat to other proximate states. Our results strongly suggest that there is a genuine neighborhood effect of armed conflict, over and beyond what individual country characteristics can account for. We then examine whether the risk of contagion depends on the degree of exposure to proximate conflicts. Contrary to common expectations, this appears not to be the case. Rather, we find that the conflict is more likely when there are ethnic ties to groups in a neighboring conflict and that contagion is primarily a feature of separatist conflicts. This suggests that transnational ethnic linkages constitute a central mechanism of conflict contagion.

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Introduction

The increased salience of civil war in the post-Cold War era has given rise to a large number of studies seeking to uncover the causes of violent conflict within societies.[1] Numerous comparative studies have explored how various characteristics of countries influence the risk of civil war (for reviews of this literature, see Sambanis 2002). As useful as these quantitative studies are, they tend to adopt a 'closed polity' approach, which assumes that the prospect for domestic conflict is a function of the specific characteristics prevailing in individual countries, and disregards the potential influence of regional factors and the international context. This assumption is highly questionable. Accounts of individual conflicts tend to attribute great importance to actors and events outside the affected state (e.g., Collier and Sambanis 2005). Moreover, we also know that armed conflicts tend to cluster spatially in certain geographic areas. A number of studies have shown that countries in proximity to states experiencing conflict are much more likely to become involved in violent conflict (Anselin and O'Loughlin 1992; Esty et al. 1995; Hill and Rothchild 1986; Most and Starr 1980; Sambanis 2001; Starr and Most 1983; Ward and Gleditsch 2002).[2]

The geographic clustering of intrastate conflicts strongly suggests that the risk of civil war is not determined merely by attributes of individual countries, and that regional factors and events in neighboring states can alter the prospects for violence. Cross-country studies of the causes of civil conflict that assume that the observations are independent of one another face the perils of the problem first noted by Galton (1889), namely that apparent structural relationships may reflect diffusion between observations. However, spatial clustering in conflict does not by itself imply a causal relationship. We know that many of the phenomena believed to influence the risk of civil war, such as country income and political institutions, also tend to cluster geographically (see Gleditsch 2002b). Hence, the observed spatial clustering of conflict could be simply due to a corresponding distribution of relevant state characteristics associated with conflict. More generally, we would face a reverse Galton's problem if we try to evaluate evidence for spatial contagion without first considering relevant unit attributes that may be both spatially clustered and potentially related to conflict.

In this article, we develop a systematic approach to evaluating whether the spatial clustering of civil war merely reflects similarly arranged country characteristics or whether there is additional evidence suggesting cross-border contagion.[3] We first examine to what extent the geographic distribution of country characteristics believed to influence the risk of conflict may account for the observed clustering of conflict. We then explore whether the likelihood of conflict is proportional to a country's interaction opportunities with and exposure to neighboring states in conflict. Seven specific factors are considered: the distance to the nearest conflict zone, the length of the boundary with the conflict neighbor, the existence of ethnic ties to the neighboring conflict population, refugee population from the conflict neighbor, the severity of the neighboring conflict, the nature of the neighboring conflict, and the size of the neighboring country experiencing conflict. We further distinguish between different types of conflict and examine to what extent secessionist insurgencies entail different propensities for contagion than conflicts over state control. To anticipate, our empirical analysis indicates that the spatial clustering of intrastate conflict is genuine, and this neighborhood effect does not disappear when taking into account other spatially clustered country-specific influences on conflict. However, the risk of contagion does not appear to increase in proportion to the density of neighboring conflict; a single neighboring conflict is sufficient to increase the risk of conflict onset and additional conflicts in the neighborhood add little to the likelihood of civil war. While most geographic and demographic characteristics of the neighboring conflicts per se are largely irrelevant, we find that transnational ethnic ties constitute an important factor contributing to the spread of conflict. Moreover, we find that contagion effects are primarily associated with separatist conflicts. Conflicts over a central government are generally not contagious and appear to be driven by other factors than territorial conflicts. Finally, we find that the risk of conflicts spreading to neighboring states is the highest when these states have characteristics that induce a higher risk of civil war at the outset.

The Origin of Conflict Clusters

Many researchers have demonstrated the existence of conflict clusters, using a variety of different sources of conflict data (see, e.g., Anselin and O'Loughlin 1992; Braithwaite 2006). This has lead some to conclude that conflicts have a propensity for contagion, in the sense that a civil conflict in one country may bring about the onset of civil conflict in a nearby country within a short time period.[4] The sequential conflicts in the Balkans and around the African Great Lakes in the 1990s clearly provide strong support for this idea. Indeed, the eruption of civil war in DR Congo (then Zaire) in 1996 was clearly influenced by events in neighboring states. For example, Rwandan Hutu militias (*Interahamwe*) operated from eastern Zaire in their fight against the Rwandan Tutsi government, joined by Zairian Tutsis (*Banyamulenge*). After the defeat of the Rwandan government by the Rwandan Patriotic Front forces operating from Uganda, Rwandan forces, under the leadership of Laurent-Desire Kabila and supported by Uganda and Burundi, in turn entered Zaire and ousted President Mobutu Sese Seko (Nzongola-Ntalaja 2004).

However, these cases aside, skeptics may contend that the apparent conflict clusters in general arise simply due to regional similarity in other country-specific features related to conflict. To illustrate the problems involved in distinguishing contagion of conflict from the clustering of similar neighbor characteristics, consider the relationship between civil war and income. A recent World Bank report asserts that poverty is strongly and consistently associated with civil war (Collier et al. 2003). The map in the left panel in Figure 1 displays the distribution of national income per capita in 2000, while the right panel provides a map of countries that experienced armed intrastate conflict between 2001 and 2005. The maps indicate that both poverty and conflict cluster in certain regions of the world, and a comparison of the two phenomena suggests considerable spatial overlap. Only one of the 38 active armed conflicts in the 2001–05 period took place in the richest quartile of the world's countries; the al-Qaeda strikes on the United States on 11 September 2001. By contrast, more than one-third (17 of 47) of the countries in the poorest quartile in the year 2000 experienced intrastate conflict within the subsequent five years. Although all continents except Oceania saw at least one intrastate conflict in this period, Africa – the least economically developed continent – was also the most severely affected by civil war, both in terms of the number of conflict years and the total number of countries with conflict on their soil.

