International Journal of Peace and Conflict Studies (IJPCS), Vol. 4, No 2, December, 2017.
Website: http://www.rcmss.com. Also available online at www.academix.ng ISSN: 2354-1598(Online)
ISSN: 2346-7258 (Print)

Md. Didarul Hasan & Sajal Lahiri, 2017, 4(2):128-151

INTERNATIONAL SANCTIONS AND THE DURATION OF CIVIL CONFLICTS

Md. Didarul Hasan[1]
Department of Economics, Asian University for Women, Chittagong, Bangladesh.

&
Sajal Lahiri
Department of Economics, Southern Illinois University Carbondale, Carbondale, IL 62901, U.S.A.
(md.hasan@auw.edu.bd and lahiri@siu.edu)

Abstract

We examine the effects of international sanctions on the expected duration of civil conflicts, using civil wars and sanctions data for the period of 1960 - 2008. We do so by estimating the hazard rate of war termination due to sanctions. Contrary to most of the previous findings, we find that international sanctions, at the aggregate level, do reduce the expected duration of civil conflicts. Our findings are robust with respect to different controls, different parametric models, and the consideration of endogeneity of sanctions. However, not all types of sanctions are equally successful in shortening conflicts. Total economic embargoes and arms sanctions are effective, but trade sanctions, aid suspension, and other sanctions are not. We also find that both multilateral and unilateral sanctions reduce the duration of civil wars.

JEL Classification*: C41, D74, F51, O19*
Key words*: Sanctions, Civil Conflicts, Duration of conflict, Hazard Model*

1. Introduction

Internal conflicts or civil wars are not only pervasive, they are also persistent. Almost 90% of all civil wars during the last decade took place in countries that had already experienced a civil war in the last 30 years (World Development Report, 2011). The average duration of civil wars is quite large compared to that of inter-state wars. For example, while Bennett and Stam (1996) found that international wars last, on an average, 11 months, according to Collier et al. (2004) the average civil war duration is 7 years in their dataset. Fearon (2004) found that the average civil war duration was even longer: 12 years in their sample.[2] The negative consequences of civil wars are not just limited within the boundary of a country, they also affect the regional and international community. Thus, conflict resolution is crucial not only for the countries involved, but also for regional and international security and stability (political, social, and economic). Considering the longevity and adverse consequences of civil wars, international community has been intervening in civil wars in many different ways.

The literature has pointed out many factors that determine the duration of civil conflicts. These factors include level of income, income inequality, natural resource abundance, geographic characteristics, ethnic fractionalizations, types of conflicts, outside interventions etc. (e.g., Collier et al., 2004; Fearon and Lation, 2003; Fearon, 2004; Regan, 2002; Regan and Aydin, 2006). Not all studies agree on specific factors, but there is a general consensus on a series of structural factors and

[1] Corresponding author.
[2]The difference in average duration of civil wars in two studies arises from the fact that they define civil war differently and their sample periods are different also. While Collier et al. (2004) define civil war in terms of 1000 deaths per year and cover the period 1960-99, Fearon (2004) define civil war in terms of 1000 deaths for the entire war and cover the period 1945-1999.

outside interventions that influence the expected duration of conflicts. In this study, we focus on the role of international sanctions as a determinant of the length of civil wars.

Sanctions are interventions based on coercive measures imposed by a country, an international organization, or a coalition of countries against a group or groups involved in a conflict, with the aim of reducing it (Escribà-Folch, 2010). In theory, the way sanctions work is simple: sanctioned countries (called targets) suffer costs resulting from actions taken by the sanctioning countries (called senders). In order to avoid the costs, the targets modify their behavior in the direction desired by the senders. Sanctions come in different shapes and sizes: total economic embargo, partial economic embargo, export/import restrictions, cancellation of foreign aid, blockade, asset freeze, travel ban, suspension of economic agreement etc. In the past few decades, the use of economic sanctions has increased noticeably. In 2012, the German Institute of Global and Area Studies (GIGA) has listed more than 120 episodes of economic sanction during 1990 to 2010. The empirical studies on the effectiveness of sanctions however find mixed results.

Hafbauer et al. (1990) found very low success rate of sanctions. Assessing UN sanctions in the 1990s, Cortright and Lopez (2000) also found that most of the sanctions failed to change the behavior of the targets. Pape (1997) argues that sanctions basically do not work, only the use of military forces that accompany sanctions might work. Analyzing 26 conflicts between 1989 and 2006, Le Billon and Nicholas (2007) conclude that military intervention and revenue sharing are usually more successful than sanctions in ending resource conflicts. However, they also find that sanctions and revenue sharing promote durable peace compared to military interventions. According to Thyne (2006), sanctions have no significant effect on the onset of civil wars. Many studies find that effectiveness of sanctions depend on other factors: the initial stability of, and the cost to, the target country (Davis and Radcliff, 1997), the political regime of the target country (Noorunddin, 2002; Lektzian and Souva, 2007), the sender's perception about the importance of the issue (Ang and Peksen, 2007), the types of conflict (Le Billon and Nicholas, 2007).

Then turning to the effect of sanctions on the duration of conflicts, the existing evidence is partial and based on only a limited number of cases (Strandow, 2006). Most of the studies suggest that outside interventions tend to extend the expected duration of civil wars. For example, using a hazard model of duration analysis, Regan (2002) finds that third party interventions tend to increase the expected duration of conflicts rather than shorten them. Intervention may exacerbate wars by reducing the cost of rebellion (Elbadawi and Sambian, 2000). Many studies even find that external or third-party interventions make it difficult to reach an agreement or a military victory, and thus lengthen the conflict. For example, Mason et al. (1999) find that third-party interventions make the negotiated settlement more unlikely. Using Correlates of War (COW) data of civil conflicts and external interventions, Balch-lindsay and Enterline (2000) find that biased interventions increase the duration of civil war. Buhaug et al. (2002) also find that intervention on the government side increases the duration of civil war.

