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Abstract

Anecdotal accounts of the geographical spread of war inevitably involve Sub-Sahara African countries. But is the conflict spillover effect effectively stronger in Sub-Saharan Africa than elsewhere? To answer this question, we specify a dynamic spatial panel model and estimate it for two separate samples: the group of all Sub-Saharan Africa countries and a group of other emerging and developing economies. It turns out that the conflict spillover is stronger and more persistent over time in the latter. However, when attention is restricted to civil wars, the contemporaneous spillover effect is stronger in Sub-Saharan Africa. One extra year of war in the neighbourhood of a generic Sub-Saharan African country in a given decade results in about three more weeks of war in that country in the same decade. This spillover effect is significantly stronger for civil wars than it is for interstate wars. However, the effect is halved after one decade. We argue that even if not quantitatively large, policymakers should not understate its importance and intervene to prevent possible regional escalation of violence.

JEL Codes: D74, C23, N47

1. Introduction

Armed conflict has a tendency to spread from country to country. Several examples come immediately to mind. In the aftermath of the Rwandan Civil War and subsequent genocide of 1994, militant section of Hutu refugees that had fled to Zaire[1] and from their camps in the eastern part of the country carried out raids against both local and Rwandan Tutsi. These raids eventually triggered the First Congo War (1996), in which several other central African countries were, directly or indirectly, involved. In western Africa, the refugee camps established on the Sierra Leone-Liberia border as a result of the First Liberian Civil War (1989-1996) provided abundant manpower for Sierra Leone's rebel army, the Revolutionary United Front (RUF). The RUF, supported by the National Patriotic Front of Liberia (NPFL) of Charles Taylor, fought a decade long civil war in Sierra Leone (1991-2002) and eventually intervened in support of Taylor during the Second Liberian Civil War (1999-2003). The Liberian conflict progressively evolved into a tri-partite conflict that also involved Guinea in addition to Sierra Leone. In southern Africa, the presence of military installations of the Zimbabwe African National Liberation Army (ZANLA) on Mozambican soil, led the Rhodesian administration to conduct military operations in Mozambique and helped to bring about the creation of the Mozambican National Resistance (RENAMO), which then became one of the two key fighting organisations during the Mozambican Civil War (1975-1992).[2]

It is perhaps not a coincidence that all these examples come from the Sub-Saharan African region. Certainly, instances of geographical spread of conflict can be found globally; consider for instance the wars in the former Yugoslavia, Lebanon, Cambodia and more recently in Syria. Nevertheless, one cannot overlook the fact that armed conflict is a frequent and often persistent event in Sub-Saharan Africa (SSA). According to the UCDP-PRIO armed conflict dataset, a total of 99 countries were involved in some form of war since 1945. Exactly one third of these war-affected countries are located in the SSA region, about 65% of the population in this region has lived in a war-affected country in the post-WWII era, and since 1960 the average SSA country has spent 6.5 years at war. These facts lead to the following question: could the high vulnerability of SSA to conflict also mean that spillover effects in SSA are stronger than elsewhere? This question is clearly important from a policy perspective. For one thing, a large body of evidence indicates that conflict has strong negative economic effects.[3] Therefore, a quantitative assessment of the strength of conflict spillover in SSA versus the rest of the developing/emerging world can help

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[1] We refer to Zaire rather than the Democratic Republic of Congo because the country changed name in 1997; that is, after the events we are describing happened.

[2] See Gerard (2009) on the Congo War, Gberie (2005) on the wars in western Africa, and Vines (2013) on the role of Rhodesia in the Mozambique war.

[3] See, inter alia, Gyimah-Brempong and Corely (2005), Bodea and Elbadawi (2008), Collier and Duponchel, (2013), and Serneels and Verpoorten (2013) for evidence on how conflict harms growth in SSA. See also Blattman and Miguel (2010) and Skaperdas (2011) for a survey of the evidence on the economic costs of armed conflict.

inform the discussion on the causes of SSA development, or lack thereof. For another, understanding regional conflict spillovers is instrumental to the successful operation of multilateral peace and security initiatives sponsored by regional economic communities and the Africa Union.

While conflict spillover is the object of a lively literature (see for instance Ward and Gleditsch, 2002, Gledistsch 2002, Brathwaite, 2005, Salehyan and Gledistch, 2006, Kathman, 2010 and 2011)[4], there is no paper (to the best of our knowledge) that separately estimates this spillover for different regions of the world. However, there are papers that study under which conditions the spillover is likely to be stronger or weaker. Brathwaite (2005) finds that territorial disputes in mountainous and resource-rich countries are more likely to experience substantial geographical spread than other disputes. In a subsequent contribution (Brathwaite, 2010), he also shows that conflict spillover is reduced when domestic state capacity is higher. Buhag and Gleditsch (2008), Bosker and De Ree (2010) and De Groot (2011) report that transnational ethnic linkages are a central mechanism of contagion. Beardsley (2011) provides evidence that peacekeeping reduces the propensity for neighbouring conflict to spur domestic conflict. Drawing on these findings, one might expect the spillover effect to be stronger in SSA than elsewhere, essentially because several of the factors that seem to facilitate conflict diffusion are more abundantly present in SSA than elsewhere. For instance, according to Carmignani and Chowdhury (2012), SSA, more than other region in the world is characterised by a combination of large natural resource endowments and weak institutions (partly resulting from bad disease environment). Also, in SSA more than anywhere else, ethnic groups tend to be split into separate adjacent countries. This high degree of ethnic partition, which is the result of the artificial borders drawn by colonisers, implies that transnational ethnic linkages are particularly strong.[5]

In spite of these considerations, the prediction on the strength of conflict spillover in SSA is somewhat ambiguous. Most countries in SSA are at high risk of war independently from what happens in their neighbourhood. War is therefore more likely to occur (and continue) because of "internal" factors rather than as a consequence of a true spillover effect. Conceptually, this argument is akin to the point made by Sambanis (2001), Hegre and Sambanis (2006), and Gleditsch (2007), who suggest that conflicts tend to cluster geographically because the determinants of conflict are clustered geographically. In econometric terms, this would imply that after controlling for domestic determinants of conflict (and persistence of conflict over time), the spillover effect might actually be weaker in SSA than elsewhere.