[Insert Figure 1 here]

Figure 2 shows the relationship between a country *i*'s GDP per capita (left) and level of democracy (right) and the corresponding average values for its neighboring countries, weighted by the inverse of the distance of each neighbor *j* to *i*, with a dashed line indicating the linear regression of each variable on the average among neighboring states. The plots demonstrate strong spatial covariance between the values for an individual country and those of its neighbors. In other words, states with low income tend to have poor neighbors, while rich states are generally surrounded by wealthy countries. Similarly, democratic institutions are much more likely to be found among other democratic states than in regions with predominantly authoritarian regimes.

[Insert Figure 2 here]

Maps and scatter plots can help visualize relationships, but establishing spatial overlaps is obviously not sufficient to determine causal relations, since such bivariate displays ignore the role of other potentially important factors that are likely to cluster spatially. Hence, to assess whether spatial position and proximity to conflicts in other states influence the risk of civil war, we must consider a multivariate model of the likelihood of conflict that captures relevant country characteristics that may induce spatial clustering. In the next sections, we develop five testable hypotheses on the origins of conflict clusters, which are then evaluated through a statistical analysis of civil conflict onset, 1950–2001.

Bad neighborhoods

As suggested above, the geographic clustering of conflict may arise due to a corresponding clustering of domestic factors believed to promote conflict. If this is the case, then any apparent distributional pattern of armed conflict should disappear once we take into account relevant domestic factors in a systematic fashion. This view is at least implicitly advanced by the large majority of conventional cross-country studies of civil war, which do not discuss the potential role of international factors and adopt research designs that only consider individual country characteristics. King (1996) provides a more explicit defense of this position. Within the context of electoral behavior, he argues that many alleged contextual effects would essentially dissipate in a properly specified model of determinants of individual preferences. Hegre et al. (2001: 41) make a similar claim with respect to civil conflict. They find little evidence of contagion of conflict from neighboring states, and conclude that the apparent clustering of civil war is fully explained by the clustering of domestic factors included in their model, mainly GDP per capita and regime type.[5] In the language of hypothesis testing, we can state this view as positing that a well-specified model of the risk of civil war should not display a residual clustering effect of conflict onset:

H1: *The positive effect of neighboring conflict on the risk of civil war disappears when taking into account relevant country-specific factors, and the clustering of these attributes.*

Obviously, researchers may differ on what is meant by a well-specified model of the risk of civil war. The extent of evidence for residual spatial clustering will clearly depend on model specification and data considerations. We return later to the issue of specifying relevant country-specific characteristics other than income and regime type that may be spatially correlated. However, for now we first wish to raise the more general point that previous tests that dismiss the role of spatial contagion in our view suffer from a number of important limitations. Hegre et al., for example, only consider major civil wars, thereby limiting the number of potential cases by excluding any possible contagion to and from less severe violent events. Second, and as we will discuss in more detail later, civil wars vary considerably in terms of the characteristics and objectives of the rebellion. Although some types of civil conflicts would seem more likely to be contagious than others, previous research has lumped all forms of civil war together. Third, most researchers use conflict measures at the country level, without a clear reference to spatial location. Fearon and Laitin (2003), for example, treat conflicts in colonies as civil wars in the metropole country. As such, the conclusion of previous studies regarding the irrelevance of spatial contagion may be a Type II error stemming from problems of research design and operationalization. Finally, few studies have considered how characteristics of neighbors other than the presence of armed conflict may influence the risk of civil war. One exception is Gleditsch (2007), who finds that states surrounded by democratic states are generally less likely to see a civil war onset, which he attributes to constraints on leaders to provide support for insurgencies in neighboring states. Such a mechanism would be a 'neighborhood effect' in the sense that country-specific attributes alone do not determine the risk of conflict, although it is potentially confounded with other determinants of contagion that also need to be considered.

Interaction opportunities and ties

Assuming for the time being that conflict clustering does not merely stem from a similar clustering in other country characteristics, many questions remain regarding the specific nature of geographical contagion effects. No study to date has adequately examined how interaction opportunities or exposure to nearby conflict influences the likelihood of conflict. Previous work are generally limited to considering contextual effects in a dichotomous fashion, and test whether the presence of conflict elsewhere increases the risk of civil conflict onset in the country of observation.[6] However, the analogy often drawn between infectious diseases and the spread of conflict (see Rapoport 1960; Davis et al. 1978; Siverson and Starr 1990) suggests that the risk of contagion should vary with interaction opportunities, just as the risk of an individual catching a disease generally increases with the number of infected people that person interacts with (Watts 2003).

The widely used gravity model depicts a more refined association whereby the extent of interaction between two units is proportional to their combined size and inversely proportional to the distance separating them (Linnemann 1966; Zipf 1949). Assuming that the same set of constraints shape the contagion of violence as other forms of social interaction, we would expect immediate neighboring states to feel the greatest negative externalities from an ongoing civil war.[7]

A number of more specific mechanisms suggested in the contagion literature support such an expectation. First, civil wars have been shown to exert a negative impact on regional economic growth (Murdoch and Sandler 2004), which lowers the opportunity costs of rebellion in neighboring states. Consequences of ongoing conflict in neighboring states, such as access to cheap arms, cross-border rebel sanctuaries, and mercenaries moving across boundaries, may also contribute to the spread of violence. The rebel factions in Liberia and Sierra Leone are examples of the latter, where warlords, thugs, and so-called sobels (soldiers at day, rebels at night) controlled vital pieces of resource-rich territory on both sides of the border. Such conflict externalities are likely to be mediated by distance. Drawing on the gravity model and other theories of interaction, we expect the spillover effects of civil war to be felt by most intensely by countries near the conflict zone, in particular those who see battles close to or crossing over their borders. Similarly, the interaction perspective would imply that the likelihood of cross-border contagion is positively associated with the length of the border to the conflict neighbor.

Second, violent mobilization in one country may lead to emulation by neighboring groups facing similar conditions. Kuran (1998) suggests that ethic conflict in other states can make groups more aware of their own grievances, raise expectations of ethnic conflict at home, and make global public opinion more sympathetic towards their political demands. In the same vein, Lake and Rothchild (1998) argue that as groups update their beliefs by observing events elsewhere, ethnic conflict can literally materialize out of thin air. Given that most forms of interaction are geographically confined, we expect reference examples and media attention to focus primarily on events in nearby states.