Regan and Aydin (2006) argue that mediation/diplomacy and timing of intervention are keys to the success of intervention in reducing the expected duration of conflicts. Some studies find that external interventions or sanctions are associated with a shorter intrastate conflict. For example, the diamond embargo imposed on warring groups of Ivory Coast, Sierra Leone, Liberia, and Angola were effective in shortening the conflicts there (Escribà-Folch, 2010; Wallensteen et al., 2006). Using data on 55 civil wars between 1960 to 2000, Collier et al. (2004) show that economic sanction has a positive but insignificant effect on the length of war, and only military intervention on the rebel side shorten war. Another study, using a sample of 213 wars and external interventions covering the period of 1867-1997, by Balch-Lindsay et al. (2008) finds that third party interventions supporting one side reduces the time until that group achieves victory, but it makes negotiated settlement more unlikely. In contrast, according to DeRouen and Sobek (2004), an intervention by UN increases the probability of truce and decreases the likelihood of one-sided victory. The strongest evidence of the effectiveness of sanctions is found by Escribà-Folch (2010). Using a sample of 87 wars between 1959 to 1999, he finds that sanctions and their duration significantly reduce civil war duration. He also shows that total economic embargoes are the most effective type of sanctions, and sanctions imposed by international organizations increase the likelihood of conflict resolution.

Many scholars also debate about what types of sanction are likely to more effective: comprehensive sanctions that target the whole country or use all type of instruments to maximize general costs, or smart sanctions that target specific group or use specific instrument to avoid suffering of general population. Empirical evidence on this is also mixed. Some suggest that comprehensive sanctions tend to be more successful (Cortright and Lopez, 2002; Nooruddin, 2002; Hufbauer, 2007). On the other hand, Strandow (2006) argues that arms embargoes if properly implemented, tend be more effective as they target the military capacity of the groups.

International sanctions are imposed either by multilateral institutions (like UN, NATO, EU) or by a state (or a small coalition of them). In this context, an issue that has been examined is: what kinds of senders are more effective: multilateral vs. unilateral? Some studies suggest that unilateral sanctions are more successful (e.g., Hufbauer et al., 1990; Drezner, 1999), while other studies find that institutional sanctions are more effective (e.g., Bapat and Morgan, 2007; Escribà-Folch, 2010).

Thus, there is no conclusive evidence either in favor of or against the effectiveness of sanctions on the resolution or on the duration of civil wars. That is why we re-examine the effects of sanctions on the duration of civil war with a new and extended dataset. By using civil war and sanctions data for the period of 1960-2008, we examine how the sanctions aiming to reduce civil conflict affect the expected duration, or the likelihood of resolution, of conflicts.

Our data include 121 civil war incidences occurring in 67 countries. By using hazard model of duration analysis, we examine the effects of different types of international sanctions on the expected duration of civil wars. While Escribà-Folch (2010) also studies the effects of sanctions on the likelihood of ending conflicts, our study is different in several ways. First, we use a new and extended dataset for the study: Escribà-Folch uses the data from 1959 to 1999, whereas we use the data from 1960 to 2008. Note, since the early 2000s, UN and other international organizations increased their policy interventions to terminate the civil conflicts. How this policy shift has helped to settle the wars can only be captured by our new data. We also use some relevant control variables that are not used in Escribà-Folch's study. Second, while Escribà-Folch uses logit model for duration analysis, we use hazard model, which is more appropriate for the duration analysis.[3] We use different robustness checks for our result, while he does not use any. Finally, the existing empirical studies including Escribà-Folch study do not deal with the issue of possible two-way causality between sanctions and the duration of civil wars, which we do.

Contrary to the most of the previous findings, we find that sanctions, at the aggregate level, reduce the expected duration of civil conflicts. Our finding is robust for different controls, different parametric models, and with consideration of endogeneity of sanctions. However, once we disaggregate sanctions into its separate components, we find that not all types of sanction are equally successful in shortening conflicts. Total economic embargoes and arms sanctions are effective, but trade sanctions, aid suspension, and other sanctions have positive but statistically insignificant effects. Both multi-lateral and unilateral sanctions (mainly U.S. sanctions) are found to be associated with shorter civil wars. Finally, we want highlight the difference between results of our study and Escribà-Folch's study. Though the result of the effect of aggregate sanction is similar in both studies, the effects of disaggregate sanctions are different. First, we find that military sanctions reduce the conflict duration, while he finds that it has no effect. Secondly, he finds that only institutional sanctions reduce the duration, while we find that both institutional and non-institutional sanctions can reduce the duration of conflict.

The rest of the paper is organized as follows. In section 2, we define our variables and mention the data sources. Section 3 outlines the model specification, and in section 4, we present and analyze our findings. Finally, section 5 makes some concluding remarks.

2. Variables and Data

Different dataset code civil war differently as there is no single agreed definition of civil war. For example, the Correlates of War (COW) considers an internal conflict as civil war if it results in at

[3]Most of the authors also use duration model to study the effects of interventions on duration of conflicts e.g., Coller et al. (2004), Regan (2002), Regan and Aydin (2006). However, unlike us and Escribà-Folch (2010), these studies do not consider the effects of different types of sanction.