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[4] Most of the papers provide evidence that conflict in a neighbouring state increases the risk of conflict in the domestic state. Two notable exceptions are Hegre *et al*. (2001) and Fearon and Laitin (2003).

[5] Alesina *et al*. (2006) provide data by country on the extent to which ethnic groups are cut by a political border line. The average "partition" for SSA is significantly higher than the average for the non SSA countries. Englebert *et al*. (2002) and Michalopoulos and Papaioannou (2013) provide evidence of the detrimental economic effects of artificial borders and the association partition/fragmentation of ethnic groups in Africa.

Ultimately, the matter has yet to be settled empirically, and this paper contributes to the literature by attempting such a study.

The logic underlying our exercise is rather straightforward. First, we identify a suitable empirical model of conflict that allows for spatial spillovers. Then, we estimate this model separately for SSA and non-SSA developing and emerging countries and compare the sign/size of the spatial effect across the two samples. Our results are as follows. First, there is evidence of a significant conflict spillover effect even after controlling for internal determinants of war and persistence of war over time. Second, compared to the rest of the developing and emerging world (RoDW), the spillover in SSA is on average weaker, but becomes stronger instead when the analysis is restricted to civil wars. Third, the spillover effect is significantly more persistent in RoDW than in SSA, independently from the type of war considered. Results are generally robust to different assumptions concerning the specification of the set of control variables in the estimating equation.

The rest of the paper is organised as follows. Section 2 introduces the econometric model and addressed a number of methodological issues. Section 3 presents the results. Section 4 discusses the policy implications of our findings and sets directions of future research. The Appendix reports some additional econometric results and a detailed description of the variables used in the estimation.

2. Methodology

2.1 Empirical measures of war

Given a period of observation of T months, the extent to which a country is afflicted by war can be measured by (i) the number of war episodes occurred in that period, (ii) the total amount of time that the country has spent at war during that period, and (iii) the total number of war-related casualties suffered in that period. Unfortunately, option (iii) suffers from incomplete data on casualties and characterised by large measurement errors, so that in the end the choice is between options (i) and (ii). While the two measures are likely to be highly correlated, we prefer option (ii) essentially because with option (i) we would be looking at the spillover effect exclusively in terms of war onset and thus miss the impact that war in the neighbourhood could have on the duration of an existing conflict. In other words, option (ii) is more likely to provide a comprehensive representation of the “commonness” of war in a given country over a given period of time. Our purpose will be then to estimate how war commonness in country i is affected by war commonness in the neighbours of country i.

To operationalise these ideas, let t be a generic period spanning over T months. If $m_{i,t}$ is the number of months that country i spent at war during period t, then war commonness for country i is defined as $y_{i,t} = m_{i,t}/T$.[6] To define war commonness in the neighbourhood of i we need first to describe the spatial arrangement of the countries in the sample. Let j denote a generic country other than i and assume that there are N countries in the sample. We define a matrix $\boldsymbol{W}$ whose generic element w_{ij} is some measure of geographical proximity between country i and country j. War commonness in the neighbourhood of i is then written as $\bar{y}_{i,t} = \sum_{j=1}^{N} w_{ij,t} y_{j,t}$, where $y_{j,t} = m_{j,t}/T$ is war commonness in generic country j. In words, war commonness in the neighbourhood of country i is a weighted average of war commonness in the countries that are geographically "close" to country i.

To operationalise $y_{i,t}$ and $\bar{y}_{i,t}$ we use information from the UDCP/PRIO Conflict Database (see Gledistch *et al*., 2002 and subsequent updates). This database reports the start and end date of every conflict since 1946. However, our sample period is restricted to 1960-2009, as other economic and political variables needed to estimate the model are generally not widely available earlier than the 1960s. T is set equal to 120 months (that is, 10 years) so that the data for estimation are stacked over five non-overlapping periods: 1960-69, 1970-79, 1980-89, 1990-99 and 2000-09.[7] This panel set-up is particularly convenient as it allows us to model war as a dynamic process and to use lagged values of potentially endogenous regressors as instruments (see explanation below).

The matrix element $w_{\mathrm{ij,t}}$ is set equal to the length of the land border between country i and country j as a proportion of the total land border of country i. The subscript t is necessary because borders do change over time and hence the elements of the weighting matrix W need to be constantly updated. A possible alternative operationalisation of $w_{\mathrm{ij,t}}$ would include maritime borders together with land borders. Our preference for considering only land borders arises from two considerations. First, spillover effects are likely to originate from large transnational movements of people (*i.e.* refugees, transnational rebels and combatants), which in turn are more likely to occur via land. Second, if we were to consider maritime border, the neighbourhood of countries with overseas territories would in some cases be unreasonably wide. For instance, the neighbourhood of the United States would include countries like Japan, Samoa, New Zealand, Tonga, and the Netherlands. Note that excluding maritime borders from the definition of matrix W does not mean that

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[6] Some countries are involved in more than one war at the time, so $y_{\mathrm{i,t}}$ might be greater than one. Moreover, in our application, the dependent variable will be first-differenced, so that for the purpose of estimation it will take both positive and negative values.