Third, conflicts may spread through transnational ethnic ties, whereby mobilization by group members in one state will change the prospects for mobilization by the same group in other countries (see Halperin 1998). The Albanian revolt in Macedonia, which occurred in the wake of prior mobilization by Albanians in the Yugoslav province of Kosovo, serves as a relevant case. If this conflict is representative of a more general relationship, then we should expect to find the likelihood of contagion to be higher whenever a country has cultural ties to groups engaged in a neighboring conflict.

Finally, some studies suggest that conflict may become more likely due to particular cross-border population movements. Salehyan and Gleditsch (2006), for example, argue that large refugee populations often create severe strains on receiving countries by exacerbating resource competition in the host community, altering the local ethnic balance, and sometimes also by containing so-called refugee warriors with potentially unhealthy aspirations and prior experience from combat. The intractable linkage between refugee flows and organized violence is evident in many parts of the contemporary world, but nowhere with a more disastrous outcome than in central Africa. We have already mentioned the intertwined conflicts in DR Congo, Rwanda, Burundi, and Uganda; a more recent example is found in eastern Chad, where Sudanese rebels raid refugee camps and forcibly recruit soldiers (UN 2007; see also UNHCR 2004a). Accordingly, we expect countries with large influx of refugees from a proximate conflict to be more at risk than otherwise similar conflict neighbors with limited or no refugee populations.

This gives the following testable hypothesis, with four specifications:

H2: *The risk of civil war is positively associated with the proximity of nearby conflicts.*

- H2a: *The risk of civil war is higher if the neighboring conflict zone abuts the common border.*
- H2b: *The risk of civil war is positively associated with the length of the border with the conflict neighbor.*
- H2c: *The risk of civil war is positively associated with the existence of transnational ethnic ties to the population in a neighboring conflict.*
- H2d: *The risk of civil war is positively associated with the influx of refugees from a conflict neighbor.*

The second component of the gravity model, i.e., size, suggests that larger units tend to interact more than smaller units across the same distance. In its directed form, the model would predict that larger units have higher influences on their neighbors than smaller ones. [8] Population size is positively correlated with the number of potential linkages to actors in other states, and economic and cultural relationships between nations can facilitate the spread of arms and ideologies conducive to insurgency. Moreover, population size is often considered as a component of potential military capabilities, and a large country is more likely to be considered a potential threat to security by its neighbors. As such, conflictual events in large countries could have a larger destabilizing effect in a region than events in smaller countries. Thus, we propose:

H3: *The risk of civil war is positively associated with the size of the conflict neighbor.*

It is certainly possible to imagine other factors related to the ease of interstate interaction that might also influence the likelihood of the spread of conflict. One such possible factor is the existence of high mountain ranges along the border, which inhibit not only trade and transportation, but also refugee flows and illegal smuggling of arms. Yet, we believe that the factors identified above are the most plausible among the mechanisms of contagion proposed in the literature.

Conflict characteristics

Another question not considered by previous research is whether various types of conflict are equally likely to diffuse. In fact, much of the literature treats civil wars as a single phenomenon and employs aggregate analysis of internal conflicts in general. Yet, civil war is a heterogeneous class of events that encompasses both efforts by peripheral groups to gain territorial concessions, such as autonomy or independence, and various forms of conflict within the center, including revolutionary movements and military coups. Furthermore, some studies have found evidence that different types of conflict display quite different characteristics. For example, Fearon (2004) reports that civil wars involving indigenous groups ('sons of the soil') last longer on average than coups and other regime-related unrest, while Sambanis (2001) shows that conflicts in neighboring states primarily increase the risk of identity-based (or 'ethnic') wars. If different types of civil war indeed pose different prospects for contagion, then lumping together all types of conflict may yield highly misleading results.

We agree that civil wars may come in different forms. However, many of the proposed categorizations of civil war – in particular, the distinction between ethnic and ideological wars – are difficult to apply in practice and often not mutually exclusive. Consider, for example, the civil war in Guatemala. Although the UNRG (Guatemalan National Revolutionary Unit) recruited among indigenous communities, this could also be considered an ideological war, given the UNRG's Marxist orientation. Buhaug (2006) suggests an alternative approach, distinguishing between civil wars based on the rebels' stated objective rather than on their base of mobilization. We adopt this classification scheme and identify conflicts as either concerning territory (i.e., secession or autonomy) or government control.[9]

Separatist conflicts typically involve peripheral ethnic minority groups that have kith and kin across the border. These conflicts should be more likely to give rise to regional demonstration effects as they appeal primarily to connected groups, in contrast to ideologically motivated revolts, which tend to employ universalistic principles (Kaufman 1996; Lake and Rothchild 1996). All else being equal, conflicts in the periphery are also likely to yield more severe conflict externalities as they often locate along porous borders and generally persist for much longer periods than do conflicts over government control. All these factors suggest that separatist conflicts should entail much greater risk of conflict in proximate states:

H4: *Separatist wars pose greater risk of inducing conflict in neighboring states than do conflicts over control of government.*

Besides the primary objective of the rebels, conflicts also differ with respect to the magnitude or degree of severity. Although we do not believe that the causes of low-intensity insurgencies are inherently different from major civil wars, we expect severe conflicts to pose greater threats to neighboring states. Bloody conflicts usually involve more combatants, cover a larger geographic area, lead to more devastating regional consequences, and generate a large number of refugees and internally displaced migrants. The largest exporters of refugees recorded in 2004 were all countries that suffered from severe internal violent conflicts, such as Afghanistan, Sudan, Burundi, D. R. Congo, and Somalia (UNHCR 2004b: 3). Incidentally, four of these five countries had contiguous neighbors that experienced armed conflict that year (see Harbom and Wallensteen 2005). Our last proposition, then, is:

H5: *The risk of civil war is positively associated with the severity of neighboring conflicts.*

Data and Research Design

We test our hypotheses using data on intrastate conflicts in independent states over the period 1950–2001, based on the UCDP/PRIO Armed Conflict Dataset (henceforth ACD), v.3.0. The ACD records all violent incidents between state governments and organized opposition groups that caused at least 25 battle-related deaths in a calendar year (Gleditsch et al. 2002). We develop two different dependent variables to evaluate the outlined expectations. Hypotheses 1–3 and 5 are tested on the outbreak of civil war in general, using a binary indicator of conflict onset in a conditional logit model. Hypothesis 4 relates specifically to the objective of the conflict, and is tested in a multinomial logit model using a three-category dependent variable that separates between no onset (reference category, coded '0'), territorial conflict onset ('1'), and governmental conflict onset ('2'). The type of conflict is determined from the 'incompatibility' indicator of the ACD. Only the year in which a conflict breaks out is counted as an onset; ongoing years of conflict are coded '0' (as opposed to missing) to allow for multiple simultaneous conflicts.[10] In conflicts where there is a lull between periods of fighting, we consider incidents that are separated by more than two calendar years of inactivity to constitute separate conflicts with individual onsets.