Table 1: List of Civil Wars: 1960-2008

Country	War years	Country	War years	Country	War years
AFGHANISTAN	1978-89	HAITI	1991-95	PHILIPPINES	1972-
AFGHANISTAN	1990-02	HAITI	2004-04	ROMANIA	1989-89
AFGHANISTAN	2003-	INDIA*	1960-	RUSSIA	1994-96
ALGERIA	1962-62	INDIA	1965-	RUSSIA	1999-05
ALGERIA	1991-00	INDIA	1984-88	RWANDA	1962-65
ANGOLA	1975-02	INDIA	1989-	RWANDA	1990-94
ANGOLA	1992-02	INDONESIA*	1960-60	RWANDA	1997-02
ARGENTINA	1974-77	INDONESIA	1975-98	SENEGAL	1989-03
AZERBAIJAN	1991-94	INDONESIA	1999-05	SIERRA LEONE	1991-02
BANGLADESH	1976-97	IRAN	1978-79	SIERRA LEONE	1997-02
BOSNIA	1992-95	IRAN	1980-93	SOMALIA	1982-91
BURUNDI	1972-73	IRAQ	1961-74	SOMALIA	1991-97
BURUNDI	1988-88	IRAQ	1994-96	SOMALIA	2001-02
BURUNDI	1993-08	IVORYCOST	2002-04	SOMALIA	2006-08
CAMBODIA	1970-75	JEORGIA	1992-94	SOUTH AFRICA	1983-94
CAMBODIA	1978-91	JORDAN	1970-70	SRI LANKA	1971-71
CAF	1996-97	LAOS	1960-73	SRI LANKA	1983-
CAF	2001-	LEBANON	1975-90	SUDAN	1963-72
CHAD	1965-79	LIBERIA	1989-96	SUDAN	1983-
CHAD	1980-88	LIBERIA	2000-03	SYRIA	1979-81
CHAD	1997-02	MALI	1989-95	TAJIKISTAN	1992-97
CHAD	2005-06	MOLDOVA	1992-92	THAILAND	1974-81
CHINA	1991-99	MOROCCO	1975-88	THAILAND	2003-05
COLOMBIA	1963-	MOZAMBIQUE	1976-92	TURKEY	1977-80
CONGO	1998-99	MYANMAR	1968-	TURKEY	1984-
CONGO	2002-03	MYANMAR	1983-	UGANDA	1980-88
CROATIA	1992-95	MYANMAR	1988-	UGANDA	1993-
CYPRUS	1974-74	NEPAL	1960-62	UK	1969-98
DJIBOUTI	1991-94	NEPAL	1996-06	VIETNAM, S.	1960-64
DOMINICAN REP.	1965-65	NICARAGUA	1978-79	YEMEN ARAB REP.	1962-69
DRC	1960-65	NICARAGUA	1981-89	YEMEN	1986-86
DRC	1977-78	NIGERIA	1966-70	YEMEN	1994-94
DRC	1996-97	NIGERIA	1980-80	YEMEN	2004-05
DRC	1998-01	NIGERIA	2004-04	YEMEN	2007-07
EL SALVADOR	1979-92	PAKISTAN	1971-71	YUGOSLAVIA	1991-91
ETHIOPIA	1974-92	PAKISTAN	1973-77	YUGOSLAVIA	1998-99
ETHIOPIA	1994-	PAKISTAN	1993-99	ZIMBABWE	1967-68
GEORGIA	1992-94	PAKISTAN	2004-06	ZIMBABWE	1972-79
GUATEMALA	1965-95	P. N.G.	1988-98	ZIMBABWE	1983-87
GUINEA	2000-02	PERU	1980-99		
GUINEA BISSAU	1998-99	PHILIPPINES	1968-		

Note: DRC- Democratic Republic of Congo, CAF-Central African Republic. * Wars started before 1960.

least 1,000 battle related death in a given year.[4] In contrast, the UCDP/PRIO Armed Conflict Dataset defines conflict as contested incompatibility that concerns government and/or territory where the use of armed force between two parties, of which at least one is the government of a state, results in at

[4]The Correlates of War project is an academic study of the history of warfare. It was started in 1963 at the University of Michigan by political scientist J. David Singer. The detail discussion of the dataset can be found in www.correlatesofwar.org.

least 25 battle-related deaths.[5] For our analysis, we use the PRIO/UCDP definition of civil conflicts, as it is used in most of the recent empirical studies on civil wars.

To determine the duration of a civil war, the start date and the end date of the war as well as choice of death thresholds are important. Different datasets record different start date and end date of civil wars. Even the start year and the end year are different depending on the definition and death threshold. A higher death threshold reduces the length of civil wars. Furthermore, a higher threshold leads to a higher number of repeat-war episodes, while lower threshold may record it as one-war episode. For our analysis, we use only the yearly information on civil war duration according to UCDP civil war criteria.

We use data on civil conflicts for the period of 1960 to 2008 (see Table 1 for the list of wars). Civil conflicts data are collected and combined from Escribà-Folch (2010) replication data (up to 1999), the COW dataset, and UCDP dataset. Our data include 121 civil war incidences occurring in 67 countries. For duration analysis, our dependent variable consists of two variables: analysis variable and event (failure) variable. Our analysis variable is civil war duration, which is the number of years a civil war has survived or is surviving (if the war is ongoing) up to a given year. Our event variable is whether or not war ends in a given year, and we use a dummy variable namely 'war end' which is coded 1 if the war ends and 0 otherwise.

Our main explanatory variables are sanctions. Data on sanctions are collected from Escribà-Folch (2010) replication data, which has data for the period of 1959-1999. These data are compared, amended, and widened using few more datasets: Hufbauer, Schott & Elliott's (2008) dataset and Threat and Imposition of Sanctions (TIES) dataset, German Institute of Global and Area Studies (GIGA) dataset. We use a dummy variable called 'sanction', coded as 1 if a country under any type(s) of sanction in a given year, 0 otherwise. The TIES dataset classifies sanctions according to the types of measure. We construct five sanction variables as follows: total economic embargo, multilateral arms embargo, trade sanction (imports and exports restrictions), aid suspension, and other measures (e.g., blockade, asset freeze, travel ban, suspension of economic agreement). As mentioned before, sanctions can be multilateral or institutional (e.g., imposed by UN, EU, or other multilateral organizations) or it can be unilateral (imposed by a country). To capture whether the effects of these two types of sanction are different, we construct two more sanction variables: one is 'unilateral sanction', which takes the value 0 if no sanction, 1 if a country is under sanctions imposed by a country; and another is 'multilateral sanction', which takes 0 if no sanction, 1 if a country is under sanctions imposed by an international institution or group of countries. Some sanctions were jointly imposed by US and UN, or US and EU, or by all three; we regard such sanctions as multilateral ones.

We use a set of country-year control variables that are used in the literature. Collier et al. (2004) argue that structural characteristics of the economy like level of income and distribution of income affect the duration of civil war. Thus, we use per capita GDP and Gini-coefficent measure of income inequality as control variables. Many studies show that the abundance or dependence of natural resources and primary commodities affect both the onset and duration of civil war (e.g., Collier and Hoeffler, 2002 & 2004; Collier et al., 2004; Ross, 2004). We use several alternative measures of resource abundance/dependence: ratio of primary commodity exports to GDP, oil rent as percentage of GDP, mineral exporter (take the value 1 if mineral exports in any year exceeded 50%, 0 otherwise), oil exporter (coded as 1 if oil exports exceed one-third of total exports, 0 otherwise), oil production per capita (in barrels), diamond production per capita or per square kilometer (in carats). Civil wars tend to last longer if the rebels have the opportunity to finance contraband (Fearon, 2004). Thus, we include a variable for the use of contraband (dummy variable taking the value 1 if the war is financed by contraband, 0 otherwise). Some researchers argue that ethno-linguistic and religious fractionalizations may affect civil war (Collier et al., 2004; Fearon and Latin, 2003). We use the Fearon (2004) measure of ethnic fractionalization, which measures the probability that two randomly selected persons from a country do not belong to the same ethnic group. Similarly, religious fractionalization is defined as the probability that two randomly selected individuals are from different religious groups. Geographic characteristics like proportion of mountainous terrain and

[5]The Uppsala Conflict Data Program (UCDP) is a data collection project on organized violence housed at Uppsala University in Sweden. It was first complied byr Gleditsch et al. (2002).

