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ABSTRACT

Ethnicity and the spread of civil war*

Civil wars critically hinder a country's development process. This paper shows that civil wars can also have severe international consequences. Anecdotal evidence highlights that civil wars sometimes spill over international boundaries. Using a more rigorous econometric approach we provide evidence that conflict spillovers are indeed quantitatively very important. Also, they are context dependent. Ethnicity in particular plays a key role in the spread of civil war. Only ethnic civil wars spill over, and only along ethnic lines. We do not find evidence that poor, ethnically heterogenous, or less populous countries are more or less susceptible to spillovers. Ethnic links to a neighbor at ethnic civil war increase the probability of an outbreak of ethnic civil war at home by 6 percentage points.

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Keywords: civil war, conflict spillovers, ethnicity

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1 Introduction

Civil war inflicts serious direct and indirect damage to a country's development process. Besides destroying lives, physical infrastructure and living standards, it also has the potential to alter a country's socio-political circumstances and may leave bitter grievances, increasing the likelihood of a future recurrence of violent conflict. Understanding the causes and consequences of civil war is of vital importance. It helps in identifying precautionary measures that can be taken to prevent conflict from breaking out in the first place, or from further escalating once the violence has already started. Blattman and Miguel (2010) forcefully argue that *"civil war ought to be central in the study of international economic development"*.

This paper aims to contribute to the existing empirical studies on the causes of civil war. All recent empirical contributions in the economic development literature find the causes of civil war to be *internal* to a country itself.[1] We take a different view in this paper. Building on insights from the international relations literature, we draw attention to an important determinant of conflict that lies *beyond a country's own borders*. In particular, we study the importance of the idea that conflicts spread beyond the borders of the country where they initiated. From several examples we know already that these, so-called, conflict spillovers matter. The conflict between Hutus and Tutsis for example, spilled over from Rwanda to the Democratic Republic of Congo (D.R.C.) partly due to the influx of refugees from Rwanda to the D.R.C.[2] Whereas the literature acknowledges the importance of conflict spillovers, two key questions have in our view not been answered. These questions are *1.* how important are conflict spillovers? and *2.* in which circumstances are conflict spillovers more (or less) likely to occur? These two questions are central to this research.

Several empirical studies in the political science and international relations literature (Hegre and Sambanis (2006); Sambanis (2001); Hegre, Ellingsen, Gates, and Gleditsch (2001); Fearon and Laitin (2003) or Buhaug and Gleditsch (2008)) already present empirical evidence

[1] Some of the notable economically-related causes of civil war that have been identified are shocks to income (Miguel, Satyanath, and Sergenti (2004); Dube and Vargas (2009)), dependence on natural resource income (Bruckner and Ciccone (forthcoming)), dependence on foreign aid (De Ree and Nillesen (2009)), openness to international trade (Martin, Mayer, and Thoenig (2008)), and the presence of high-value (contestable) natural resources (Angrist and Kugler (2008)). Moreover, a country's political situation (Collier and Rohner (2008)) and ethnic composition (Montalvo and Reynal-Querol (2005)) have been shown to affect its chances on experiencing civil war.

[2] Other examples of conflict spillovers are between Liberia to Sierra Leone, from Croatia to Bosnia to Kosovo, from Afghanistan to Pakistan, or from Sudan to Chad.

regarding the relevance of conflict spillovers. Their empirical approaches typically include a *neighbor at war* variable in a pooled civil war onset model to capture the spillover effect[3]. In addition, they control for various observable country or region specific determinants of civil war onset. The availability of panel data however allows to control for unobservable, albeit time invariant, characteristics as well. To us this is a missed opportunity, and in this paper we extensively discuss why this matters in particular for studying spillovers.

We show that allowing for fixed effects plays a key role in answering our first research question: how important are spillovers? In a fixed effects model we find sizable and statistically significant spillover effects. A country's probability of conflict onset increases by about 3 percentage points when one of its neighbors is at civil war. These effects are notably much higher than the 0.7 percentage points effect we find in pooled models, or the insignificant effects we find in models that allow for fixed continent or subcontinent effects.

To answer our second main research question, we depart from the assumption that all countries are equally susceptible to conflict spillovers. Virtually all existing empirical studies on conflict spillovers make this assumption.[4] It effectively means that having a neighbor at war is equally risky irrespective of, for example, the type of war, the strength of the government army, or the types of linkages to the warring neighbor. A range of earlier, largely narrative contributions in the international relations literature however, strongly contradicts such an a priori supposition [Lake and Rothchild (1998a), Brown (1996)].

Following the insights of this literature we adapt our baseline fixed effects model. In particular, we allow the spillover effect to depend on characteristics of the country at risk, the type of neighboring conflict, and the existence and nature of the transnational ties between a country and its conflict neighbor. Our results identify a key role for ethnicity in the spread of civil war: *only ethnic wars spill over, and only along ethnic lines.* The presence of ethnic links to a neighbor at ethnic civil war increases the probability of an outbreak of ethnic civil war at home by about 6 percentage points. These results are qualitatively and quantitatively important. We do not find evidence that poor, ethnically heterogenous, or less populous

[3]A neighbor at war variable may have different appearances. The literature often codes a dummy variable that is 1 if at least one of the country's neighbors is at war [see e.g. Hegre and Sambanis (2006)]. Another possibility would be to construct a weighted average of the conflict variable in neighboring countries. The weights are then determined on the basis of contiguity or distance.

[4]Two exceptions are Buhaug and Gleditsch (2008) and De Groot (forthcoming). Their respective empirical approaches however are not applicable to fully answer the two central questions of this paper. We return to this issue in section (2.3) and in appendix B.

countries are more or less susceptible to spillovers.

Finally, based on our results, we answer a third question: where are conflict spillovers more likely to occur? We provide a detailed geographical account of which countries are most prone to spillovers. We do this by localizing countries that have suffered, and still suffer, from a deadly mix of spillover intensifying factors: many cross-border ethnic links and many ethnic wars. We find that mainly African countries and, to a lesser extent, Asian countries have these properties. Parts of Europe and Latin America may in principle also be prone to spillovers as there exist ethnic links across international borders. However, because both continents have stayed largely deprived from ethnic civil wars after WWII, actual conflict spillovers are rarely observed (the conflict episodes in former Yugoslavia are of course an exception).

Blattman and Miguel (2010) (p.30) already hinted at the importance of adding an international dimension to the study of civil war. Our findings show that the causes of civil war are indeed not exclusively found within a country's own borders. A neighboring conflict poses a quantitatively important risk to a country's own stability. Besides that, we reveal a second role of ethnicity in the process of conflict ignition. Not only are ethnically divided countries more likely to experience the devastating consequences of civil war in the first place (Montalvo and Reynal-Querol (2005)), once ethnic war breaks out, it potentially draws other (ethnically interlinked) countries into a vicious cycle of ethnic conflict as well. This international dimension of internal conflict stresses the need for additional precautionary measures: besides trying to end the domestic violence in a country at civil war, preventing it from spreading to neighboring states should be high on the political agenda.

2 The spread of conflict: insights from the international relations literature

The onset of civil war is clustered in particular regions of the world. Figure 1 shows that in the second half of the 20th century civil war predominantly broke out in (Sub-Saharan) Africa, Asia, and Central and South America.

This empirical fact has not remained unnoticed. Based on an abundance of anecdotal evidence[5], conflict spillovers are one of the prominent explanations for this pattern posed in

[5]Conflict spilled over in e.g. Bosnia, Jordan, Sierra Leone, Rwanda, Zaire, Uganda, Pakistan, etc.

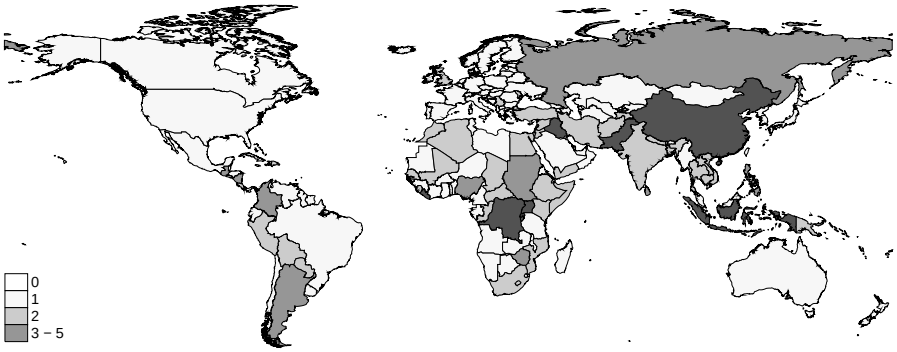

Figure 1: Number of civil war onsets by country, 1945-2000

the international relations literature [see e.g. Hill and Rothchild (1986), Brown (1993), Brown (1996), Lake and Rothchild (1998a)]. In this literature there exists a long history of looking beyond international borders for the causes of a country's own internal political or social circumstances [see e.g. early contributions by Putnam (1967), Midlarsky (1970), Morrison and Stevenson (1972), Li and Thompson (1975) or Collier and Messick (1975), but also more recently Simmons and Elkins (2004), Murdoch and Sandler (2004), or Gleditsch and Ward (2006)].

Conflict in one nation may affect the likelihood of conflict in neighboring countries in various ways[6]. The most obvious type of conflict spillovers occurs when the fighting itself, or its immediate consequences, straddles international boundaries. Examples of such direct spillovers are refugee flows, armed rebel groups seeking refuge and/or wreaking havoc in neighboring countries, alliances between transnational kin groups, irredentist demands that involve territory in two nations, or even active action (be it openly or covertly) of a state to internationalize its own domestic conflict. Conflict abroad can also indirectly ignite conflict at home. Most notably, it can change people's beliefs about the likelihood of conflict at home [see Kuran (1998) for a good overview]. The neighboring conflict may trigger previously dormant grievances between domestic groups, inspire domestic groups to start making more extreme demands (similar to those of one of the fighting factions in its neighboring country),

[6]Our discussion is aimed to set out the main mechanisms behind conflict spillovers that have been stressed in the international relations literature. We do not provide an exhaustive discussion of all possible kinds of conflict spillovers. The literature for example, discriminates "diffusion", "escalation", "demonstration", or "contagion" effects [see Brown (1996) or Lake and Rothchild (1998a) for a much more thorough overview]. All these different types of conflict spillovers however can be modeled/captured by including some kind of neighbor at war variable in an empirical onset model [as we do in section 2.1].

or, as Fearon (1998) puts it, people may simply start to belief that *"if it can happen there, why couldn't it happen here?"* (p.112), which in turn could become a self-fulfilling prophecy.

