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Aid for War?

An Empirical Assessment of Donor Interventions in Civil Wars

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Abstract. Since 1960 foreign aid to countries with civil war has exceeded \$300 billion in Sub-Saharan Africa alone. Using a global panel of more than 150 civil wars between 1945 and 2000, this paper evaluates the impact of diplomatic, military and economic interventions on conflict duration. Moreover, specific conditions are identified under which foreign interventions work best. The results suggest that diplomatic and military interventions can shorten civil wars, while economic interventions do not affect the duration of civil wars but may play a conditioning role. The findings are obtained by fitting a proportional hazard model for time-varying covariates. They are confirmed using a logit model, which is then expanded for additional robustness checks.

JEL Classification:

Keywords: Aid, Civil War, Interventions, Duration Analysis, Cox, Logit, Heterogeneity

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1 Introduction

Over the last decade the effectiveness of aid flows to poor countries has come under extensive scrutiny. Yet, one field of economic assistance remains largely unexplored – aid to conflict countries. This lack of research is surprising, given that since the 1960s more than \$300 billion in aid went to civil war countries in Sub-Saharan Africa alone (Fitzpatrick et al. 2007). Since conflicts have an enormous effect on the human, social and economic life it is highly unlikely that any type of assistance is given to a civil war country without considering how the assistance will affect the conflict (Lindgren 2004).

This makes clear that economic interventions, just like other forms of foreign intervention, can be assessed in terms of their impact on the conflict. In particular, when looking at the available forms of foreign interventions, what are their effects on the duration of civil wars? Moreover, under what circumstances do they work best?

Foreign interventions are commonly categorized into economic, military and diplomatic. First, economic interventions describe material support, mainly in form of financial aid, but they include food and other non-military goods as well. On top, they include economic sanctions. Second, military interventions include anything from sharing intelligence over logistic support to sending military officers, but they also contain weapon embargoes. Third, diplomatic interventions describe mediation efforts by third parties as an attempt to bring the conflicting parties on one table. Mediation is conceptually different from other forms of intervention. While military and economic involvement can affect the different parties' ability of victory by weakening or strengthening them, diplomatic intervention seeks to avoid a clear victory and usually requires both sides to forgo certain objectives.

In order to assess the impact of diplomatic, military and economic interventions a panel of more than 150 civil wars between 1945 and 2000 is used. The results suggest that diplomatic and military interventions can shorten civil wars, while economic interventions do not seem to have any impact on the duration of civil wars. The findings are obtained by fitting a proportional hazard model for time-varying covariates. They are confirmed using a logit model, which is then expanded for additional robustness checks.

The next section provides an overview of the empirical knowledge regarding conflict interventions and the ‘anatomy’ of civil war. It concludes with a discussion of variables that have been identified to influence the duration of civil war and might also affect the impact of foreign interventions. Section three explains the data and the econometric methodology that is used. In section four the results are presented, followed by the concluding section five.

2 Empirical Literature on Civil War

Academic interest in civil conflicts has increased substantially during the last years. It focussed on two main areas. First, and most prominently, scholars have tried to identify the causes of civil wars in an attempt to facilitate conflict prevention. A second and much smaller group is investigating the role of foreign interventions in terms of accelerated conflict resolution.

Although this study belongs to the latter faction concerned with the effectiveness of foreign interventions it is worth looking at the reasons leading to civil war, as they might be crucial when it comes to the effectiveness of foreign interventions. Therefore the next section presents the main empirical findings concerning the roots of civil conflict with a focus on the relevant explanatory variables. It is followed by an overview of the quantitative studies regarding foreign interventions. The section concludes with a summary of variables which are potentially related to the success of foreign interventions.

2.1 The Cause of Civil War

The empirical findings regarding the cause of civil war can be broadly categorised into a political and an economic line of argument which contradict each other. The former relates to the ability of political participation and self-determination. According to this reasoning civil wars are more likely to erupt in countries with lower levels of democratic rule (Reynal-Querol 2002; Henderson and Singer 2000) or when there are dominant ethnic or religious groups (De Soysa 2002).

The economic line of argument holds that the onset of civil wars depends on a cost benefit analysis by the rebels. This is expressed by the trade-off between their ability to finance an insurgency and the opportunity cost of fighting (Collier and

Hoeffler 2001, p. 16). First, the authors argue that natural resources are used for financing purposes, which is supported by their results. Ross (2002) refines this assertion with an empirical study where he shows that only easily extractable resources, namely narcotics and gemstones, are significantly related to the onset of civil war. Second, higher opportunity costs make the beginning of civil war more unlikely. Opportunity costs, which are interpreted as forgone earnings, are positively associated with GDP per capita (Collier and Hoeffler 1998, 2001; Reed 2000), economic growth (Collier and Hoeffler 2001; Miguel, Satyanath and Sergenti 2004) and male secondary enrolment (Collier and Hoeffler 2001).

Most of these studies prove each other wrong. Thus, at best the results are mixed, but one might be tempted to call the overall findings quite contradictory. Either way, the different results of these studies suggest that the interaction of political and economic factors is not straightforward and might be sensitive to model specifications. Three additional papers exist, however, which might help explain the mixed results. First, Hegre and Sambanis (2006) replicate some of the most cited papers and find that the definition of conflict makes a big difference. While most datasets do not capture conflicts with less than 1000 casualties per year some scholars are using thresholds as low as 200 casualties in total. This primarily affects the significance of ethnic diversity which tends to be more important in low scale conflicts.

A second quantitative study which reconciles the mixed findings from above is a recent paper by Buhaug (2006). He includes a new dichotomy of conflicts – the objective of the insurgents – which seem to be determined by very different causal mechanisms. In territorial conflicts the rebels' objective is secession, while in governmental conflicts the opposition is trying to overthrow the ruling government via a *coup d'état* or a revolution. There are four main results. First, Buhaug's findings

suggest that secessions are typical for large and ethnically diverse countries, while coups and revolutions are predominantly attempted in smaller and more homogenous societies. Second, the risk of a secessionist movement tends to increase with institutional and economic stability, both indicators of a superior political environment. Governmental conflicts on the other hand are more likely in poor countries with inconsistent policies and resource-dependent regimes. Third, the paper also sheds some light on the mixed findings concerning the role of democracy. In more democratic countries governmental conflicts are highly unlikely since governments replace themselves. The contrary is true for secessionist movements which often find themselves discriminated by a democratically ruling majority. In that case, democracy can increase the risk of civil war. Four, the author finds that studies which do not account for the objectives of the rebel groups are likely to underestimate the impact of ethnic diversity. That is because in total governmental conflicts are more common than territorial disputes, however ethnic diversity does not have a significant impact on the risk of governmental conflicts. Thus, controlling for rebel objective provides a nuanced picture concerning the role of democracy, ethnic diversity, and GDP and seems to explain the conflicting findings of previous studies.

The third and last theory which reconciles the mixed findings above is provided by the protagonists of the economic reasoning. Using an improved version of the original dataset Collier, Hoeffler and Rohner (2006) conclude that neither political nor economic motivation can explain the onset of civil war. Instead, their results indicate that civil war is a matter of feasibility and will take place whenever possible. The authors find that insurgency becomes more feasible when geographic conditions enhance guerrilla fighting, something also identified by Fearon and Laitin (2003). Furthermore Collier, Hoeffler and Rohner find that conflict becomes more likely when

the proportion of young men is higher in a society, since it might foster recruitment. And lastly, civil war is more common when government forces cannot draw on military support of their former colonial rulers. The last point is in line with the findings of Reed (2002), who identified the relative power structure of the warring factions as the main indicator of domestic conflict. The results of Grieco (2001) also support the feasibility theory. His findings indicate that a recent conflict makes countries more prone to renewed violence, since they can build on existing organisational and armament structures.

To sum up this section, various studies suggest three types of variables to be related to the onset of civil war. The first type underlines the importance of economic motivation and includes income, economic growth, natural resources, and male secondary enrolment. The second group of variables could be labelled political motivation and contains the level of democratic rule, the degree of ethnic and religious polarization and the objective of the insurgents. The third group contains variables in favour of the feasibility theory. It consists of the geographic conditions, the share of young men in the conflict country, the relative power structure of the government and insurgents, and the number of recent conflicts. Besides, the results seem to depend on the fatality threshold used to define conflict.

2.2 Conflict Intervention

While the study of conflict onset with its policy implications for conflict prevention often enjoys priority given the human, social and economic cost associated with war (Lindgren 2004), prevention is evidently not always taking place. For those cases the question remains how to bring a conflict to an end once it has started. In particular, what can foreign actors do in the face of civil war?

Studies on diplomatic intervention often apply game theoretic bargaining models (Wagner 2000) or psychological concepts (Carnevale and Choi 2000) to demonstrate how mediation works.¹ Empirical work in this field is rather scarce, with Fearon (1994) being a rare exception. In his effort to test a number of conflicting theories the author finds that the effectiveness of diplomacy depends on the mediator's ability to lower the transaction costs for the conflicting parties of obtaining information about each other. Such information includes military abilities, the detailed objectives of the parties, and most importantly the available range for negotiations. Despite this work most studies on mediation are of qualitative nature and occasionally seem to oversell diplomacy as a panacea for conflict resolution (Kaufmann 1996; Luttwak 1999).

Military interventions have received only modest empirical attention. Despite substantial theoretical work little is known about when and if military force can be used to bring civil war to an end. An exception is the panel study by Balch-Lindsay and Enterline (2000). Covering military interventions in civil wars between 1820 and 1992 their results indicate the importance of the geopolitical context. Interventions during the Cold War for instance might have been working differently, not only because of external circumstances but also because of different intentions of the intervening country. Besides, the authors point out that military interventions, when supporting opposing parties, can lead to a military deadlock and consequently prolong civil war.

The literature on economic interventions is twofold, consisting of studies of economic sanctions and a much smaller part focussing on the effect of economic aid to countries in civil war. According to theory sanctions impose economic costs on the conflicting parties, ideally inducing a change in their behaviour (Drury 2001, p. 488). However, using cross-sectional analysis several empirical studies find little support of

¹ The term mediation is used synonymously with diplomacy.

the effectiveness of economic sanctions (Drury 1998; Pape 1997, 1998), a conclusion that is also underlined by microeconomic theory (Morgan and Bapat 2003). In turn, very little is known about the effect of financial flows to warring parties, although the World Bank has initiated an evaluation of its lending to “low income countries under stress”, which includes several countries affected by civil war (World Bank 2004).