[7] By collapsing the time dimension of the panel in ten-year periods we implicitly assume that the effect of a year in country *j* on war commonness in country *i* does not depend on when exactly in decade *t* war in country *j* happens. If we were trying to estimate a detailed time profile of contagion or if we relied exclusively on the chronological sequence of wars to identify the causality effect of war in country *j* to war in country *i*, then this assumption would be rather strong. However, our objective is to estimate the *average* effect of war commonness in country *j* on war commonness in country *i* and our identification procedure exploits a set of moment restrictions to estimate the causal effect of regressors. This means that for the purpose of our estimation, the implicit assumption arising from stacking the time dimensions over decades is unlikely to bias our results.

island states are excluded from the sample. It does mean, however, that for island states the war commonness in the neighbourhood, $\bar{y}_{i,t}$, is equal to zero.

2.2 Econometric specification

Our estimating equation is:

$$(1)\quad y_{i,t} = \rho y_{i,t-1} + \delta \bar{y}_{i,t-k} + \boldsymbol{x}_{i,t}\boldsymbol{\beta} + \mu_i + \varepsilon_{i,t}$$

Where $\boldsymbol{x}$ is a row vector of observation on time-varying control variables, μ is a country fixed effect, ε is an i.i.d. error term for i and t with zero mean and finite variance, k is a non-negative integer, and the other variables are as in Section 2.1. The presence of k allows for war in the neighbourhood to affect war in country i with a lag. In Section 3 we will present results for k = 0 (contemporaneous effect) and k = 1 (one decade lagged effect). Our interest is on the estimation of the spatial spillover effect δ, the coefficient of the lagged dependent variable ρ, and the vector of slope coefficients $\boldsymbol{\beta}$.

The vector of controls $\boldsymbol{x}$ includes time-varying factors that determine war commonness in addition to the lagged and spatial effects respectively captured by $y_{i,t-1}$ and $\bar{y}_{i,t}$. Two such factors that have been emphasised by previous research on war onset (and duration) are country i's stage of economic development and level of democracy. To proxy for development, we use the log of real per-capita GDP (sourced from the Penn World Tables). As a measure of democracy, we choose the index of polity quality (sourced from the Polity IV Project Database). This index takes values from -10 (perfect autocracy) to +10 (perfect democracy). Both per-capita GDP and polity are measured at the beginning of each decade. We separately consider two specifications: one that allows for just a linear effect of these two control variables and the other that instead allows their effect to be quadratic. It turns out that results concerning the spillover effect are generally robust across the two specifications.

Other commonly used determinants of war include mainly time-invariant factors, such as the geo-morphological characteristics of the terrain, the geographical location of the country, colonial history, and natural resource endowments. Factors like ethnic, linguistic, and religious fragmentation are also normally measured by variables that are constant over time. Therefore, all of these other determinants of war are captured by the country-fixed effect μ_i in our model. Finally, we run some sensitivity tests on the model specification by adding time-variant demographic variables (*i.e.* the log of total population, the log of population by different age groups, the proportion of female population on total population) and we find that (i) their estimated coefficient are generally not different from zero and (ii) the results for the spillover effect are substantially unchanged.

2.3 Estimation

Equation (1) is a dynamic spatial panel data model whose estimation involves a number of complications: (i) the lagged dependent variable $y_{i,t-1}$ by construction is correlated with the error term; (ii) the spatial variable $\bar{y}_{i,t}$ is potentially endogenous, in the sense that war in country *i* can determine war in country *j* and/or war in countries *i* and *j* can be determined by some unobserved common factor; and (iii) the time-variant regressors in $\boldsymbol{x}$ are also potentially endogenous to the dependent variable. The Maximum Likelihood (ML), Quasi Maximum Likelihood (QML), and biased-corrected Least Squares Dummy Variables (LSDV) estimators proposed by Elhorst (2005), Yu *et al*. (2008), and Lee and Yu (2010) do not allow for endogenous $\boldsymbol{x}$ and hence they are not suitable in our case. Korniotis (2010) combines the LSDV approach with the instrumental variable estimator developed by Anderson and Hsiao (1982) for dynamic panels where $\delta = 0$. The underlying intuition is to instrument endogenous regressors dated at time *t* with any variable dated at time $t - \tau,\ \tau > 0$. While this "hybrid" estimator does not account for the endogenous spatial effect, the idea that lagged values might be efficiently used to estimate the parameters of a dynamic spatial panel model is theoretically attractive.