The many examples of conflict spillovers that are used to illustrate the relevance of (particular kinds of) conflict spillovers, are found throughout the world: in Africa (Rwanda, Burundi, Uganda, and the Democratic Republic of Congo; Sierra Leone and Liberia; Zimbabwe, Mozambique and South Africa; Senegal and Guinea-Bissau; or, more recently Chad and Sudan), Asia (Turkey and Iraq; the Caucasus; or, Afghanistan and Pakistan), as well as in Europe (the Balkans) and Central America (Nicaragua and El Salvador).

Whereas these examples show that civil wars do have effects beyond a country's own borders, *conflict spillovers do happen*, they do not provide a clear-cut indication as to how big the role of conflict spillovers is in igniting domestic conflict. Maybe spillovers contribute only marginally to a country's conflict risk, compared to the domestic explanations for the outbreak of civil war (e.g. a country's own level of economic development, ethnic heterogeneity, political stability, etc). As Lake and Rothchild (1998b) put it (p.27) *"events abroad may appear to cause the outbreak of civil war [...] but conditions at home are the real driving forces."* In other words, the observed clustering in the outbreak of civil war shown in Figure 1 may as well be explained by a similar clustering of the domestic drivers of civil war. Any empirical identification strategy therefore faces the task of disentangling both effects.

2.1 Evidence from recent empirical studies

Recently, started with work by Sambanis (2001), an increasing number of empirical studies have appeared that take explicit note of this. Instead of relying on anecdotal evidence from specific case studies, they specify an empirical model that assesses the relevance of conflict spillovers while also taking account of possible domestic determinants of civil war onset[7]. In particular they specify the following civil war onset model:

$$P\left(c_{it}=1|c_{it-1}=0,X_{it},n_{it-1}\right)=F\left(\gamma+\rho n_{it-1}+X_{it}\beta\right) \tag{1}$$

where c_{it} is a dummy variable indicating whether or not country i is at civil war in year t. The probability of a country experiencing the onset of civil war is allowed to depend on both (observable) domestic conditions X_{it}, and on n_{it-1}, a dummy variable indicating whether one

[7]In a way that was already suggested more than twenty years earlier by Klingman (1980).

of a country's neighbors was in a state of civil war in the previous year (γ is a constant). A significantly positive estimate of ρ is taken as evidence for conflict spillovers, i.e. a neighbor at civil war increases the likelihood of civil war onset at home. F typically denotes the CDF of the logistic or of the standard normal distribution and determines whether logit or probit techniques are used to estimate the parameters of (1).

Although some authors do not find evidence for conflict spillovers (e.g. Hegre, Ellingsen, Gates, and Gleditsch (2001) and Fearon and Laitin (2003)), the majority of existing empirical studies estimating different variants of (1) finds that they are important even when controlling for various domestic or regional factors associated with civil war onset (e.g. Sambanis (2001), Salehyan and Gleditsch (2006), Gleditsch (2007) and Buhaug and Gleditsch (2008)). Indeed, in a recent robustness analysis into the causes of civil war Hegre and Sambanis (2006) report that conflict spillovers are one of the most robust explanations for the observed patterns of civil war onset around the world. This is a comforting result because the abundance of real world examples for spillovers suggests that one should also find evidence for them in an empirical analysis.

Our empirical analysis is in many respects based on these earlier contributions in the international relations literature. However, we adapt the standard empirical strategy in two important ways. First, we take explicit note of unobserved heterogeneity in our empirical onset model. Doing so, we find a much more important role for spillovers than in (standard) pooled models used by earlier studies. Second, we allow for the possibility that conflict spillovers depend on context. In particular, we allow a country's spillover susceptibility to depend on the type of civil war abroad, the nature of its links to its conflict neighbor, as well as on domestic circumstances of the country at risk. We discuss the relevance of these two extensions and their relation to the standard onset model (1) in more detail in the next two subsections.

2.2 Conflict spillovers or the clustering of domestic drivers of civil war

By employing an empirical specification like (1) earlier studies into the role of conflict spillovers explain the observed clustering in the outbreak of civil war by either a similar clustering of the domestic drivers of civil war, or by conflicts spilling over across international boundaries (or a mix of the two). After controlling for several observable domestic determinants, X_{it}, of civil

war onset [see e.g. Sambanis (2001), Hegre, Ellingsen, Gates, and Gleditsch (2001), Fearon and Laitin (2003), or Buhaug and Gleditsch (2008)], any remaining spatial dependence in conflict onset is picked up by the neighbor at war variable n_{it-1} and interpreted as a spillover effect. This identification strategy critically depends on the assumption that the included regressors filter out all spatial dependence in the domestic drivers of civil war.

A priori, however, it is unlikely that one fully captures all crucial causes of conflict ignition by including a limited set of regressors in a pooled model like (1). Ethnic tensions, economic hardship, institutional performance or historical events are all likely causes of civil war. These concepts are often not observed or only incompletely accounted for by the standard regressors in an empirical model. For example, an indicator of ethnic heterogeneity is only a proxy of a broader theoretical idea and easily misses out on the specific nature of the ethnic grievances in a particular society.

If these unobservables exhibit some form of spatial dependence, their existence should be taken seriously. If the unobserved causes of civil war are correlated in space the estimated parameter for ρ in (1) no longer exclusively measures the extent of spillovers. Instead, the neighbor at war variable picks up the joint impact of the unobservables and the spillover effect. Inferences about spillovers on the basis of these estimates could therefore be misleading. We briefly discuss how different spatial patterns in the unobservables could lead to such wrong inference.

Perhaps the most intuitive case is where unobservables are positively correlated in space. This means for example, that countries in the same region have similar climatic, geographic, political or historic features impacting on their likelihood of experiencing civil war. Consequently, countries within those regions have a similar unobserved baseline probability of civil war onset. When this positive spatial dependence in the unobservables is not accounted for in an empirical analysis, it will produce an upward bias on the estimated spillover parameter. Quite easily, part of the variation that is due to these spatially clustered unobservables will be spuriously attributed to conflict spillovers. It would appear that neighbors keep ending up in war because conflict is spilling over between countries, yet it only reflects the fact that all countries suffer from similar underperforming regimes or bad geography for example.

The second case is perhaps less intuitive, but potentially equally relevant. There may be substantial differences between neighboring countries in their baseline probability of civil

war onset that are unobserved (or not modeled). Contrary to the example above, this may lead to an underestimation of spillover effects in pooled models. For example, when a high onset risk country (e.g., Zimbabwe) is bordering a low onset risk country (e.g., Botswana) we will see a lot of conflict in the high risk country and much less conflict (if any) in the low risk country.[8] As a result, we observe a peaceful country bordering a country that is frequently at civil war. A naive explanation for this pattern would be that spillovers are not that important. Indeed, if we do not consider the differences between Zimbabwe and Botswana, how else could we explain the fact that Botswana has never experienced the onset of a civil war since independence? A pooled empirical model, like (1), formalizes this naive explanation and would estimate a spillover effect that is consequently smaller than the true effect. In other words, in case of such negative spatial dependence in the unobservables, pooled models may write off the relevance of conflict spillovers too quickly.[9]

The conclusions regarding the relevance of conflict spillovers may thus be quite sensitive to how one deals with unobserved heterogeneity. We therefore argue that the existing empirical literature on conflict spillovers has not taken full advantage of the opportunities to control for unobserved heterogeneity provided by the time-series-cross-section nature of almost all of the available data sets on civil war (our data set e.g. covers 175 countries over an average period of 40 years[10]). At best, some existing studies include continent or world region dummies to filter out any continent specific heterogeneity [see e.g. Hegre, Ellingsen, Gates, and Gleditsch (2001), Fearon and Laitin (2003) or Hegre and Sambanis (2006)]. Instead of assuming that all countries have the same baseline onset probability, these studies allow countries in different regions of the world (e.g. continents) to differ in this probability. That is, the constant in (1) is replaced by a region specific constant:

$$P\left(c_{it}=1|c_{it-1}=0,X_{it},n_{it-1}\right)=F\left(\gamma^{r}+\rho n_{it-1}+X_{it}\beta\right) \tag{2}$$

where γ^r is the same for all countries located in region r.

[8]Note that the Botswana-Zimbabwe example is suggestive as we do not know a priori how large the unobservables are. Nevertheless many pairs of neighbors seem to exhibit similar differences. Compare Tanzania's stability to Rwanda's instability, Cameroon's stability to Nigeria's instability, or Ecuador's stability vs. Peru's or Colombia's instability.

[9]This potentially important mechanism for bias is not often highlighted in empirical studies into the relevance of conflict spillovers, or spillovers more generally.

[10]The median number of years covered is 41.

In this paper we further generalize this identification strategy and allow each country to have its own baseline probability on conflict onset, i.e. the constant in (1) is replaced by a country-specific intercept:

$$P\left(c_{it}=1|c_{it-1}=0,X_{it},n_{it-1}\right)=F\left(\gamma^{i}+\rho n_{it-1}+X_{it}\beta\right) \tag{3}$$

Whereas in the economics literature estimating a fixed effects model is the rule rather than the exception [see e.g. Miguel, Satyanath, and Sergenti (2004), Bruckner and Ciccone (forthcoming), De Ree and Nillesen (2009)], none of the existing empirical studies on conflict spillovers reports fixed effects results[11]. In fact, the fixed effects model is under continuous and critical debate in the political science and international relations literature, especially for models with binary dependent variables [see e.g. Green, Kim, and Yoon (2001), Beck and Katz (2001) and Oneal and Russett (2001)]. Our reading of this literature is that the fixed effects model is sometimes considered too general to be useful by filtering out all cross-sectional variation in the data. This leaves little variation to identify potentially important correlations. Due to strong cross-sectional correlations of some important political variables (such as e.g. democracy) and other economic or social variables (such as e.g. GDP or population size) that exhibit relatively little variation over time, fixed effects results can easily paint a distorted picture of reality as one may wrongly interpret insignificant parameters as evidence against important hypotheses (i.e. type II errors). Although we do not deny the validity of this critique, we show in section 4.1 that if one's main goal is to study the relevance of conflict spillovers, making type II errors is not an issue. We find that both country dummies as well as conflict spillovers are significant at the same time. This indicates that there is enough (within-)variation left, rendering the discussion about making type II errors irrelevant.