To date, the Binghamton Center on Democratic Performance offers the only known dataset of combined diplomatic, economic and military interventions (Regan 2000).² Using this panel Regan and Aydin (2006) find that economic and military interventions are counterproductive in speeding up conflict resolution. Only diplomatic involvement seems to be able to shorten conflicts. However, economic interventions show a shortening effect when combined with diplomatic efforts. The study indicates that several variables influence the result of interventions. These include ethno-religious identity as a reason of fighting, the troop size of the opposition forces, the intensity of fighting in terms of total casualties, and the existence of opiates in the country, which are already identified in the previous section.

To come back to the original question what foreign actors can do, the international community – according to the empirical findings so far – is well advised to focus primarily on diplomatic interventions, which can be combined with economic aid, while both economic sanctions and military initiatives are largely ineffective in bringing civil wars to an end.

2.3 Relevant Variables

This section provided an overview of the empirical findings regarding the causes of civil war and the effectiveness of foreign interventions. The following list provides an overview of the variables that have been identified to affect either the onset of civil

² For a comprehensive overview of conflict related datasets see Eck (2005).

war or the success of interventions. Of course effective conflict resolution may not entirely depend on the same characteristics as the beginning of conflict, yet they are likely to be related (Elbadawi and Sambanis 2000). Since these variables provide the basis for the methodological discussion in the next section it is useful to categorize them into political, economic and military aspects. Variables in support of the feasibility theory are allocated with military aspects, since they are related to the ability to fight.

Table 1 Overview of Relevant Variables

<i>Political Aspects</i>	<i>Economic Aspects</i>	<i>Military Aspects</i>
<ul style="list-style-type: none"> • Level of democratic rule • Degree of ethnic and religious polarization • Objective of insurgents • Geopolitical context, i.e. Cold War 	<ul style="list-style-type: none"> • GDP per Capita • GDP per capita growth • Natural resources, i.e. gemstones and narcotics • Male secondary enrolment 	<ul style="list-style-type: none"> • Geographic conditions, i.e. proportion of mountainous terrain • Share of young men in the conflict country, i.e. potential recruits • Relative power structure of the government and insurgents • Military stalemate, i.e. balance of opposing military interventions • Repeated conflict
<p>Other</p> <ul style="list-style-type: none"> • Fatality threshold of the operational definition of conflict 		

Note: For the categorisation of these variables refer to the discussion above (Section 2). Other variables that one might expect here are not listed because they showed up as insignificant in all the studies mentioned in this section. This affects among others income inequality and diaspora contributions (see for example Collier, Hoeffler (2001)).

3 Data and Research Design

This section commences by briefly introducing the variables used for the statistical analysis. It proceeds by presenting two models frequently used for duration analysis and a number of relating robustness checks.

3.1 Data

The main dataset comes from Regan and Aydin (2006) in a form of an unbalanced panel containing 12,573 conflict months covering 153 civil wars between 1946 and 2000.³ Conflicts are defined by a relatively low fatality threshold of 200 deaths during the entire conflict. The outcome variable is a dummy coded 1 if the conflict ended in a particular month and zero otherwise. Conflict end, or peace, is defined as the onset of a period of at least six months without any casualties. Most of the preferred variables can be found here, including whether the conflict took place during the Cold War, GDP per capita⁴ and its growth, the presence of gemstones and opiates, the troop size of the opposition forces, the number of opposing military interventions, the number civil wars since World War II as a proxy of repeated conflict and of course diplomatic, military interventions as well as economic sanctions and aid. On top, the dataset includes the direction of the interventions, i.e. whether it is neutral, in support of the government, or the opposition.

The remaining variables are taken from Buhaug (2006).⁵ This concerns the objective of the insurgents, the portion of mountainous terrain in the affected country, and oil as another natural resource. Two variables remain missing, the share of young

³ An extensive description of the coding of variables can be found at http://bingweb.binghamton.edu/~pregan/replication_data.html. Last accessed on March 20, 2007.

⁴ To reduce endogeneity bias GDP is lagged by one year. Most missing values for GDP per capita are amended using data of the World Development Indicators 2005 (World Bank 2006). For the remaining years where no GDP data is available linear intra- and extrapolation is used to complete the variable.

⁵ The original data and a description of the coding can be found at: <http://www.prio.no/jpr/datasets>. Last accessed on March 20, 2007.

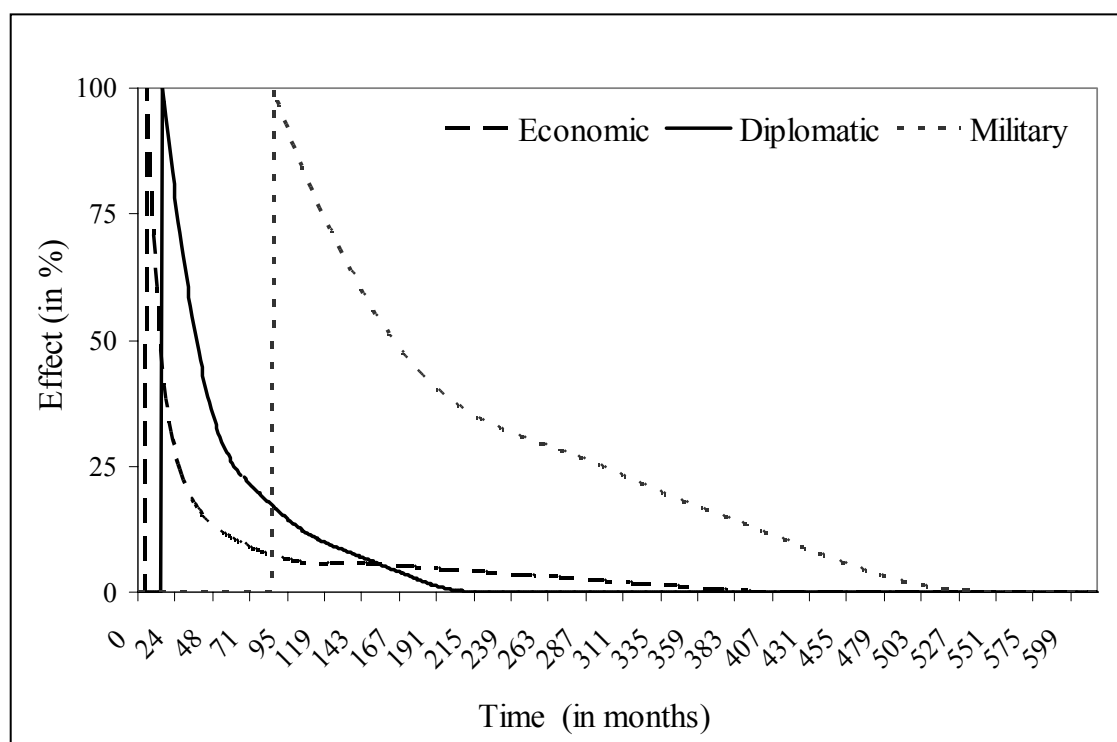
men in the conflict country and male secondary enrolment.⁶ First, the share of young men in the conflict country is used as a proxy for the ability of the rebels to recruit (Collier, Hoeffler and Rohner 2006, p. 15). Since the actual troop size of the rebels is available in this dataset there is no need to use enrolment. Second, male secondary enrolment is initially used as a proxy for expected future income and is meant to account for income forgone during future conflict (Collier and Hoeffler 2001, p. 4). Since this study is concerned with conflicts that have already started, future income prospects are better proxied by current (or lagged) GDP per capita. This is regardless of whether the war ends or not, given that it is unlikely that a post-conflict economy recovers quickly (Collier and Hoeffler 2002).

One remaining issue relates to the coding of interventions. In the original dataset foreign interventions are dummies, coded 1 in the month in which they occur and zero otherwise. However, it is plausible to assume that the effect of interventions lasts beyond the first month in which they take place and slowly decreases over time (Regan and Aydin 2006). It is also possible that the effect of interventions does not set in immediately, but only after some time has passed. To account for this ‘lagged decay function’ of foreign interventions their effect is estimated for all possible time lags, beginning from zero up to 600 months. The lagged estimates represent the effect of the intervention for the respective lag. When plotted against time the coefficients follow a decay function, which can be used to model the total effect of the interventions. These estimated lagged decay functions are adjusted by setting their mode equal to one. This is important for comparison and interpretation, since it implies that during the month when an intervention fully unfolds the effect equals a unit change. The resulting estimates represent the effect of an intervention at time t more accurately. Figure 1 shows the

⁶ Potentially both variables might be available from national censuses, which would be preferable over the theoretical exclusion of this paragraph.

lagged decay functions for the three intervention types. They are very similar in their functional form, although military interventions seem to persist longer. On average, economic interventions unfold fully after 6 months. For diplomatic and military interventions the lag is 16 and 86 months, respectively. The relatively long delay of military interventions is somewhat surprising since one might expect that military interventions show an immediate effect in military confrontations. This is an important finding since, regardless of the direction of their effect military interventions do not bring change about quickly.

Figure 1. Effect of Foreign Interventions in Civil Wars, 1945-2000



Source: Author's calculations

3.2 Cox Model

Once the data has been explained the methodology can be introduced that will be used to investigate by how much the duration of civil wars can be altered through foreign interventions. The models used here belong to the field of duration analysis,

where the duration of conflicts is modelled as a stochastic process in which the outcome of each conflict month is a dichotomous event – either peace or war.

Consider a random variable $T_i > 0$ indicating the duration of a civil war i , where t is the realization of T_i . The distribution function of the duration is given by $F(t)$, where

$$F(t) = \int_0^t f(s)ds = \Pr(T_i \leq t) \text{ for } t > 0. \quad (1)$$

This function can be used to determine the probability that a war lasts at least t months by setting up the survival function, defined as

$$S(t) = 1 - F(t). \quad (2)$$

Yet, the survival function doesn't quite capture what this study is concerned with, namely the probability of a conflict to end at time t , given of course that it has lasted that long. This is best expressed by the hazard function or hazard rate, given by the ratio of density and survival function

$$h(t) = \frac{f(t)}{S(t)}. \quad (3)$$

The hazard rate can be modelled as a function of the independent variables, which yields the following proportional hazards model

$$h(t, x_i) = h_0(t) \exp(x_i' \beta), \quad i = 1, \dots, N \quad t = 1, \dots, T \quad (4)$$

where the dependent variable is determined by a baseline hazard $h_0(t)$ and a vector of explanatory variables x_i . The baseline hazard describes how time affects the probability of peace in the absence of any characteristics, i.e. when all explanatory variables are set zero. Since a parametric estimation of the coefficients requires prior knowledge of the baseline hazard, it is natural to begin with a semi-parametric Cox model (Cox, 1972). It has the advantage of not requiring any specifications about the shape of the baseline hazard.