Our main explanatory factor is the presence of conflict among nearby states, similar to the notion of a spatial lag of the dependent variable (see Anselin 1988; Beck et al. 2006). We use a variety of operationalizations of neighboring conflict: The simplest is a binary indicator of whether at least one neighboring state experiences a civil war in a given year. We only code directly contiguous states as neighbors, since most factors believed to contribute to the spread of conflict apply primarily to land borders. We also consider a more complex measure weighting all conflicts by their proximity to the unit of observation. More specifically, we use an inverse distance-weights matrix **W**, given by

$$\mathbf{W}_{ij} = \frac{1/d_{ij}}{\sum_{j=1}^{n} 1/d_{ij}},$$

where d is the distance between a pair of countries i and j. In words, the inverse of the distance between i and j is normalized by the sum of the inverse distances between i and all other countries j, so that conflicts close to i count more than more distant ones. The weighted neighboring conflict measure can take on values between 0, in the event that there are no intrastate conflicts in the international system in the given year, and 1, in the extreme case of conflict in all other countries in the system in the given year. To determine the distance between pairs of states, we relied on minimum distance data for states separated by up to 950 km from Gleditsch and Ward (2001), supplemented by intercapital distances, generated by EUGene, for states that are further apart (Bennett and Stam 2000).[11]

The second stage of our analysis considers the relative risk of civil war among contiguous neighbors to countries with conflict. We investigate a number of different measures of opportunity for contagion: the (logged) length of the common boundary between the observation country and the neighbor in conflict; the (logged) minimum distance from the country to the neighboring conflict zone; a discrete measure indicating whether the neighboring conflict zone abuts the boundary of the country; the (logged) number of accumulative battle-related deaths in the neighboring conflict; the (logged) population size of the conflict neighbor; the (logged) size of the refugee population from the conflict neighbor; and whether there are ethnic ties between a country and the population in the neighboring conflict zone. The boundary length measure is based on Furlong and Gleditsch (2003). Minimum distances from the conflict zones to all neighboring countries (rounded downwards to the nearest 50 km) were measured by combining the location variables in the ACD (see Buhaug and Gates 2002) with a GIS layer on national boundaries using ArcGIS 8.3. We also used ArcGIS to identify the conflicts that extended to the border of a neighboring state. As a proxy for the magnitude of the neighboring conflict, we include the cumulative number of battle-deaths, using annual casualty estimates from Lacina and Gleditsch (2005). Data on the population size of the conflict neighbor are based on the Correlates of War project's National Material Capabilities dataset, v. 3.02 (Singer et al. 1972). Data on the size of dyadic refugee flows were derived from the UNHCR's online database. Moreover, we used a digital version of the ethnographic *Atlas Narodov Mira* (see Cederman et al. 2006) in combination with the spatial conflict data to assess whether a country has ethnic linkages to groups residing in the neighboring conflict zone. Finally, we test whether the regional effect of conflict is conditional on the nature of the conflict by including a territorial conflict dummy to distinguish between separatist conflicts and the reference category, conflicts over government control. In cases of multiple neighboring conflicts, the locational attributes refer to the nearest conflict.

Our baseline model builds on existing studies of civil conflict and includes controls for exogenous and plausibly spatially correlated factors. We use the natural logarithm of GDP per capita data (from Gleditsch 2002a) with a one-year time lag, except in the initial year of observation. The availability of GDP data limits the temporal scope of our analysis to the 1950–2001 period. We also control for regime characteristics, using the democracy-autocracy index as well as a squared term from the Polity IV project (see Gurr et al. 1989). Earlier research has shown that more populous countries are disproportionately often involved in civil war. This is an important control variable for our purposes, as large countries are also more likely to have transnational groups, longer boundaries, and more neighboring countries. Hence, we add the natural log of population size, derived from the COW capability dataset, as a final country-specific control.

Lastly, we consider whether the post-Cold War period (1989–2001) is associated with a systematic difference in the baseline risk of armed conflict, as the demise of the bi-polar system is often held to have changed the nature of armed conflict (see Mueller 1989). Pooling observations over time raises the likelihood of duration dependence and possible overconfidence in coefficient estimates.[12] We address the temporal ordering of the observations by including a count of the number of consecutive years of peace and reporting robust standard error estimates.[13] We have also considered a number of other factors frequently included in quantitative studies of civil war, such as political instability, rough terrain, ethnolinguistic fractionalization, and measures for resource-dependent economies. Although some of these factors on occasions gain statistical significance, we do not report these results here, as they do not affect the results for our key explanatory variables.

Empirical Results

Models 1 and 2 in Table 1 provide an initial test of the impact of neighborhood characteristics. Aside from the peace-years variable and the post-Cold War dummy, we only consider regional measures of intrastate conflict, democracy, and development. The two models differ only in the operationalization of neighboring conflict; the first uses the inverse distance-weighted average of regional conflict density whereas the second uses the dummy variable for contiguous neighboring conflict. Model 1 provides little evidence of conflict contagion. The coefficient estimate for the weighted neighboring conflict variable, while positive, is not significantly discernable from zero. In fact, the only explanatory factor to make a substantial impact on the fit of the first model is neighboring level of development. All else being equal, countries in a poor neighborhood are considerably more likely to experience domestic conflict at any given time than countries with wealthy neighbors.