In section 4.1, we show the impact of allowing for unobserved heterogeneity at four, ever more detailed, geographical scales. We find that pooled models [like (1)] and models that use dummies for (sub) continents [like (2)] severely underestimate the importance of conflict spillovers. A fixed effects specification (3) identifies a spillover effect of around 3 percentage point, much stronger than the 0.7, or even 0.2, percentage point spillover effect we find in a pooled model or in models including (sub)continent dummies respectively.[12] Moreover,

[11] Also, in the context of conflict spillovers, two notable variables that are captured by the fixed effects are a country's area and, relatedly, a country's total number of neighbors.

[12] Of course, we are still unable to exclude the possibility that our evidence for spillovers is in effect due to

combining the results of these four different models reveals useful information about the spatial distribution of the unobservables.

2.3 Asymmetry in spillover susceptibility

Our second extension involves the implicit assumption made when estimating (1), (2) or (3), that conflict spillovers are equally relevant to all countries: all else equal, the effect of having a neighbor at war is the same for all countries[13]. Existing empirical studies on conflict spillovers rely on this assumption [see e.g. Sambanis (2001), Hegre and Sambanis (2006), Salehyan and Gleditsch (2006), or Fearon and Laitin (2003)].

We start with providing preliminary evidence against such an a priori supposition on the basis of a simple empirical exercise. For each country we calculate the difference between the empirical onset probability conditional on having a neighbor at war, and the empirical onset probability conditional on having all neighbors at peace, or more formally:

$$\frac{\sum_t I(c_{it}=1|c_{it-1}=0,n_{it-1}=1)}{\sum_t I(c_{it-1}=0,n_{it-1}=1)} - \frac{\sum_t I(c_{it}=1|c_{it-1}=0,n_{it-1}=0)}{\sum_t I(c_{it-1}=0,n_{it-1}=0)} \tag{4}$$

where I denotes the indicator function taking the value 1 when its argument is true and zero otherwise.

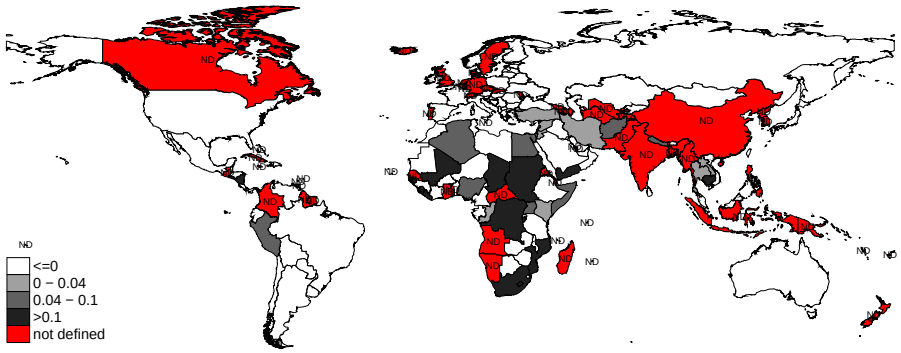

Figure 2: Spillover susceptibility: a first look

Note: Each country's spillover susceptibility is calculated as in (4). Countries in red and labeled "ND" are countries for which this measure is not defined (see footnote 14).

Figure 2 maps this simple indicator of countries' spillover susceptibility. The larger this

any unobserved time-varying spatially clustered domestic causes of civil war onset.

[13] In other words, ρ is the same for all countries.

measure, the more often a country ended up in civil war with one of its neighbors at civil war than with all its neighbors at peace, i.e. the more susceptible to spillovers it appears to be. If conflict spillovers are equally relevant for all countries we would expect to see that all countries score similarly on this measure. Figure 2 indeed suggests otherwise. African or Asian countries appear more prone to spillovers than most countries in Latin America or Europe for example.[14]

Apart from these differences between continents we also observe important differences within continents. In Africa for example, Sudan, the Democratic Republic of Congo, Sierra Leone or Chad score higher than Ethiopia, Kenya, Cote d'Ivoire or Zimbabwe even though all these countries experienced multiple onsets of civil war. Also in Asia and South America we observe differences between countries. Peru, Cambodia, Nicaragua, Turkey or Afghanistan for example score much higher on our indicator than Ecuador, Vietnam, Honduras, Saudi Arabia or Malaysia.

Note however, that figure 2 merely provides suggestive evidence that countries differ in their spillover susceptibility. The analysis does not control for possible confounding factors and does not say anything about statistical significance. More importantly perhaps, even if figure 2 reflects the actual differences in conflict spillover susceptibility, it does not say much about the reasons behind these differences.

The international relations literature does provide valuable insights why conflict from one country does not necessarily have the same likelihood of spilling over to each of its neighbors. As put by Lake and Rothchild (1998a) (p.25) *"The seeds of conflict, while possibly blown in from abroad, germinate and take root only in fertile soil"*. So what makes for this fertile soil? Our reading of the literature is that basically three broad categories can be distinguished that together determine a country's spillover-susceptibility:

1. *Domestic circumstances.*

[14]For a notable group of countries the spillover susceptibility indicator in (4) is not defined when at least one of the denominators of the first and the second term in (4) is zero [in figure 2 these countries are indicated in red and labeled "ND"]. First we have the island nations who do not have any neighbors (New Zealand, The Philippines, Iceland, Cuba, or Madagascar). Second, due to the lack of civil conflict occurrence in Europe or North-America for example, some countries never had a neighbor at civil war (Germany, Sweden, Canada, The Netherlands, or Portugal). Third, some countries always had a neighbor at civil war when they were in peace themselves (China, Pakistan, India, Central African Republic, Namibia, Ghana, South Korea or Djibouti). And finally some countries were themselves involved in civil war throughout our sample period (Angola). For each of these groups one of the two counterfactuals is never observed (i.e. being in peace with a neighbor at civil war, or being in peace without a neighbor at war), making it a priori hard to make any empirical claims about the role of conflict spillovers for these countries.

2. *A country's ties to its conflict neighbor*

3. *The type of neighboring conflict*

Domestic circumstances refer to the idea that some countries are by themselves more vulnerable to neighboring conflict than others [see e.g. Lake and Rothchild (1998b), Kuran (1998) or Smith and Crano (1977)]. Ethnically fractionalized, economically struggling, or militarily weak countries are arguably more easily affected by their conflict neigbor(s) than politically stable and/or economically well-developed nations that can more easily cope with the negative consequences of a neighboring conflict (e.g. by fending off foreign rebel groups or by dealing with (large) groups of refugees).

Second, transnational links are stressed to play an important role in conflict spillovers. A country with direct ties to a conflict neighbor is more likely to be affected than a country without such ties. Although these ties can in principle take many forms, it is the existence of ethnic links between countries that is widely viewed as most important [see e.g. Lake and Rothchild (1998a), Keller (1998), Buhaug and Gleditsch (2008)]. This is exemplified by Brown (1996) (p.595) stating that *"Internal conflicts are most likely to spark conflicts in neighboring states when ethnic groups straddle formal international frontiers: divided ethnic groups are particularly effective conflict transmitters."* Indeed, many examples can be found where an ethnic group (or groups) that is divided between different countries played a significant role in spreading the ethnic violence across international boundaries (Rwanda, Burundi, and the DRC; Sierra Leone and Liberia; Pakistan and Afghanistan; Guinea-Bissau and Senegal, the Balkan, etc.).

Finally, the type of neighboring conflict is argued to matter. Some conflicts are more likely to become transnationalized than others [see e.g. Brown (1996) or Stedman (1996)]. Not surprisingly, ethnic conflict is again the main candidate here (especially in combination with internationally dispersed ethnic groups). But wars over territory, where group(s) fight for independence, have also been argued to more easily spread than wars over government, where groups fight for control of their country [see e.g. Buhaug and Gleditsch (2008)].

Based on these three arguments we allow conflict spillovers to depend on context. In particular, in section 4.2 we adopt (3) by allowing the spillover parameter ρ to depend on *1.* the type of neighboring conflict (i.e. ethnic or not), *2.* on the existence of ethnic ties to the conflict neighbor, and/or *3.* on domestic circumstances of the country at risk of a conflict

spillover (in particular, we allow rich or poor countries, more or less populous countries, and ethnically heterogenous or ethnically homogenous countries to differ in their spillover susceptibility).

Further specifying a country's susceptibility to neighboring conflict in this way is an empirical novelty. It allows us to test whether certain conditions make a country more, or less, vulnerable to neighboring conflict than others. Earlier contributions by Buhaug and Gleditsch (2008) and De Groot (forthcoming) also allow for heterogeneity in the conflict spillover parameter in some specific way.[15] However, and contrary to our approach, the empirical approaches adopted in these two studies do not straightforwardly allow for statistical tests whether context (types of wars, links, or domestic factors) matters for spillovers [we further elaborate on this in appendix B]. Moreover, both studies do not allow for unobserved heterogeneity which we show in this paper to be very important when studying the relevance of conflict spillovers.

Our results in section 4.2 show that spillover effects are indeed context dependent. We find that ethnicity plays a key role: only *ethnic wars spillover along ethnic lines.* A neighboring ethnic civil war significantly increases the probability of ethnic civil war onset at home, but only in the presence of ethnic linkages between the two countries. We do not find evidence that poor, ethnically heterogenous, or populous countries are more, or less vulnerable to outside influences.