However, also the Cox model comes with a constraint, namely the proportional hazard (PH) assumption. Following equation (3), any change in the conflict characteristics x_i might affect the hazard function $h(t, x_i)$. The PH assumption says that

the effect of a unit change of the explanatory variables on the hazard function does not change over time. As a result, the different probability of peace of two conflicts will be proportional at any time t and can be directly attributed to the difference in their characteristics x_i .

When the PH assumption is violated, the multiplicative effect of a unit change of the explanatory variables is not constant over time and as a result the estimates can be biased. In particular, when the true hazard ratio is increasing (decreasing) over time the estimate will be biased upward (downward) (Box-Steffensmeier, Reiter and Zorn 2003, p. 35).⁷ If the true hazard ratio converges towards zero over time the estimate will also be biased towards zero.

Several tests exist regarding the violation of the proportional hazard assumption. One statistic with favourable characteristics was introduced by Schoenfeld (1982). Rearranging (4) yields equation (5), where the estimate β is independent of time and thus constant throughout the conflict. This assumed property is used for the null hypothesis of the Schoenfeld test. Its rejection implies non-proportional hazards.

$$\frac{h(t, x_i)}{h_0(t)} = \exp(x_i' \beta), \quad i = 1, \dots, N \quad t = 1, \dots, T \quad (5)$$

Some remedies exist in case of non-proportional hazards. They are directly related to how conflict characteristics, expressed by the explanatory variables, affect conflict duration. As such, the following four points are very useful in revealing the complexity of civil wars, since these modifications help explain under which conditions the PH assumption holds.

First, some variables might be affected by what is called duration dependence. For example, it could be that during the early months of fighting natural resources such as opiates or precious minerals have little effect on the expected duration, since they are

⁷ For a definition of the hazard ratio, which is not to be confused with the hazard rate, see below in section 4.2.

not yet being used for financing. Over time, however, when other financial means become relatively scarce, the existence of natural resources might substantially prolong conflict duration by providing income sources to the insurgents or paramilitaries (Collier, Hoeffler and Rohner 2006, p. 5). According to this theory the effect of natural resources on the expected duration increases over time. When a PH test is rejected for a particular variable it is often due to duration dependence (Box-Steffensmeier, Reiter and Zorn 2003, p. 36). A simple way to control for it is by fitting the model after multiplying the affected variable by a function of time, such that

$$\ln(h(t, x_i)) = \ln(h_0(t)) + x_i' \beta + f(t) v_i' \delta, \quad i = 1, \dots, N \quad t = 1, \dots, T \quad (6)$$

where v_i contains the duration dependent covariates and δ is their scalar of coefficients. Because of the interaction with time the changing effect of v_i is captured by δ . Otherwise the parameter estimates would be biased. The decision over which function to use for $f(t)$ depends on how the effect of v_i changes over time. In most applications the variable is either interacted with time to account for a linear change in the log hazard ratio, or it is interacted with the log of time to account for some convergence of the log hazard ratio.

Second, the proportional hazard assumption might be violated because hazards are different within distinct time intervals. For instance, the explanatory variables might affect the probability of peace differently during early periods of conflict (Zartman 1985). To see if that is the case, the model can be estimated separately for different time intervals (or pieces). The underlying theory for such piecewise estimation can be quite similar to that of duration dependence. However, here the contribution to the probability of peace is assumed to be constant within each interval, while it is a function of time when accounting for duration dependence. The cutting points for a piecewise estimation

need to be chosen somewhat arbitrarily. However, a graph of the hazard function can guide the decision.

Third, it is possible that the data contain two (or more) distinct types of conflict. These types may differ in how interventions affect the probability of peace in their respective conflicts. When estimated simultaneously the hazards may not be proportional, even if the conflict type is controlled for (Kalbfleisch and Prentice 1980). The solution lies in separate estimation. For instance, if one suspects territorial conflicts to be different from governmental conflict the Cox model should be estimated separately for both types. If these two conflict types differ indeed in how interventions and other characteristics affect the probability of peace one would expect different coefficients for both types, and possibly proportional hazards within each group. Separate estimation is not limited to conflict types like the objective of the insurgents but can be done with any categorical variable, especially when it has failed to satisfy the PH test (Jenkins 1995).

Fourth, stratified estimation is yet another approach, and is indeed very similar to separate estimation. Again, the model is estimated along conflict types (or any other categorical variable such as natural resources, short vs. long total conflict duration, etc.). However, stratified estimation assumes identical contributions of the explanatory variables to the hazard in each conflict subset. Only the baseline hazard is allowed to differ between the sets. As such, stratified estimation is more restrictive than separate estimation because the latter allows for different baseline hazards and different coefficients in the data subsets. Therefore, separate estimation provides a good check for the validity of stratified estimation. Only if the coefficients are similar within subsets stratified estimation is sensible.

Using these four modifications individually and in combination can help to make sure the PH assumption is satisfied. However, there are a few more issues concerning the robustness of the estimates which will be addressed in the following section.

3.3 Logit Model

An alternative and useful complement to proportional hazard Cox models is the logit model. It comes with some advantages over the model used so far. First, the logit model assumes discrete time intervals, which actually describes the present data better than the continuous time assumption posed by the Cox duration models. Although time runs continuously, events are recorded discretely in monthly intervals, which in principle may have some minor effects on the estimates when there are only few time intervals (Meyer 1995).⁸ Second, the logit model does not require proportional hazards, which makes model specification more flexible. This is of particular advantage in this study with its focus on time-varying foreign interventions. Third, the baseline hazard can be identified relatively easy by using a set of dummies for each failure time period (Sueyoshi 1995). Therefore, the logit approach is a suitable method to use in addition to the Cox model. Furthermore, modifications of the logit model can be used to test for the frequent problem of unobserved heterogeneity, which is defined as a vector of missing variables which act through the error term. This issue will be more thoroughly addressed below.

To begin with the standard logit model, consider N civil wars that are observed over a maximum of T time periods. The model can then be formulated as

$$\Pr(y_{it} = 1) = \frac{\exp[\beta' X_{it}]}{1 + \exp[\beta' X_{it}]}, \quad i = 1, \dots, N \quad t = 1, \dots, T \quad (7)$$

⁸ While this may affect the Cox estimates when it comes to short conflicts the results show little difference between the Cox and the Logit model – an indication that the discrete coding does not affect the results of the Cox model.

where the dependent variable represents the probability that a conflict i comes to peace at time t ; X_{it} denotes the set of explanatory variables that influence the probability and D_{it} represents the baseline hazard.

The logit model also comes with some critique. As such the first point is related to the increased sample size. Following Jenkins (1995) the dataset needs to be reorganized to allow one observation for each period for each conflict. This changes the dataset with 153 conflict observations to an unbalanced panel with 12,573 conflict-month observations. However, using available computational resources the increased sample size does not pose a problem. Besides, the transformation is also needed to estimate the Cox model to account for the time-varying covariates. A second critique relates to the assumed independence of subsequent conflict months. In the logit estimation each conflict month is treated as an independent draw, i.e. the absence of duration dependence is assumed. However, correlation over time is quite possible, which makes it necessary to address the matter. Essentially, duration dependence can be treated as unobserved heterogeneity which will be controlled for below by the estimation of fixed and random effects logit models.⁹

The issue with unobserved heterogeneity goes a bit further and also affects the Cox model. The reason is that there is a multitude of possible explanatory variables that might be somehow related to conflict duration and interventions. However, only a handful of these possible predictors are available in the current dataset and many decision making processes are not measured and would require huge data collection

⁹ In principle duration dependence may be controlled for by interacting the affected variables with a function of time, just as it is done above when controlling for duration dependence in the Cox model. However, it is not clear how to identify variables that are duration dependent other than using the Schoenfeld test results from the Cox model as an indication.

efforts to be proxied adequately. This lack of potentially relevant variables might cause some unobserved heterogeneity, which can lead to too small standard errors of the parameter estimates and subsequently to overly narrow confidence bands. The estimation is nevertheless consistent, i.e. the parameter estimates are not biased in case of unobserved heterogeneity, as long as the sample size is large (Robinson 1982, p. 29).

A common method to deal with unobserved heterogeneity is fixed effects regression. It exploits variation within individuals, specifically the variation of variables across the observations of each conflict. By doing so conflict fixed effects regression controls for omitted variables that might cause unobserved heterogeneity. Fixed effects regression has the additional advantage of controlling for endogeneity, which occurs when explanatory variables are (partly) determined by other covariates in the model or omitted variables. Here the selection process of foreign interventions might be source of endogeneity. For instance, it is plausible to assume that the decision to intervene is influenced by ethnic kin, colonial history, or other time invariant factors. Besides, these variables might also directly affect conflict duration and thus be a source of unobserved heterogeneity. Fixed effects regression controls for these unmeasured factors by only exploiting time-varying covariates in the data. The logit fixed effects model can be written as

$$\Pr(y_{it} = 1) = \frac{\exp[\beta' X_{it} + \alpha_i]}{1 + \exp[\beta' X_{it} + \alpha_i]}, \quad i = 1, \dots, N \quad t = 1, \dots, T \quad (8)$$

where the left-hand side depends on a vector β of common structural variables and a unit specific incidental parameter α_i . Chamberlain (1980) provides the statistic $\sum_{t=1}^T y_{it}$ to consistently estimate the structural parameters when incidental parameters are present.¹⁰

¹⁰ For a discussion of this conditional and alternative unconditional statistics for fixed effect logit estimation see Katz (2001).

Fixed effect regression is quite useful here, because it can provide robust estimates for time varying covariates such as conflict interventions. However, more importantly, it is a convenient tool to test for homogeneity across conflicts. Following Jenkins (1995), a Hausman test can be used to test for the joint statistical significance of the fixed effects by comparing the results for the fixed effects model to those of a regular pooled logit model with a constant baseline hazard. Under the null hypothesis both models are correct but the fixed effects model leads to inefficient estimates, while under the alternative hypothesis only fixed effects estimation is consistent. In other words, rejecting the null hypothesis would provide evidence of unobserved heterogeneity.

Alternatively, one could treat the omitted individual effects as randomly distributed, similar to linear random effects models. Following Greene (1997) the logit random effects model can be formulated as

$$\Pr(y_{it} = 1) = \frac{\exp[\beta' X_{it} + u_i]}{1 + \exp[\beta' X_{it} + u_i]}, \quad i = 1, \dots, N \quad t = 1, \dots, T \quad (9)$$

where u_i is also normally distributed with zero mean. Its variance is defined as

$$\sigma_u^2 = \frac{\rho}{1 - \rho}, \text{ where } \rho \text{ indicates a correlation of the omitted variable over time.}$$

As such the random effects model can be used to test for duration dependence, which can be rejected when ρ equals zero. More generally speaking, this also provides a test for neglected heterogeneity. Whenever ρ is zero, so will be σ_u^2 and subsequently u_i . Thus, when ρ is not significantly different from zero conflict specific unobserved random heterogeneity can be rejected.