Table 2a: List of Variables and Data Sources

Variable	Data Source
War duration (in year)	COW, UCPD, Escriba`-Folch (2010)
War end (dummy)	COW, UCPD, Escriba`-Folch (2010)
Sanction (dummy)	Escriba`-Folch (2010), Hufbauer, Schott & Elliott's (2008), TIES, GIGA
Total economic embargo (dummy)	Escriba`-Folch (2010), Hufbauer, Schott & Elliott's (2008), TIES, GIGA
Aid end (dummy)	Escriba`-Folch (2010), Hufbauer, Schott & Elliott's (2008), TIES, GIGA
Trade sanction (dummy)	Escriba`-Folch (2010), Hufbauer, Schott & Elliott's (2008), TIES, GIGA
Other sanctions (dummy)	Escriba`-Folch (2010), Hufbauer, Schott & Elliott's (2008), TIES, GIGA
Arms embargo (dummy)	Escriba`-Folch (2010), Hufbauer, Schott & Elliott's (2008), TIES, GIGA
Multi-lateral sanction (dummy)	Escriba`-Folch (2010), Hufbauer, Schott & Elliott's (2008), TIES, GIGA
Unilateral sanction (dummy)	Escriba`-Folch (2010), Hufbauer, Schott & Elliott's (2008), TIES, GIGA
Population (in thousands)	WB, Pen World Table 7.1
Per capita GDP (2005 constant $)*	WB, Pen World Table 7.1
Per capita GDP in PPP (international $)*	WB, Pen World Table 7.1
Gini-efficient (index, 0-100)*	WB
Male secondary school enrollment ratio*	WB
Army size (per 1000 population)	Escriba`-Folch (2010)
Battle death per year	Escriba`-Folch (2010), UCPD
polity2 (index, -10 to +10)	Polity IV project, CSP
Mountainous area (% of total land)	Escriba`-Folch (2010), Fearon (2004)
Forest area (% of total land)*	WB
Ethnic fractionalization (index, 0-1)	Escriba`-Folch (2010), Fearon (2003)
Religious fractionalization (index, 0-1)	Escriba`-Folch (2010), Fearon (2003)
Number of border	Escriba`-Folch (2010)
Primary commodity exports (% of GDP) *	Escriba`-Folch (2010), Fearon (2005)
Oil rent (% of GDP, interpolated) *	WB
Mineral exporter (dummy)	Escriba`-Folch (2010), Fearon (2005)
Oil exporter (dummy)	Escriba`-Folch (2010), Fearon (2005)
Oil production per capita (in barrels) *	Escriba`-Folch (2010)
Diamond production per capita (in carats) *	Escriba`-Folch (2010)
Diamond production per square kilometer (in carats) *	Olsson (2007), Geology.com
Contraband (dummy)	Escriba`-Folch (2010)
Military intervention (dummy)	Escriba`-Folch (2010)
External intervention (dummy)	Cunningham (2010)
Ethnic war (dummy)	Escriba`-Folch (2010), Fearon (2004)
Sons of civil war (dummy)	Escriba`-Folch (2010), Fearon (2004)
Post-cold war (dummy)	= 0 if the year is before 1990, =1 if 1990 and after
Non-member of UNSC	= 0 if member of SC during the war, 1 = otherwise

Notes: * interpolated for missing values, COW-Correlates of War, UCPD-Uppsala Conflict Data Program, TIES-Threat and Imposition of Sanctions, GIGA-German Institute of Global and Area Studies, WB-World Bank, UNSC-United Nation Security Council.

jungles also affect the duration of civil war (Buhaug et al., 2005). So, we include two separate variables to capture these geographic characteristics: the proportion of mountainous area in total area, the proportion of forests in total area. Government military capacity definitely affect the duration of civil war. Thus, we include the size of army (per 1,000 inhabitants) to represent the government capability to fight. However, the effects of government capability might be quadratic, i.e., civil wars

might be short for very week and very strong government. Thus, we also include square of army size as explanatory variable. Regan (2002) argues that third party or external intervention on either government side or rebel side or both sides affect the duration of civil war. We include a dummy variable called 'external intervention', which is coded 1 for the year the country is under some sort of external military intervention. Fearon (2004) shows that some categories of civil war tend to last longer than others. We consider two types of civil war variables: 'ethnic war' which takes value 1 if the ongoing war is an ethnic nature and 0 otherwise, and 'sons of soil' conflict (dummy variable tanking 1 or 0) that typically involve land conflict between a peripheral ethnic minority and state-supported migrants of a dominant ethnic group. Other relevant control variables that are used in our analysis include: country's population, average battle related death per year, and polity 2 (polity IV project).

The data for our control variables are collected from different sources (see Table 2a for the list of variables and data sources). We use data of many control variables from Escribà-Folch (2010) replication dataset, Fearon & Lation (2003) dataset, and Fearon (2004 & 2005) dataset. These variables include primary commodity exports, mineral and oil exporters, oil and diamond productions, ethnic and religious fractionalizations, the use of contraband, mountainous terrain, army size, civil war, sons of soil war. We extend these datasets using other sources whenever required. We collect population and per capita GDP data from World Bank and Pen World Table 7.1. Gini-coefficient, oil rents, and forest area data are collected from world Bank's World Development Indicator dataset. Battle related death data are available at PRIO/UCDP dataset and Escribà-Folch (2010) replication dataset; we compare and contrast both dataset. External intervention data are collected and combined from two sources: Cunnigham (2010), and Escribà-Folch (2010). Polity 2 data are collected from Center for Systemic Peace's Polity IV project. For some time-varying covariates, the data of all years are not available. In such cases, we interpolate the missing years' data using the data of the closest years available.

Table 2b reports the summary statistics of our variables. It shows that mean war duration in our sample is more than 11 years, with minimum of 1 year and maximum of 62 years. Thus, as mentioned before, on average civil wars are long-lasting. Out of 1020 total war years in the sample, 36% are under some types of sanction. 55% of these sanctions are unilateral sanctions and the rest are multilateral sanctions. Note, different types of sanction are not mutually exclusive. Arms embargo is most common types of sanction, followed by aid suspension and trade sanction. Our sample countries vary vastly in terms of both population size and per capita income. The average per capita income of sample countries is only $1833, which implies that conflict affecting countries are generally poor. Average male secondary school enrollment ratio is quite low (38.7%) in these countries. The intensity of civil wars in terms of battle related deaths is high with average deaths of 11000 per year.