3 Data

Before presenting our empirical findings, we first discuss the data that we use throughout the paper.[16] We take the data set from Hegre and Sambanis (2006) as our baseline data set. We also use their definition of civil war, which is the civil war definition of Sambanis (2004b). A civil war is defined as an armed conflict between an internationally recognized state and (mainly) domestic challengers able to mount an organized military opposition to the state. A war must have caused more than 1,000 battle deaths in total and within at least a three-year period. The data covers 175 countries over the period 1945-2000. In total, civil war broke out

[15]Other studies by e.g. Gleditsch (2007) or Salehyan and Gleditsch (2006) look at different international causes of domestic civil conflict than conflict spillovers such as e.g. refugee flows, or trade. They do not allow the likelihood of a conflict spillover itself to depend on these characteristics.

[16]These data were also used to construct Figure 1 and 2.

102 times in 66 countries (22 countries experienced the onset of civil war more than once).

To control for observed country specific attributes related to the onset of civil war, we include the following variables as regressors (X_{it} in (1)) in our onset models: *ln* gdp per capita, *ln* population size, and peace duration (that we capture by three dummy variables indicating whether or not the country has been in peace for at least 2, 3 or 5 years respectively). Hegre and Sambanis (2006) take these as their "core variables" to be included in any study on civil war onset. Furthermore, we include the growth rate of per capita gdp [following Miguel, Satyanath, and Sergenti (2004)], a measure of ethnic heterogeneity [the Vanhanen (1999) index][17], an oil exporter dummy and a measure of democracy based on the Polity IV index.

As our baseline conflict spillover variable (n_{it-1} in (1)), we use a dummy variable indicating whether one or more of a country's neighbors experienced civil conflict in period $t-1$. Our results may therefore be readily compared with e.g. Sambanis (2001), Fearon and Laitin (2003), or Hegre and Sambanis (2006) who use a similar time lagged neighboring conflict variable.

In section 4.2 we further distinguish between ethnic and nonethnic civil wars. In doing so, we rely on the classification of ethnic civil wars provided by Fearon and Laitin (2003) and classify each of the civil wars identified by Hegre and Sambanis (2006) as either ethnic or nonethnic. The conflict data of Hegre and Sambanis (2006) largely overlaps with Fearon and Laitin (2003). A few wars coded in Hegre and Sambanis (2006) were not coded by Fearon and Laitin (2003). We code these civil wars as ethnic or nonethnic wars on the basis of Sambanis' coding notes (Sambanis 2004a). With this information, we construct a dummy variable indicating whether at least one neighbor was at ethnic civil war in period $t-1$, and a dummy variable indicating whether one or more neighbors were at civil war in period $t-1$ with *none* of these civil war classified as ethnic. Furthermore, we construct a dummy variable indicating whether a country itself was involved in ethnic civil war in year t.

In the same section, we also allow conflict spillovers to depend on the presence of ethnic linkages between countries. We determine the existence of such linkages on the basis of the data collected by the Joshua Project (`http://www.joshuaproject.com`), a Christian missionary initiative that seeks to *"develop a list of all ethnic peoples that is as com-*

[17] Other studies have used different ethnicity measures, most notably a country's ethnic polarization (see e.g. Montalvo and Reynal-Querol (2005)). Given that the currently available ethnicity measures are all time-invariant, the specific choice of which one to include is irrelevant in our preferred fixed effects specifications.

plete as possible" in order to identify the *"ethnic groups of the world with the least followers of Christ"*. They provide comprehensive, well-documented data on the ethnic composition of all countries in the world, classifying ethnic groups at three ever-more detailed scales. The finest scale is the individual people group. They identify 9773 different people groups around the world. Various people groups together are grouped in 251 people clusters, and these people clusters are subsequently grouped in 16 affinity blocs [see `http://www.joshuaproject.net/peoples-tree.php` for a complete ethnic peoples tree, and for the exact definitions of the three ethnicity scales they identify]. For example, the *Kikuyu*, the largest people group in Kenya, belong to the *Banta, Gikuyu-Kamba* people cluster (as does e.g. the *Kamba* people group in Kenya, and the *Meru, Rwo* people group in Tanzania). This *Banta, Gikuyu-Kamba* people cluster falls under the *Sub-Saharan African* affinity bloc (as do most of the African people clusters like the *Kru* or *Nilotic* people cluster). A particularly nice feature of this data set is that it classifies people groups, people clusters and affinity blocs on a consistent and transparent basis, without using different names for the same groups, clusters or blocs in different countries. This, in our view, improves upon earlier used data sets (such as the Encyclopedia Brittanica or the CIA World Factbook; used by e.g. Alesina, Easterly, and Matuszeski (forthcoming)), where similar groups sometimes get different names in different countries.

We identify the existence of ethnic linkages between countries on the basis of people clusters rather than affinity blocs or people groups. The latter two classifications provide a too wide or too narrow definition of ethnic groups respectively. Affinity blocs consider e.g. the Armenians and the Anglo-Celts, or the Urdu Muslim and Tamil to belong to the same ethnic group. On the other hand, people groups consider e.g. the Flemish and the Dutch, or the Lango and Luo, that are ethno-linguistically closely related, to be separate ethnic groups. In the former case, we would overstate the degree of ethnic similarity between countries, whereas in the latter we would understate it. Using people clusters takes the middle ground in this.

For similar reasons, we only classify an ethic link between two countries if an ethnic group constitutes at least 5% of the population in both countries. This cutoff identifies ethnic groups that are sufficiently important. Given the extreme detail of the Joshua Project database, not using such a cutoff would yield ethnic links between almost all countries in the world (e.g. Iceland and Malawi would be classified as having an ethnic link given that 0.02% of Malawi's

and 0.34% of Iceland's populations fall in the Germanic people cluster). The 5% cutoff is arbitrary, but the main results in section 4.2 hold up to using a 2% or 10% cutoff instead.

The data on ethnic links is combined with the data on neighbors at war to construct two additional neighbor-at-war variables. A dummy variable indicating whether a country has at least one neighbor at ethnic civil war in period $t-1$ with which it has an ethnic link, and a dummy variable indicating whether a country has at least one neighbor at ethnic civil war in period $t-1$ but *without* an ethnic link to any of these conflict neighbors.

Finally, we augment the data set with three measures that we use to control for unobserved heterogeneity: 6 continent dummies, 21 subcontinent dummies [see Appendix A for their definition], or individual country dummies.

4 Results

This section constitutes the empirical backbone of our paper. In subsection 4.1 we start by establishing the importance of conflict spillovers, taking account of both observed and unobserved heterogeneity. Next, in subsection 4.2 we show when, why, and where conflict spillovers are a more pregnant concern by allowing countries to differ in their spillover susceptibility.

4.1 The magnitude of the spillover effect: the importance of controlling for unobserved heterogeneity

We first estimate a standard pooled civil war onset model (1). This is the type of model that has been estimated by earlier empirical studies into the relevance of conflict spillovers. The results of this model are used as a benchmark to analyze how our conclusions change when we control for unobserved heterogeneity at a geographically ever-more narrow scale. We first allow countries on different continents or subcontinents to differ in their baseline onset probability by including continent or subcontinent dummies [as in (2)]. Finally, we turn to our preferred specification and estimate a country fixed effects model [see (3)].

Table 1 shows the results of doing this. In all our models, F [see (1), (2), or (3)] denotes the logistic CDF, so that we use standard logit maximum likelihood techniques to estimate the parameters of the model. All the results in the paper can be reproduced using probit or linear probability techniques instead.[18] Furthermore, we argue that incidental parameter

[18] When adopting a linear probability fixed effects model (as in e.g. Miguel, Satyanath, and Sergenti (2004))

problems resulting from the inclusion of country dummies in a logit model are limited given the long time span of our data ($T = 40$ on average). Such problems are typically associated with large N small T data sets [see e.g. Heckman (1981)]. Reassuringly, we find very similar results when using a conditional logit specification to control for unobserved heterogeneity at the country level. Given this similarity in results, we opt for including country dummies in a logit model. A drawback of using a conditional logit specification is that it does not allow for the calculation of marginal effects. This would leave us unable to say anything about the magnitude of the spillover effect.

The pooled logit results in column 1 replicate the standard result from the literature. The neighboring conflict variable is significant and positive, yet the magnitude of the effect is arguably not very large: a neighbor at war increases the probability of civil war onset by 0.7 percentage points[19]. Column 2 shows that this result disappears immediately once we include continent dummies: the parameter on the neighboring conflict variable becomes much smaller and turns insignificant. Moreover, the model that includes continent dummies is statistically preferred over the pooled model. This indicates the statistical importance of systematic unobserved differences between continents [see the LR-test in the table[20]].

We obtain a similar result when further narrowing down the geographical scale at which we control for any unobserved heterogeneity to the level of subcontinents. Again the parameter on the neighboring conflict variable is insignificant and even smaller than in column 2. The model including subcontinent dummies is statistically preferred over the model including continent dummies (and thus implicitly also over the pooled model) indicating significant additional heterogeneity within continents [again see the results of a LR-test in the Table[21]].

Finally, column 4 shows that controlling for fixed effects changes the results quite dramatically. The neighbor at war variable reappears as a significantly positive contributor to the probability of civil war onset. The LR-test reveals that this model is preferred over the

we however obtained about 30% predictions outside the 0 - 1 interval.

[19]It is however hard to perfectly compare our results with earlier results from the literature given that this literature does not report marginal effects, see e.g. Sambanis (2001), Hegre and Sambanis (2006), Gleditsch (2007) and Buhaug and Gleditsch (2008)].

[20]The LR-test statistic is $2 \times (-431.6 - -440.7) = 18.2$, which is distributed $\chi^2(5)$ as we loose 5 degrees of freedom by including the continent dummies (4 extra parameters are estimated and 1 because North-America predicts the r.h.s. perfectly).

[21]The LR test statistic is $2 \times (-419.0 - -431.6) = 25.2$, which is distributed $\chi^2(13)$ as we lose 5 additional degrees of freedom by including the subcontinent dummies (14 subcontinent dummies are estimated and 4 subcontinents predict the r.h.s. perfectly, minus the 5 that we had already lost by including continent dummies).

