journal of peace RESEARCH

© 2004 Journal of Peace Research,
vol. 41, no. 3, 2004, pp. 253–273
Sage Publications (London, Thousand Oaks, CA
and New Delhi) www.sagepublications.com
DOI 10.1177/0022343304043769 ISSN 0022-3433

# On the Duration of Civil War*

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This article explores empirically the duration of civil war. It relates the duration of civil war to two alternative models of conflict and culls testable hypotheses from the case study literature on civil war. Using a comprehensive dataset on large-scale violent civil conflicts covering the 1960–2000 period, a wide range of hypotheses are tested by means of hazard function regressions. The results show that the duration of conflict is systematically related both to structural conditions prevailing prior to conflict and to circumstances during conflict. The key structural characteristics that lengthen conflict are low per capita income, high inequality and a moderate degree of ethnic division. The key variable characteristics that shorten conflict are a decline in the prices of the primary commodities that the country exports and external military intervention on the side of the rebels. Furthermore, the results indicate that the chances of peace were much lower in the 1980s and 1990s than they had been previously. Three empirical explanations are suggested as different approaches to civil war: rebellion-as-investment, in which the critical incentive is the post-conflict payoff; rebellion-as-business, in which the critical incentive is the payoff during conflict; and rebellion-as-mistake, in which military optimism prevents the recognition of any mutually advantageous settlement. The article concludes that the empirical evidence is incompatible with the first of these approaches but consistent with the others.

## Introduction

This article explores empirically the duration of civil war. The subject is of interest both for policy and as a means of distinguishing between alternative theories of civil conflict. From the policy perspective, a distinctive feature of civil war is its persistence. The average civil war lasts over six times longer than the average international war.[1] Given the long duration of civil wars, an important policy question is how civil wars can be shortened. Fortunately, it is possible to access a comprehensive dataset of international policy interventions in civil wars, thus making it possible to investigate the efficacy of these interventions in shortening conflict. Furthermore, this article investigates the effect of changes in the price of exported primary commodities. This approximates the curtailment of rebel incomes from the plunder of natural resources. Several initiatives now have this objective, most

* An earlier version of this article was presented at the conference 'The Economics of Civil Wars, Crime, and Violence', University of California, Irvine, 18–20 May 2001. We thank Håvard Hegre and four anonymous referees for very helpful comments. All errors are our own. Anke Hoeffler gratefully acknowledges funding from the Research Council of Norway (NFR). The data used in this article are available at http://users.ox.ac.uk/~ball0144. Correspondence: paul.collier@economics.ox.ac.uk.

[1] Civil wars last on average seven years, while Bennett & Stam (1996) find that international wars last for about 11 months.

notably the newly launched Kimberley Process of diamond certification. From the perspective of theory, this analysis casts some light on whether rebel groups regard civil war as a cost of achieving post-conflict change, or as itself constituting an improvement upon the pre-conflict state.

## The Duration of Conflict in the Literature on Civil War

The literature on civil war currently offers three rival conceptualizations of rebellion. The first is rebellion-as-investment. In this approach, the payoff to rebellion, whether political or material, is treated as being contingent upon rebel victory. The articulated aspirations of rebel groups suggest that this eventual payoff is political, and a large case-study literature usually takes this at face value.[2] The celebrated model of Grossman (1991) makes the same assumption about the importance of victory, but treats the payoff as financial. These models have testable implications for the duration of conflict. Although the original Grossman model was not temporal, in a simple extension Grossman (1995) introduced a discount rate into the analysis. Building on this, Collier & Hoeffler (1998) modelled the benefits as accruing upon victory, *while during the period of fighting the rebels incurred net costs*. In this structure, the longer the expected duration of war, the higher are the costs and the more heavily discounted are the benefits. *A key prediction is that the higher the payoff from victory, the longer would be the warranted rebellion*. The payoff from victory might be narrowly self-serving, such as the capture of government revenues, or altruistic, such as the release from repression. Collier & Hoeffler (2004) have found that one of the risk factors making a country prone to civil war is the presence of primary commodity exports with large rents. If the payoff to civil war is the post-conflict control of such resources, then a testable prediction is that *the duration of civil war should be increased by the extent of pre-conflict primary commodity exports*. If, instead, the post-conflict payoff is more altruistic, such as release from repression, then a testable prediction would be that *the duration of civil war is increased by the severity of pre-conflict political repression*.

The second conceptualization is rebellion-as-mistake. Hirshleifer (2001), in particular, has stressed the scope for misperceptions: for example, analogous to the 'winner's curse', each side may overestimate its military prospects. Such misperceptions would evidently make wars more likely to start, but they might also explain why they persist. Because civil war is socially costly, it is always possible to envisage an agreed return to peace in which both protagonists are better off. However, if each side overestimates its prospects of victory, there may be no peaceful outcome in which both protagonists *recognize* that they are better off. In this case, a settlement recognized as mutually beneficial being infeasible, the conflict continues. In the sample used for this article, the mean duration of conflict is seven years, and this very persistence of costly conflicts demonstrates the considerable difficulties in achieving a settlement that both protagonists consider advantageous. In order to apply this explanation for the persistence of rebellion to *differences* in the duration of conflict, we need to establish the circumstances in which misperceptions have particularly debilitating effects on the ability to reach a settlement.

Clearly, one differentiating feature is the magnitude of the misperceptions of military prospects. Other things being equal, the larger is the mutual degree of overoptimism about military prospects, the longer will the

[2] For a recent collection of the literature which brings out these rival conceptualizations, see Berdal & Malone (2000).

war last. This trivial-sounding proposition turns out to have a readily testable proxy. In the context of civil war, the winner's curse implies that in those situations in which war is initiated, both parties will tend to be overoptimistic. Evidently, where both sides are overpessimistic, they will strive hard to avoid escalating the situation to the point of warfare. However, as the war proceeds, the continuing flow of new military information inconsistent with these initially overoptimistic expectations should gradually force their revision towards greater realism. As expectations converge towards reality, it becomes progressively easier to find a settlement recognized as mutually beneficial. Hence, if errors in expectations are the chief explanation for the persistence of civil war, a testable prediction is that *the probability of reaching a settlement in each time period should rise over time*. That is, the chance of ending the war would be higher in the third year than in the second, and so on.

Now consider whether, for a given degree of overoptimism about military prospects, there are other features which differentiate the duration of conflict, but which indeed depend upon some degree of overoptimism? One such feature is the cost of conflict to society. If the cost is sufficiently large, then even with substantial overoptimism, it should be possible to find a mutually beneficial settlement. The social cost of conflict is indeed likely to vary systematically across societies. One reasonable proposition is that the higher the costs of conflict, the higher is the per capita income of the society prior to the conflict, implying the testable proposition that *the duration of war would be negatively related to initial per capita income*. Unfortunately, this proposition does not distinguish the war-as-mistake approach from the other approaches. This is because a high social cost of conflict is likely also to imply a high private cost of conflict for the rebel group, and both the other approaches would predict that higher private costs of rebellion would imply shorter conflicts.

A second feature which differentiates the duration of conflict is the extent to which the share of income accruing to each protagonist depends upon victory versus defeat. If the distributional swing dependent upon the military outcome is very large, then even a modest degree of overoptimism will be sufficient to preclude a settlement recognized as mutually beneficial. A fairly good proxy for the distributional swing contingent upon the military outcome is the distribution of income in the society prior to the conflict. The more unequal is the society, then the larger are the gains from moving to the top of the ranking, which victory can be presumed to achieve, leading to the testable prediction that *initial inequality is positively related to the duration of conflict*. Again, unfortunately, this does not distinguish the conflict-as-mistake approach from the other approaches. The greater is inequality for a given level of income, the worse off are the poor and so the lower the cost of rebel recruitment. Both the other approaches would predict that the lower the costs of recruitment, the longer would be the duration of conflict.