To illustrate the estimation approach, let's first re-write equation (1) in a more compact form:

$$(2)\quad \boldsymbol{y}_t = \rho \boldsymbol{y}_{t-1} + \delta \bar{\boldsymbol{y}}_{i,t-k} + \boldsymbol{X}_t \boldsymbol{\beta} + \boldsymbol{\mu} \quad + \boldsymbol{\varepsilon}_t$$

where $\boldsymbol{y}$, $\boldsymbol{\mu}$, $\boldsymbol{\varepsilon}$ are $N \times 1$ vectors of observation on war commonness, country fixed effects, and error terms respectively, $\bar{\boldsymbol{y}} = \boldsymbol{W}\boldsymbol{y}$ and $\boldsymbol{W}$ is the $N \times N$ weighting matrix introduced in section 2.1 to describe the spatial arrangement of countries in the sample, and $\boldsymbol{X}$ is a $N \times 2$ matrix on observations on per-capita income and democracy. The country fixed effect can then be removed by taking the first differences of (2):

$$(3)\quad \Delta \boldsymbol{y}_t = \rho \Delta \boldsymbol{y}_{t-1} + \delta \Delta \bar{\boldsymbol{y}}_{i,t-k} + \Delta \boldsymbol{X}_t \boldsymbol{\beta} + \Delta \boldsymbol{\varepsilon}_t$$

OLS estimates of (3) are certainly inconsistent because $\Delta \boldsymbol{y}_{t-1}$ is correlated with $\Delta \boldsymbol{\varepsilon}_t$. For $\delta = 0$, Anderson and Hsiao (1982) derive a consistent instrumental variable estimator using $\boldsymbol{y}_{t-2}$ and $\boldsymbol{X}_{t-1}$ as instruments. Arellano and Bond (1991) extend the approach and design a GMM procedure (known as "difference-GMM") that efficiently uses additional lags of the endogenous regressors. However, lagged levels are likely to be poor instrument for first differences if the variables are closed to a random walk. In this case, Arellano and Bover (1995) and Blundell and Bond (1998) suggest a further extension to incorporate additional moment conditions in the GMM procedure. This is achieved by jointly estimating equations (2) and (3) as a system and using first-differences of the endogenous regressor as instrument in the level equation (2) (their estimator is therefore called "system-GMM").

Elhorst (2010) investigates the properties of the difference-GMM estimator for the general case of a spatial dynamic panel model (*i.e.* when δ is no longer restricted to be equal to 0) and finds that it can be severely biased with respect to the estimates of δ. However, as shown by the Monte Carlo simulations of Kukenova and Monteiro (2009), this bias is significantly reduced when the system-GMM version of the estimator is used. Cizek *et al*. (2011) modify the system-GMM to account for potential spatial error correlation; that is, for a version of equation (1) where error term is specified as $\varepsilon_{i,t} = \lambda \sum_{j=1}^{N} w_{ij} \varepsilon_{j,t} + \omega_{i,t}$, and $\omega_{i,t}$ has zero mean and finite variance.

In light of the above discussion, our preferred choice of estimator is the system-GMM. The consistency of this estimation approach relies on three critical assumptions: (i) the error in levels (*i.e.* the $\varepsilon_{i,t}$ in equation (1)) are serially uncorrelated, (ii) lagged levels of endogenous variables are valid instruments for first differences, and (iii) lagged differences are valid instruments for variables in levels, which in turn requires first differences to be uncorrelated with the unobserved country-specific effect. If errors in level were auto-correlated, then some lags might not be valid instruments. Similarly, if lagged levels were not exogenous, then the GMM procedure delivers biased estimates. Finally, if lagged differences were correlated with the unobserved country effects, then system-GMM should not be used and the model should be estimated only by difference-GMM. In order to assess whether these assumptions hold, we perform three key specification tests. One is a test for zero auto-correlation in first-differenced errors. The null hypothesis of this test is that there is no auto-correlation at order k, $k > 0$. By construction, the null must be rejected for $k = 1$. However, if errors in levels are effectively serially uncorrelated, then the null must be rejected at higher orders. The second test is a standard Hansen test of over-identifying restrictions. Under the null hypothesis, the over-identifying restrictions implied by the choice of instruments are valid. Non-rejection of the null therefore indicates that the instruments as a group appear to be exogenous. The last test is a difference-in-Hansen test for the equation in levels. This is a test of validity of the instruments in the level equation and it is therefore indicative of whether system-GMM should be preferred to difference-GMM.

A final estimation issue concerns the risk of instrument proliferation in system-GMM. Roodman (2009) notes that as the time series dimension of the panel increases above $T = 3$, system-GMM generates a large number of instruments, with the consequence that these numerous instruments fail to expunge the endogenous component of instrumented variables and hence bias coefficient estimates towards those from OLS estimators. A typical symptom of instrument proliferation is a very high p-value (*e.g.* p-value = 1) for the Hansen test of overidentifying restrictions. If this occurs, then Roodman suggests using only certain lags (instead of all available lags) for instruments and/or to collapse instruments by combining them through addition into smaller sets. In the case of our application, the maximum value of T is 5. However, since our equation includes a one period lagged dependent variable, estimation is done on a time dimension of $T = 4$ and in some cases $T = 3$ because not all the variables are available in all decades for all countries. In these circumstances, the total number of instruments generated by system-GMM is 23 and hence "small" relative to the cross sectional dimension of the panel ($N = 145$). Furthermore, as shown in the next section, the p-value of the Hansen test is never remotely close to 1, thus suggesting that instrument proliferation is effectively not a problem. Heading 4

3. Results

3.1 Baseline estimates

We start by estimating our equation on the full sample of all countries. The usefulness of this exercise is twofold. First, it allows us to check the statistical performance of the model. Second, results provide a useful benchmark for our subsequent analysis of SSA. The estimated coefficients are shown in Table 1. The equation in Column 1 includes a contemporaneous effect of war in the neighbourhood while the equation in Column 2 allows for a one-period lagged effect. The evidence suggests that the spillover effect is positive and significant. Depending on the length of the border between country *i* and country *j*, one extra year of war in country *j* causes increases war commonness in country *i* by something between one and five weeks. This effect is however halved after a decade. Of the other regressors, only the lagged dependent variable has a significant coefficient. Taken at face value, the point estimate of ρ implies that one extra year of war in decade *t*-1 translates into about six more months of war in decade *t*. Polity and per-capita GDP instead appear to have no significant linear effect on war commonness. Finally, all diagnostic tests support our estimation approach. The hypothesis of second order auto-correlation of first differenced errors in equation (3) is rejected, while the null hypothesis that the over-identifying restrictions are valid cannot be rejected at usual confidence levels. The difference in Hansen test also suggests that the instruments used for the level equation (2) are valid and hence that the system-GMM estimator is more efficient than the difference-GMM estimator.