Model 2, which drops the complex spatially lagged dependent variable in favor of a simpler dummy indicator, presents a quite different picture. Contrary to the claims of Hegre et al. (2001) and others, we find that civil conflicts exert a significant destabilizing effect on the neighborhood. All else held at median values, a country is nearly twice as likely to experience an outbreak of conflict if at least one of its neighbors is involved in conflict. The contrasting findings of these two models suggest that the risk of becoming involved in civil war does not increase in proportion to the share of nearby states in conflict. Rather, the risk of conflict assumes a discrete function where the major divide runs between countries without neighboring conflict and countries with at least one conflict at their borders. The other contextual covariates behave largely as in Model 1; regional economic development is associated with a significant decrease in the risk of conflict – consistent with the spatial overlap illustrated in Figure 1 – while regional level of democracy has a negligible effect.

In Models 3 and 4, we introduce four country-specific controls: democracy, democracy squared, GDP per capita, and population size. Not surprisingly, we see that taking into account domestic attributes substantially decreases the apparent clustering effect of civil war. The parameter estimate for the inverse distance-weighted measure of regional conflict incidence (Model 3) decreases by more than 60% compared to Model 1 and is far from statistical significant. The simpler dummy for neighboring conflict (Model 4) is also weaker than in Model 2, but only by 30%, and remains statistically significant. The other contextual variables all fail to add significantly to the fit of the model; apparently, neighboring levels of democracy and development are not nearly as important as a country's own political and economic capacities. The control variables confirm the dominant view in comparative civil war research (see e.g. Hegre and Sambanis 2006): institutionally inconsistent, poor, and more populous countries are more prone to experience domestic turmoil than are other states. In terms of relative impact, country size is associated with the largest variation in the estimated risk of conflict. A change in population from the 5th percentile value to the 95th percentile corresponds to a fourfold increase in conflict propensity.[14]

[Insert Table 1 here]

Additional tests using the complete list of country-specific covariates suggested by Fearon and Laitin (2003) did not alter the main results reported here. Although we have considered the main suspects based on previous research on civil war, it is of course possible that *some other* omitted country specific covariate is associated with both civil war and the spatial covariates. One possible approach to ensure that our results do not arise from such unmeasured features of states is to estimate a model with country fixed effects (see Green et al. 2001). This is a very conservative test to assess potential unmeasured heterogeneity as it uses dummies to remove any constant feature of states that may underlie variations in risk. We are generally skeptical of the merits of fixed effects analysis for binary dependent variables (see Beck and Katz 2001), in particular as this requires us to exclude as non-informative all countries where we do not observe variation in the response or any instances of civil war (in this case 68 countries). Indeed, many country-specific covariates, such as GDP per capita, become insignificant in the fixed effects specification; few wealthy states experience civil war and much of the association here is cross-sectional rather than cross-temporal. However, our binary indicator for neighboring conflict retains a significant positive coefficient even in a fixed-effects model, indicating that our results cannot be dismissed as an artifact of merely failing to control for unit-specific differences.

Our reported estimates of the effect of conflict in neighboring countries are generated from observational data. However, as Ho et al. (2007) point out, many reported empirical estimates of the effect of a covariate in observational studies (i.e., the 'treatment') may be driven by influential data points that are far from the distribution of the data points where we observe the treatment. Whereas experiments ensure that other differences between treated and untreated observations should be random, in imbalanced samples in observational studies the estimated effects may be highly dependent on other differences between the samples of treated and non-treated observations. Matching provides one way to reduce model dependence by pre-processing data to achieve greater similarity between the distribution of the covariates in the treated and non-treated samples. In our case, using nearest neighbor matching and discarding all non-matched observations, however, still indicates a clear positive effect of conflict in a contiguous country. As such, we are confident that the reported results are not driven by extreme observations and systematic sample differences between cases where we see a civil conflict in a neighboring state and cases where we do not.[15]

These results provide tentative answers to some of the questions we have raised on conflict contagion. Most importantly, we find that there is a genuine neighborhood effect of civil war. Since this effect remains even after controlling for plausible third factors, the clustering of conflict cannot be dismissed as a mere artifact arising from similarly clustered country characteristics. The results further suggest that the contextual effect stems primarily from conflicts among contiguous countries, and taking into account more distant civil wars yields little additional information.

Next, we turn to the issue of how characteristics of a neighboring conflict influence the prospects that a new conflict will emerge. Table 2 reports a series of estimates for alternative specifications of risk of contagion, where the sample is limited to contiguous neighbor states of countries at war. More specifically, Models 5 and 6 consider the roles of various interaction opportunities with the conflict neighbor, as expressed in Hypotheses 2a-d, as well as characteristics of the neighboring conflict (Hypotheses 3-5).

Contra H2a and H2b, we do not find that conflicts are more likely to spread to neighbors with greater geographical opportunities for interaction. The estimates for the log of shared boundary length actually suggest a negative relationship, although the comparatively large standard errors imply that the coefficient for the variable is essentially undistinguishable from 0. More surprisingly, the estimate for the distance to the (nearest) conflict in the neighborhood (Model 5) and the dummy for conflicts extending to the border (Model 6) are also insignificant. Geography in its simplest sense is thus unable to shed additional light on the origins of conflict clustering and where conflict is more likely to occur. The size of the refugee population from a conflict neighbor (H2d) also fails to explain the variation in conflict propensity. Indeed, the only proposed mechanism underlying the interaction perspective that is in line with the empirical results concerns ethnicity (H2c). Models 5 and 6 indicate that countries with ethnic ties to groups in a neighboring conflict zone are more at risk of civil war.

Our remaining indicators of exposure to proximate conflicts and opportunities for spillovers display remarkably feeble links to the probability of conflict onset. We find no evidence that conflicts in larger countries have a larger destabilizing effect on its neighbors (H3); the insignificant coefficient estimate even has the wrong sign. Similarly, we also fail to uncover a robust positive association between the risk of conflict and the severity of the neighboring conflict (H5).

[Insert Table 2 here]

The clustering of armed conflict and the corresponding process of contagion are mainly a characteristic of separatist insurgency. Models 5 and 6 both indicate that the estimated probability of conflict is roughly doubled if the neighboring conflict concerns territory rather than control of the government. Given the weak findings for the boundary length and distance measures, this effect is unlikely to stem from separatist conflicts more often extending to or across the border of neighboring states (which they indeed often do). Rather, we interpret this finding as additional indication of the transnational aspect that often characterizes these conflicts. Even though we do include a rough indicator of ethnic ties as a separate factor, this measure is by no means perfect, and it is very likely that the powerful effect of the territorial dummy captures residual influence of transnational linkages. In addition, the pernicious impact of territorial conflict on the neighborhood may also be due to various demonstration effects, which are likely to be less central when neighbors experience conflicts over governmental issues.