The third conceptualization is rebellion-as-business. In this literature, the rebellion pays off through income or satisfaction *during* the fighting: rebels gain despite the costs to society. Collier (2000) models the rebellion as 'quasi-criminal', the opportunity for looting during the conflict providing the motivation.[3] Collier & Hoeffler (2004) allow the rebellion to be motivated by non-economic objectives, but with financial and military viability during conflict as the binding constraint. They suggest that all

[3] Indeed, the payoff may even depend upon the continuation of a state of lawlessness. For example, the drug revenues received by several rebel groups depend upon their controlling territory outside the control of a recognized government.

societies have at least a few hundred people attracted by the role of the rebel and willing to engage in sustained violence for hazy objectives. If such subjective grievance is indeed widespread across societies, it cannot explain why some societies experience civil wars whereas most do not. In this case, the relevant explanatory variables for the occurrence of conflict are those that determine the relatively rare conditions in which aggrieved people can combine into an organization that is financially and militarily viable. Rebellions will occur where and only where they are profitable (although they need not be motivated by profit). They will be profitable where revenues during conflict are atypically high and costs atypically low. One component of rebel revenue stressed by Collier & Hoeffler (2004) is the extortion of revenues from primary commodity exports, most notably natural resources where the rents are often very high. A testable prediction of this approach is that the duration of conflict should be related to the world price of these exports. Like the test of the war-as-mistake approach, this has the advantage of being time-variant. That is, *prospects of peace should improve when world prices are low, implying a squeeze on rebel finances, and deteriorate when world prices are high*. The base period against which to compare prices would be the time at which the conflict started. When world prices of the commodity exported by the country are below this level, rebellion is less profitable than it was initially; when they are above this level, the rebellion is more profitable than it was initially.

In effect, whereas in the second conceptualization the persistence of rebellion reflects mistakes, now rebellions persist *unless* they are mistakes. One prediction of this approach is that unless the revenues or costs change, *the chance of peace per period will be negligible and will not change significantly.* As noted, the cost of conflict enters in all three approaches but with differences. The rebellion-as-investment approach assumes that during conflict, rebels have net costs, whereas the rebellion-as-business approach assumes that rebel revenue exceeds costs. The rebellion-as-mistake approach is concerned with the overall costs to society.

In addition to this economic analysis of the duration of conflict, different literatures have also proposed social, geographic, historical, psychological and policy influences. Although these are not integrated into a single model, they are evidently worth investigation and we briefly discuss them. Peer pressure and solidarity are critical to military effectiveness. There is some evidence that social diversity makes cooperation more difficult, leading to the prediction that *socially diverse rebellions should be shorter*.[4] A further speculation from the military literature is that some terrain is particularly suitable to sustained rebellion. The use of 'agent orange' to defoliate areas of Vietnam was a dramatic instance of the hypothesis that forest cover sustains conflict. The rise of global private and clandestine markets in both armaments and illicit primary commodities, such as 'conflict' diamonds, timber and drugs, has led some analysts to speculate that conflict has become increasingly viable and so easier to sustain.[5] Finally, international relations scholars have discussed the extent to which international policy interventions have succeeded in shortening wars (Regan, 2002).

## Data and Econometric Model

The analysis of war duration is critically dependent upon definitions of what constitutes a war, its start and its end. Following the Correlates of War (CoW) project, we

[4] See Collier (2001) for a discussion of the evidence relating to the difficulties of cooperation posed by ethnic diversity.

[5] See Le Billon, Sherman & Hartwell (2002) for a review.

define civil wars as violent conflicts that resulted in at least 1,000 battle-related deaths per annum.[6] In addition, these conflicts are internal to a country, and the nongovernmental forces are responsible for a minimum of 5% of the deaths in order to distinguish civil wars from massacres. For many conflicts, the CoW project offers start and end dates and thus can be used for duration analysis. Trigger events, for example the assassination of Rwanda's president on 6 March 1994, often spark a civil war. If the conflict ended in a peace treaty, ceasefire or military defeat, it is also easy to date the end of the civil war. However, often the violence escalates over some period of time before it reaches the relevant threshold and thus can be defined as a civil war. Military victories and peace treaties are also relatively rare in civil wars, and dating the end of the civil wars is often difficult. In many cases, the number of fatalities falls beneath the threshold and is thus not counted as a civil war, although the country is not at peace. Thus, higher death thresholds result in recording shorter civil wars. For example, in our dataset the mean duration of civil war is about seven years, while the civil wars in Fearon's (2004) sample have an average of about 12 years. The difference is because Fearon includes conflicts which resulted in a minimum of 1,000 deaths over their entire duration as civil wars. Furthermore, a higher threshold leads to a higher number of repeat war episodes. Take, for example, a conflict during which the number of battle-related deaths is lower than the per annum threshold for some period before the level of violence escalates again. A rigid application of the absolute threshold criterion could lead to the classification of two conflicts for a high-threshold definition and to the classification of one conflict for a low-threshold definition. By our definition, 77 civil wars started during the period 1960–99.[7]

This article uses a new dataset on international policy interventions in civil war, collected by Regan (2002).[8] His operational definition of third-party interventions in intrastate conflicts includes 'convention-breaking military and/or economic activities in the internal affairs of a foreign country targeted at the authority structures of the government with the aim of affecting the balance of power between the government and opposition forces' (Regan, 2000: 2). The notion of convention breaking allows him to discriminate between the normal course of international influence and interventions. Regan lists a number of different economic and military interventions. Economic interventions include grants, loans, non-military equipment or expertise, credits, relief of past obligations and economic sanctions. For this article, a dummy variable 'economic intervention' was created, taking a value of 1 if any of these interventions took place. Regan distinguishes between six different types of military intervention: troops, naval forces, equipment or aid, intelligence or advisers, air support and military sanctions. If any of these six interventions occurred, we classified it as a military intervention. Furthermore, he distinguishes between how the intervention is targeted: in favour of the government, in favour of the opposition, or neutrally, and we retain this disaggregation.

Our other explanatory variables are from the dataset of Collier & Hoeffler (2004). This attempts to collect socio-economic, political and geographic data for all the 77 civil wars, based on the definition discussed above. Data limitations, notably missing data on key variables, reduce the effective sample to 55 wars, which are listed in

[6] The CoW dataset is available from Singer & Small (1994), and a detailed discussion of their definition can be found in Small & Singer (1982).

[7] For a discussion of data issues in the study of civil war, see Collier & Hoeffler (2002).

[8] Data can be downloaded from http://bingweb.binghamton.edu/~pregan/.

Appendix Table AI. Summary definitions of the socio-economic variables are given in the Appendix. Table AII presents descriptive statistics of all the variables, giving means for all the conflicts in the sample, disaggregated into those for shorter and longer conflicts.