The risk that war might spill over across the border should induce the domestic country to take appropriate precautionary measures once a conflict erupts in a neighbouring nation. This response of the domestic country should take into account the type of conflict that war in the neighbourhood is likely to generate. It is therefore interesting to disaggregate the dependent variable of the econometric model by type of war. The UDCP/PRIO Conflict Database identifies two main types: civil war and interstate war. Accordingly, in Columns III and IV, $y_{i,t}$ is equal to the number of months of civil war in country *i* in decade t divided by 120. Similarly, in columns V and VI, $y_{i,t}$ is equal to the number of months during which country *i* was involved in an interstate war in decade *t*. The central finding concerning the spillover effect is qualitatively unchanged: both types of war become more common in country *i* when war in the neighbourhood increases. Quantitatively, the contemporaneous spillover effect is stronger on interstate war, but the one-period lagged effect is stronger on civil war. This means that when country *j* is at war, the "immediate" risk for country *i* is mainly the involvement in an interstate war (possibly because of alliances with/against country *j*) rather than a domestic civil war. However, in the longer term, civil war becomes progressively more likely than interstate war. Finally, the disaggregation between the two types of war might help explain why per-capita GDP appeared to have no significant impact in the regressions of columns I and II. As can be seen, a more advanced stage of economic development makes civil war less common and interstate war more common. Hence, it is possible that when aggregating the two types of war in a single indicator of war commonness, the positive and negative effect of per-capita GDP cancel out, thus yielding a non-significant coefficient. The role of democracy, instead, remains rather unclear, as its estimated coefficient is basically insignificant bar one specification.

Table 1: Spatial model of war with linear specification

	I War: all types	II War: all types	III War: only civil war	IV War: only civil war	V War: only interstate	VI War: only interstate
Lagged war	.5528***	.5770***	.5846***	.6087***	.2402***	.2619***
Polity	.0004	-.0003	.0003	.0007	.0004***	.0001
GDP pc (log)	-.0060	-.0072	-.0127**	-.0165**	.0087***	.0063***
War in neighbourhood	.0859***	..	.0377**	..	.0581***	..
Lagged war in neigh.	..	.0489**	…	.0463**	..	.0100***
Diagnostics						
AR(1)	-3.17	-3.16	-3.97	-4.09	-2.17	-2.12
(p value)	(0.002)	(0.002)	(0.000)	(0.000)	(0.030)	(0.034)
AR(2)	0.79	0.72	0.04	-0.02	1.61	1.49
(p value)	(0.431)	(0.469)	(0.964)	(0.984)	(0.107)	(0.137)
Hansen	45.30	46.00	46.69	44.95	63.82	59.03
(p value)	(0.418)	(0.238)	(0.362)	(0.272)	(0.127)	(0.183)
Diff in Hansen	28.73	29.85	27.45	25.13	30.52	22.72
(p value)	(0.479)	(0.274)	(0.548)	(0.512)	(0.388)	(0.649)
N. Observations	505	505	505	505	505	505
N. instruments	49	45	49	45	49	45

Notes: The diagnostics are as follows. AR(1) is the Arellano-Bond test for first order auto-correlation in first differenced errors. AR(2) is the Arellano-Bond test for second order auto-correlation in first differenced errors. Hansen is the test of over-identification restrictions. Difference in Hansen is the tests of exogeneity of instrument subsets. ***, **, * denote statistical significance at 1%, 5%, and 10% confidence level respectively.

To some extent, the problem with interpreting the impact of democracy and per-capita GDP on war commonness may relate to the use of a linear specification. In fact, it can be argued that both variables affect war non-linearly. For instance, democratisation could initially make a country more vulnerable to war and subsequently bring peace once institutions have consolidated. Similarly, economic development increases the opportunity cost of war, but also raises the reward from fighting (and winning) a conflict. It is possible that at initially early stages of development, the opportunity cost falls short of the expected reward, thus implying a positive effect of per-capita income on war. But as per-capita income grows, the opportunity cost rises and the relationship might turn from positive to negative. The linear specification in Table 1 is not suited to pick this type of non-linear effects. If the underlying effect of democracy and/or per-capita GDP on war commonness were non-linear, then the best fit provided by a linear specification is likely to be a horizontal line and the estimated slope coefficients turn out to be insignificant. While democracy and per-capita income are not our primary interest, we need to ensure that the results on the spillover effect are not affected by the way in which these two variables enter the regression model.

Table 2 reproduces the same regressions presented in Table 1, but with the addition of the squared values of democracy and per-capita GDP to the set of regressor. The sign of the estimated coefficients indicate that the relationship between these two variables and war commonness might be an inverted U-shaped; that is, democracy and per-capita GDP initially increase and then decrease war commonness. However, these estimated coefficients do not always pass a zero restriction test. More importantly, the evidence on the spillover effect is qualitatively unaffected by the change in the specification of the vector of regressors. If anything, the marginal effect of war in the neighbourhood seems to be now quantitatively larger. All the statistical tests are also satisfactory. Therefore, for the purpose of our analysis of spillover effects, choosing between a linear and a quadratic form for democracy and polity is not crucial.