The results for the country attributes in Models 5 and 6 indicate that the patterns of conflict onset among countries with one or more neighbors at civil war resemble the determinants of civil war onset in general. Wealthier states are much less likely to become 'infected' by the neighboring conflict, while larger states are more likely to experience onset, all else being equal. Accordingly, the risk of contagion is highest amongst those neighboring states that share characteristics associated with an elevated likelihood of civil conflict more generally. The only country-specific factor that behaves differently in the reduced sample is democracy, which no longer displays a significant parabolic effect.

So far, we have found that countries in conflict are hazardous to their immediate neighborhood, over and beyond what the spatial distribution of adverse country characteristics can account for. We have also found that countries with ethnic linkages to groups in conflict across a shared border are significantly more at risk of civil war than other conflict neighbors. Moreover, we have found that the trajectory of contagion tends to follow a weakest link mechanism, where it is the presence of a contiguous neighbor involved in a conflict that increases the risk of conflict, and where exposure to additional conflicts does not noticeably contribute further to the risk of conflict. The fact that the states that are the most prone to civil war also are more likely to see conflict among their neighbors implies that conflict in proximate states would be most likely to make an impact at the margin, or tip the balance towards violent conflict. Finally, we have found that the neighborhood effect of civil war mainly stems from separatist rebellions. All of these findings are consistent with our claim that transnational cultural ties contribute to the spread of armed conflict.

One more test remains to be conducted, however. If ethnicity indeed is a driving force of contagion, and the prevalence of a separatist conflict across the border increases the risk of civil war, this should primarily apply to territorial conflict. Model 7 presents the results from a multinomial logit model that evaluates correlates of territorial and governmental conflict separately.[16] In line with the above reasoning, we find that a neighboring territorial rebellion only increases the risk of territorial conflict, not violent conflicts over control of the government. The ethnic linkages dummy, in contrast, suggests that shared cultural ties are at least as important in predicting diffusion of conflicts over government control (although the difference between the coefficients is not statistically significant). More surprisingly, we find that refugee populations have opposing effects on the two forms of conflict; whereas they increase the likelihood of conflicts over government control, they actually appear to reduce the risk of separatist conflict in the host country. The latter finding is particularly unexpected as it runs counter to arguments concerning increased resource competition and ethnic polarization in the host state.

More generally, our results suggest that the causes of governmental conflict onset seem quite different from those of secessionist conflicts, supporting the conclusions of Buhaug (2006) and Sambanis (2001). Within the context of conflict neighbors, conflicts over governmental control are less likely in the most repressive and most democratic societies, but political institutions do not appear to have a comparable impact on the likelihood of territorial conflict. None of the other right-hand side variables are significantly associated with conflict over government control – including the covariates most influential for secessionist conflict, namely per capita income and country size.[17]

Discussion

We have shown that the spatial clustering of intrastate conflicts cannot be dismissed as a mere product of a clustering in similar country characteristics associated with conflict. This provides evidence that there indeed *is* a neighborhood effect of civil war, where something about armed conflict in one state makes neighboring countries more prone to violence. We then asked to what extent interaction opportunities influence the risk of contagion. Our results here suggest that proximity and exposure play minor roles; neither the distance to the nearest conflict, the weighted density of conflict in the neighborhood, the influx of refugees from a conflict neighbor, nor the severity of the neighboring conflict explains the trajectory of contagion. Rather, we find that transnational ethnic ties seem to be an important catalyst of contagion. Our results also suggest that the demonstrated contextual effect of civil war is largely a feature of secessionist conflict. We interpret this as additional evidence that cultural ties constitute a significant risk factor that contributes to the spread of armed conflict. More generally, the analysis implies that the increase in risk from proximate insurgencies will be the largest among those states that are the most likely to experience civil war in general, as these will have a higher baseline risk at the outset.[18] By contrast, small, democratic, and wealthy states would be less sensitive to disruption from conflict in neighboring states, regardless of their level of exposure to conflicts among neighbors.

Most separatist movements revolve around ethnonational minority groups, and such groups often exist in more than one country. Recent research indicates that civil conflicts are more likely the higher the number of transnational ethnic groups in a country (Gleditsch, 2007) and the larger the politically excluded ethnic minorities (Buhaug et al., 2007). The potential role of such groups in contributing to the spread of violence across international boundaries certainly deserves future attention. However, this will require better data on ethnic groups than what is currently available. Data sources, such as the Minorities at Risk (MAR) data (Gurr 1993), Fearon (2003), and Vanhanen (1999), include counts of the share of a state's population that belong to each particular group, defined on the basis of language, risk, and ethnicity. The MAR database also includes indicators of the extent to which individual groups are at risk of repression or rebellion, the political aim of the group, and whether the group has ethnic kin in other countries, but it does not provide data on where the groups are present, nor does it include groups not considered at risk. In that regard, we welcome ongoing work to develop the old Soviet *Atlas Narodov Mira* into a spatio-temporal database on politically relevant ethnic groups (see Cederman et al. 2006; Buhaug et al. 2007), which will allow tracing the role of transnational ethnic linkages in further detail.

Our ability to answer some questions about the nature of conflict contagion in turn raises new questions regarding how identity ties between states may make conflicts likely to diffuse. Inter-group linkages, rather than geography *per se*, appear to play a significant role here. Ultimately, we believe that new data can help further opening the black box of contagion and allow testing additional specific connections between conflict actors and the risks of conflict among neighbor states. The UCDP/PRIO Armed Conflicts Data have recently been recoded in a dyadic format with information on non-state actors as well as actor relations to other states (see Cunningham et al. 2005). In the longer run, we hope to be able to capitalize on new data reflecting the geographic distribution of ethnic groups that will permit identifying linkages from an opposition group, or territory under contention, to communities in other states.