4 Empirical Results

4.1 Relevant Variables

Using stepwise introduction the available variables mentioned in section two are tested for significance. As a result, several variables are barred from the succeeding regressions. The excluded variables are the level of democratic rule, the degree of ethnic and religious polarization, GDP per capita growth, the proportion of mountainous terrain, the relative power structure of the government and insurgents, military stalemate, i.e. balance of opposing military interventions, and the number of previous civil wars since World War II. On top, also crude oil reserves, as expressed by more than 1/3 in export revenues coming from oil, showed to have no effect on the probability of civil war. These variables are from all three theoretical fields described in the second section – political, military and economic.

The remaining set of variables is made up from a dummy indicating the Cold War, a dummy coded one for territorial conflicts and zero for governmental conflicts, monthly GDP per capita in 1995 US\$ lagged by one year, two dummies representing the presence of opiates and gemstones, the mean monthly casualties, and the various interventions.¹¹ Economic interventions contain sanctions and aid, unless at least one of them is significant individually, in which case they are reported separately. The direction of the interventions, i.e. whether supporting the government or the opposition, is also found to be insignificant and is therefore not included below. The following sections present the results of the Cox model and the logit model specifications. Note that a description of the estimation procedure can be found in Appendix VI.

¹¹ Cold War is operationally defined to have ended on December 31, 1989. This means conflicts which were ongoing while the Cold War ended appear in both categories (Cold War and past Cold War). This affects some of the longer conflicts. Alternatively, civil wars are coded as Cold War conflicts throughout their duration if they started during the Cold War. This has only small effects on the results and does not change the direction of the significant coefficients.

4.2 Cox Model

Following the variables selection, the impact of economic, military and diplomatic interventions and other control variables can now be estimated. Using a Cox model Appendices I-III present the exponentiated estimates which can be interpreted as hazard ratios. The hazard ratios are obtained by taking the ratio of the hazard function for different levels of the explanatory variables (equation 10). In order to obtain the hazard ratio of a unit increase the ratio is calculated for covariate levels of zero and unity,

$$\text{hazard ratio}(x,t) = \frac{h(t, x_1, \beta)}{h(t, x_0, \beta)}. \quad (10)$$

Substituting the right hand side with equation 1 yields

$$\text{hazard ratio}(x,t) = \exp(\beta(x_1 - x_0)), \quad (11)$$

which can be easily interpreted. When x increases by one unit the hazard ratio describes by how much the probability of peace increases at time t . For dummy variables the hazard ratio can be interpreted as the relative probability of peace between the two categories.

Column one of Appendix I presents the estimates when none of the covariates are interacted with time. The results suggest a positive effect of diplomatic and military interventions in terms of an increased chance of peace. However, the chi-square statistic of the Schoenfeld residual test, as displayed in the square brackets, shows that none of the significant estimates complies with the proportional hazards assumption. Also the global test for all covariates rejects the PH assumption. The individual tests are useful to see which covariates are most affected by non-proportional hazards. Given the possible bias in the estimates no conclusions should be made at this point.

Duration Dependence

Instead, the model can be re-specified to account for possible duration dependence by interacting variables with functions of time. Using smoothed plots of Schoenfeld residuals against time can give an idea of the type of duration dependence. When the PH assumption is fulfilled the smoothed function of the residuals runs parallel to the time axis. In order to control for duration dependence conflict type ‘Territory’ is interacted with time and monthly GDP per capita is interacted with the log function of time $\ln(t)$. This makes interpretation a bit tricky for these estimates. Still, the Schoenfeld test shows that the proportional hazard assumption cannot be rejected under these specifications (Appendix I, column 2). The hazard ratios of the variables can only be interpreted in comparison to their reference category. A hazard ratio between zero and one indicates that such conflict is less likely to come to an end in comparison to its reference category. When the reported hazard ratio is bigger than one, peace is more likely than in the reference category. The effect of variables which are interacted with a function of time can be interpreted in the same way, however, the effect becomes more pronounced the longer the conflict continues. To give an example, conflict type defined by rebel objective significantly influences the probability of peace. During the first month a territorial conflict it is almost as likely to come to an end as is a governmental conflict, the reference category. The hazard ratio of 0.996 indicates that secessionist conflicts are – compared to coups and revolutions – on average 0.4 percentage points less likely to come to an end during the first month. Since the variable is interacted with time this value gets higher as time goes on. After 24 months of civil war a territorial conflict is 9.8 percentage points less likely to come to an end than a governmental conflict.¹²

¹² The log hazard ratio is time dependent for variables v_i which are interacted with time. Following equation (3) it can be written as $\ln(HR_t) = \delta v_i t$. The resulting hazard ratio HR_t follows an exponential

These result stems from the fact that on average civil wars are shorter when the insurgents or the military attempts to overthrow of the ruling government. Controlling for conflict type shows that secessionist conflicts lasted 76 months while governmental conflicts lasted for approximately 54 months – almost two years less. However, the results also indicate that during the early months there is little difference between attempted secessions and governmental conflicts in their probability to come to an end. This could imply that secessionist conflicts deserve more attention when they break out, since they become relatively more difficult to resolve as they continue.

Piecewise Estimation

When the model is estimated piecewise the timeline is split in two (or more) pieces in which the covariates are assumed to differ in how they influence the dependent variable. Here two cutting points are chosen after investigating the smoothed hazard function (Appendix V).

First the timeline is split at 108 months (or 9 years). For the earlier period only diplomatic interventions are significant on a 10% level (Appendix I, column 3). However, the PH assumption is rejected for diplomatic interventions, which is why the reported hazard ratio of 1.125 should be interpreted with hesitation. Of the significant control variables, only the presence of opiates passes the PH test. It can be said that on average during the first nine years conflicts countries with opiates are only 25.9 percent as likely to come to an end as otherwise identical conflicts in countries without narcotics. This finding underlines the economics of conflict – when lootable resources are available to insurgents and government forces conflicts last longer, perhaps because

function of time. For ‘Territory’ the hazard ratio at time 1 is equivalent to the reported hazard ratio, $HR_1 = 0.996$. Compared to its reference category, it is $100 - 99.6 = 0.4$ percentage points less likely to come to an end. After two years of conflict the hazard ratio is much lower at $HR_{24} = 0.902$. Compared to its reference category it is $100 - 90.2 = 9.8$ percentage points less likely to come to an end. Note that for variables which are interacted with log time the log hazard ratio is $\ln(HR_t) = \delta v_i \ln(t)$.

it provides relatively easy access to finance. However, the reason why narcotics and gemstones tend to increase conflict duration might not only be due to the financial means they provide for fighting. Rather, they can aggravate the creation of “war economies”, whose beneficiaries benefit personally from conflict and thus may no longer be interested in victory or negotiations, but in the continuation of conflict (see for example Arnson and Zartman (2005)).

For conflict times after 108 months military interventions tend to shorten conflicts (Appendix I, column four). Given the time lag of 85 months before military interventions show any effect it needs to be said that especially military interventions are indeed significant during earlier conflict times. However, since their effect is delayed they only have an influence during the later periods. This has to do with the coding of the interventions. In order to account for the full effect of interventions they are not measured when they formally take place, but when they unfold their effect. Besides military interventions, also diplomatic interventions seem to shorten conflict duration after 108 months, however the PH test is rejected. Similar to the unrestricted estimation, the secessionist conflicts have a lower chance of coming to an end. Also, the Cold War seems to play a role in the expected duration of long conflicts. In particular, a civil war which is still going on after nine years is more than four times as likely to end during any following month as it would be if the Cold War would still be going on.¹³ Loosely put, civil wars can be expected to be shorter now that the Cold War is over.¹⁴

¹³ The reported Hazard Ratio of 0.242 implies that a conflict during the Cold War is only 24.2% as likely to end compared to its reference category, which is when the Cold War has ended. Taking the inverse of the Hazard Ratio turns Cold War into the reference category. The result $1/HR = 4.17$ is the relative probability of a post-Cold-War conflict to end compared to an otherwise identical Cold War conflict, conditional on both conflict having been going on for at least 9 years. Such a post-Cold War conflict is more than four times as likely to end.

¹⁴ This finding is possibly related to a sample selection bias since the dataset ends only ten years after the Cold War. Since the estimate is conditional on conflict duration of 9 years, this leaves only 1 year of available data to draw from. It might be questionable, however, if this one year is representative for all post-Cold War conflicts lasting longer than 9 years. Therefore this result should be tested with an updated dataset including information for times from the year 2000 onwards.

Second, the timeline is cut at 236 months, which seems to fit the data quite well for the first piece (Appendix I, column five). The PH assumption holds for several significant covariates – conflict type, opiates and GDP per capita. On a global level the PH test is only marginally rejected at the 10% level. The estimates seem to indicate that conflicts in slower growing nations are more likely to end. This adverse effect of increasing GDP is interesting because it contradicts most previous studies on Civil War (see for example Collier and Hoeffler (2001) and Fearon and Laitin (2003)). It seems that a decrease in GDP per capita facilitates conflict resolution, which might be related to the opportunity cost of ongoing fighting. In other words, once the GDP per capita has decreased by a certain degree the insurgents might lose public support, since households and firms may fear a further (unbearable) decrease in income if the fighting continues.

However, two alternative explanations exist. First, the estimate might be caused by the feedback conflict has on GDP. That is, if a civil war generally disrupts the economy it leads to lower GDP per capita. As such, GDP per capita would be lower when a conflict ends. In that case, lower GDP per capita values are the result of the war, but not necessarily the reason why the war ends. This is essentially a question about the direction of causality. In order to discriminate between the alternative explanations GDP per capita is lagged by one year which means that last year's GDP per capita is regressed on this year's peace, i.e. it can be established that lower GDP per capita leads to an increased probability of peace. However, a one year lag may not be sufficient if GDP per capita is generally decreasing over time, because it would still be the feedback that is captured. For that reason the lag is increased to 5 years, which does not change the results. This provides some indication that it is in fact the lower GDP per capita that leads to an increased probability of peace, and not the other way around.