3. Model Specification

The purpose of the study is to estimate the effects of sanctions on civil war duration and on the likelihood of ending of the conflict. A useful way to think about the effect of interventions (sanctions) on a conflict duration is to treat it as if an intervention is taking place at a discrete point in time. As a result of an intervention, the conflict either remains at the status quo condition or moves to an alternative state, which we will call the termination of the conflict. The usual approach of testing such effects is to the use of a duration or hazard model (Allison, 1984; Bennett, 1999; Box-Steffensmeier and Jones, 2004). A Hazard model allows us to determine the likelihood of a transition to state t_i, given it is at state t_0, due to changes in a series of explanatory variables. For conflict duration analysis, the hazard model estimates the chance of a conflict termination at *t*, given that it has survived until *t*.

We use an event history approach to model the expected duration of civil conflicts. Among the competing parametric models of hazard (or survival) analysis, we have chosen to test the model

Table 2b: Summary Statistics

Variable	Observation	Mean	Standard Deviation	Min.	Max.
War duration (in year)	1020	11.32647	11.22533	1	62
War end (dummy)	1020	0.101961	0.3027452	0	1
Sanction (dummy)	1020	0.364706	0.4815837	0	1
Total economic embargo (dummy)	1011	0.038576	0.1926767	0	1
Aid end (dummy)	1010	0.105941	0.307914	0	1
Trade sanction (dummy)	1011	0.087043	0.2820369	0	1
Other sanctions (dummy)	1005	0.065672	0.2478306	0	1
Arms embargo (dummy)	1019	0.144259	0.3515248	0	1
Multi-lateral sanction (dummy)	1020	0.162745	0.369314	0	1
Unilateral sanction (dummy)	1020	0.201961	0.4128485	0	1
Population (in thousands)	1020	79625.98	202311.4	612.589	1300000
Per capita GDP (2005 constant $)	1020	1833.806	4239.119	50.0422	33344.1
Per capita GDP in PPP (international $)	1020	1834.949	2632.704	94.7089	23850.2
Gini-efficient (index, 0-100)	941	40.91148	9.567844	22.78	61.33
Male secondary school enrollment ratio	950	38.70466	24.65422	2.138	103.322
Army size (per 1000 population)	1020	12.40768	12.54353	0.49678	84.9979
Battle death per year	1020	10919.6	97173.36	40	3000000
polity2 (index, -10 to +10)	1005	-0.26866	6.245434	-10	10
Mountainous area (% of total land)	1020	25.71216	23.8946	0	81
Forest area (% of total land)	1020	30.8112	22.03939	0.241588	77.5879
Ethnic fractionalization (index, 0-1)	1020	0.565014	0.2454206	0.003996	1
Religious fractionalization (index, 0-1)	1020	0.41486	0.0990027	0.0958	0.6418
Number of border	1020	4.272549	2.427402	0	14
Primary commodity exports (% of GDP)	1005	0.103157	0.0974174	0.005	0.547
Oil rent (% of GDP, interpolated)	1020	3.54235	9.755719	0	66.47643
Mineral exporter (dummy)	1020	0.111765	0.3152313	0	1
Oil exporter (dummy)	1020	0.130392	0.3368996	0	1
Oil production per capita (in barrels)	1010	0.008099	0.0242019	0	0.1956964
Diamond production per capita (in carats)	1017	0.022554	0.1004609	0	1.104837
Diamond production per square kilometer (in carats)	1020	0.327549	1.382838	0	8.41
Contraband (dummy)	1020	0.322549	0.4676811	0	1
Military intervention (dummy)	1014	0.211045	0.4082519	0	1
External intervention (dummy)	1012	0.171937	0.377512	0	1
Ethnic war (dummy)	1016	0.534449	0.4990575	0	1
Sons of civil war (dummy)	1004	0.311753	0.4634404	0	1
Post-cold war (dummy)	1020	0.519608	0.4998605	0	1
Non-member of UNSC	1020	0.642157	0.4796008	0	1

with a Weibull parameterization.[6] The Weibull model allows us to test for duration dependency in the termination of civil conflict, which is an advantage over other event history analysis methods.

[6]There are alternative specifications of hazard model, e.g., Exponential model, Gompertz model, Log logistic model, Log normal model, Cox proportional hazard model. Each model makes different assumptions about duration dependence. The advantage of Weibull specification is that it does not assume a functional form of the dependence parameter, instead allows one to test for the existence of duration dependence.

Without any covariates, the basic functional form of the hazard rate, $h(t)$, using a Weibull specification is the following:

$$h(t_i) = \lambda p(\lambda t)^{p-1} = pt^{p-1}\lambda^p ; t > 0, p > 0, \lambda > 0, \quad (1)$$

where $h(t)$ is the estimated hazard rate at time t, p is the shape parameter, and λ is the positive scale parameter. The parameter p accounts for duration dependence. When p =1, there is no duration dependence, and the hazard rate, $h(t) = \lambda$, is constant. When 0< p<1, the hazard rate decreases monotonically over time. When p>1, the hazard rate increases monotonically, although not necessarily linearly. Covariates X (independent variables) can be added into the model as influences on the hazard rate by specifying the following:

$$h(t_i) = pt^{p-1}\lambda_i^{\ p} = h_0(t)\lambda_i^{\ p}, \text{ and } \lambda_i = e^{-\beta X_I} \quad (2)$$

where $h_0(t)$ is called baseline hazard, when all covariates are zero.[7] Positive β implies that hazard decreases and average survival time increases as X increases.

For our cross-sectional time series analysis of war duration, we specify the following proportional hazard model:

$$h(t_i / I_{it}, C_{it}) = pt^{p-1}.\exp(\beta I_{it} + \gamma C_{it}) \quad (3)$$

In this functional form, $h(.)$ reflects the rate at which a civil conflict terminates at time t given that it has survived until t, p is the duration dependency parameter, I is the vector denoting interventions (sanctions), and C is the vector denoting control variables. β and γ represent the vectors of the coefficients on the variables of interest. Positive duration dependency (p>1) suggests that the conflict is more likely to terminate with the passing of time, whereas negative duration dependency (0<p<1) suggests the institutionalization of the conflict: as the adversaries continue fighting, their chances of settling the conflict also decrease over time. Note, our explanatory variables include both time invariant and time-varying covariates. Thus, we estimate a hazard model that accounts for the impact of a series of covariates on the expected duration of a conflict. For estimation purpose, we use Maximum Likelihood (ML) estimation method.