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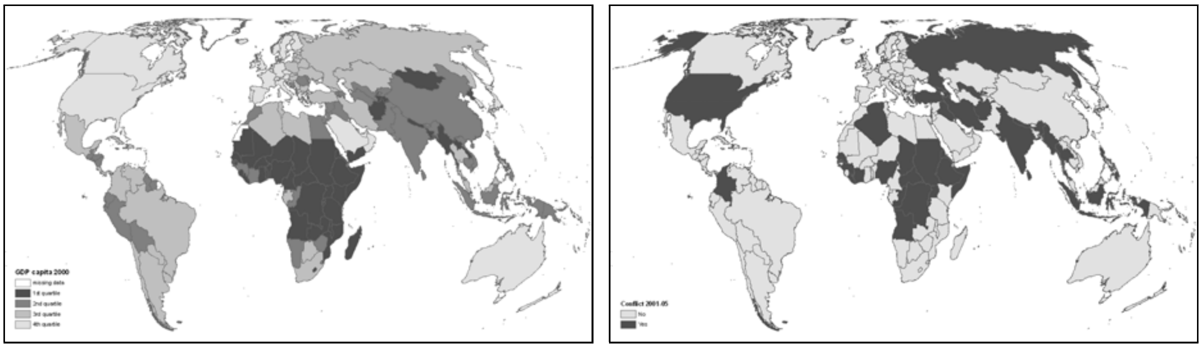
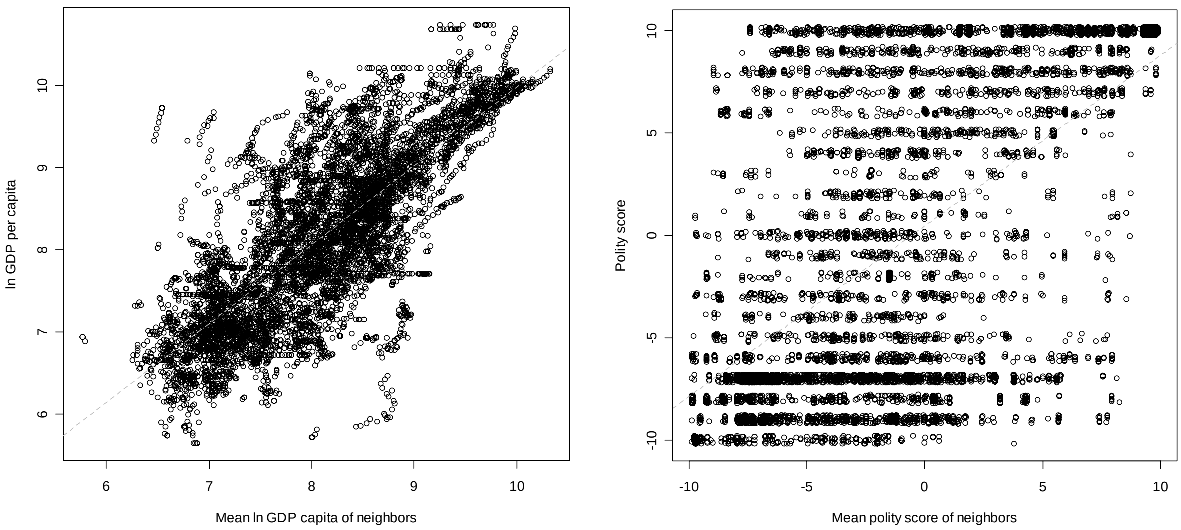
Tables and Figures

Figure 1. Poverty and Intrastate Armed Conflict in the World



Note: GDP per capita data from Gleditsch (2002a), conflict data from the UCDP/PRIO Armed Conflict Dataset (Gleditsch et al. 2002).

Figure 2 Spatial Association of Wealth and Democracy, 1950–2001



Note: The scatter plots display the association between a country's own level and the inverse distance-weighted average level among neighboring countries for economic development (left panel) and extent of democratic institutions. The Polity scores for each country have been jittered by adding a small amount of noise to each observation to avoid completely overlapping points for each of the 21 possible Polity values. The correlation between country *i* and the weighted average for neighboring states *j* are estimated at r=0.77 for GDP per capita and r=0.59 for democracy score.

Table 1. Onset of Intrastate Conflict, 1950–2001

	(1)	(2)	(3)	(4)
Neighborhood conflict incidence (wa)	0.365		0.129	
	(1.51)		(0.46)	
Neighborhood conflict dummy		0.555		0.382
		(3.79)**		(2.53)*
Neighborhood democracy (wa)	-0.012	-0.006	-0.017	-0.014
	(0.62)	(0.30)	(0.75)	(0.61)
Neighborhood democracy sq.'d (wa)	-0.004	-0.003	0.0005	0.001
	(1.21)	(0.93)	(0.15)	(0.39)
Neighborhood GDP per capita (wa)	-0.392	-0.367	-0.052	-0.040
	(3.55)**	(3.32)**	(0.34)	(0.26)
Democracy			-0.0002	0.002
			(0.01)	(0.11)
Democracy squared			-0.008	-0.008
			(2.92)**	(2.98)**
GDP per capita (ln)			-0.285	-0.267
			(2.31)*	(2.13)*
Population size (ln)			0.302	0.283
			(6.34)**	(5.99)**
Post Cold War	0.755	0.680	0.668	0.609
	(5.18)**	(4.68)**	(4.32)**	(3.96)**
Peace years	-0.022	-0.020	-0.015	-0.015
	(3.50)**	(3.32)**	(2.47)*	(2.44)*
Constant	-0.308	-0.710	-3.285	-3.503
	(0.36)	(0.81)	(3.03)**	(3.22)**
Log pseudolikelihood	-906.22	-899.97	-878.68	-875.67
N	6,591	6,591	6,591	6,591

Note: Logit estimates with robust absolute z scores in parenthesis; wa = weighted average, ln = natural logarithm. * p<.05; ** p<.01.