Table 2: Spatial model of war with non-linear specification

	I War: all types	II War: all types	III War: only civil war	IV War: only civil war	V War: only interstate	VI War: only interstate
Lagged war	.5110***	.5175***	.5544***	.5496	.2286***	.2519***
Polity	.0004	.0004	-.0001	.0003	.0005***	.0003***
Polity squared	-.0011***	-.0012***	-.0008***	-.0008***	-.0001***	-.0001***
GDP pc (log)	.1370**	.0923	.0284	.0301	.0551***	.0474***
GDP pc squared (log)	-.0080**	-.0056	-.0019	-.0021	-.0032***	-.0029***
War in neighbourhood	.1080***	..	.0515***	..	.0625***	..
Lagged war in neigh.	..	.0880***	..	.0875***	..	.0106***
			Diagnostics			
AR(1)	-3.27 (0.001)	-3.24 (0.001)	-3.97 (0.000)	-3.96 (0.000)	-2.17 (0.030)	-2.11 (0.035)
AR(2)	0.70 (0.481)	0.61 (0.542)	-0.06 (0.955)	-0.16 (0.870)	1.65 (0.101)	1.50 (0.134)
Hansen	70.92 (0.381)	72.70 (0.213)	63.50 (0.632)	70.15 (0.279)	88.08 (0.151)	69.50 (0.298)
Diff in Hansen	49.83 (0.287)	45.93 (0.313)	41.21 (0.633)	39.06 (0.601)	56.25 (0.121)	42.36 (0.456)
Obs	505	505	505	505	505	505
N. instruments	75	41	75	71	75	71

Notes: The diagnostics are as follows. AR(1) is the Arellano-Bond test for first order auto-correlation in first differenced errors. AR(2) is the Arellano-Bond test for second order auto-correlation in first differenced errors. Hansen is the test of over-identification restrictions. Difference in Hansen is the tests of exogeneity of instrument subsets. ***, **, * denote statistical significance at 1%, 5%, and 10% confidence level respectively.

3.2 Sub-Saharan Africa (SSA) vs. rest of the emerging and developing world (RoDW)

We now estimate the model separately on the sample of SSA countries.[8] As a further comparison, we also present estimates for the group of non-SSA developing and emerging economies (as listed in the IMF World Economic Outlook). Results are summarised in Table 3 (contemporaneous spillover effect, *i.e.* k = 0) and Table 4 (one-period lagged spillover effect, *i.e.* k = 1). The information in these two tables are organised as follows. The top half (panel A) reports the estimated coefficients of the neighbourhood variable $\bar{\boldsymbol{y}}$ for the SSA sample. The first row refers to specifications where democracy and per-capita GDP are entered linearly; the second row instead refers to the specification with squared values. Each specification is estimated using three different definitions of war commonness: (i) all types of war, (ii) only civil wars, and (iii) only interstate wars. The bottom half of the table (panel B) is similarly organised, with the difference that coefficients are estimated for the group of RoDW countries. The estimated coefficients of the other regressors in the model are reported in the Appendix for the SSA sample with the linear specification. All the other results can be obtained from the authors upon request.

Starting with Table 3, our model suggests that there is a significant contemporaneous spillover effect in SSA, but this is about half the size of the spillover effect in the RoDW group. In quantitative terms, one extra year of war in the neighbourhood of a generic SSA country increases war commonness in that SSA country by less than one to up to three weeks (depending on the length of the border between the neighbour country at war and the SSA country). In the RoDW, the spillover effect associated with one extra year of war in a neighbourhood country amounts to anything between one week and five weeks (again, depending on the length of the border shared between the domestic country and its neighbour). In fact, the RoDW spillover effect is of the same magnitude as the one estimated from the full sample of all countries (see Table 1, column I). The disaggregation by type of war however provides some important qualifications. When restricting attention to civil wars, the spillover effect in SSA is considerably stronger than in the RoDW. In fact, it seems that in SSA, the extent to which war in the neighbourhood increases domestic country's involvement in an interstate war is small and possibly even statistically insignificant (in the quadratic specification). The opposite instead is true for the RoDW group, where the spillover effect on civil wars is comparatively small.

[8] Countries outside SSA are still included in the definition of neighbourhood if they share a land border with a SSA country.

Table 3: Contemporaneous Effect of War in the Neighborhood

Panel A: Sample of Sub-Saharan African Countries

	Type of war in country *i*		
	All types	Civil only	Interstate only
War in the Neighbourhood			
Linear specification	.0421***	.0436***	.0062***
Quadratic specification	.0493***	.0352***	.0010
No. of Obs.	174		
No. of countries	44		

Panel B: Sample of non-Sub-Saharan African Developing Countries

	Type of war in domestic country		
	All types	Civil only	Interstate only
War in the Neighbourhood			
Linear specification	.0855***	-.0018	.0949***
Quadratic specification	.1145***	.0145**	.0895***
No. of Obs.	231		
No. of countries	59		

Notes: The table report the estimated coefficient δ (war in the neighbourhood) for different specifications of the spatial regression. Estimates of the other coefficients are available in the Appendix (linear specification, sample of Sub-Saharan countries) or upon request. In both panels ***,** and * denote statistical significance at the 1, 5% and 10% levels of significance respectively.