The regression analysis is based on maximum likelihood estimation, where the econometric specification is a hazard model of the monthly transition rates from war to peace. To illustrate the model, let $\tau = 1, \ldots, T$ denote calendar time (in months), where $\tau = 1$ represents January 1960 and $\tau = T$ December 1999, and let $t$ denote the duration of a war that started at a point in time $a$ between $\tau = 1$ and $\tau = T - 1$. We assume a proportional hazard function of exponential form:

$$h(t; x_\tau, \mu, \theta) = \exp(x_\tau \beta + \mu) h^B(t), \quad (1)$$

where $x_\tau$ is a vector of observed exogenous variables, $\theta$ is a vector of unknown parameters of which $\beta$ is a sub-vector, $\mu$ is a country-specific unobserved random effect assumed orthogonal to $x_\tau$ and $h^B$ is the baseline hazard. Subscript $\tau$ on the vector of explanatory variables indicates that the explanatory variables may be, but are not necessarily, time-varying.[9] For the baseline hazard, a piecewise exponential specification was adopted. Specifically, we divide the time axis into $W$ intervals by the points $c_1, c_2, \ldots, c_W$, and constant baseline hazard rates within each interval were assumed:

$$h^B(t) = \exp\left(\alpha + \sum_{w=2}^{W} \lambda_w d_w(t)\right), \quad (2)$$

where $d_w(t)$ is a duration dummy variable equal to 1 if $c_{w-1} < t \leq c_w$ for $c_0 = 0$ and $c_W = \infty$, and 0 otherwise; $\alpha$ is an intercept; and $\lambda_2, \ldots, \lambda_W$ are baseline hazard parameters to be estimated. Thus, the baseline hazard is allowed to vary freely between intervals, which imposes few restrictions on duration dependence. Because an intercept was included in the model and the first duration dummy (i.e. $d_1$) was excluded, negative (positive) coefficients on $\lambda_2, \ldots, \lambda_W$ imply that the hazard is lower (higher) than in the first interval.

Now consider the likelihood function. A completed ($\delta = 1$) or censored ($\delta = 0$) war of length $t$ is observed with likelihood

$$L(t; x_\tau, \mu, \theta) = \left[h(t; x_\tau, \mu, \theta)\right]^\delta \cdot S(t; x_\tau, \mu, \theta) \quad (3)$$

where $S(t;\cdot) = \exp(-\Lambda(t;\cdot))$ is the survivor function and $\Lambda(t;\cdot)$ is the integral of the hazard function over time with limits $(a, a + t)$.[10] While this likelihood depends on the unobserved country random effect $\mu$, the parameter vector $\theta$ can still be estimated consistently if $\mu$ is integrated out.[11] This is a relatively straightforward task under the maintained assumption that $\mu$ is orthogonal to $x_\tau$. If there is no unobserved heterogeneity in the data, that is, zero variance of $\mu$, the likelihood simplifies to $L(t{:}x_\tau, \theta) = [h(t;x_\tau, \theta)]^\delta \cdot S(t;x_\tau, \theta)$.

The sample likelihood, finally, is simply the product of the relevant individual likelihood contributions. With $N$ countries and $R(i)$ wars occurring in country $i$ over the sampling period, the sample likelihood of the model with unobserved heterogeneity is

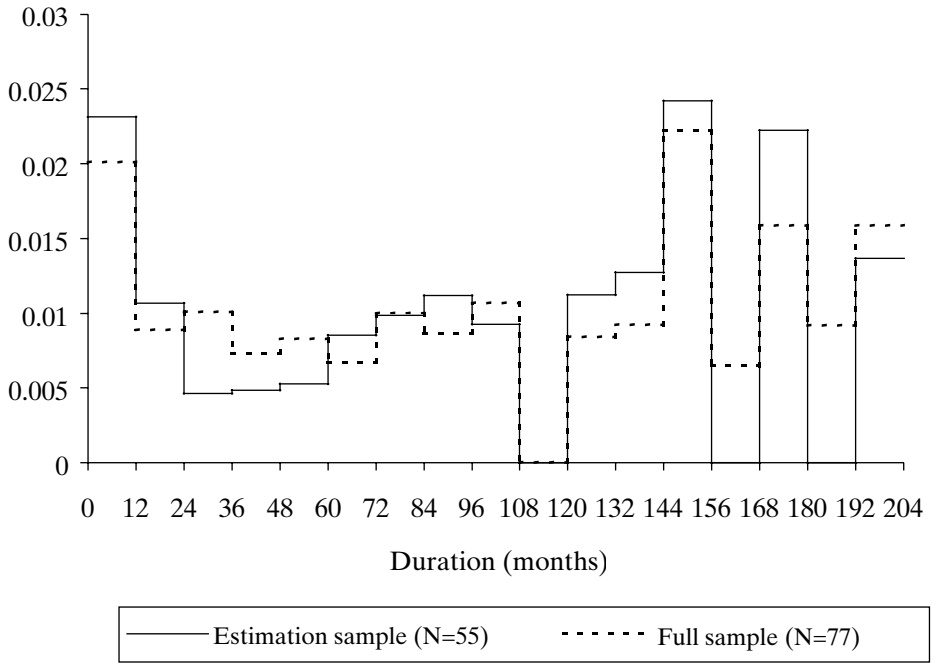
$$\lambda_{UH} = \prod_{i=1}^{N} \int \prod_{r=1}^{R(i)} L_{ir}(t; x_\tau, \mu, \theta)\, g(\mu)\, d\mu, \quad (4)$$

[9] In the empirical analysis, one category of variables is time-varying both across and within wars; a second category is time-varying across, but time-invariant within, wars (typically measured at the start of each war or where this is not possible, for the period closest to this date); and a third category is time-invariant.

[10] See Chapter 2 in Lancaster (1990) for details on the relations between the hazard function, the integrated hazard and the survivor function.

[11] It is well known that if there is unobserved heterogeneity in the data and this is not controlled for, this will result in spurious negative duration dependence and biased parameter estimates for the explanatory variables (see e.g. Lancaster, 1990: ch. 4).

Figure 1. Estimates of the Hazard Function Without Controls for Explanatory Variables




The hazard function is calculated using the formula $\exp\left[\hat{\alpha} + \sum_w \hat{\lambda}_w d_w(t)\right]$, where $\hat{\alpha}, \hat{\lambda}_2, \cdots, \hat{\lambda}_w$ are parameter estimates obtained by means of maximum likelihood. The underlying regression is not reported but is available on request from the authors.

where $L_{ir}$ is the likelihood of the $r$th war in country $i$ and $g(\mu)$ is a density function. Without unobserved country heterogeneity, no integration is necessary, hence

$$\lambda = \prod_{i=1}^{N} \prod_{r=1}^{R(i)} L_{ir}(t; x_\tau, \theta). \qquad (5)$$

Given the functional form of (1), this is the same likelihood as that of the ordinary exponential regression, generalized to allow for a piecewise baseline hazard.

## Empirical Results

As a first step, estimates of the parameters of a hazard function in which explanatory variables and unobserved heterogeneity are absent are derived:

$$h(t) = \exp\left(\alpha + \sum_{w=2}^{W} \lambda_w d_w(t)\right) \equiv h^B(t). \qquad (6)$$

This, of course, is restrictive, but serves as a usual benchmark. The underlying assumption is that the monthly hazard rates are constant over a period of 12 months. Rather than reporting the estimates of the parameters $\alpha$, $\lambda_2$, . . . , $\lambda_W$, Figure 1 shows the estimated hazard function up to 17 years.[12] The graph shows estimates based on both the full sample of 77 wars and the estimation sample of 55 wars, and the results are similar. Clearly, the hazard of peace falls quite rapidly in the early phases of war and then starts to increase after three years of war, albeit slowly. In the following discussion, the effects of including explanatory variables are considered.

Table I presents the preferred reference model of conflict duration with eight

[12] Only 4 of the 77 wars in the full sample last longer than 17 years.