Second, the result might also stem come from the difference in GDP per capita between civil wars. That is, because the estimates are based on the changes in GDP per capita over time within each conflict as well as on the different GDP per capita between conflicts. Thus, the same results would be obtained if civil wars in poorer countries are shorter on average. This seems to be the case here. When the model is estimated with a constant GDP per capita of the year previous to the beginning of civil war the results do not change, but the hazard ratio of GDP per capita is slightly closer to unity, but still significant. This implies that it is not necessarily the change in income during conflict but the difference in income between conflicts that contribute to the variation in expected durations of civil wars.¹⁵ In other words, civil wars in countries with lower GDP per capita tend to be shorter, which provides some evidence for the economic feasibility of conflict: Conflicts in richer countries have a lower probability to come to an end because more financial means are available to support continued fighting.

To demonstrate what this means it might be useful to provide a numerical example of two identical conflicts with mean characteristics that only differ in their GDP per capita. In the country A the mean income is 136.84 USD per month, the sample mean.¹⁶ In country B income is 5% higher, meaning that the mean income is 143.68 USD per month. Since GDP per capita is interacted with log time to control for duration dependence the actual effect of GDP per capita increases with increased conflict duration. Thus, one year into the civil war country B is approximately 1 percentage point less probable to end than the conflict in country A. After 10 years of fighting the effect is larger. Now the probability to of peace of the richer country is almost 2 percentage points below the poorer country, *cetirus paribus*.

¹⁵ In fact, the results of the logit fixed effects estimation imply that GDP per capita changes within conflicts have no significant effect on the probability of peace (see Appendix IV, column 4).

¹⁶ Note that GDP per capita is recorded on a monthly basis and measured in 1995 USD. It is lagged for 1 year to reduce endogeneity bias.

The estimation after 236 months is not possible because of a lack of observations. There are only seven conflicts ending after 236 months which makes it impossible to estimate nine coefficients. Conflicts lasting longer than 236 months might also be considered as outliers. Nevertheless, for the following specification tests they continue to be included.

Separate Estimation

The next examination concerns different typologies of conflicts. First, this concerns the objective of the insurgents, coded dichotomously in territorial and governmental conflicts (Buhaug 2006). A second candidate for separating the dataset is the existence of natural resources, defined by opiates and/or gemstones. Such ‘lootable’ resources have been identified to influence the onset of civil war (Ross 2002), giving rise to the assumption that natural resource wars might be intrinsically different from other types of civil wars (Azam 2002; Lujala, Gleditsch and Gilmore 2005). Third, conflicts will be separated by the Cold War to get a better understanding if and how conflicts have changed since the 1990s.

First, when looking into the different effects for the insurgent motivation the Cox model is estimated separately for territorial and governmental conflicts. The estimated coefficients are similar in both subgroups (columns one and two of Appendix II). However, they vary greatly in significance, indicating that conflict type should be considered when a particular intervention is chosen. In secession attempts only economic interventions seem to make a difference, while governmental conflicts can be significantly shortened by both diplomatic and military interventions. Since the PH assumption is rejected for these covariates the effect is not constant over time, making a quantitative interpretation of the estimates insensible. Furthermore, the probability of peace does not seem to be related to per capita GDP or natural resources in civil wars

where the insurgents attempt to secede. This is rather surprising and contradicts the findings of Buhaug (2006).

Second, conflicts are divided according to the presence of natural resources. When either opiates and/or gemstones are present none of the possible interventions shows any effect (see columns 3 and 4 of Appendix II). This provides quite a challenge to the international community who has been intervening in many resource wars. In contrast, when countries do not possess lootable resources, diplomatic and military interventions tend to shorten the expected duration. When a military intervention shows its full effect the probability of peace increases by 18.6 percentage points. The effect of diplomatic interventions is very similar, yet the PH assumption is rejected for diplomacy, i.e. its effect changes over time.

Third, the dataset is split at the year 1990, to account for the effect of the Cold War. As can be seen in column 5 of Appendix II foreign interventions were not significant during the Cold War. After the Cold War both diplomatic and military interventions show a reducing effect on the expected duration (column 6). This finding is in line with the results when only duration dependence is controlled for. Yet again, the PH assumption is rejected, making inferences about the precise effect difficult.

Despite these insights into the structure of civil wars separate estimation does not show useful for the Cox Model. In fact, the proportional hazard assumption is rejected on most individual and all global levels when the model is fitted separately for subsets. This makes it necessary to look into other ways of dealing with the PH requirements, such as stratified estimation.

Stratified Estimation

As outlined in section 3 stratified estimation is sensible when the coefficients of the separate estimation are similar between subsets, since they are assumed to be identical between the strata. Only the baseline hazard is allowed to vary between the subsets. Given that most coefficients are indeed very similar when the model is estimated separately for conflict type and natural resources (see Appendix II), stratification is plausible.¹⁷

The results of the stratified estimation support the previous findings when the model was estimated for the complete dataset. When stratified by the objective of the insurgents, diplomatic and military interventions tend to shorten conflict duration, while economic sanctions do not make any significant difference (see column one of Appendix III). Again, the presence of opiates and gemstones adversely affects the probability of peace. An increasing number of fatalities also tends to shorten conflict duration, although it is difficult to quantify the effect precisely, given the rejection of the PH assumption.

Similar results are obtained when the model is stratified by natural resources. Again, diplomatic and military interventions make peace more likely. Economic interventions are not significant. Secessionist conflicts are relatively less likely to end, while an increasing number of fatalities contributes to conflict resolution. Virtually identical hazard ratios are returned when the model is stratified by Cold War. Diplomatic and military interventions show significant, while economic interventions do not seem to affect the probability of peace. Of the other explanatory variables all but GDP per capita are significant. Yet again, these results should be taken with caution, given that stratification does not improve the results of the PH test.

¹⁷ This comparison can be done using a Chi square test, though it implicitly assumes that a possible bias has the same direction in both subgroups of the separate estimation.

Summary of Cox Model

Despite various different specifications to capture the structure of civil wars the results of the Schoenfeld test do not improve once duration dependence is controlled for (column 2 of Appendix I). No matter which specification is used, the PH assumption does not hold for most variables. However, there are several results which are conditional on the specifications above. To begin with rebel objective, territorial conflicts are generally less likely to come to an end than governmental conflicts. The effect becomes more pronounced over time, though it is not significant when the country does not possess natural resources.

Concerning the Cold War, it seems that diplomatic and military interventions are more effective now that the Cold War is over. This is good news for the international community, since it underlines the increasing effectiveness of foreign interventions.¹⁸ It also indicates that conflict resolution may not have been the primary reason for foreign interventions during the Cold War, which would be in line with the ‘proxy wars’ theory according to which the superpowers used to fight each other outside their respective territories (Regan and Aydin 2006, p. 743). On top, it helps to shed some light on the nil findings associated with the end of the Cold War in previous studies (see for example Collier, Hoeffler and Rohner (2006) and Tuscisny (2004)). It seems that the Cold War only shows relevant when interventions are controlled for.

In general, conflict intensity has a shortening effect on conflict duration. Yet it is difficult to conclude how an increase in killed combatants influences the probability of peace, since only mean values are available. However, when looking at these average fatalities, controlled for total conflict duration, higher casualties tend to increase the

¹⁸ Collier, Hoeffler and Rohner (2006) find that “the end of the Cold War did not have lasting effects on the risk of civil war” (p. 16).

probability of peace. This might be related to increased international pressure as a civil war becomes 'bloodier' but could also stem from defeat of one of the warring parties. The finding also implies that conflicts with few casualties are likely to last longer.

There is some evidence that higher GDP per capita adversely influences the probability of peace. The effect is duration dependent and significant only for the first 20 conflict years, although the hazard ratio is similar for most other specifications. There remains some doubt about the direction of causality however, since conflicts are typically associated with decreasing income even with a one year time lag. Thus, while it is not difficult to imagine how conflict disrupts economic activity and subsequently reduces the income, it remains unclear if the lower income can convince the warring factions to end the violence.

The presence of natural resources seems to extend the conflict duration during the first nine years, as well as in governmental conflicts. However, the effect of natural resources is insignificant in the post-Cold War period.

Diplomatic interventions seem to increase the probability of peace by about 1 percentage point when they unfold their full effect. However, the effect is not significant in countries with natural resources and in secessionist conflicts. Military interventions can help to increase the probability of peace, although the effect is less pronounced than that of diplomatic interventions and much more delayed. This finding contradicts other studies using the same dataset without a lagged effect of military interventions (Regan 2002, Regan and Aydin 2006). Given that most conflicts end well before military interventions unfold they do not seem very effective compared to diplomatic interventions (see section 3, figure 1). However, it might be argued that the long delay of the effect of military interventions is due to the operational definition of peace. It is defined as a subsequent period of six months without any battle related

deaths. Thus, if military interventions do not bring the number of casualties down to zero they appear as ineffective, although they might in fact substantially improve security in areas that were previously fiercely fought over. To control for this effect the dataset would need to be amended by a variable containing the monthly fatalities.

Economic interventions, such as aid and sanctions, do not seem to have much impact on the probability of peace. Only in secessionist conflicts economic aid seems to contribute to the peace process, although the estimate is not robust as the PH assumption is rejected. It is noteworthy that economic interventions never show up as significant when other interventions do. There are various explanations for this, but it is clear that economic aid is not turned into military goods, otherwise the effect of military and economic intervention would be somewhat similar. Instead, economic support is used for other purposes, possibly humanitarian needs of the population. As such, the nil finding of economic interventions with regard to the probability of peace could be interpreted as an endorsement for humanitarian interventions, such as food programmes and refugee camps. Contrary to claims against such humanitarian interventions (Sørensen 2006; Lischer 2005) they do not seem to extend conflict durations, including secessionist conflicts.

Overall, the PH assumption can be accepted as valid for individual covariates but as the results show there continue to be some variables whose contribution to the overall probability of peace is not constant over time even when duration dependence is controlled for. This calls for alternative specifications, one of which is the discrete-time logit model. Besides relaxing the PH assumption, the logit approach can be used to test for two kinds of unobserved heterogeneity.

4.3 Logit Model

Before the logit model can be estimated the data needs to be slightly rearranged. Allowing a flexible baseline hazard based on intercepts of individual months requires a failure in each of the observations. In other words, at least one conflict should come to an end during each of the 422 periods of which conflicts are observed in the data. Since not more than 124 civil wars end in the dataset some periods need to be combined into time bundles in order to estimate a baseline hazard which can completely replace the constant term. Naturally, the bundles get longer over time, since there are few conflicts lasting very long and subsequently less failures during the later periods. Up to the 14th conflict month each month enters, between the 15th and the 30th month periods enter pairwise. Up to conflict month 120 the monthly periods are combined into 6 month bundles, after which the bundles comprise 36 months. The last combined period, running from month 373 to 422 months, lasts 50 months because there are very few conflicts ending that late. This method assures that there is at least 1 failure in each time bundle. Interventions are counted cumulatively, e.g. if there was a single intervention in two of the, say, 6 months combined, the bundle has the value 2 for that particular intervention. For GDP per capita the mean value across the combined periods is used for the bundles. The constant variables are not altered when combining the periods.