Previous studies on the effectiveness of sanctions ignore the possibility of endogeneity of the sanction variable. However, we consider this issue in our paper. Two possible sources of bias in estimation are: (i) selection bias - the threat of sanction might be more effective than imposed sanction, and (ii) omitted variable bias - unobserved factors may affect both sanctions and war duration, which are not included as regressors in our model. These unobserved factors include political grievance, culture, institutions, poverty, relationship with other countries, international geo-political situation, international institutions, and the like.

To test for selection bias, we include a variable called 'non-imposed sanction threat' (take the value 1 if a country is threatened but eventually sanctions are not imposed, 0 if no threat or sanction is applied) as a regressor in our model, and test whether non-imposed sanction threat affect the war duration. To test for unobserved heterogeneity, we estimate the *frailty* model of hazard function, which test for unobserved variation in the hazard rate. A frailty model is a survival model with unobservable heterogeneity, or frailty. At the observation level, frailty is introduced as an unobservable multiplicative effect, α, on the hazard function, such that $h(t/\alpha) = \alpha h(t)$. The frailty, α, is a random positive quantity and, for model identiability, is assumed to have mean 1 and variance θ. We test the presence of unobserved heterogeneity by the likelihood ratio test of $H_0 : \theta = 0$.

According to Masuhara (2013), in case of duration model, only controlling for unobserved heterogeneity is not sufficient to deal with endogeneity. It is important to consider both heterogeneity and endogeneity in duration analysis. One possible source of endogeneity is reverse causality.

[7]The above model allows for the presence of an intercept term, β_0, within X_i. Thus, the baseline hazard function is actually equal to $h_0(t).\exp(\beta_0)$.

Sanctions may go to the conflicts that international community perceive would be long-lasting. In this case causation run in opposite direction and we may find a positive association between sanctions and war duration. To deal with endogeneity problem we need to find proper instrument(s).[8] Even if one finds an instrument, there is no established or standard methodology for applying instrumental variable technique in case of duration model in particular and nonlinear model in general. Terza et al. (2008), and Atiyat (2011) suggest a two-stage regression method, like *two-stage least squares* (2SLS) technique in case of linear models. In the first-stage, the endogenous variable is regressed on the appropriate instrumental variable(s) and other exogenous regressors in the system. An appropriate non-linear model is used to estimate this first-stage model and residuals are estimated from it. The first-stage residuals are used as a regressor along with endogenous regressor and other variables in the second-stage regression. This is called *two-stage residual inclusion* (2SRI) method. Terza et al. (2008), and Atiyat (2011) show that 2SRI method produces consistent estimators.[9]

For our sanction variable, we consider two instruments: (i) post-cold war period, and (ii) Security Council membership of the conflict affected country. The episodes of sanctions have increased significantly after the end of cold war. This is because the end of cold war has given more freedom to both US and UN Security Council to impose sanctions without opposition from the former USSR. We construct a dummy variable 'post-cold war', which takes the value 1 if the conflict year is 1990 or later, 0 otherwise. Thus, we expect a positive relationship between sanctions and 'post-cold war' variables. We consider temporary membership of a country in the Security Council (SC) is an indicator of good international relationship of the country. We expect that a country will less likely to be under sanction, if it has the membership in the SC during the war years. We generate a variable called 'non-membership in SC', taking the value 1 if the country is not a member of the SC any time during the war, 0 otherwise.[10] Thus, we expect a positive relationship between sanctions and 'non-membership in SC' variables.

As an alternative specification, we also estimate the logit model to test how sanctions affect the likelihood of war termination.[11] However, we think that hazard model is more appropriate for our case. Note, logit model is appropriate for discrete time analysis and if the event is not duration dependent. However, if the duration of time leading up to the event is important, as is the case of civil war, then event history model is more appropriate. Moreover, event history model performs better than logit model if there are time varying covariates in regression. Truncation and censoring can also be better dealt with event history model. Censoring, especially right censoring is important for civil war because some of the wars in the sample might be ongoing even if the sample period ends.[12] Thus, for our study the preferred model is event history model/hazard model. The logit model is used for robustness check on our estimates.

4. Empirical Results

To start with, we present survival probabilities of wars over time. The non-parametric Kaplan-Meier survival functions of conflicts under sanctions versus conflicts without sanctions are shown in

[8]A good instrument must satisfy the following three conditions: (1) it cannot be correlated with first-stage disturbance term, (2) it must be sufficiently correlated with endogenous regressor for which it is used (i.e. it must not be 'weak'), and (3) it can neither have a direct influence on dependent variable nor be correlated with the error term in second-stage regression.

[9]They also show that in this case two-stage predictor substitution (2SPS) method, which is the rote extension to nonlinear models of 2SLS method, do not provide consistent estimators. In the first-stage of 2SPS, auxiliary (reduced form) regressions are estimated, and the results are used to generate predicted values for the endogenous variables. The second-stage regression is then conducted for the outcome equation of interest after replacing the endogenous variables with their predicted values.

[10]Information of Security Council membership is available on UN website.

[11] In our case, we can specify the following logit model:

$\Pr ob.(warend_{it} = 1/I_{it}, C_{it}, \alpha_i) = \Phi(\alpha_0 + \alpha_1 + \beta I_{it} + \gamma C_{it})$ where $\Phi(.)$ is logistic cumulative distribution,

$warend_{it}$ is a country-year dummy variable taking the value 0 if the war is ongoing and 1 if the war ends.

[12]In our study at the end of 2008, 16 wars were still ongoing. Hazard model takes in to account all these wars in the analysis of duration of wars.