Turning to Table 4, it seems that the spillover effect is more persistent in the RoDW than in SSA. In fact, after one decade, the increase in domestic war commonness associated with an extra year of war in the neighbourhood is almost halved in SSA, while it stays practically the same in the RoDW. Importantly, the lagged spillover effect in the RoDW is stronger than in SSA independently from the type of war.[9] Finally, in the RoDW the lagged effect is stronger for civil wars than for interstate wars. That is, it appears that in the RoDW, war in the neighbourhood is initially likely to push the domestic country towards interstate war, but as time goes by this involvement in interstate war translates into greater commonness of domestic civil war.

[9] In fact, the coefficient estimated for the SSA sample is even negative, meaning that more war in the neighbourhood would marginally reduce country *i*'s involvement in an interstate war. While this is certainly a fascinating interpretation that would deserve more attention in future work, we stress that for practical purposes the coefficient is very close to zero.

Table 4: Lagged Effect of War in the Neighborhood

Panel A: Sample of Sub-Saharan African Countries

	Type of war in domestic country		
	All types	Civil only	Interstate only
War in the Neighborhood			
- Linear specification	.0241***	.0261***	-.0034***
- Quadratic specification	.0385***	.0160**	-.0030***
No. of Obs.		174	
No. of countries		44	

Panel B: Sample of non-Sub-Saharan African Developing Countries

	Type of war in domestic country		
	All types	Civil only	Interstate only
War in the Neighborhood			
- Linear specification	.0871***	.0897***	.0198***
- Quadratic specification	.1062***	.0985***	.0195***
-			
No. of Obs.		231	
No. of countries		59	

Notes: The table report the estimated coefficient δ (war in the neighborhood) for different specifications of the spatial regression. Estimates of the other coefficients are available in the Appendix (linear specification, sample of Sub-Saharan countries) or upon request. In both panels ***,** and * denote statistical significance at the 1, 5% and 10% levels of significance respectively.

4. Discussion and conclusions

Our estimates suggest that the war spillover effect in the rest of the developing and emerging world is stronger and more persistent over time than in SSA. Disaggregation of war by type, however, reveals a more complicated picture and the key differences between the two groups of countries can be summarised as follows:

When restricting attention to the commonness of civil wars, the contemporaneous spillover effect is stronger in SSA than in the RoDW. However, in SSA the strength of this spillover effect diminishes quite significantly over time, so that at a one-period lag the spillover effect on civil war is stronger in RoDW than in SSA.

In SSA, there is little evidence of war in the neighbourhood causing a greater involvement of the domestic country in an interstate war. In fact, the contemporaneous spillover effect on interstate war is positive and significant, but quantitatively very low. After a decade, this spillover effect turns negative, but very close to zero. In the RoDW instead, most of the contemporaneous spillover effect relates to interstate war. This is however not persistent over time, so that after one decade, most of the largest spillover effects in the RoDW centres on civil wars.

Putting these findings together, the story of war contagion in SSA seems to go as follows. When war in neighbourhood countries becomes more common (*e.g.* more war onsets and/or longer duration of existing wars), then war in the domestic country also becomes more common. Quantitatively, if all neighbourhood countries of the SSA spent one extra year at war (of any type) in a given decade, then the domestic SSA countries would experience an extra three weeks of war, most likely civil war, within the same decade. This effect does however, dissipate over time. For instance, after one decade the extra time of war in the domestic country is reduced to one or possibly two weeks for every additional year of war in the neighbourhood. As well, we note that the impact on neighbourhood war on the domestic country in the SSA falls mainly in the sphere of civil war. War in the neighbourhood is unlikely to cause the domestic SSA country to become involved in more interstate conflict. In fact, in due course (*i.e.* the lagged neighbourhood war effect), being surrounded by neighbours at war might actually shield the domestic country from interstate conflict.

The reasons behind this bifurcated effect of civil and interstate war deserves further consideration, especially considering that the effect for the RoDW is similar in direction between the two spheres of war (though the magnitudes differ markedly). As previously noted, the African continent is replete with colonial era borders that paid scant attention to ethnicities. This leads to wars in the neighbourhood, especially civil wars linked to ethnicity to spill over to a domestic country that has ethnic links with the country in which the conflict broke out. Bosker and de Ree (2010) note that this is a more likely outcome for countries in Africa compared to the rest of the world (with the exception of some Asian countries). These ethnically linked conflicts then draw in their ethnic brethren across borders, but in the form of raising ethnic-linked grievances that cause civil strife in the domestic country rather than leading to an outright war between governments across international borders. This study is however focussed on the relationship and nature of conflict spillover in the SSA, and does not enter into a detailed investigation of the causes of conflict spillover in the SSA, which is subject to future research.

Having established that every year of war in the neighbourhood extends domestic war by three weeks over the space of a decade, it is pertinent to ask whether this is, or indeed should be of concern to policymakers. We argue in the affirmative, even if the direct matter of concern is not days of war (which is a symptom) but instead the spread of war (the genesis of direct concern). More than the exact number of extra weeks of war, what seems to matter here is that war is indeed contagious. If policymakers were to neglect the risk of contagion, then war could potentially spread faster and, possibly, the rate of contagion itself would increase. In other words, without interventions aimed at preventing the transmission of conflict across borders, the very mechanisms that are responsible for this transmission might grow stronger and hence cause the spillover effect to strengthen over time. As previously noted, in the SSA region, these transmission mechanisms are likely to operate via ethnic linkages and the movement of refugees.