Table 1: Civil war onset regressions – heterogeneity in unobservables

Variables	(1) POOLED	(2) + continent dummies	(3) + subcontinent dummies	(4) FIXED EFFECTS
Neighboring	0.007**	0.004	0.002	0.031**
conflict $t-1$	(2.11)	(1.52)	(0.69)	(2.51)
ln gdp per	-0.008***	-0.005***	-0.005***	-0.022
capita	(-4.57)	(-3.18)	(-2.90)	(-1.44)
ln population	0.002***	0.002***	0.004***	0.023
size	(2.80)	(2.87)	(3.84)	(1.31)
growth	-0.041***	-0.034***	-0.047***	-0.099**
	(-2.89)	(-2.75)	(-2.85)	(-2.02)
ethnic heterogeneity	0.000	-0.000	-0.000	-
	(0.77)	(-0.067)	(-0.43)	-
oil	0.006	0.002	0.001	-0.012
	(1.17)	(0.57)	(0.17)	(-0.49)
polity IV	0.000	0.000	0.000	0.002
	(0.99)	(1.39)	(1.43)	(1.41)
peace years				
≥ 2 years	-0.003	-0.004	-0.005	-0.004
	(-0.48)	(-0.58)	(-0.62)	(-0.18)
≥ 3 years	-0.001	-0.001	-0.000	0.008
	(-0.15)	(-0.17)	(-0.037)	(0.58)
≥ 5 years	-0.010*	-0.008*	-0.010*	0.003
	(-1.91)	(-1.92)	(-1.84)	(0.32)
Observations	5235	5127	4414	1927
F-test peace years	p=0.02	p=0.01	p=0.02	p=0.80
log likelihood	-440.7	-431.6	-419.0	-338.5
LR-test stat. unobs. het.	-	18.2	25.2	161.0
5% critical value	-	11.1=$\chi^2_{crit.}$ (5)	22.4=$\chi^2_{crit.}$ (13)	137.7=$\chi^2_{crit.}$ (112)

Notes. Robust z statistics in parentheses. *** p<0.01, ** p<0.05, * p<0.10. We report marginal effects. The marginal effects are evaluated at the sample mean of the covariates. The dependent variable is a civil war dummy. In the regressions we use only the observations (country-year) that were in peace at $t-1$.

models controlling for unobserved heterogeneity at a higher geographical scale, indicating the presence of significant additional unobserved heterogeneity between countries within subcontinents[22]. Moreover, the individual effects (jointly) and the neighboring conflict variable (individually) are both significantly different from zero *at the same time*. In our case therefore, the concern of making type II errors when using a FE approach (briefly raised in section 2.2) is not an issue[23].

Most importantly, the spillover effect we find in column 4 is much larger than the baseline result of column 1. Where column 1 suggests that having at least one neighbor at war increases a country's chances on the onset of civil war by less than 1 percentage point, in column 4 we find an effect of approximately 3 percentage points instead[24]. Failure to account for unobserved differences between countries in their baseline onset probability [in column 1 - 3] results in a severe *under*estimation of the size of the spillover effect.

Combining the results from column 1 - 4 of table 1 also gives useful ex-post insights in the nature of the unobserved heterogeneity. First of all, continent or subcontinent dummies are too broadly defined and do not adequately account for the heterogeneity in unobserved effects between countries within the same continent or subcontinent. Although the average African or Asian country is more likely to experience the onset of civil war than the average Latin-American or European country for reasons that we do not observe, there also exists important unobserved heterogeneity between countries within (sub)continents.

This finding resonates very well with many real world examples. In Africa relatively stable countries like Guinea, Cameroon, Botswana or Namibia border war-prone countries like Liberia, Chad, Angola or the Democratic Republic of Congo. Also, within Europe the tumultuous former Yugoslavian republics are located next to peaceful countries like Italy,

[22]The LR test statistic is $2 \times (-338.5 - -419.0) = 161.0$, which is distributed $\chi^2(112)$ as we lose 112 additional degrees of freedom by including the country dummies (61 country dummies are estimated and 91 country dummies predict the r.h.s. perfectly, minus 18 from model (2) and (3) minus an additional 22 countries that we already lost by including subcontinent dummies).

[23]We note however that the fixed effects approach does come, perhaps unfortunately, at the expense of making possible type II errors on some of the other included regressors like *ln* gdp per capita and *ln* population size that are not significantly different from zero in the fixed effects results. These results should be interpreted as a by-product of the main purpose of our paper: to establish the relevance of conflict spillovers. Whether these results imply that these other variables are not important for conflict onset, or that we lack sufficient within-country variation to identify their respective impacts, is beyond the scope of this study. Djankov and Reynal-Querol (forthcoming) also report a non-significant effect of *ln* gdp per capita on civil war after including fixed effects even while extending the time dimension of the data to 1825.

[24]As a robustness check on our results we have recalculated the marginal effect of column 1 on the basis of the same mean we use in column 4. This does not affect the results.

Austria or Hungary. And, in Asia civil war-affected countries like Myanmar, Indonesia or India border relatively peaceful nations such as Laos, Malaysia or Bhutan. Our results indicate that the reasons for these differences are not completely captured by the regressors in a pooled conflict onset model, nor in a conflict onset model including (sub)continent dummies.

We conclude that the remaining unobserved heterogeneity within subcontinents is to blame for the underestimation of the spillover effects in column 1 - 3. More specifically, the absence of a significant spillover effect in column 3 and the highly significant and sizable spillover effect in column 4 suggest that within subcontinents, the unobserved country-specific effects are negatively correlated with the neighbor at war dummy. The existence of such negative correlation is best explained by an example. The fact that we find statistically significant heterogeneity across countries within our narrowly defined subcontinents, suggests that relatively[25] high γ_i countries often border relatively low γ_i countries. If we do not fully control for the unobserved differences in the baseline probability of war between, say, peaceful Tanzania and war-ridden Rwanda or Burundi, we would infer from the observation that Tanzania remains in peace during the civil wars in Rwanda and Burundi that spillovers are not that important[26]. A competing explanation for this pattern is however, that the civil wars in Rwanda and Burundi did pose a risk to Tanzania, but that Tanzania remained in peace because, after independence, it has been ruled by a government actively promoting the integration of Tanzania's many different ethnic groups. The latter factor is typically not fully accounted for by the typical regressors in a pooled civil war onset model. Allowing for such unobservable characteristics and spillovers simultaneously, attributes the variation in onsets to the right cause.

One could be misled by the fact that the fixed effects procedure appears to eliminate around 60% of our observations: from 5235 observations in column 1 to 1927 observations in column 4. This, however, does not mean that these observations are endogenously selected out. Instead this is no problem for our purposes and can be easily understood. The observations that drop out mostly concern countries that either never experienced civil war during our sample period or that were at civil war throughout our sample period. As a result, the included fixed effects perfectly predict these countries' time-invariant conflict history. Actu-

[25] "Relatively" here means relative to the average within a subcontinent.

[26] Similar stories can be told for many other neighboring countries where one is war-prone and another peaceful, like Ecuador and Colombia.

ally, this issue directly relates to the example we provided above. From observing Tanzania (or any other country for that matter) in peace over the entire sample period, we cannot infer whether the country has low individual effects or whether spillovers are not that important. Countries that remain in peace over the entire period of observation therefore do not bear any useful variation for the slope parameters in a fixed effects model, including the spillover parameter ρ. A pooled model instead is more restrictive and keeps Tanzania in the sample. But, this comes at the expense of attributing part of its peaceful history to the relative unimportance of spillover effects[27]. The possibility of underestimating spillover effects in pooled, or region effects models is potentially relevant for any empirical enquiry into spillovers effects, be it civil wars, democracy (Gleditsch and Ward (2006)), or specific policies (Simmons and Elkins (2004)).

Overall, based on the results in Table 1, we are now in a position to answer our first research question: how important are conflict spillovers? We find that they are very important. A neighbor at civil war increases the likelihood on civil war onset at home by 3 percentage points. This is a substantial effect considering the unconditional world-wide frequency of civil war onset of around 1.7%.

4.2 Asymmetry in spillover susceptibility: ethnicity matters

Having established the importance of conflict spillovers, we now turn to our second research question. Our results so far assume that all countries are equally susceptible to conflict spillovers. In this section we drop this assumption and allow conflict spillovers to be context dependent.

4.2.1 Ethnicity and the spread of civil war

In table 2 we study the role of ethnicity in conflict spillovers. In column 1 we test whether different types of neighboring conflict, ethnic or nonethnic civil war, imply a different spillover risk, i.e. we estimate:

$$P\left(c_{it}=1|c_{it-1}=0,X_{it},n_{it-1}\right)=F\left(\gamma^{i}+\rho_{1}n_{it-1}^{nonethnic}+\rho_{2}n_{it-1}^{ethnic}+X_{it}\beta\right) \tag{5}$$

[27] Heckman (1981) further elaborates on this issue more generally: *"While this may be intuitively displeasing, because it apparently manufactures a form of small sample selection bias, as T goes to infinity this problem becomes unimportant."*

where $n_{it-1}^{nonethnic}$ is a dummy variable indicating whether one or more neighbors are at civil war at $t-1$, none of which classified as ethnic. And n_{it-1}^{ethnic} is a dummy variable indicating whether at least one neighbor is at ethnic civil war at $t-1$.

We find that neighboring ethnic civil war increases the probability of a civil war onset by 4 percentage points. There is no evidence that nonethnic civil wars carry any spillover risks. However, we can also not reject the hypothesis that ethnic or nonethnic civil war pose the same risk to a neighboring country (i.e. the p-value of a test on the equivalence of ρ_1 and ρ_2 is 0.16).

In column 2 we further specify ρ_2 in (5). We allow the risk on an ethnic conflict spillover to depend on the existence of ethnic ties to the neighboring ethnic conflict [see section 3 for the definition of these ties], i.e. we estimate:

$$P\left(c_{it}=1|c_{it-1}=0, X_{it}, n_{it-1}\right) = F\left(\gamma^i + \rho_1 n_{it-1}^{nonethnic} + \rho_{2^a} n_{it-1}^{ethnic_{-link}} + \rho_{2^b} n_{it-1}^{ethnic_{+link}} + X_{it}\beta\right) \quad (6)$$

where $n_{it-1}^{ethnic_{-link}}$ is a dummy variable indicating whether one or more neighbors are at ethnic civil war at $t-1$, but there exist *no* ethnic ties to any of these neighbors. And $n_{it-1}^{ethnic_{+link}}$ is a dummy variable indicating whether one or more neighbors are at ethnic civil war at $t-1$, and there exist ethnic ties to at least one of those neighbors.