Appendix IV shows the results of the logit estimations using the above combination of periods. The first column presents the maximum likelihood estimates of the baseline hazard without any explanatory variables. The estimates are the log odd ratios of survival which can be transformed into Kaplan Meier hazard rates using $[1 + \exp(-\beta)]^{-1}$. These follow the distribution function on conflict durations, but account for censoring of conflicts, i.e. when a conflict continues beyond the study period. In order to obtain the effect for a specific month the estimate needs to be adjusted for the

length of the relevant time bundle. Notably, hazard rates of conflicts lasting longer than 300 months (25 years) are not significant. This loss of explanatory power is common to survival studies and is caused by wider confidence intervals due to the decreasing number of individuals in the study at these later periods (Jenkins 2005, p. 80).

The second column shows the estimates with covariates. The baseline estimates change as expected, suggesting some explanatory power of the introduced covariates. Exit rates after 10 conflict years are not significant, again underlining the power of the explanatory variables. During the period when diplomatic and military interventions show their strongest effect they make peace by 13.1 and 8.3 percentage points more likely, respectively.¹⁹ Economic interventions do not have a significant impact on the probability of peace. On average secessionist conflicts are 41 percentage points less likely to end in a given period compared to an otherwise identical governmental conflict. Similarly, the probability of peace is respectively 59.0 and 35.2 percentage points lower when opiates and gemstones are present. Conform to the estimates of the above Cox model higher per capita GDP is associated with a decreased probability of peace. Civil wars with higher mean fatalities tend to last slightly shorter.

The third column contains the estimates of the pooled logit model using a single constant to represent the baseline hazard. The estimates can be compared to the previous model with a flexible baseline hazard using a likelihood ratio test. The results suggest that a flexible baseline specification fits the data better.

Column four presents the results for the fixed effects model. It only contains estimates for variables that change over time, excluding most variables and 36 conflicts

¹⁹ The odds ratio for diplomatic interventions of 1.131 implies that the full effect of a diplomatic intervention makes peace 13.1 percentage points more likely. Note that the estimates are obtained by using the mean effect of the interventions within each time bundle.

which are censored or only lasted one month. In this setting, all interventions are significant at conventional levels, as is the impact of the Cold War. Per capita GDP has no significant effect. These findings should be interpreted cautiously however, since no between-conflict-variance is used to calculate these estimates. Nevertheless, the fixed effects model is useful to investigate the presence of unobserved conflict specific heterogeneity. By testing the estimates of the conflict fixed effects model and the pooled logit model with a constant for significance the Hausman test can discriminate between the null hypothesis, under which both models are consistent but the fixed effects model is inefficient, and the alternative hypothesis, under which only the fixed effects model is consistent. In other words, rejecting the null hypothesis provides evidence of unobserved heterogeneity (see Appendix VI.). The test value is estimated for $19.8 \sim \chi_6^2$. The null is rejected, suggesting significant evidence of variation in the conflict fixed effects. Note however, that the test is carried out using the constant baseline specification, which does not accurately resemble the data (see above). It is not possible to use a flexible baseline because the estimation does not converge for the fixed effects model.²⁰ Thus, the test does not provide a definite proof of conflict specific unobserved heterogeneity.

As pointed out above there are several reasons why the model might be affected by unobserved heterogeneity. First, some of the included variables might be duration dependent, second the selectivity of interventions may not be sufficiently controlled for, and third relevant factors which affect conflict duration may have been excluded. The first issue, duration dependence, can be addressed by interacting all duration dependent variables with a monotone function of time. Since two variables (GDP per capita and rebel objective) are already identified to be serially correlated under the Cox model it is

²⁰ This might be an indication that countries which are overly prone to conflict entered excessively into the sample, possibly making general inferences problematic (see Elbers and Ridder 1982).

straightforward to include them in their interacted form here as well. In fact, this seems to resolve the problem of unobserved heterogeneity. When GDP per capita is interacted with a log function of time and rebel objective is interacted with time the Chi² statistic of the Hausman test is insignificant on conventional levels, implying that presence of neglected heterogeneity can be rejected.²¹ This also shows how the Cox model is more robust when duration dependence is controlled for.

The last column of Appendix IV presents the estimates for the random effects logit model with a flexible baseline hazard. The estimates for both σ^2 and ρ are not significantly different from zero, indicating that there are no cross-period correlations. Thus, the hypothesis of randomly distributed heterogeneity can be rejected. Since ρ cannot be distinguished from zero the estimates of this model are similar to the ones of the pooled logit model with a flexible baseline hazard (Column 2).

Summary of Logit Model

The logit model specification supports the main findings of the Cox model. Interpretation for the time-varying covariates requires adjustment for the grouping of time periods. A likelihood ratio tests show that the baseline hazard is most adequately expressed by a flexible specification. Furthermore, using fixed and random effects logit models there is some indication of conflict specific unobserved heterogeneity. However, both conflict specific and randomly distributed heterogeneity are rejected, once duration dependence is controlled for. These robustness checks are important since interventions and civil wars might be determined by strategic behaviour not completely controlled for. The tests indicate that such behaviour does not affect the results. In effect, the standard logit model with a flexible baseline is preferred over other logit specifications.

²¹ For further information on the other possible sources of unobserved heterogeneity see for example Heckman (1979), Meernik (2000), and Signorino (2002) for the selectivity of interventions; and Allison (1999), Signorino and Yilmaz (2003), Sowmya and Schoenfeld (2007), and Wooldridge (2002) on omitted variable bias.

5 Conclusion

This study is concerned with the effect of foreign interventions in civil wars. The overall results suggest a rather complex nature of civil wars, making effective intervention difficult. However, using a Cox model for time-varying covariates it is shown that diplomatic and military interventions are able to significantly shorten conflict durations. Economic interventions have no significant effect on the duration civil war, except in secessionist conflicts where economic aid tends to increase the probability of peace.

The nil finding regarding the effect of economic interventions under most Cox model specifications does not necessarily imply that economic measures are ineffective, since aid might be used for other objectives, including humanitarian relief. Furthermore, it is shown that the presence of natural resources, higher GDP per capita and territorial conflicts are associated with a decreased probability of peace, i.e. longer conflict duration. An increase in conflict intensity and governmental conflicts are associated with an increased probability of peace. Several of these findings depend on the geopolitical context. For example, the effectiveness of interventions has increased substantially after the end of the Cold War. Also natural resources play a less important role since the iron curtain has been removed. The parameter estimates of the logit model support the main findings of the Cox estimation.

This thesis comes with a number of innovations over previous work in the field. First, the full effect of interventions is accounted for by a two-step procedure. In step one the decay functions of the impact of interventions over time are estimated. The resulting functions resemble the impact of the available interventions more accurately than earlier work with a subjectively defined decay. In step two the model is estimated,

using the impact functions of foreign interventions. Second, the original dataset is amended by a number of variables which are identified as relevant by previous empirical work. In particular, including the objective of the opposition proves powerful in distinguishing between opposing theories. Ethnic diversity and related issues of ethno-religious identity that might cause conflict do not influence the effectiveness of interventions. Third, a series of robustness checks are conducted not found in earlier work on interventions. These include fixed and random effects logit models to account for unobserved heterogeneity. No evidence of conflict specific or random unobserved heterogeneity is found and thus the standard logit model is preferred, as long as duration dependence is controlled for. This accentuates the importance to test for duration dependence and incorporate it when specifying the model, as it is done here.

To analyse the impact of interventions in more detail it would be useful to update the available dataset by monthly fatalities, since the protection of human life is often a desired effect of foreign interventions, currently not controlled for. Also, future empirical research might be well advised to focus on the nexus between conflict resolution and peace building, since former conflicts often reignite, arguably rendering previous interventions ineffective.

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Appendix

Appendix I. Cox Model: Duration Dependence and Piecewise Estimation

	1	2	3	4	5
	Cox Model	Cox Model with Duration Dependence ¹	Piecewise time ≤ 108 ¹	Piecewise time > 108 ¹	Piecewise time ≤ 236 ¹
<i>Political Aspects</i>					
Cold War	0.883 (0.600) [5.490]	0.855 (0.750) [7.900]	1.058 (0.270) [0.810]	0.242 (2.040)** [2.170]	0.919 (0.400) [4.380]
Territory	0.602 (2.410)** [0.060]	0.996 (1.660)* [13.420]°°°	0.992 (1.610) [0.300]	0.994 (2.050)** [0.320]	0.990 (3.110)** [2.730]°
<i>Economic Aspects</i>					
GDP/cap	0.999 (1.220) [0.620]	1.000 (1.030) [8.180]	1.000 (1.160) [1.720]	1.000 (0.360) [10.110]	0.999 (1.780)* [3.180]°
Opiates	0.549 (1.770)* [0.730]	0.562 (1.730)* [1.980]	0.259 (2.140)** [2.910]°	2.284 (1.420) [0.090]	0.564 (1.740)* [3.270]°
Gemstones	0.683 (1.740)* [1.970]	0.690 (1.730)* [5.170]°°	0.571 (1.950)* [0.080]	1.684 (0.920) [3.230]	0.602 (2.080)** [0.060]
<i>Military Aspects</i>					
Intensity	1.000 (7.000)** [0.460]	1.000 (7.400)** [0.710]	1.000 (7.210)** [0.000]	0.983 (0.400) [1.340]	1.000 (7.360)** [0.250]
<i>Interventions</i>					
Diplomatic	1.154 (2.900)** [0.070]	1.152 (2.880)** [1.190]	1.125 (1.730)* [0.030]	1.204 (2.110)** [0.820]	1.136 (2.620)** [0.020]
Military	1.078 (3.440)** [0.160]	1.073 (3.040)** [3.840]°°	1.027 (0.370) [0.270]	1.065 (2.630)** [2.620]	1.056 (2.540)** [0.010]
Economic	0.959 (0.130) [0.000]	0.986 (0.050) [0.420]	0.953 (0.140) [0.390]	0.728 (0.370) [1.360]	0.961 (0.120) [0.130]
Global PH Test	8.82	29.45°°°	6.99	15.35°	14.62
Log likelihood	-498.707	-499.800	-444.268	-47.071	-483.456
N	12573	12573	8155	4418	11250
Conflicts	153	153	153	36	153

Note: Estimates are reported in exponentiated form so they can be interpreted as hazard ratios. Round brackets contain absolute z values. Asterisks (*, **, ***) indicate significance at the 10, 5, and 1% level, respectively. Square brackets contain Chi² values of the Schoenfeld residual test. Degrees (°, °°, °°°) indicate the proportional hazard assumption is not rejected on the 10, 5, and 1% level, respectively.