To this purpose, domestic governments, in cooperation with the international community (and the United Nations High Commission for Refugees), should deploy security and monitoring personnel to refugee hosting areas to assess the risk of camps losing their humanitarian character. Furthermore, camps should be set up as distant as possible from the borders, with the presence of military protections officers, and organised in such way to limit their population density and overall size. Encouraging the integration of refugees among domestic country citizens might also help militarisation by increasing the cost of using violence. In turn, integration might be facilitated by placing refugee camps in urban and rural settings rather than in isolated areas.

Finally, when a country gets involved in a war, the other countries in the region should take a pro-active role in peace-keeping and peace-making. This can be achieved through the mandate of existing regional economic communities (REC). In fact, while originally born to foster intra-regional trade, some of these RECs, especially in Africa, now aim at fostering cooperation in a variety of areas, including diplomacy. In a few cases (*e.g*. Central African Republic), these REC have played an active role in peace-keeping. With the support of the international community, this role can be strengthened and extended, thus creating the basis for a regional response to the risk of regional conflict spillovers.

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Appendix

Table A1: Variables description, sources and summary statistics

Variable name	Definition	Source	Mean (full sample)	Mean (SSA)	Std dev. (full sample)	Std dev. (SSA)
War (all types)	Commonness of war in the domestic country *i*. It is defined as the number of months that country *i* spent at war during decade *t* divided by 120 (or the total number of months in decade *t* during which the country was in existence)	Authors' calculations from data in UDCP/PRIO Conflict Database (Gledistch *et al*. 2002)	.127	.136	.309	.294
War (civil war only)	Commonness of civil war in the domestic country *i*. It is defined as the number of months that country *i* spent at civil war during decade *t* divided by 120 (or the total number of months in decade *t* during which the country was in existence)	Authors' calculations from data in UDCP/PRIO Conflict Database (Gledistch *et al*. 2002)	.109	.117	.281	.276
War (interstate war only)	Commonness of civil war in the domestic country *i*. It is defined as the number of months that country *i* spent in interstate war during decade *t* divided by 120 (or the total number of months in decade *t* during which the country	Authors' calculations from data in UDCP/PRIO Conflict Database (Gledistch *et al*. 2002)	.017	.019	.086	.097

	was in existence)					
War in the neighbourhood	Commonness of war in the neighbourhood of country *i*. Weighted average of war commonness in all the countries that share a land border with country *i*. For each neighbour *j*, the weight is set equal to the length of the land border between *i* and *j*, divided by the total land border of country *i*.	Authors' calculations from data in UDCP/PRIO Conflict Database (Gledistch *et al*. 2002)	.159	.183	.267	.268
Polity	Value of the polity index at the start of each decade. The index is defined on a scale from -10 (perfect autocracy) to +10 (perfect democracy)	Polity IV database	.230	-2.924	7.439	5.714
Per-capita GDP	(Log of) per-capita GDP at constant prices in PPP US dollars at the start of each decade	Penn World Tables	8422	1861	11680	2961

Table A2: Full set of estimates for the linear specification and contemporaneous spatial effect, Sub-Saharan Africa sample

	I War: All types	II War: Civil wars	III War: Interstate wars only
Lagged war	.5989***	.5745***	.44821***
Polity	.001***	.0009***	.0004***
GDP pc (log)	-.028***	-.0223***	0.07***
War in neighbourhood	.0421***	.0436***	.0062***
	Diagnostics		
AR(1) (p value)	-2.37 (0.018)	-2.63 (0.008)	-1.91 (0.056)
AR(2) (p value)	0.47 (0.637)	-0.41 (0.683)	0.83 (0.408)
Hansen (p value)	36.07 (0.797)	38.42 (0.709)	35.42 (0.818)
Diff in Hansen (p value)	25.34 (0.661)	28.19 (0.508)	38.98 (0.102)

Notes: The diagnostics are as follows. AR(1) is the Arellano-Bond test for first order auto-correlation in first differenced errors. AR(2) is the Arellano-Bond test for second order auto-correlation in first differenced errors. Hansen is the test of over-identification restrictions. Difference in Hansen is the tests of exogeneity of instrument subsets. ***, **, * denote statistical significance at 1%, 5%, and 10% confidence level respectively.

Table A3: Full set of estimates for linear specification and lagged spatial effect, Sub-Saharan Africa sample

	I War: All types	II War: Civil wars	III War: Interstate wars only
Lagged war	.5961***	.5707***	.4477***
Polity	.0017***	.0013***	.0003***
GDP pc (log)	-.0387***	-.0379***	-.0010***
War in neighbourhood	.0241***	.0261***	-.0034***
	Diagnostics		
AR(1) (p value)	-2.38 (0.018)	-2.61 (0.009)	-1.92 (0.055)
AR(2) (p value)	0.40 (0.686)	-0.49 (0.627)	0.66 (0.508)
Hansen (p value)	40.18 (0.4462)	36.11 (0.646)	41.90 (0.388)
Diff in Hansen (p value)	20.73 (0.756)	24.96 (0.521)	34.93 (0.113)

Notes: The diagnostics are as follows. AR(1) is the Arellano-Bond test for first order auto-correlation in first differenced errors. AR(2) is the Arellano-Bond test for second order auto-correlation in first differenced errors. Hansen is the test of over-identification restrictions. Difference in Hansen is the tests of exogeneity of instrument subsets. ***, **, * denote statistical significance at 1%, 5%, and 10% confidence level respectively.