We find that a neighbor at ethnic civil war increases a country's probability of a civil war onset by around 6 percentage points, but only if there exist ethnic linkages between the two countries. We do not find evidence for spillovers if such linkages do not exist. Moreover, we find that neighboring ethnic civil wars with ethnic links carry significantly higher spillover risks than either neighboring ethnic wars without such linkages or neighboring civil wars that are nonethnic in nature. The p-values associated with a test of the equivalence of ρ_{2^a} and ρ_{2^b} and of the equivalence of ρ_1 and ρ_{2^b} are both 0.08. By contrast, the p-value of a test for the equivalence of ρ_1 and ρ_{2^a} is 0.62. Note that the column 2 results also serve as a kind of robustness check to our earlier finding that only ethnic wars tend to spill over. It would be harder to explain if we would have found that ethnic wars only spill over in the absence of ethnic links between the two countries.

The results so far suggest a further generalization of the model. If an ethnic war spills

Table 2: ONSET REGRESSIONS – HETEROGENEITY IN THE CONFLICT SPILLOVER PARAMETER

	LOGIT		MULTINOMIAL LOGIT	
	(1)	(2)	(3a)	(3b)
VARIABLES			nonethnic war onset	ethnic war onset
neighbor at war $t-1$	0.003	0.003	0.001	0.002
(none of them is ethnic)	(0.140)	(0.140)	(0.140)	(0.140)
neighbor at ethnic war $t-1$	0.040***	-	-	-
(at least one is ethnic)	(2.646)	-	-	-
neighbor at ethnic war $t-1$	-	0.017	0.002	0.015
(no ethnic link to ethnic war)	-	(0.989)	(0.353)	(1.155)
neighbor at ethnic war $t-1$	-	0.068***	0.007	0.062***
(with ethnic link to ethnic war)	-	(2.918)	(1.053)	(3.270)
ln gdp per	-0.022	-0.021	-0.008	-0.013
capita	(-1.432)	(-1.415)	(-1.415)	(-1.415)
ln population	0.023	0.018	0.006	0.011
size	(1.294)	(1.032)	(1.032)	(1.032)
growth	-0.101**	-0.094*	-0.035*	-0.057*
	(-2.051)	(-1.924)	(-1.924)	(-1.924)
oil	-0.011	-0.009	-0.003	-0.006
	(-0.483)	(-0.376)	(-0.376)	(-0.376)
polity IV	0.002	0.002	0.001	0.001
	(1.508)	(1.537)	(1.537)	(1.537)
peace years				
≥ 2 years	-0.004	-0.004	-0.001	-0.002
	(-0.202)	(-0.183)	(-0.183)	(-0.183)
≥ 3 years	0.009	0.009	0.003	0.006
	(0.605)	(0.668)	(0.668)	(0.668)
≥ 5 years	0.003	0.004	0.001	0.002
	(0.294)	(0.370)	(0.370)	(0.370)
observations	1927	1927	1927	
F test peace years	$p = 0.80$	$p = 0.74$	$p = 0.74$	
log likelihood	-337.6	-336.0	-396.1	

NOTE. Robust z statistics in parentheses. *** p<0.01, ** p<0.05, * p<0.10. We report marginal effects. The marginal effects are evaluated at the sample mean of the covariates. The dependent variable is a civil war dummy in column 1 and 2. In column 3 - 4 we model the onset of an ethnic and nonethnic war simultaneously. In the regressions we use only the observations (country-year) that were in peace at $t-1$.

over from one country to its neighbor, we would expect this to lead to an ethnic conflict on the neighbor's territory as well. Again, any other finding would be more difficult to explain. We test this hypothesis in a multinomial logit model by allowing for a differential impact of a neighboring ethnic conflict (both with and without ethnic links) on the onset of an ethnic and nonethnic civil war respectively. For parsimony we restrict the parameters on all other variables, including the fixed effects, to be the same for both ethnic and nonethnic civil war onset.[28]

Column 3a and 3b show the results of doing just such a "robustness check". Indeed, the results in column 3a and 3b confirm the key role of ethnicity in the spread of civil war. Compared to having all neighbors in peace, ethnic ties to a neighbor in ethnic civil war increase a country's chances of ethnic civil war onset by 6 percentage points. This effect is about ten times larger than the spillover effect we initially found in a standard pooled spillover model [see column 1 of table 1].

The insignificance of all the other key spillover parameters in the model suggests that if any of the preconditions is not met –no ethnic linkages, no ethnic war at your neighbors, or no ethnic war onset at home– spillovers are not important. However, this finding could also be due to limited variation in any of these other possible "spillover channels". Generally, limited variation leads to large standard errors. For example, whereas the estimated threat posed by a neighboring nonethnic civil war is insignificantly different from zero (as we see in all columns of table 2), it might also be insignificantly different from the threat posed by a neighbor at ethnic civil war. We address this concern by testing separately, whether the statistically significant parameter in column 3b is equal to any of the other five key spillover parameters in column 3a and 3b. We always reject this hypothesis at the 5% significance level. We can thus summarize our findings in the following schematic way:

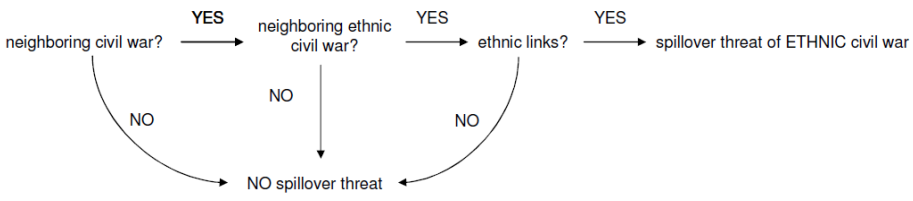

Figure 3: Ethnicity's role in conflict spillovers

[28]Note that the marginal effects differ for all variables in column 3a and 3b, also the ones that we have restricted. This is due to the nonlinear structure of the model.

These results constitute the key finding of our paper: not all civil wars pose a significant spillover risk, nor does a neighboring civil war pose the same spillover risk to each and every one of its neighbors. *Only ethnic wars spillover, and only along ethnic lines.* This sheds an interesting light on the recent finding by Alesina, Easterly, and Matuszeski (forthcoming) that countries with transnational ethnic links to their neighbors generally have lower income levels than less partitioned nations. Our results suggest that cross-border spillovers of ethnic conflict are one of the mechanisms explaining their finding.

4.2.2 Domestic circumstances determining a country's spillover susceptibility

Countries can also differ in their susceptibility to spillovers due to differences in their own domestic circumstances [see e.g. Kuran (1998), Hill and Rothchild (1986) or Smith and Crano (1977)]. Ethnically fractionalized, economically struggling, or militarily weak countries are arguably more easily affected by their warring neighbor(s), than politically stable, economically well-developed nations.

To shed empirical light on these claims, we allow the risk of an ethnic war spillover to depend on three different characteristics of a country at risk. The first factor we consider is GDP per capita. We expect that richer countries are better able to build a strong army to deter emerging conflict [see also Kuran (1998)]. Moreover, they may also be better equipped to properly host refugee flows from neighboring conflict states. A second factor we consider is population size. Smaller countries are arguably more vulnerable to conflict spillovers. For example, the same number of refugees has a bigger impact on the balance of power in a thinly populated country than in a thickly populated country [see Salehyan and Gleditsch (2006)] . Finally, we consider a country's degree of ethnic heterogeneity. On the one hand, ethnically diverse countries may be more easily affected by a neighboring conflict. If groups abroad set the unfortunate example, groups at home, that may even be very different groups, may be more likely to pick up arms as well. Instead, it has also been argued that ethnically fractionalized countries are less prone to spillovers. Hill and Rothchild (1986) for example claim that *"countries that are marked by extreme heterogeneity will be less exposed to a contagion effect, as their groups will be so small and fractionalized relative to the whole that foreign civil strife will be less instructive for their purposes."*.

Table 3 shows the results of again estimating the same multinomial logit model as in

Table 3: ONSET REGRESSIONS – HETEROGENEITY IN THE CONFLICT SPILLOVER PARAMETER

VARIABLES	(1a) nonethnic	(1b) ethnic	(2a) nonethnic	(2b) ethnic	(3a) nonethnic	(3b) ethnic
neighbor at war $t-1$	0.002	0.003	0.001	0.002	0.002	0.003
(none of them is ethnic)	(0.205)	(0.205)	(0.170)	(0.170)	(0.220)	(0.220)
neighbor at ethnic war $t-1$	0.004	0.018	0.003	0.017	0.001	0.012
(no ethnic link to ethnic war)	(0.524)	(1.307)	(0.428)	(1.210)	(0.145)	(0.949)
neighbor at ethnic war $t-1$	0.009	0.091***	-	-	-	-
(ethnic link + LOW GDP)	(1.025)	(3.411)				
neighbor at ethnic war $t-1$	0.004	0.012	-	-	-	-
(ethnic link + HIGH GDP)	(0.312)	(0.487)				
dummy low GDP	-0.011	-0.018	-	-	-	-
	(-1.306)	(-1.306)				
neighbor at ethnic war $t-1$	-	-	0.016	0.047*	-	-
(ethnic link + LOW ETH.HET.)			(1.232)	(1.836)		
neighbor at ethnic war $t-1$	-	-	0.002	0.088***	-	-
(ethnic link + HIGH ETH.HET.)			(0.341)	(3.061)		
dummy low ethnic heter.	-	-	-0.011	-0.018	-	-
			(-0.862)	(-0.862)		
neighbor at ethnic war $t-1$	-	-	-	-	-0.002	0.108**
(ethnic link + SMALL POP.)					(-0.0711)	(2.559)
neighbor at ethnic war $t-1$	-	-	-	-	0.010	0.057**
(ethnic link + LARGE POP.)					(1.194)	(2.476)
dummy small population	-	-	-	-	-0.025*	-0.043*
					(-1.914)	(-1.914)
ln gdp per	-0.009	-0.014	-0.007	-0.012	-0.009	-0.015
capita	(-1.504)	(-1.504)	(-1.367)	(-1.367)	(-1.573)	(-1.573)
ln population	0.009	0.014	0.006	0.011	0.005	0.009
size	(1.251)	(1.251)	(1.022)	(1.022)	(0.854)	(0.854)
growth	-0.037**	-0.060**	-0.034*	-0.058*	-0.033*	-0.056*
	(-2.061)	(-2.061)	(-1.921)	(-1.921)	(-1.918)	(-1.918)
oil	-0.004	-0.006	-0.004	-0.006	-0.003	-0.004
	(-0.465)	(-0.465)	(-0.420)	(-0.420)	(-0.297)	(-0.297)
polity IV	0.001	0.001	0.001	0.001	0.001	0.001
	(1.374)	(1.374)	(1.483)	(1.483)	(1.576)	(1.576)
peace years						
$\geq$ 2 years	-0.002	-0.003	-0.001	-0.002	-0.001	-0.002
	(-0.224)	(-0.224)	(-0.188)	(-0.188)	(-0.168)	(-0.168)
$\geq$ 3 years	0.004	0.006	0.003	0.006	0.003	0.005
	(0.773)	(0.773)	(0.679)	(0.679)	(0.648)	(0.648)
$\geq$ 5 years	0.001	0.002	0.001	0.002	0.002	0.003
	(0.277)	(0.277)	(0.347)	(0.347)	(0.457)	(0.457)
observations	1927		1927		1927	
log likelihood	-394.1		-394.5		-392.7	