Table I. Econometric Estimates of Hazard Function Parameters

| | *(1)* | *(2)* | *(3)* | *(4)* |
|---|---|---|---|---|
| Income inequality | –0.1244 | –0.1258 | –0.1186 | –0.1262 |
| | (0.0284)*** | (0.0283)*** | (0.0306)*** | (0.0289)*** |
| Missing inequality | –5.868 | –5.8717 | –5.6289 | –5.9118 |
| | (1.2774)*** | (1.2689)*** | (1.3644)*** | (1.2800)*** |
| Per capita income | 0.3651 | 0.4043 | 0.4452 | 0.3633 |
| | (0.1322)*** | (0.1370)*** | (0.1736)*** | (0.1332)*** |
| Ethnic fractionalization | –0.0628 | –0.0695 | –0.0788 | –0.0624 |
| | (0.0259)** | (0.0261)*** | (0.0270)*** | (0.0258)** |
| Ethnic fractionalization$^2$ | 0.0006 | 0.0007 | 0.0008 | 0.0006 |
| | (0.003)** | (0.0003)** | (0.0003)*** | (0.003)** |
| ln population | –0.3164 | –0.2860 | –0.2373 | –0.3318 |
| | (0.1231)*** | (0.1225)** | (0.1587) | (0.1325)*** |
| 1970s | 0.0078 | 0.1298 | 0.0064 | 0.0041 |
| | (0.4625) | (0.4677) | (0.5049) | (0.4622) |
| 1980s | –1.4202 | –1.3830 | –1.5404 | –1.4282 |
| | (0.5203)*** | (0.5256)*** | (0.5865)*** | (0.5202)*** |
| 1990s | –1.1621 | –1.1810 | –0.9705 | –1.1796 |
| | (0.5417)** | (0.5405)** | (0.6332) | (0.5469)** |
| 3rd and 4th years of war ($\lambda_2$) | –0.8067 | –0.8747 | –1.2103 | –0.8146 |
| | (0.5743) | (0.5790) | (0.6590)* | (0.5746) |
| 5th and 6th years of war ($\lambda_3$) | –0.0011 | 0.0044 | –0.0460 | –0.0170 |
| | (0.5606) | (0.5601) | (0.5619) | (0.5614) |
| 7th year of war and beyond ($\lambda_4$) | 0.6098 | 0.7100 | 0.7471 | 0.5966 |
| | (0.4464) | (0.4440) | (0.4592)* | (0.4470) |
| Change in commodity price index (CPI) | | 1.7669 | 2.1929 | |
| | | (1.0629)* | (1.0304)** | |
| Primary commodity exports/GDP (sxp) | | 10.6114 | 12.2126 | |
| | | (5.9267)* | (5.6428)** | |
| CPI*sxp | | –11.3237 | –13.2774 | |
| | | (5.8518)** | (5.7058)** | |
| Democracy | | | | 0.0189 |
| | | | | (0.0584) |
| Constant | 7.4331 | 5.2044 | 3.8077 | 7.7223 |
| | (2.7079)*** | (3.0352)* | (3.4190) | (2.8481)*** |
| Log likelihood | –80.43 | –78.18 | –67.05 | –80.38 |
| Number of observations | 55 | 55 | 51 | 55 |

variants. The reference model is reached after a series of iterations in which insignificant variables are deleted and variants of economic, social, geographic and historical explanatory variables are then tested in turn. Given the small sample, it is desirable to keep the number of segments of the baseline hazard as small as possible without imposing too strong restrictions on the data. Guided by Figure 1, the interval between 0 and 72 months was divided into three two-year periods, while beyond 72 months the hazard was assumed to be flat. Experimentation with the data showed that the main results are robust to alternative definitions. Throughout, no evidence was

Table I. (continued)

| | *(5)* | *(6)* | *(7)* | *(8)* | *(9)* |
|---|---|---|---|---|---|
| Income inequality | –0.1247 | –0.1264 | –0.1312 | –0.1314 | –0.1264 |
| | (0.0284)*** | (0.0275)*** | (0.0306)*** | (0.0320)*** | (0.0293)*** |
| Missing inequality | –5.868 | –5.7676 | –6.0510 | –6.1956 | –6.1673 |
| | (1.2798)*** | (1.2254)*** | (1.3523)*** | (1.4352)*** | (1.3247)*** |
| Per capita income | 0.3820 | | 0.3114 | 0.3581 | 0.4264 |
| | (0.1401)*** | | (0.1482)** | (0.1356)*** | (0.1353)*** |
| Ethnic fractionalization | –0.0635 | –0.0685 | –0.0691 | –0.0647 | –0.0803 |
| | (0.0260)** | (0.0260)*** | (0.0270)*** | (0.0261)*** | (0.0273)** |
| Ethnic fractionalization$^2$ | 0.0006 | 0.0007 | 0.0007 | 0.0006 | 0.0008 |
| | (0.003)** | (0.0003)** | (0.0003)** | (0.0003)** | (0.0003)** |
| ln population | –0.3217 | –0.4768 | –0.3523 | –0.3163 | –0.3500 |
| | (0.1224)*** | (0.1283)*** | (0.1388)*** | (0.1266)*** | (0.1299)*** |
| 1970s | 0.0160 | 0.0962 | 0.0350 | 0.0844 | –0.0771 |
| | (0.4637) | (0.4586) | (0.4657) | (0.4727) | (0.4742) |
| 1980s | –1.4021 | –1.4378 | –1.4530 | –1.4146 | –1.7871 |
| | (0.5228)*** | (0.5137)*** | (0.5253)*** | (0.5222)*** | (0.5615)*** |
| 1990s | –1.1823 | –1.2212 | –1.1042 | –1.1072 | –1.4467 |
| | (0.5506)** | (0.5309)** | (0.5564)** | (0.5541)** | (0.5678)*** |
| 3rd and 4th years of war ($\lambda_2$) | –0.8013 | –0.8458 | –0.8189 | –0.8055 | –0.7438 |
| | (0.5748) | (0.5691) | (0.5759) | (0.5747) | (0.5854) |
| 5th and 6th years of war ($\lambda_3$) | 0.0072 | –0.1095 | –0.0334 | –0.0056 | 0.0421 |
| | (0.5619) | (0.5476) | (0.5597) | (0.5612) | (0.5786) |
| 7th year of war and beyond ($\lambda_4$) | 0.6176 | 0.4758 | 0.5852 | 0.5784 | 0.4948 |
| | (0.4504) | (0.4182) | (0.4442) | (0.4514) | (0.5001) |
| Primary commodity exports/GDP (sxp) | –0.6079 | | | | |
| | (1.6205) | | | | |
| Male secondary school enrolment rates | | 0.0272 | | | |
| | | (0.0092)*** | | | |
| Religious fractionalization | | | –0.0136 | | |
| | | | (0.0399) | | |
| Religious fractionalization$^2$ | | | 0.0001 | | |
| | | | (0.0006) | | |
| Mountains | | | | –0.0068 | |
| | | | | (0.0084) | |
| Forests | | | | 0.0011 | |
| | | | | (0.0094) | |
| Pro-government economic intervention | | | | | –0.0167 |
| | | | | | (0.1592) |
| Pro-rebel economic intervention | | | | | –0.1747 |
| | | | | | (0.4180) |
| Pro-government military intervention | | | | | –0.0127 |
| | | | | | (0.0806) |
| Pro-rebel military intervention | | | | | 0.1994 |
| | | | | | (0.0933)** |
| Constant | 7.5691 | 9.9927 | 8.6983 | 7.9985 | 8.3512 |
| | (2.7225)*** | (2.7974)*** | (3.2204)*** | (2.9428)*** | (2.8474)*** |
| Log likelihood | –80.56 | –80.03 | –80.09 | –80.10 | –76.93 |
| Number of observations | 55 | 55 | 55 | 55 | 55 |

*z*–statistics are based on asymptotic standard errors. Significance at the 10%, 5% and 1% level is indicated by *, ** and ***, respectively. All results have been calculated using Stata Release 8.0 (StataCorp, 2003).

found that unobserved heterogeneity was present in the data, and therefore results are shown without controls for unobserved heterogeneity.[13] The translation from the hazard (which is a density function) to a single summary number for the duration of conflict is non-trivial: an increase in the 'hazard' of peace will evidently shorten the expected duration of conflict, but the relationship is not linear. While the regression results directly report the parameters of the hazard function, the effects of important variables upon the expected duration of conflict are reported below.