¹ Territory and GDP per capita are interacted with time and ln(time), respectively.

Appendix II. Cox Model: Separate Estimation

	1	2	3	4	5	6
	Secession vs. Coup ¹		Natural resources ¹		Cold War ¹	
	Territorial Conflicts	Governmental Conflicts	Resource Conflicts	No Natural Resources	During Cold War	After Cold War
<i>Political Aspects</i>						
Cold War	0.604 (1.350) [0.860]	1.086 (0.320) [3.970]	0.552 (1.200) [3.800]	1.024 (0.100) [2.080]	-	-
Territory	-	-	0.990 (1.930)* [3.850] ^{oo}	0.999 (0.400) [10.170]	0.989 (2.280)** [0.000]	0.988 (2.750)** [0.610]
<i>Economic Aspects</i>						
GDP/cap	1.000 (0.520) [0.110]	0.999 (1.710)* [0.840]	1.000 (0.340) [4.330]	1.000 (0.870) [10.740]	0.999 (1.510) [3.250]	1.000 (0.660) [0.130]
Opiates	0.493 (1.120) [0.120]	0.601 (1.190) [0.380]	-	-	0.587 (1.350) [1.670]	0.357 (1.280) [0.000]
Gemstones	0.828 (0.400) [0.070]	0.559 (2.210)** [2.210]	-	-	0.544 (1.940)* [0.680]	0.795 (0.580) [0.120]
<i>Military Aspects</i>						
Intensity	1.004 (5.860)** [0.010]	1.000 (6.920)** [0.280]	1.004 (3.950)** [0.030]	1.000 (7.250)** [0.170]	1.000 (7.090)** [0.030]	1.001 (4.040)** [0.000]
<i>Interventions</i>						
Diplomatic	0.979 (0.160) [0.000]	1.179 (3.100)** [0.760]	1.080 (0.910) [0.530]	1.183 (2.740)** [0.720]	1.062 (0.420) [0.020]	1.148 (2.480)** [0.180]
Military	1.012 (0.020) [0.490]	1.071 (2.730)** [0.050]	1.032 (1.270) [0.640]	1.186 (2.400)** [6.740] ^{ooo}	0.972 (0.440) [0.020]	1.101 (2.030)** [0.050]
Economic	-	0.671 (1.030) [0.520]	0.375 (1.560) [0.000]	1.171 (0.580) [0.520]	1.218 (0.590) [0.000]	0.461 (1.470) [0.350]
Sanction	2.078 (0.600) [0.070]	-	-	-	-	-
Aid	1.483 (1.750)* [0.010]	-	-	-	-	-
Global PH Test	1.46	9.11	7.49	22.64 ^{ooo}	6.49	1.42
Log likelihood	-118.369	-288.582	-64.152	-369.707	-301.325	-116.872
N	6667	5906	4306	8267	7898	4675
Conflicts	56	97	36	117	114	75

Note: Estimates are reported in exponentiated form so they can be interpreted as hazard ratios. Round brackets contain absolute z values. Asterisks (*, **, ***) indicate significance at the 10, 5, and 1% level, respectively. Square brackets contain Chi² values of the Schoenfeld residual test. Degrees (°, °°, °°°) indicate the proportional hazard assumption is not rejected on the 10, 5, and 1% level, respectively.

¹ Territory and GDP per capita are interacted with time and ln(time), respectively.

Appendix III. Cox Model: Stratified Estimation

	1	2	3
	Secession vs. Coup ¹	Natural resources ¹	Cold War ¹
<i>Political Aspects</i>			
Cold War	0.889 (0.580) [3.190]	0.856 (0.750) [5.610]	-
Territory	-	0.996 (1.840)* [5.910] ^{oo}	0.989 (3.260)*** [0.690]
<i>Economic Aspects</i>			
GDP/cap	1.000 (1.190) [3.820]	1.000 (0.860) [8.480]	1.000 (1.390) [1.930]
Opiates	0.517 (1.900)* [0.380]	-	0.536 (1.780)* [0.960]
Gemstones	0.657 (1.800)* [1.310]	-	0.633 (2.000)** [1.010]
<i>Military Aspects</i>			
Intensity	1.000 (6.940)*** [0.210]	1.000 (7.470)*** [0.320]	1.000 (7.070)*** [0.050]
<i>Interventions</i>			
Diplomatic	1.156 (3.040)*** [0.360]	1.138 (2.660)*** [0.490]	1.111 (2.230)** [0.330]
Military	1.072 (2.840)*** [0.360]	1.054 (2.180)** [1.890]	1.061 (2.500)** [0.040]
Economic	0.956 (0.140) [0.000]	1.015 (0.050) [0.350]	0.939 (0.180) [0.260]
Global PH Test	8.37	18.76 ^{oo}	4.66
Log likelihood	-423.057	-441.059	-423.100
N	12573	12573	12573
Conflicts	153	153	153

Note: Estimates are reported in exponentiated form so they can be interpreted as hazard ratios. Round brackets contain absolute z values. Asterisks (*, **, ***) indicate significance at the 10, 5, and 1% level, respectively. Square brackets contain Chi² values of the Schoenfeld residual test. Degrees (°, °°, °°°) indicate the proportional hazard assumption is not rejected on the 10, 5, and 1% level, respectively.

¹ Territory and GDP per capita are interacted with time and ln(time), respectively.

Appendix IV. Logit Model

	1	2	3	4	5	6
	Pooled Logit	Pooled Logit	Pooled Logit	Fixed Effects Logit	Random Effects Logit	Random Effects Logit
	Flexible Baseline	Flexible Baseline	Constant Baseline	Constant Baseline	Constant Baseline	Flexible Baseline
<i>Political Aspects</i>						
Cold War	-	0.951 (0.240)	0.867 (0.680)	0.000 (11.510)***	0.867 (0.700)	0.946 (0.250)
Territory	-	0.607 (2.240)**	0.720 (1.610)	-	0.720 (1.620)	0.590 (2.260)**
<i>Economic Aspects</i>						
GDP/cap	-	0.999 (1.440)	1.000 (0.330)	0.994 (0.630)	1.000 (0.380)	0.999 (1.530)
Opiates	-	0.458 (2.040)**	0.566 (1.470)	-	0.566 (1.430)	0.428 (1.920)*
Gemstones	-	0.671 (1.550)	0.674 (1.510)	-	0.674 (1.540)	0.648 (1.550)
<i>Military Aspects</i>						
Intensity	-	1.001 (3.420)***	1.000 (3.770)***	-	1.000 (5.440)***	1.001 (5.060)***
<i>Interventions</i>						
Diplomatic	-	1.146 (1.960)**	1.253 (3.930)***	3.938 (3.350)***	1.253 (3.630)***	1.151 (1.860)*
Military	-	1.091 (2.800)***	1.149 (4.990)***	4.095 (3.540)***	1.149 (4.800)***	1.095 (2.430)**
Economic	-	0.991 (0.030)	0.998 (0.010)	1.971 (1.960)**	0.998 (0.010)	0.981 (0.070)
<i>Baseline</i>						
Constant	-	-	0.042 (14.250)***	-	0.040 (14.130)***	-
each month	Month 1	0.048 (7.850)***	0.019 (5.160)***	-	-	0.018 (5.600)***
	Month 2	0.074 (7.970)***	0.084 (6.250)***	-	-	0.080 (5.960)***
	Month 3	0.046 (7.370)***	0.057 (5.880)***	-	-	0.056 (5.780)***
	Month 4	0.032 (6.790)***	0.047 (5.540)***	-	-	0.046 (5.580)***
	Month 5	0.033 (6.730)***	0.049 (5.550)***	-	-	0.048 (5.490)***
	Month 6	0.034 (6.660)***	0.052 (5.340)***	-	-	0.051 (5.400)***
	Month 7	0.009 (4.740)***	0.013 (4.240)***	-	-	0.013 (4.230)***
	<i>Continues on next page</i>					
Log-likelihood ¹	-495.614	-457.221	-492.382	-229.144	-492.382	-457.354
N	3522	3522	3522	2470	3522	3522
Conflicts	153	153	153	117	153	153

Note: Estimates are reported in exponentiated form to be interpreted as odd ratios. Round brackets contain absolute z values. Asterisks (*, **, ***) indicate significance at the 10, 5, and 1% level, respectively.

¹The log-likelihood of the fixed effects logit model cannot be compared to other log-likelihood values.

	1	2	3	4	5	6	
	Pooled Logit	Pooled Logit	Pooled Logit	Fixed Effects Logit	Random Effects Logit	Random Effects Logit	
	Flexible Baseline	Flexible Baseline	Constant Baseline	Constant Baseline	Constant Baseline	Flexible Baseline	
<i>Continued from previous page</i>							
each month	Month 8	0.017 (5.680)***	0.027 (5.060)**	-	-	-	0.027 (4.860)***
	Month 9	0.009 (4.720)***	0.014 (4.150)***	-	-	-	0.014 (4.150)***
	Month 10	0.036 (6.510)***	0.059 (5.390)***	-	-	-	0.059 (5.090)***
	Month 11	0.009 (4.670)***	0.015 (4.150)***	-	-	-	0.015 (4.100)***
	Month 12	0.009 (4.660)***	0.015 (4.000)***	-	-	-	0.015 (4.090)***
	Month 13	0.009 (4.650)***	0.015 (4.120)***	-	-	-	0.015 (4.070)***
	Month 14	0.009 (4.640)***	0.015 (4.230)***	-	-	-	0.015 (4.060)***
	Months 15-16	0.019 (5.530)***	0.031 (4.760)***	-	-	-	0.031 (4.640)***
	Months 17-18	0.020 (5.510)***	0.031 (4.580)***	-	-	-	0.032 (4.620)***
	Months 19-20	0.020 (5.460)***	0.032 (4.690)***	-	-	-	0.033 (4.560)***
	Months 21-22	0.021 (5.430)***	0.032 (4.480)***	-	-	-	0.033 (4.550)***
	Months 23-24	0.032 (5.870)***	0.050 (4.700)***	-	-	-	0.051 (4.700)***
	Months 25-26	0.011 (4.510)***	0.017 (3.990)***	-	-	-	0.017 (3.930)***
	Months 27-28	0.022 (5.340)***	0.034 (4.470)***	-	-	-	0.035 (4.450)***
Months 29-30	0.011 (4.480)***	0.017 (3.870)***	-	-	-	0.017 (3.910)***	
2 months bundles	Months 31-36	0.059 (6.160)***	0.090 (4.750)***	-	-	-	0.093 (4.520)***
	Months 37-42	0.012 (4.400)***	0.017 (3.830)***	-	-	-	0.018 (3.870)***
	Months 43-48	0.078 (6.020)***	0.114 (4.770)***	-	-	-	0.120 (4.210)***
	Months 49-54	0.070 (5.730)***	0.108 (4.110)***	-	-	-	0.114 (4.060)***
	Months 31-54	0.059 (6.160)***	0.090 (4.750)***	-	-	-	0.093 (4.520)***
6 months bundles	Months 31-36	0.059 (6.160)***	0.090 (4.750)***	-	-	-	0.093 (4.520)***
	Months 37-42	0.012 (4.400)***	0.017 (3.830)***	-	-	-	0.018 (3.870)***
	Months 43-48	0.078 (6.020)***	0.114 (4.770)***	-	-	-	0.120 (4.210)***
	Months 49-54	0.070 (5.730)***	0.108 (4.110)***	-	-	-	0.114 (4.060)***
	Months 31-54	0.059 (6.160)***	0.090 (4.750)***	-	-	-	0.093 (4.520)***
<i>Continues on next page</i>							
Log-likelihood ¹	-495.614	-457.221	-492.382	-229.144	-492.382	-457.354	
N	3522	3522	3522	2470	3522	3522	
Conflicts	153	153	153	117	153	153	