NOTE. Robust z statistics in parentheses. *** p<0.01, ** p<0.05, * p<0.10. We report marginal effects. The marginal effects are evaluated at the sample mean of the covariates. In column 3 - 4 we model the onset of an ethnic and nonethnic war simultaneously. In the regressions we use only the observations (country-year) that were in peace at $t-1$.

column 3a - 3b of table 2, where we now allow a country's susceptibility to ethnic civil war spillovers to depend on its level of economic development (columns 1a and 1b), its degree of ethnic heterogeneity (columns 2a and 2b), or its population size (columns 3a and 3b). For each of these three country-specific characteristics we construct two dummy variables that equal 1 if a country at risk scores above or below the world median in a given year respectively [$D_{high-GDP}$ and $D_{low-GDP}$, $D_{high-pop.}$ and $D_{low-pop.}$, and $D_{high-eth.het.}$ and $D_{low-eth.het.}$]. To test whether one of these three domestic circumstances affects the likelihood of a conflict spillover, we interact our neighboring-ethnic-civil-war-with-ethnic-links dummy variable with each of these dummy variables. Using interaction terms with dummy variables identifies differences between well defined groups. Continuous interaction terms are harder to interpret [see Braumoeller (2004) and Brambor, Clark, and Golder (2006)], especially in logit or probit models [see Ai and Norton (2003)].

The results in column 1a, 2a, and 3a all confirm our previous finding that only ethnic civil wars spill over and only along ethnic lines. Moreover, the results in column 2b and 3b show that this key channel for spillovers does not depend on the size of the population, nor on the degree of ethnic heterogeneity of a country at risk. Although the respective sizes of the marginal effects are different (e.g. according to the point estimates a small country is more at risk than a more populous country, and an ethnically heterogenous country is more at risk than an ethnically homogenous nation), these differences are not statistically significant.

The results are different when considering a country's level of economic development. In column 1b we only find evidence for ethnic wars spilling over along ethnic lines in case of low-income countries. There appears to be some truth to the notion that economically more developed nations are better able to cope with their warring neighbors. However, the difference in spillover susceptibility between high and low income countries is not statistically significant (p-value: 0.16). We can not exclude the possibility that high-income countries are subject to the same risk as low-income countries, but that the insignificant parameter estimates are driven by a lack of variation. This finding is consequently much weaker than our earlier findings on the role of ethnicity.

We can now give a clear answer to our second main research question: in which circumstances are conflict spillovers more (or less) likely to occur? We find that spillovers are not always a threat. It strongly depends on the type of neighboring conflict and on the existence

and nature of ties to a neighboring conflict. Our key finding is that only ethnic wars tend to spill over, and only along ethnic lines. It points to a second important role for ethnicity in conflict ignition. Not only are ethnically divided countries more likely to experience the devastating consequences of civil war in the first place (Montalvo and Reynal-Querol (2005)), once ethnic war breaks out, it potentially draws other (ethnically interlinked) countries into a vicious cycle of ethnic conflict as well.

4.3 Localizing spillover susceptibility

Figure 2 of section 2.3 already provided a tentative geographical account of the cross-country heterogeneity in susceptibility to spillovers. Clearly, with the evidence of the previous sections in hand we can provide a more informed geographical account of where spillovers matter most and answer the question: where are conflict spillovers more likely to occur? We do this by on the one hand localizing countries where ethnic conflicts occurred most frequently, and on the other hand localizing countries that are ethnically linked to their neighbors. Such an exercise is useful as it can, for example, serve policy purposes in highlighting regions where the possibility of conflict spillovers is a more pregnant concern than in others.

We start by mapping ethnic civil war incidence by country. Figure 4 shows that ethnic civil wars especially plague African and Asian nations. Apart from the ethnic civil wars in the Balkan and the long conflicts in Northern Ireland and Guatemala, European, North-American, and interestingly (given that civil conflict frequently occurs there) also Latin-American countries stayed largely void of ethnic conflicts. Table 4 summarizes that the average African or Asian country's unconditional probability of experiencing ethnic civil strife is 16% and 13% respectively. These are high numbers compared to the mere 2% and 3% in Latin-American and European countries respectively.[29]

The second risk factor is having ethnic links with neighbors. Figure 5 plots each country's average number of ethnic links to each of its neighbors. This reveals that transnational ethnic links can be found all over the globe. However, African nations stand out again [see also table 4]. On average, African countries have ethnic links to 74% of their neighbors. Moreover, the average African country has much more ethnic links with its neighbors than countries on other continents. African countries have 1.2 ethnic links with their average neighbor,

[29]Excluding the conflict in the U.K. from this statistic would lead to a prevalence rate of around 1% in Europe.

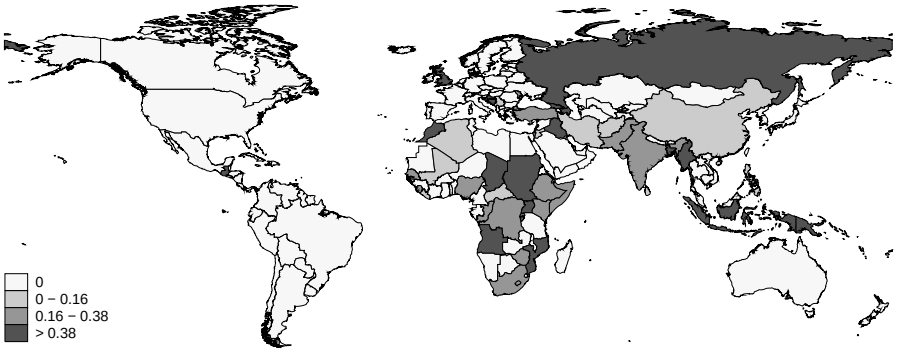

Figure 4: Ethnic civil war incidence, 1945-2000

almost double the number in Asia, Latin America or Europe. This clearly reflects the many "artificial states" in Africa that were created by former colonial powers, without much regard for the existing geographical distribution of ethnic peoples. Not only did this create ethnically diverse nations [see Alesina, Easterly, and Matuszeski (forthcoming)] it also led to a separation of ethnic groups in two (or more) new states [Englebert, Tarango, and Carter (2002) note that 177 African cultural or ethnic groups are partitioned across borders]. Besides that, figure 5 and table 4 also show that many European countries are ethnically interlinked (the average European country has ethnic links to 60% of its neighbors). Especially countries on the Balkan, but also in (North-)Western Europe, the Baltic and Scandinavia are ethnically intertwined.

Apart from these important differences between continents, there are also differences within continents. Within Asia for example, the East and South-East Asian countries are much less ethnically related than countries in Central Asia (partly a result of forced migration during Soviet times) and the Middle-East. Also in Latin-America, the Central American nations are typically ethnically much stronger interlinked than their counterparts in South-America.

Table 4: Criteria for spillovers by continent.

Group type	Africa	Asia	Latin America	Europe
average % of neighbors with at least one ethnic link	74%	45%	48%	60%
mean # ethnic links with neighbors	1.2	0.6	0.6	0.7
ethnic war prevalence	16%	13%	2%	3%

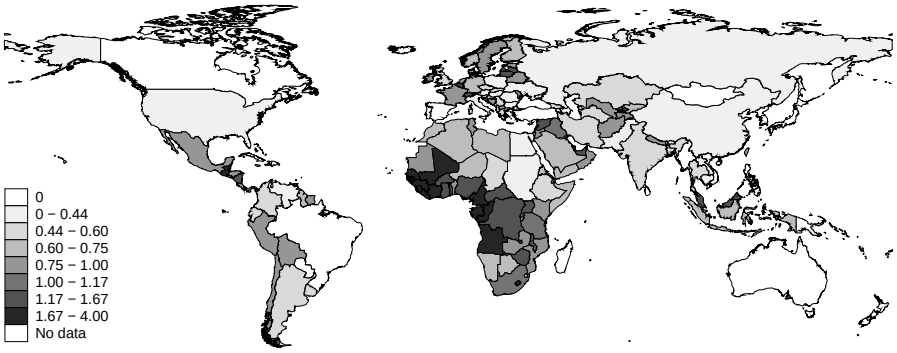

Figure 5: Average number of ethnic links to a neighboring country

Overall, our analysis indicates that spillovers of ethnic conflict could be a potential problem anywhere in the world. However, the problem is much bigger in Africa and Asia than in Europe and Latin-American. Many European and Latin-American countries do have ethnic links with their neighbors, making them in principle vulnerable to ethnic conflict spillovers. However, because ethnic civil wars on both continents are relatively rare these countries largely stay safe. As such, concluding that conflict spillovers are no threat to European or Latin-American countries is somewhat too preliminary. Lake and Rothchild (1998b) elaborate as follows: *"we need not observe conflict actually spreading to conclude that it is indeed a potential problem."*