First, the results of the reference model (column 1) are considered. The most significant variable in the model is income inequality, as measured by the Gini coefficient. Globally comparable data on income inequality are limited. To preserve sample size, missing values of the Gini coefficient were replaced by zeros and a dummy variable was added, taking the value 1 for missing observations and 0 for complete ones.[14] Income inequality, as measured by the Gini coefficient, is not just statistically highly significant, it has large effects. At the mean of other variables, a ten-point increase in the Gini coefficient (from 41 to 51) increases the expected duration of conflict from 59 months to 144 months – an elasticity of duration with respect to inequality of 4. Although income inequality is evidently important, the routes by which it affects the duration of conflict are open to multiple interpretations. It might be proxying the difference between the victory and defeat payoffs, as suggested by the rebellion-as-mistake approach. It might also be proxying the costs of recruitment: since recruits tend to come from the poor, for a given mean per capita income, the greater is inequality the lower will be recruitment costs. In turn, lower costs will imply a longer conflict in both the rebellion-as-investment and the rebellion-as-business approaches.

The next significant variable is per capita income. Higher income increases the hazard of peace and thus reduces the expected duration of conflict. A 10% increase in per capita income is associated with a 5% reduction in the duration of conflict, the elasticity being –0.5. Per capita income is correlated with so many other characteristics that it is difficult to interpret. However, the higher is per capita income prior to the conflict, the higher is the opportunity cost of conflict to society, and so one interpretation is that it is proxying this social cost.[15] No other variables characterizing the economy just prior to the conflict are significant in explaining the duration of conflict.[16]

Ethnic fractionalization is important for the duration of conflict. We measure it on a scale of 0 to 100. This measures the probability that two randomly drawn

[13] To test for unobserved country heterogeneity, we used a semi-parametric approach, proposed by Heckman & Singer (1984), in which $g(\mu)$ is approximated by a discrete multinomial distribution. This is a flexible approach (Mroz & Guilkey, 1995; Mroz, 1999). Allowing for two mass points in the distribution of $\mu$, we never obtained a significant increase in the log likelihood compared to the model without heterogeneity. Thus, the assumption that the variance of $\mu$ is zero is not rejected by the data.

[14] This procedure is known as a modified zero-order regression (Greene, 2003: 60). The estimates of the coefficients on inequality and on the dummy for missing inequality can be used to determine what the value of the inequality measure would have to be to produce the same hazard rate as that conditional on inequality missing, all other factors held constant. Specifically, dividing the coefficient on the dummy by the coefficient on inequality gives the hypothetical value of inequality conditional on which the hazard rate equals the hazard rate conditional on missing data on inequality, all other factors equal. Thus, if this hypothetical value is very different from the sample mean of the observed data on inequality, this indicates that countries with missing data on Gini, for one reason or another, record atypically long or short durations, conditional on the other explanatory variables in the model.

[15] We have tested for the significance of non-linear income effects on the hazard. The estimated coefficients on income squared and income interacted with inequality (as pointed out by one of the referees, inequality tends to be relatively high in middle-income countries, and so it could be that the inequality effect proxies a middle-income effect) are both insignificant at the 10% level.

[16] Using a different dataset and model, Fearon (2004) reaches a similar conclusion.

individuals do not belong to the same group. Thus, a value of 0 characterizes perfect homogeneity and 100 complete heterogeneity. The measure is based on the data from Atlas Narodov Mira (USSR, 1964). The effect of ethnic diversity is significant and substantial, but it is non-monotonic: the duration of conflict is at its maximum when ethnic fractionalization is around 50 on its 0–100 range. This typically occurs when the society has two or three large ethnic groups. One interpretation of this is that when there are only two major groups, one is the government side and the other is the rebel side, so that this degree of diversity at the national level actually increases social cohesion on the rebel side. Beyond this, higher levels of national diversity introduce diversity into the rebel side and so reduce cohesion. Evaluated at the mean values of the other variables, ethnic fractionalization of 50 is associated with a duration of conflict of 84 months. This falls to 59 months if the fractionalization score is 25, and to 70 months if the fractionalization score is 75. Collier & Hoeffler (2004) find a similar effect in their analysis of conflict initiation.

In addition to the social composition of the population, its size is significant: more populous countries tend to have longer wars. Doubling the population increases the duration of conflict by 18%. However, this result needs to be interpreted with caution. It need not mean that any particular rebellion continues for longer in more populous countries. More populous countries tend to have more rebellions, though not necessarily more than proportionately to their population, and so may have several under way at the same time. A conflict is coded as continuing if any rebellion is continuing, so that this alone will tend to produce a correlation between country size and the duration of conflict. Hence, the results do not imply that a continent divided into many countries would have shorter wars than an otherwise identical continent divided into few countries.

Decade dummies have significant and substantial effects. At the means of other variables, the hazards of peace prevailing during the 1960s and 1970s were identical, implying an expected duration of 43 months, evaluated at the mean values of the other regressors. In the 1980s, the hazard of peace was much lower, implying an expected duration of 122 months. In the 1990s, there was a modest improvement, with the expected duration declining to 101 months. Evidently, from the model we cannot account for this large lengthening of conflict. A phenomenon stressed in the case study literature is the growing role of global markets, both for the illicit sale of natural resources by rebel groups and for the illicit purchase of armaments. These results are consistent with this literature. The small reduction in the expected duration of conflict during the 1990s is quite probably accounted for by a temporary surge in peace settlements following the end of the Cold War. During the Cold War, both superpowers had illicitly funded rebel groups opposed to governments in the rival bloc, and this source of finance rapidly declined once it was no longer useful.

The final parameter estimates in the reference model refer to the shape of the baseline hazard. As already discussed, the baseline hazard is assumed to be constant within, but potentially varying between, the intervals (0,24], (24,48] and (48,72] months, and flat beyond 72 months. Based on the estimates of the baseline, we investigate one prediction of the rebellion-as-mistake approach, namely, that the chances of peace should increase as each party is disabused of its initial military optimism. The broad pattern of the period-specific hazard is U-shaped. However, the only statistically significant component of the U-shape is that beyond six years of a conflict, where the chances of reaching peace are significantly better than

during the third and fourth years.[17] This improvement over time in the chance of peace is consistent with the rebellion-as-mistake approach, but this should be set against the failure to find any more general tendency for chances to improve over time. Indeed, the more reasonable reading of the estimated baseline hazard parameters is the absence of any strong trend, something which is more consistent with the rebellion-as-business approach. We also investigated whether the characteristics that influence the duration of conflict are differentially important in different phases of conflict. For this, taking each significant variable in turn, interaction terms between the variable and the phase of conflict dummies were added but none was significant. Figure 2 shows the baseline hazard, evaluated at mean values of the explanatory variables. For reference, the dashed line shows the baseline hazard obtained without controls for explanatory variables. As expected, the evidence suggests that the latter exhibits some degree of bias towards negative duration dependence.