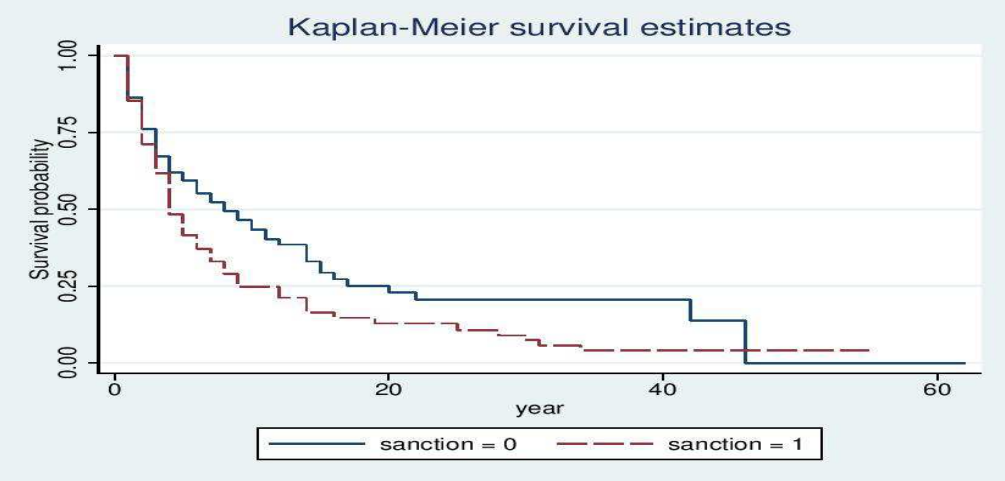

Figure 1: Survival probabilities of wars over time

Figure 1.[13] In this figure the vertical axis represents the probability of a conflict ongoing at any given time, and horizontal axis shows the number of years the conflict is ongoing. The survival curves indicate that the civil wars become slightly less likely to survive with each passing year. Moreover, survival probabilities are lower for the conflicts with sanctions than those without sanction, except for very long-lasting conflicts. A statistical test – the log rank test for the equality of survivor functions – demonstrates that the difference is statistically significant. But this test alone cannot tell whether there are significant differences in survival probabilities (i.e., expected duration of war) in the two cases, because we have not controlled for other determinants of war duration. To determine the causal relationship between sanctions and expected war duration, we have to use regression analysis which controls for other determinants of war duration. The regression results are presented in the following sections.

4.1 Effects of Sanctions

To begin with, we examine the effects of all sanctions in aggregate on the expected duration of civil wars. We estimate the hazard rate of conflict termination using Weibull parameterization, with the unit of analysis being the conflict year. The coefficients of hazard rate are presented to see whether hazard rate increases or decreases with a covariate.[14]

Table 3a reports the estimated coefficients of hazard rate for different regression functions. We see that even without controlling for other covariates, the coefficient of sanction variable is positive and statistically significant (model 1), which implies that sanctions increase the hazard rate of war termination. As we add more and more relevant control variables, the magnitude of sanction coefficient increases and become more significant. Our reference model (# 8) suggests that international sanctions significantly reduce the expected duration of conflict.

[13] Kaplan-Meier estimator of survival function: $S(t_i) = \Pi(1 - d_i / n_i)$, where n_i is the number of observation at risk, and n_i is the number of event (i.e., $d_i = n_i$ is the hazard rate).

[14] We can also present the results in proportional hazard (PH) metric and accelerated failure-time (AFT) metric forms. PH metric shows the effects of explanatory variables on the hazard rate, whereas the AFT metric shows the effects of explanatory variables on the expected duration of conflicts. In the PH model the hazard function is $h(t_j) = h_0(t)g(X_j) = h_0(t)\exp(X_j\beta)$. On the other hand, in the AFT model, the natural logarithm of the survival time, log *t*, is expressed as a linear function of the covariates, yielding the linear model: log $t_j = X_j\beta + z_j$, where z_j is the error with density *f*(.).

Table 3a: Effects of Sanctions on Civil War Duration

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sanction	0.384*	0.341*	0.339*	0.493**	0.513**	0.560**	0.681***	0.679***
	(0.05)	(0.09)	(0.10)	(0.02)	(0.02)	(0.01)	(0.00)	(0.00)
Log of population		-0.319***	-0.383***	-0.391***	-0.250***	-0.309***	-0.380***	-0.303***
		(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.01)
Log of GDP per capita		0.025	-0.215*	-0.343***	-0.391***	-0.349***	-0.417***	-0.425***
		(0.79)	(0.06)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)
Male secondary school enrolment			0.017***	0.016***	0.015***	0.015***	0.017***	0.016***
			(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Oil rent to GDP				0.026***	0.028***	0.030***	0.035***	0.042***
				(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Diamond production per capita				2.829***	2.555***	2.196**	2.454***	3.052***
				(0.00)	(0.00)	(0.02)	(0.01)	(0.00)
Contraband				-1.336***	-1.206***	-1.316***	-1.322***	-1.376***
				(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Son of civil war					-1.284***	-1.283***	-1.404***	-1.655***
					(0.00)	(0.00)	(0.00)	(0.00)
Army size						-0.038	-0.048**	-0.054**
						(0.11)	(0.05)	(0.03)
Army size sq.						0.0003	0.0003	0.0005*
						(0.24)	(0.12)	(0.08)
Log of battle death per year							-0.196**	-0.189**
							(0.01)	(0.02)
Number of border								-0.100*
								(0.10)
Constant	-2.275***	0.452	2.029**	2.812***	1.786*	2.359**	4.820***	4.582***
	(0.00)	(0.61)	(0.03)	(0.01)	(0.08)	(0.03)	(0.00)	(0.00)
Ln (p)	-0.137*	-0.019	-0.019	0.080	0.152*	0.185**	0.235***	0.232***
	(0.07)	(0.81)	(0.82)	(0.30)	(0.06)	(0.03)	(0.01)	(0.01)
N	1013	1013	943	940	927	927	927	927
LL	-193.3	-183.4	-165.0	-145.6	-136.2	-134.5	-131.4	-130.0
AIC	392.6	376.7	342.0	309.3	292.5	292.9	288.9	288.0

p values in parentheses: *p<.10, **p<.05, ***p<.01

This result is robust to the inclusions of other control variables, like Gini-coefficient, external intervention, mountain, forests, ethnic and religious fractionalizations, ethnic war, and polity2 (Table 4). The result is also robust to the use of alternative measure of natural resource abundance (Table 5). Thus, contrary to the findings of most other studies, our findings suggest that sanctions do reduce the war duration. Note that Table 3a shows only the direction of change in hazard rate, it does not show the estimated hazard rates. Table 3b reports the estimated hazard rates for the corresponding models of Table 3a. The reference model 8 in Table 3b shows that sanctions increase the hazard rate of war termination by 97% after controlling for all other relevant variables. Figure 2 shows the estimated hazard functions for the Weibull regression with sanctions and without sanctions. We see that, for each year, the hazard rate is significantly higher under sanction than without sanction.