5 Conclusions

In the aftermath of the 1994 Rwanda genocide, Hutu militias fled Rwanda into the refugee camps in the neighboring Democratic Republic of Congo (D.R.C.) These Rwandese militias eventually teamed up with the Congolese F.A.Z. to fight the Tutsi's in the Eastern parts of the D.R.C. Examples of such international consequences of internal civil wars can be found all over the world. A large body of scholarly work has tried to understand and shed light on the nature of these, so called, conflict spillovers. In this paper we provide empirical answers to two pending questions that, to our judgement, have not been sufficiently addressed before. The first is *how important are conflict spillovers?*, and the second question follows up on this by asking *in what circumstances are conflict spillovers more (or less) likely to occur?*

We use data on all civil wars since WWII and estimate a typical empirical civil war onset model. Conflict spillovers are allowed for by including a neighbor-at-war dummy as one of the

regressors. A statistically significant neighbor-at-war dummy is then interpreted as evidence for conflict spillovers. A key identifying assumption for this approach is that the included controls should filter out the spatial dependence in civil war onset that is due to domestic factors. Otherwise, the neighbor-at-war dummy no longer exclusively picks up the spillover effect. The current empirical studies into the relevance of conflict spillovers take this to heart and control for a plethora of observable characteristics related to civil war onset. The nature of the typical data set on civil war however, covering over 130 countries for on average 40 years, also allows to control for unobserved time invariant characteristics. We argue that this is a missed opportunity to these earlier studies.

We show that allowing for unobserved heterogeneity has serious consequences for the conclusions reached about the importance of conflict spillovers, and thus for answering our first research question. In a fixed effects model, we estimate that a neighbor at war increases the probability of war onset by 3 percentage points. This sharply contrasts the small 0.7 percentage point spillover effect we find in a pooled model or the even smaller and insignificant effects in a model with continent or subcontinent dummies. The fact that a fixed effects model identifies much larger spillover effects than a pooled, or a continent effects model is a surprising, non-obvious result that we extensively discuss in this paper.

To answer our second research question we adapt our baseline specification and allow conflict spillovers to be context dependent. This is an empirical novelty. Based on insights from earlier (largely narrative) contributions from the international relations literature, we allow for a differential impact of spillovers, depending on the type of conflict at home and abroad (ethnic or not ethnic), the existence of transnational ethnic ties between a country and its conflict neighbor, and a number of domestic characteristics (rich versus poor, populous versus sparsely populated, and ethnically heterogenous versus ethnically homogenous countries).

Our results reveal that ethnicity plays a key role in conflict spillovers: only ethnic civil wars spill over, and only along ethnic lines. Ethnic links to a neighbor at ethnic civil war increases a country's probability of an ethnic civil war onset by 6 percentage points. This is a substantial effect given the unconditional world-wide frequency of civil war onset of 1.7%. We do not find evidence that poor, ethnically heterogenous, or populous countries are more, or less vulnerable to outside influences.

Finally, with these results in hand, we provide a detailed geographical account of where

the preconditions for spillovers (ethnic links to neighbors and the incidence of ethnic civil wars) are most prevalent. This reveals that Africa and, to a lesser extent, Asia suffered (and still suffer) in particular from a deadly mix of these two preconditions.

Overall, our findings show that the causes of civil war are not exclusively found within a country's own borders. Ethnic conflict in particular can easily spread across international boundaries. This international dimension of internal conflict stresses the need for possible precautionary measures: besides trying to end the violence in the country at civil war, preventing it from spreading to its neighbors should be high on the political agenda.

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A Continents and subcontinents

We define continents and subcontinents according to the United Nations classification [see `http://www.un.org/depts/dhl/maplib/worldregions.html`].

Continents

Africa, Asia, Europe, Latin America, North America and Oceania.

Subcontinents

Eastern Africa, Middle Africa, Northern Africa, Western Africa, Southern Africa, Eastern Asia, South-central Asia, South-eastern Asia, Western Asia, Eastern Europe, Northern Europe, Southern Europe, Western Europe, Caribbean, Central America, South America, North America, Australia & New Zealand, Melanesia, Micronesia, and Polynesia.

B Limitations of existing alternative modeling approaches

Two earlier papers also allow for heterogeneity in the spillover parameter (Buhaug and Gleditsch (2008) and De Groot (forthcoming)). The empirical approaches in these studies however, do not allow for performing all empirical tests that are of interest in this paper. We are interested in estimating all parameters of (5) and (6) as well as those in the even more flexible multinomial logit specifications for three reasons. First, to be able to calculate (context dependent) marginal effects in order to get an estimate of *the magnitude* of the spillover effect. Second, to test whether spillovers are different depending on differences in context. And third, to test whether spillovers (possibly depending on context) matter at all. We clarify the difference between our empirical approach and the approaches by Buhaug and Gleditsch (2008) and De Groot (forthcoming) respectively, by working out an example.

Suppose one is interested in analyzing whether dummy variable D is a risk factor for conflict spillovers. We abstract from other confounding factors (i.e., other covariates or fixed effects for example) to maintain a good overview of the problem at hand. The example model (7) is essentially the same model as the models we specify and estimate in the main text [e.g. (5) and (6)].

$$P\left(c_{it}\middle|n_{it-1}, D_{it}, c_{it-1}=0\right) = F\left(\gamma + \rho_1 n_{it-1} D_{it} + \rho_2 n_{it-1}\left(1-D_{it}\right) + \beta D_{it}\right) \tag{7}$$

ρ_1 measures the effect of a neighbor at war when $D_{it} = 1$, ρ_2 measures the effect of a neighbor at war when $D_{it} = 0$. β measures the direct effect of D_{it}, and γ is a constant. The parameters of equation (7) may be estimated with standard logit techniques ($F = \Lambda$) on a sample of countries that is restricted on the basis of $c_{it-1} = 0$, the onset sample [this is the standard approach in the literature]. All parameters of (7) are identified such that one can perform a series of tests that are of possible interest. One may test $\rho_1 = 0$ to test whether spillovers matter when $D = 1$. Second, one may test $\rho_2 = 0$ to test whether spillovers matter when $D = 0$. Furthermore, one may test whether $\rho_1 = \rho_2$ to test if spillover effects are different if $D = 0$ or if $D = 1$. To us testing all three hypothesis are of key relevance and are the kind of tests we perform in this paper. Also, because we estimate all parameters of the model, we can calculate the marginal effects that are important to answer the first key question of this paper: how important are spillovers?

Buhaug and Gleditsch (2008) estimate a model that is essentially the same as (7), yet they estimate the parameters of (7) after further restricting the sample of countries on the basis of having a neighbor at war, $n_{it-1} = 1$. This selection creates collinearity issues such that the parameters of (7) are no longer separately identified. Two of the three variables must be omitted to reach identification. As a result, context dependent marginal effects can no longer be calculated and some of the tests of interest (explained above) can no longer be performed.

Because $n_{it-1}(1 - D_{it}) = n_{it-1} - n_{it-1}D_{it}$ we may rewrite (7) without loss of generality:

$$P\left(c_{it}\middle|n_{it-1}, D, c_{it-1} = 0\right) = \Lambda\left(\gamma + \rho_2 n_{it-1} + (\rho_1 - \rho_2)\, n_{it-1}D_{it} + \beta D_{it}\right) \tag{8}$$

Restricting the sample on the basis of $n_{it-1} = 1$ means that ρ_2 cannot be discriminated from the constant γ. Furthermore, $\rho_1 - \rho_2$ cannot be discriminated from β:

$$P\left(c_{it}\middle|n_{it-1}, D, c_{it-1} = 0, n_{it-1} = 1\right) = \Lambda\left((\gamma + \rho_2) + (\rho_1 - \rho_2 + \beta)\, D_{it}\right) \tag{9}$$

Buhaug and Gleditsch (2008) therefore obtain estimates for (linear) combinations of the parameters of interest. As a result, it does not allow for a test of whether ρ_1 and ρ_2 are separately significantly different from zero. In other words, one can not test whether a neighbor at war in any situation (when $D = 1$ OR when $D = 0$) poses a greater risk of conflict than having no neighbor at war at all. The estimated combinations of parameters in

(9) only have clear interpretations if one is willing to make some additional assumptions. It may for example be reasonable to assume that $\beta = 0$. It is perhaps hard to think of cases where having ethnic links to a neighbor has a direct impact on conflict onset.[30] If one is willing to assume $\beta = 0$ one obtains an estimate for $\rho_1 - \rho_2$ from estimating (9), which is an interesting parameter. It allows for testing whether ρ_1 is different from ρ_2, i.e. whether spillovers matter more in the presence of ethnic links than in the absence of ethnic links. However, in many cases the assumption that $\beta = 0$ is uncomfortably restrictive. It is likely that, besides affecting the relevance of conflict spillovers, there are direct effects of for example low levels of economic development, bad institutions or the degree of ethnic heterogeneity. In that case the estimated parameter on D_{it} in (9) does not have a clear interpretation.

Furthermore, because not all of the model parameters in (7) can be separately identified (and estimated) when estimating (9), it becomes impossible to compute context dependent marginal effects.

De Groot (forthcoming) instead, directly models the spillover effect to be context dependent. He constructs a neighbor at war variable that is a weighted average of all neighboring conflicts. The weights are determined on the basis of distance and the ethno-linguistic affinity between countries. De Groot (forthcoming) however, does not simultaneously include a baseline neighbor at war variable, with weights based on distance only for example. He can therefore not formally test whether conflict spillovers matter more for ethno-linguistically related neighbors than for neighbors that do not share such ethno-linguistic ties, i.e. he can not infer whether spillovers are different depending on context or not.

Finally, a more general objection to the approaches of Buhaug and Gleditsch (2008) and De Groot (forthcoming) is that they do not allow for fixed effects. We show in this paper that this is of key importance for estimating the magnitude of the spillover effect.

[30] Because ethnic structure is practically time invariant, any direct effect of ethnic structure (links or otherwise) is captured by the fixed effects in most of our specifications.