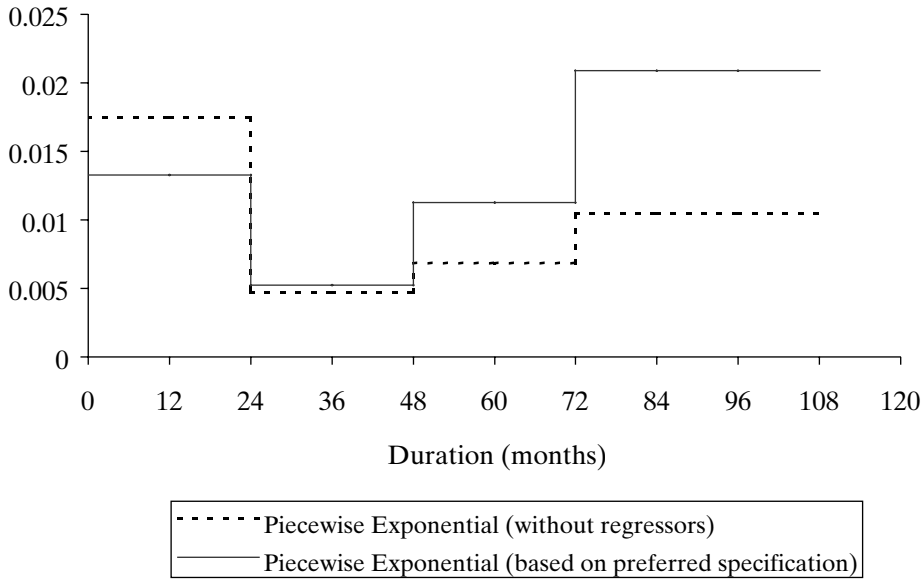
This evidence on the U-shaped path of the peace hazard can be related to the path of the risk of conflict initiation. One of the results from the analysis of the initiation of conflict is that the risk is temporarily much higher in post-conflict situations. Evidently, something happens during the conflict that increases this risk. For example, one obvious hypothesis is that since conflict causes grievances, it is these accumulated grievances that make the reversion to war more likely. Although the concept of conflict continuation is not the same as that of conflict re-ignition, it is reasonable to suppose that the risk of conflict re-ignition, which is latent during conflict, broadly follows the observed risk of conflict continuation. While the risk of conflict *initiation* is much higher after a conflict than before it, the risk of conflict *continuation* does not rise continuously during the conflict – indeed, if anything it declines. Hence, the latent risk of conflict re-ignition probably jumps with the onset of the conflict. This jump may be more related to the establishment of the rebel military organization than to any sudden upsurge in grievances.[18]

Columns 2 and 3 present variants of this reference model that are on the borderline of statistical significance and are pertinent for testing the rebellion-as-business approach. In these variants, we introduce data on changes in the world price of the country's primary commodity exports. The case study literature suggests that rebel groups often rely upon the plunder of natural resources and, sometimes, other primary commodities, in order to sustain conflict financially (see Klare, 2001, for a discussion). Lower world prices would therefore squeeze rebel finances and potentially shorten the conflict. Evidently, governments also get revenue from primary commodities, and the net effect in any particular instance would depend upon which party was more dependent upon commodity taxation for its finance, but a presumption might be that normally rebel groups have fewer alternative sources of finance than governments. The issue is of some policy importance since international efforts are currently under way to reduce rebel access to global commodity markets, notably for diamonds and timber. The effect of such efforts is not literally to exclude rebels from the market, but rather to create a price discount for rebel supplies – industry sources suggest that the early effects of the Kimberley Process of diamond certification have been to create a discount of around 10% for illicit diamonds. A reduction in the world price to an extent simulates such rebel-specific discounts. For this analysis, a weighted export price series

[17] The hypothesis that $\lambda_2 = \lambda_4$ is rejected at the 5% level of significance.

[18] Glaeser (2002) presents a political economy model of hatred and argues that hatred is not the result of past grievances but generated by political entrepreneurs to achieve their own goals. Our empirical evidence provides some support for this hypothesis.

Figure 2. Piecewise Exponential Estimates of the Hazard Function




The hazard function based on the preferred specification (i.e. the reference model) is calculated using the formula $\exp\left[\bar{x}_\tau \hat{\beta}\right] \cdot \exp\left[\hat{\alpha} + \sum_w \hat{\lambda}_w d_w(t)\right]$, where $\hat{\beta}, \hat{\alpha}, \hat{\lambda}_2, \cdots, \hat{\lambda}_W$ are the parameter estimates and $\bar{x}_\tau$ denotes a vector of sample means of the explanatory variables. The hazard function without regressors was calculated as explained in the notes to Figure 1. The underlying regression is not reported but is available on request from the authors.

constructed by Dehn (2000) was used. Specific export price indices, for which the base period is the year of the onset of the conflict, were constructed.[19] The resulting indices are year-by-year time-varying (for a given war), and we are, therefore, investigating how price changes from the time of conflict initiation affect duration. Evidently, such changes in prices are only even potentially important to the extent that the country has such exports, and so the relevant concept is the price index multiplied by the pre-conflict share of primary commodity exports in GDP. To enable the interpretation of this interaction term, its two components were also separately introduced into the regression. The effect of a change in commodity prices then has to be evaluated for each particular level of commodity dependence, as the net effect of the three variables. Because Dehn's series is not available for a few countries, we investigate two approaches to missing variables. In column (2), we impose the mean value of the variable on missing observations, and in column (3) we delete missing observations. The two approaches produce very similar results, although the latter yields higher levels of statistical significance. The new variables are each significant at 5%, although jointly they are not quite significant.[20] The effect of changes in world prices depends upon the initial level of a country's exports. For low levels of dependence upon primary commodity exports, world prices have

[19] That is, the index is equal to 1 at the onset of the conflict and then takes (during the course of the war) subsequent values determined by the changes in the original Dehn series since the onset of the conflict.

[20] A log likelihood ratio test of the hypothesis that the three commodity price and primary exports coefficients are jointly zero can be carried out, based on the log likelihood values for specifications (1) and (2). The test statistic is 2[80.43 – 78.18] = 4.5 and so, with three degrees of freedom, the associated *p*-value is 0.21.

no significant effect. However, taking a fairly high but not atypical initial level of such exports in GDP of 30%, a sustained reduction in the world price of 10% would shorten the duration of conflict by 12%. This result gives some support to the rebellion-as-business approach.

The next variants consider those variables that might proxy the expected payoff to victory. Recall that this payoff may be political or financial, and according to the rebellion-as-investment approach, it should increase the duration of conflict. The expected political payoff to victory is proxied by a measure of pre-conflict political repression. If rebellions are commonly struggles to end elite oppression, which is the media image that rebel groups usually portray, it might be expected that the more severe the repression prior to the conflict, the longer would the conflict tend to persist. As a proxy for political repression, the Polity III dataset measure of the openness of the political institutions (Jaggers & Gurr, 1995) is used. The scale runs from 0 (least open) to 10 (most open). In contrast to inequality, this proxy for the political payoff to victory is insignificant (column 4).[21]

The expected financial payoff to victory is proxied by the pre-conflict share of primary commodity exports in GDP. Primary commodity exports are the most readily taxable sector of an economy, and their capture might well be the lure for victory. However, the regression results find that primary commodity exports have no significant effect on the duration of conflict (column 5). The rebellion-as-business approach would suggest that the reason for this is that the plunder of primary commodities does not require victory, but can be conducted during the conflict. Taken together, the absence of significant effects from both political repression and primary commodities cast some doubt upon the key assumption of the rebellion-as-investment approach.