Table 2. Onset of Intrastate Conflict for Conflict Neighbors, 1950–2001

	(5)	(6)	(7)	
			Terr.	Gov't.
Distance to neighboring conflict zone (ln)	-0.025			
	(0.56)			
Neighboring conflict zone at border		0.116		
		(0.44)		
Boundary length to conflict neighbor (ln)	-0.017	-0.015		
	(0.17)	(0.15)		
Ethnic linkages to neighboring conflict	0.509	0.521	0.587	0.652
	(1.93)	(2.00)*	(1.41)	(2.07)*
Refugees from conflict neighbor (ln)	0.011	0.012	-0.071	0.054
	(0.48)	(0.49)	(1.86)	(2.18)*
Population size of conflict neighbor (ln)	-0.062	-0.065		
	(0.77)	(0.81)		
Neighboring conflict over territory	0.643	0.643	1.165	0.141
	(2.96)**	(2.96)**	(3.61)**	(0.56)
Battle-deaths of neighboring conflict,	-0.046	-0.045		
cumulative (ln)	(1.06)	(1.04)		
Democracy	0.013	0.013	0.026	-0.009
	(0.78)	(0.78)	(1.04)	(0.40)
Democracy squared	-0.003	-0.003	0.002	-0.009
	(0.75)	(0.77)	(0.36)	(2.24)*
GDP per capita (ln)	-0.289	-0.291	-0.765	0.043
	(2.33)*	(2.34)*	(3.40)**	(0.31)
Population size (ln)	0.314	0.315	0.545	0.059
	(5.34)**	(5.36)**	(6.28)**	(0.80)
Post Cold War	0.748	0.746	1.609	0.197
	(3.42)**	(3.43)**	(4.76)**	(0.74)
Peace years	-0.015	-0.015	-0.030	-0.004
	(2.08)*	(2.09)*	(1.97)*	(0.52)
Constant	-3.403	-3.517	-5.082	-4.798
	(2.53)*	(2.55)*	(2.36)*	(3.46)**
Log pseudolikelihood	-475.74	-475.80	-537.88	
N	2,632	2,632	2,646	

Note: Logit and multinomial (Model 7) logit estimates with robust absolute z scores in parenthesis; ln = natural logarithm. * p<.05; ** p<.01.

Notes

[1] Although some researchers restrict the term 'war' to severe conflicts, typically requiring at least 1,000 deaths, we here use the terms 'war' and 'conflict' interchangeably. More generally, we do not expect the causes of conflicts to differ fundamentally between larger and smaller conflict.

[2] Anselin and O'Loughlin (1992) find strong evidence of spatial autocorrelation for both international conflict and cooperation using the COPDAB event data, but little evidence of clustering in internal conflict, as reflected in the coup d'état data compiled by Jackman (1978).

[3] Civil wars in one state may also give rise to forms of violent conflict other than civil war outside the boundary of the conflict country, including militarized conflict between states. Gleditsch et al. (2007), for example, find that dyads where one state have a civil war have a significantly higher risk of interstate disputes, and that the positive effect is of similar magnitude as the much better known negative effect of dyadic democracy. In this paper, we limit ourselves to how civil wars may affect the onset of civil war in other states. The closed polity assumption is obviously not common in studies of interstate conflict or transnational terrorism, although we recognize their linkages to civil war as an area worthy of a further research. We refer to Gleditsch et al. (2007), Davis and Moore (1997), and Moore and Davis (1998) for further discussion of linkages from civil war to interstate conflict, and Enders and Sandler (2005) on the motivations for transnational terrorism.

[4] Unlike many studies of diffusion of international war, we distinguish cross-country *contagion* from conflict *escalation* that is due to third-party intervention in ongoing civil wars. See Lake and Rothchild (1998) for a similar distinction. Midlarsky (1978) and others distinguish between diffusion processes, where events are independent but have consequences that in turn make other events more likely, and contagious processes, where events arise as a result of emulation of previous events. In this paper, we treat our concept of contagion of conflict as an observed characteristic, and evaluate hypotheses on specific mechanisms that may underlie the increased risk of conflict and account for variation on this.

Elkins and Simmons (2005) or Most and Starr (1990) provide more general discussions of the concept of diffusion.

[5] Other factors that might exhibit similar non-random spatial patterns include religion, oil wealth, and colonial heritage. We have considered these in our empirical analysis, but do not discuss these in further detail since we found no evidence that these factors were strongly associated with civil war or affected the findings for our key features of interest. We will return to this issue later when we discuss the robustness of the results.

[6] Exceptions here include Ward and Gleditsch (2002), who consider the number of conflicts in neighboring states, and the appendix with additional results for Fearon and Laitin (2003) available at http://www.stanford.edu/group/ethnic/workingpapers/addtabs.pdf.

[7] For a related argument regarding the diffusion of democracy, see O'Loughlin et al. (1998) and Cederman and Gleditsch (2004).

[8] The gravity model is often applied to study the volume or net interaction between units without distinguishing the direction of interaction. A strict interpretation of the analogy to the gravity model in its typical undirected form could be taken to suggest that the prospects for conflict should increase with the size of both parties (i.e., the country of observation and the conflict neighbor). Here we are clearly interested in the direction of contagion influences, and it would seem unreasonable to expect that conflict should be generally more likely to spill over to larger states, although larger states are known to be associated with a higher risk of conflict in general, and the population size of the country of observation is already included a separate country-specific control. We found no evidence of interaction between the two country size measures on the risk of conflict.

[9] This typology, too, involves some problematic cases, in particular where the objective seems to change during the course of conflict, such as in the civil war in Southern Sudan.

[10] We also include a 'peace year' count of the number of years since the end of the previous conflict, or independence to correct for duration dependence (see below).

[11] We also experimented with lagging the spells of neighboring conflict since there may be a time lag to diffusion, but the substantive implications of the estimates from the alternative models did not differ notably from the reported results.

[12] A more troubling possibility is that many right hand side variables may be endogenous to conflict (see Christin and Hug 2005).

[13] We also considered potential non-linear duration dependence using the Beck et al. (1998) approach, but this did not noticeably improve model fit or affect the estimates for the key covariates of interest.

[14] All other covariates in Model 4 held at median values. Marginal effects were calculated using the CLARIFY extension package to Stata (Tomz et al. 2003).

[15] For the estimates of these additional tests, see the web appendix.

[16] We only report a reduced model here, where insignificant factors tested in the earlier models are excluded. A full regression model produces almost identical results.

[17] That said, it is possible that the latent risk of one type of conflict makes the other type more likely. For example, governments weakened by separatist strife may be more likely to be challenged over state authority, and separatist movements may try to seize upon coups and general instability to extract concessions.

[18] To be precise, we have also considered interactive specification between conflicts in neighboring states and country characteristics, but find no evidence of significant differences in country characteristics for states with and without neighboring conflicts. However, the coefficients in a logit model indicate the effects in terms of the log odds of the response, and the implied probabilities will depend on the baseline odds given by the combination of the covariates.