Note: Estimates are reported in exponentiated form to be interpreted as odd ratios. Round brackets contain absolute z values. Asterisks (*, **, ***) indicate significance at the 10, 5, and 1% level, respectively.

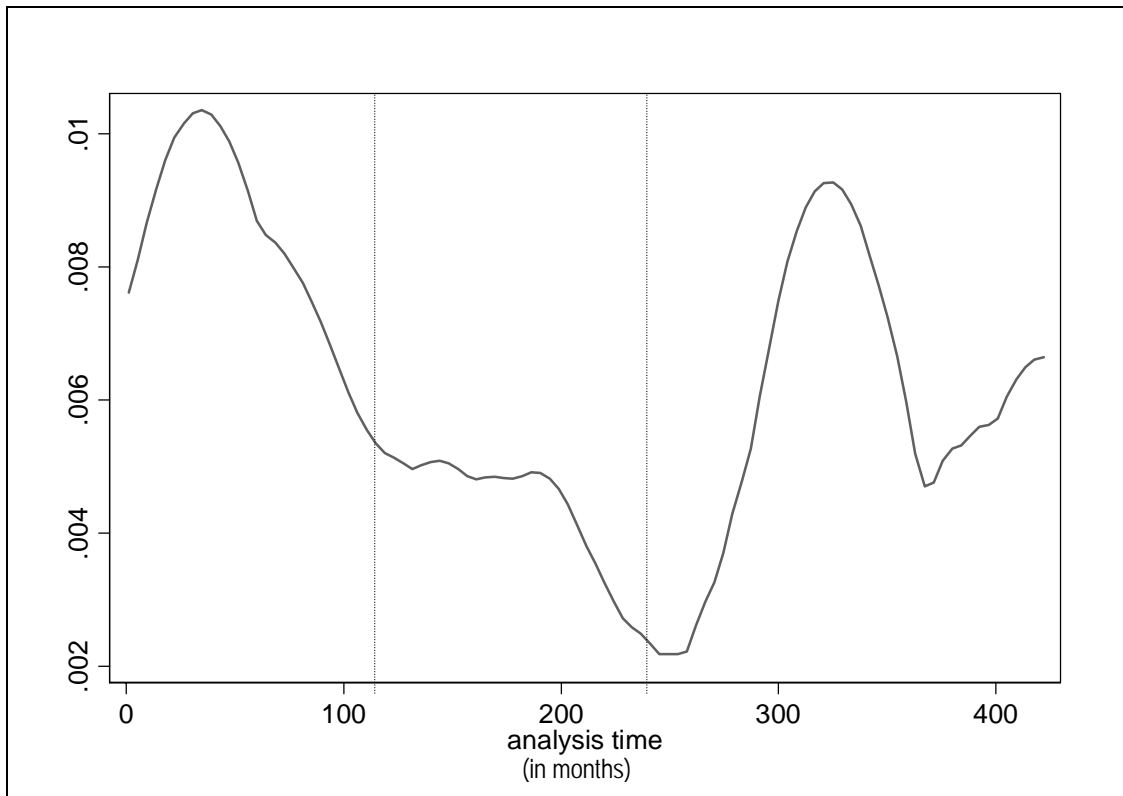
¹The log-likelihood of the fixed effects Logit model cannot be compared to other log-likelihood values.

	1	2	3	4	5	6
	Pooled Logit	Pooled Logit	Pooled Logit	Fixed Effects Logit	Random Effects Logit	Random Effects Logit
	Flexible Baseline	Flexible Baseline	Constant Baseline	Constant Baseline	Constant Baseline	Flexible Baseline
<i>Continued from previous page</i>						
6 months bundles	Months 55-60	0.060 (5.480)***	0.094 (4.070)***			0.100 (3.930)***
	Months 61-66	0.098 (5.420)***	0.155 (3.890)***	-	-	0.169 (3.410)***
	Months 67-72	0.052 (5.000)***	0.081 (3.900)***	-	-	0.088 (3.670)***
	Months 73-78	0.018 (4.010)***	0.026 (3.420)***	-	-	0.029 (3.360)***
	Months 79-84	0.036 (4.600)***	0.053 (3.990)***	-	-	0.058 (3.600)***
	Months 85-90	0.038 (4.550)***	0.054 (3.940)***	-	-	0.059 (3.590)***
	Months 91-96	0.020 (4.550)***	0.026 (3.940)***	-	-	0.029 (3.590)***
	Months 97-102	0.098 (4.440)***	0.139 (3.010)***	-	-	0.154 (2.970)***
	Months 103-108	0.027 (3.560)***	0.036 (3.150)***	-	-	0.040 (2.950)***
	Months 109-114	0.059 (3.890)***	0.076 (2.870)***	-	-	0.086 (2.920)***
	Months 115-120	0.063 (3.800)***	0.078 (2.720)***	-	-	0.089 (2.810)***
	36 months bundles	Months 121-156	0.103 (3.740)***	0.107 (3.100)***	-	-
Months 157-192		0.273 (2.820)***	0.337 (2.130)**	-	-	0.392 (1.480)
Months 193-228		0.059 (2.750)***	0.098 (2.030)**	-	-	0.114 (1.960)**
Months 229-264		0.077 (2.470)**	0.154 (1.730)*	-	-	0.180 (1.540)
Months 265-300		0.100 (2.200)**	0.216 (1.390)	-	-	0.256 (1.210)
Months 301-336		0.600 (0.700)	1.367 (0.360)	-	-	1.685 (0.590)
Months 337-372		0.333 (0.950)	0.667 (0.300)	-	-	0.849 (0.130)
50	Months 373-422	0.500 (0.570)	0.913 (0.100)	-	-	1.219 (0.130)
	ρ	-	-	-	0.000	0.045
	σ	-	-	-	0.001	0.392
	Log-likelihood ¹	-495.614	-457.221	-492.382	-229.144	-492.382
	N	3522	3522	3522	2470	3522
	Conflicts	153	153	153	117	153

Note: Estimates are reported in exponentiated form to be interpreted as odd ratios. Round brackets contain absolute z values. Asterisks (*, **, ***) indicate significance at the 10, 5, and 1% level, respectively.

¹The log-likelihood of the fixed effects Logit model cannot be compared to other log-likelihood values.

Appendix V. Smoothed Hazard Function



Note: The vertical axis displays instantaneous hazard. Dotted lines indicate cutting points for piecewise estimation.

Appendix VI. Estimation in Stata

This appendix is meant as a brief guide to the commands and computational procedure applied to obtain the results above. It is organised according to the structure of the thesis. All estimations are done with Stata 9.0 (StataCorp 2005a). For a more detailed explanation please refer to the manuals of the corresponding commands (StataCorp 2005b).

1. Cox Model

The Stata command to estimate the Cox model is `stcox` (StataCorp 2005b). It uses maximum likelihood to fit the proportional hazard model and allows for multiple records per individual (conflict) to account for time-varying covariates. One of the advantages of the Cox model is that it also uses information from censored conflicts.

2. Decay Function of Interventions

The Cox model is used to estimate the decay function of the foreign interventions. The model is fit at each possible lag for the interventions ($t-1$, $t-2$, ..., $t-T$). The estimates are saved and smoothed using Stata's `lowess` command (StataCorp 2005b). The result gives the approximate function of the effect of each intervention over time. To functions are transformed by setting their modes to unity to represent the period with the strongest effect.

3. Logit Model

The command used here is `logit` (StataCorp 2005b). Also the logit model uses maximum likelihood for the regression. Logit does not handle censored conflicts, nor periods without failures (i.e. peace), which is why the data has to be rearranged into time bundles of several months each before the estimation.

4. Fixed Effects Logit

The Stata command is `clogit` (StataCorp 2005b). Fixed effects logit requires that conflicts come to an end, hence the name 'conditional logit'. Stata does not make use of maximum likelihood to fit the model, but rather an OLS estimator, since the results would be biased otherwise.

5. Random Effects Logit

The model is fit through maximum likelihood using the `logit` command with the random effects option (StataCorp 2005b).

6.a Hausman Test for Unobserved Heterogeneity

The test requires the estimation and storing of the model which is to be tested for unobserved heterogeneity and a second model which is not affected by unobserved heterogeneity but is inefficient. For further information see the hausman command (StataCorp 2005b).

6.b Results of Hausman Test for Unobserved Heterogeneity

The test compares the results of a random effects and a fixed effects estimation, both of which are correct under the null hypothesis. However, the fixed effects model is inefficient compared to the random effects model. Under the alternative hypothesis the random effects is not correct. Thus, when the null is rejected the fixed effects model is preferred. Otherwise the random effects model is preferred.

1. Hausman Test without controlling for duration dependence:

$$\chi^2_6 = 19.8***$$

Result: H0 can be rejected in favour of fixed effects logit model, i.e. there is evidence of conflict specific unobserved heterogeneity.

Note: Asterisks (***) indicate significance on 1% level.

2. Hausman Test when duration dependence is controlled for:

$$\chi^2_6 = 0.00$$

Result: H0 cannot be rejected, i.e. there is no significant evidence of conflict specific unobserved heterogeneity.

Note: GDP per capita is interacted with the log of conflict time; and rebel objective is interacted with conflict time.