In the reference model, per capita GDP is significant, and we have suggested that one interpretation of this variable is that it is proxying the social cost of conflict. In column 6, we try an alternative variable which can also be seen as proxying these costs, namely, the proportion of young males enrolled in secondary schooling. To the extent that the pertinent opportunity cost is that of rebel recruits, who are drawn predominantly from young males, this proxy may be more accurate than per capita GDP. The coefficient on school enrolment is also significant and positive: societies with higher enrolment rates for males in secondary school have higher hazards of peace and thus shorter expected conflicts. Unfortunately, per capita income and secondary school enrolment are too highly correlated to be included in the same regression, and statistically there is little to choose between them.

In column 7, religious fractionalization is introduced; the proxy is analogous to ethnic fractionalization. The religious diversity measure is based on the *World Christian Encyclopaedia* (Barrett, 1982). Unlike ethnic fractionalization, it has no significant effect, whether entered as a linear or a quadratic relationship. Possibly, religion is less useful than ethnicity as a force for military cohesion.

In column 8, proxies for geography are investigated. Specifically, the extent of forest coverage and the extent of mountainous terrain are investigated. Neither is significant. Evidently, mountains and forests offer safe havens for rebels in necessity. Perhaps, however, the results suggest that in the circumstances where it has proved feasible to escalate a conflict to a substantial scale – so that it is included in our sample – it can be sustained militarily even without favourable geography.

The final variant introduced is international policy interventions (column 9). Four types of intervention can be distinguished, according to whether they are economic or

[21] We have also tested for a quadratic effect of the openness of political institutions. We find no evidence for quadratic effects as the estimated coefficient on openness of political institutions squared is far from significant.

military, and according to whether they are pro-government or pro-rebel. Each intervention is dated by month. This gives the option of treating an intervention as only likely to be effective during that month, or allowing it to have a continuing effect. In the reported results, the latter was assumed although the former was also investigated. Specifically, a measure that cumulates the number of months of each type of intervention from the onset of the war (at time $a$) to each of the periods ($a + 1$, $a + 2, \ldots, a + t$) was constructed. For example, sustained pro-government military assistance is therefore entered not as a constant dummy variable, but with a steadily rising value over time. Hence, our interventions variables are month-by-month time-varying. Since interventions are purposive, the results must be treated with caution. For example, interventions might be targeted on situations that had become militarily critical, so that even if they had no effect, they would be correlated with the proximate subsequent end of conflict. Similarly, interventions might have the intention of aligning the intervening power with whichever side is set to win. In the event, the economic interventions, whether pro-government or pro-rebel, are completely insignificant. It is possible that economic interventions are systematically targeted to the side in danger of defeat, so that the apparent lack of an effect is because these interventions lengthen what would otherwise be conflicts in their last stages. However, a less contrived interpretation is surely that economic interventions have not usually been large enough to have a significant effect. Military intervention on the side of the government is also ineffective. The only intervention that is significant is military intervention on the side of the rebels. A possible interpretation is that with sufficient military support for rebels, government forces can be defeated: for example, the recent US support for the Northern Alliance in Afghanistan had this effect. By contrast, because rebel groups have the option of concealment, military support for the government may not produce a decisive military outcome.

Overall, the results are not decisive between the three different approaches outlined in the theoretical framework above. However, they offer some support for both rebellion-as-mistake and rebellion-as-business, while tending to reject rebellion-as-investment. The two better-supported approaches are not incompatible, and can be synthesized into a theory suggesting that long conflicts may be persistent because they are characterized by a double-bind. First, in long conflicts rebellion is likely to be sustainable as a going concern, as implied by the rebellion-as-business approach. If the gross costs of rebellion to the rebel group are low (low opportunity costs for recruits) and if rebel revenues during conflict are high (high commodity prices), rebels may be better off than prior to the conflict and therefore under little pressure to reach a settlement. This is then compounded by the difficulty of reaching a settlement recognized as mutually advantageous. The potential mutual gains to peace are modest (per capita income is low), relative to the gap between the defeat and victory payoffs (inequality is high).[22]

[22] With respect to the interventions effects, our results are quite different from those reported by Regan (2002), despite the fact that we use his data on interventions. Regan's results 'suggest that third-party interventions tend to extend expected durations rather than shorten them' (Regan, 2002: 55). Using Weibull duration regressions, Regan reports very large positive effects of interventions on the expected duration of war (see Table I in the paper). However, the empirical specification assumes that interventions have contemporaneous effects only, which, with monthly data, we think is too restrictive. Looking closer at Regan's data, it is clear that there are no observations for which an economic intervention, or an intervention opposing the government, coincides with the end of a war in the same month. This explains the very large estimates of the interventions coefficients reported by Regan. If, as seems plausible, the effects of interventions operate with a lag of at least a month, then Regan's specification is likely to give misleading results. Our method of cumulating interventions in order to allow for dynamic effects should be more robust to such timing errors. Further, it is also the case that Regan's sample, which includes 150 conflicts during the period 1945–99, is rather different from ours, which could be another reason why our results differ.

Finally, the implications for policies to shorten conflict are briefly considered. The potential for shortening conflict is partly through operating on the structural variables that characterize countries prior to conflict and partly through operating on the variables that change during conflict. At the structural level, low per capita income and high inequality were identified as lengthening conflict. Hence, the results suggest some support for regarding equitable economic development as a way of reducing the duration of conflict. With regard to the variables that can be changed during conflict, the results can be interpreted as indicating three variable forces for peace: a squeeze on rebel finances (a decline in commodity prices shortens conflict); a more realistic assessment of military prospects (by the seventh year of a conflict, the chances of peace per year have risen); and an exogenous change in the balance of military power (external military support for the rebels shortens conflict). Of these, the first appears to offer the clearest opportunities for international intervention.

## Conclusion

A comprehensive dataset on large-scale violent civil conflicts since 1960 was used to analyse the duration of conflict by means of estimated hazard functions. The duration of conflict is systematically related both to structural conditions prevailing prior to conflict and to circumstances during conflict. The key structural characteristics that lengthen conflict are low per capita income, high inequality and a moderate degree of ethnic division. The key variable characteristics that shorten conflict are a decline in the prices of the primary commodities that the country exports and external military intervention on the side of the rebels. The internal clock of the conflict has relatively little effect on the chances of reaching peace, but the external clock seems to have been important. The chances of peace were much lower in the 1980s and 1990s than they had been previously. It was speculated that this may reflect the easier access of rebel groups to global markets for the sale of plundered commodities and for the purchase of armaments. An attempt was made to relate these results to three different approaches to civil war: rebellion-as-investment, in which the critical incentive is the post-conflict payoff; rebellion-as-business, in which the critical incentive is the payoff during conflict; and rebellion-as-mistake, in which military optimism prevents the recognition of any mutually advantageous settlement. This article suggests that the evidence was incompatible with the first of these approaches but was consistent with the others.

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## Appendix

Table AI. Sample of 55 Wars

| *Country* | *Start of the war* | *End of the war* |
|---|---|---|
| Algeria | 07/62 | 12/62 |
| Algeria | 05/91 | ongoing |
| Angola | 02/61 | 05/91 |
| Angola | 09/92 | ongoing |
| Burma/Myanmar | 68 | 10/80 |
| Burma/Myanmar | 02/83 | 07/95 |
| Burundi | 04/72 | 12/73 |
| Burundi | 08/88 | 08/88 |
| Burundi | 11/91 | ongoing |
| Chad | 03/80 | 08/88 |
| China | 08/66 | 07/69 |
| Columbia | 04/84 | ongoing |
| Cyprus | 07/74 | 08/74 |
| Dominican Republic | 04/65 | 09/65 |
| El Salvador | 10/79 | 01/92 |
| Ethiopia | 07/74 | 05/91 |
| Guatemala | 07/66 | 07/72 |
| Guatemala | 03/78 | 03/84 |
| Guinea-Bissau | 12/62 | 12/74 |
| India | 08/65 | 08/65 |
| India | 84 | 94 |
| Indonesia | 06/75 | 09/82 |
| Iran | 03/74 | 03/75 |
| Iran | 09/78 | 12/79 |
| Iran | 06/81 | 05/82 |
| Iraq | 09/61 | 11/63 |
| Iraq | 07/74 | 03/75 |
| Iraq | 01/85 | 12/92 |
| Jordan | 09/71 | 09/71 |
| Morocco | 10/75 | 11/89 |
| Mozambique | 10/64 | 11/75 |
| Mozambique | 07/76 | 10/92 |
| Nicaragua | 10/78 | 07/79 |
| Nicaragua | 03/82 | 04/90 |
| Nigeria | 01/66 | 01/70 |
| Nigeria | 12/80 | 08/84 |
| Pakistan | 03/71 | 12/71 |
| Pakistan | 01/73 | 07/77 |
| Peru | 03/82 | 12/96 |
| Philippines | 09/72 | 12/96 |
| Romania | 12/89 | 12/89 |
| Rwanda | 11/63 | 02/64 |
| Rwanda | 10/90 | 07/94 |
| Sierra Leone | 03/91 | 11/96 |
| Somalia | 04/82 | 12/92 |
| Sri Lanka | 04/71 | 05/71 |
| Sri Lanka | 07/83 | ongoing |
| Sudan | 07/83 | ongoing |
| Turkey | 07/91 | ongoing |
| Uganda | 05/66 | 06/66 |
| Uganda | 10/80 | 04/88 |
| Yugoslavia | 04/90 | 01/92 |
| Zaire/Dem. Rep. of Congo | 07/60 | 09/65 |
| Zaire/Dem. Rep. of Congo | 09/91 | 12/96 |
| Zimbabwe | 12/72 | 12/79 |

Table AII. Summary Statistics: Means and Standard Deviations

| | *All wars, N = 55* | *Wars ≤ 48 months, N = 22* | *Wars > 48 months, N = 33* |
|---|---|---|---|
| Income inequality (Gini coefficient) | 40.7 | 36.3 | 44.51 |
| | (8.9) | (7.0) | (8.8) |
| Per capita income, 1985 constant US$ | 1707 | 2202 | 1376 |
| | (1411) | (1759) | (1022) |
| Male secondary school enrolment rates (%) | 29.2 | 35.7 | 24.9 |
| | (23.3) | (25.8) | (20.7) |
| Primary commodity exports/GDP | 0.15 | 0.14 | 0.15 |
| | (0.13) | (0.12) | (0.14) |
| Commodity price index (CPI) | 1.28 | 1.10 | 1.38 |
| | (0.64) | (0.27) | (0.76) |
| Democracy, index 1–10 | 1.97 | 2.1 | 1.9 |
| | (3.2) | (3.4) | (3.1) |
| Ethnic fractionalization, index 1–100 | 51.9 | 42.3 | 58.3 |
| | (29.2) | (31.4) | (26.2) |
| Religious fractionalization, index 1–100 | 35.5 | 31.4 | 38.3 |
| | (24.8) | (24.4) | (25.1) |
| ln population | 16.48 | 16.41 | 16.53 |
| | (1.47) | (1.72) | (1.31) |
| Mountainous terrain (%) | 24.5 | 28.6 | 21.7 |
| | (23.4) | (25.1) | (22.1) |
| Forest coverage (%) | 23.6 | 13.8 | 30.1 |
| | (18.8) | (11.0) | (20.2) |
| Economic pro-government interventions (months) | 0.42 | | |
| | (1.08) | | |
| Economic anti-government interventions (months) | 0.15 | | |
| | (0.52) | | |
| Military pro-government interventions (months) | 1.84 | | |
| | (3.03) | | |
| Military anti-government interventions (months) | 1.71 | | |
| | (3.07) | | |

Standard deviations in parentheses.

*Data Sources*

**Commodity Price Index** Weighted export price series compiled by Jan Dehn, based on the methodology developed in Deaton & Miller (1996). Source: Dehn (2000).

**Democracy** The degree of openness of democratic institutions is measured on a scale of 0 (low) to 10 (high). Source: http://www.cidcm.umd.edu/polity/index.html. The data are described in Jaggers & Gurr (1995).

**Ethno-linguistic and Religious Fractionalization** We proxy social fractionalization in a combined measure of ethnic and religious fractionalization. Ethnic fractionalization is measured by the ethno-linguistic fractionalization index. It measures the probability that two randomly drawn individuals from a given country do not speak the same language. Data are available only for 1960. In the economics literature, this measure was first used by Mauro (1995). Using data from Barrett (1982) on religious affiliations, we constructed an analogous

religious fractionalization index. Following Barro (1997), we aggregated the various religious affiliations into nine categories: Catholic, Protestant, Muslim, Jew, Hindu, Buddhist, Eastern Religions (other than Buddhist), Indigenous Religions and no religious affiliation.

The fractionalization indices range from 0 to 100. A value of 0 indicates that the society is completely homogenous whereas a value of 100 would characterize a completely heterogeneous society.

We calculated our social fractionalization index as the product of the ethno-linguistic fractionalization and the religious fractionalization index plus the ethno-linguistic or the religious fractionalization index, whichever is the greater. By adding either index, we avoid classifying a country as homogenous (a value of 0) if the country is ethnically homogenous but religiously diverse, or vice versa.

**Forest Coverage** We used the FAO measure of the proportion of a country's terrain which is covered in woods and forest. Source: http://www.fao.org/forestry.

**GDP Per Capita** We measure income as real PPP adjusted GDP per capita. The primary dataset is the Penn World Tables 5.6 (Summers & Heston, 1991). Since the data are available only from 1960–92, we used the growth rates of real PPP adjusted GDP per capita data from the World Development Indicators (World Bank, 1998) in order to obtain income data for the 1990s.

**Income Inequality** Income Inequality is measured by the Gini coefficient. Source: Deininger & Squire (1996).

**Interventions** We used Patrick Regan's data on interventions. He defines third-party interventions in intrastate conflicts as 'convention breaking military and/or economic activities in the internal affairs of a foreign country targeted at the authority structures of the government with the aim of affecting the balance of power between the government and opposition forces'. Economic interventions include: grants, loans, non-military equipment or expertise, credits, relieve past obligations and economic sanctions; and military interventions include: troops, naval forces, equipment or aid, intelligence or advisers, air support and military sanctions. Furthermore, he distinguishes between different targets, in favour of the government, in favour of the opposition and neutral interventions. Data can be downloaded from http://bingweb.binghamton.edu/~pregan/.

**Male Secondary School Enrolment Rates** We measure male secondary school enrolment rates as gross enrolment ratios, that is, the ratio of total enrolment, regardless of age, to the population of the age group that officially corresponds to the level of education shown. Secondary education completes the provision of basic education that began at the primary level and aims at laying the foundations for lifelong learning and human development by offering more subject- or skill-oriented instruction using more specialized teachers. Source: World Bank (1998).

**Population** Population measures the total population. The data source is World Bank (1998). Again, we measure population at the beginning of each subperiod.

**Primary Commodity Exports/GDP** The ratio of primary commodity exports to GDP proxies the abundance of natural resources. The data on primary commodity exports as well as GDP were obtained from the World Bank. Export and GDP data are measured in current US dollars.

**War Duration** A civil war is defined as an internal conflict in which at least 1,000 battle-related deaths (civilian and military) occurred per year. We use mainly the data collected by Singer & Small (1994), and according to their definitions (Small & Singer, 1982) we updated their dataset for 1992–99.

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