Table 3b: Effects of Sanctions on Civil War Duration: Hazard Rates

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sanction	1.468*	1.407*	1.403*	1.636**	1.670**	1.751**	1.975***	1.973***
	(0.05)	(0.09)	(0.10)	(0.02)	(0.02)	(0.01)	(0.00)	(0.00)
Log of population		0.727***	0.682***	0.676***	0.779***	0.734***	0.684***	0.738***
		(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.01)
Log of GDP per capita		1.025	0.807*	0.709***	0.676***	0.706***	0.659***	0.654***
		(0.79)	(0.06)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)
Male secondary school enrolment			1.017***	1.016***	1.015***	1.015***	1.017***	1.016***
			(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Oil rent to GDP				1.027***	1.028***	1.030***	1.035***	1.043***
				(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Diamond production per capita				16.935***	12.866***	8.989**	11.632***	21.152***
				(0.00)	(0.00)	(0.02)	(0.01)	(0.00)
Contraband				0.263***	0.299***	0.268***	0.267***	0.253***
				(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Son of civil war					0.277***	0.277***	0.246***	0.191***
					(0.00)	(0.00)	(0.00)	(0.00)
Army size						0.963	0.953**	0.948**
						(0.11)	(0.05)	(0.03)
Army size sq.						1.000	1.000	1.000*
						(0.24)	(0.12)	(0.08)
Log of battle death per year							0.822**	0.828**
							(0.01)	(0.02)
Number of border								0.905*
N	1013	1013	943	940	927	927	927	927
LL	-193.3	-183.4	-165.0	-145.6	-136.2	-134.5	-131.4	-130.0
AIC	392.6	376.7	342.0	309.3	292.5	292.9	288.9	288.0

p values in parentheses: *p<.10, **p<.05, ***p<.01.

From Table 3a we also see that the estimate of the shape parameter p is greater than 1 (as $\log(p)$ is positive) and statistically significant, which implies that the hazard rate is increasing over time.

The interpretations for other statistically significant variables are as follows (see Table 3a and Table 3b). A large population decreases the hazard rate of war termination, implying that more populous countries tend to experience longer civil wars. A higher per capita income increases the expected duration of wars. One might suspect a reverse causality from war duration to per capita

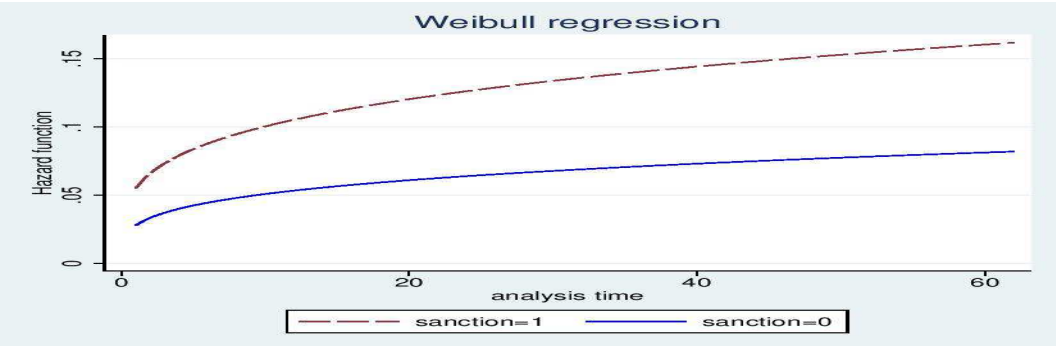

Figure 2: Estimated hazard rates of wars over time

income. To eliminate the possibility of reverse causality, we run separate regression by including the initial level of per capita income of a country for all war years instead of each country-year per capita income. But, we find that the coefficient of per capita income is still negative and statistically significant. This result implies that though lower per capita income is likely to increase the risk of conflict onset (e.g., Collier and Hoeffler, 1998 & 2002; Fearon, 2005), a relatively higher per capita income tends to lengthen conflicts, once they start. A higher male secondary school enrollment reduces the war duration. This finding is intuitive in the sense that higher male secondary school enrollment decreases the opportunity for rebel recruits, as rebel groups typically recruit fighters from young male. Natural resource abundance, measured by both 'oil rent to GDP ratio' and 'diamond production per capita', reduces war duration. A possible interpretation is that higher resource availability increases the government revenue and thus government can build a strong army, which helps the government to win war within a short time. However, more resource rents might encourage rebel groups to fight harder and longer, thus tend to lengthen the war. In our sample, the first effect dominates the second one, resulting in a net reduction of war duration. The opportunity of contraband by rebel groups increases the expected duration of war. The interpretation is straightforward: the rebel groups can finance war by selling natural resources or drugs, and so can fight longer wars. The coefficient of 'Son of civil war' is positive and significant, implying that these types of wars are comparatively longer than other types of civil wars. Army size of a country has a negative but diminishing effect on hazard rate, implying that larger army size increases the war duration, but a very large army can win war quickly. A higher battle related death tends to lengthen wars, implying that more deaths increase grievances among groups and lead to a lengthy war. More neighbors with common borders tend to increase the expected duration of war.

Robustness Checks

Table 4 presents regression equations by adding other relevant control variables to our reference equation. We find that our estimates are robust in general to the inclusion of others but statistically insignificant control variables. Contrary to the findings of Collier & Hoeffler (2004), and Escribà-Folch (2010), we find that ethnic fractionalization does not have a significant effect on war duration. Similarly, the effect of religious fractionalization is not statistically significant. Geographic characteristics, like mountain and forest areas, are not statistically significant as well. Though Balch-lindsay and Enterline (2000), and Regan (2002) find that external or third-party intervention tend to increase the war duration, we do not find such evidence in our estimation. Similar to most of the earlier findings, we find that regime type indicator variable 'polity2' do not have statistically significant effect on war duration.

We also check for the robustness of our estimates by using alternative indicators for resource abundance (Table 5). Instead of oil rent to GDP ratio, we use oil production per capita, oil exporter, mineral exporter variables, and primary commodity export to GDP ratio.[15] With each alternative, our estimates are found to be robust except for the 'number of border' variable.

We also estimate some of the other parametric models of hazard function to see which model fits the data best. Table 6 shows the estimates of different parametric models: Weibull model, Exponential model, Gompertz model, and Cox proportional hazard model. We see that Weibull model gives the best fit with the highest log likelihood value. Weibull model is also preferred model with the smallest AIC.[16] We also estimate a logit model to see how the likelihood of war termination is affected by the sanctions (model 5 in Table 6). We see that the coefficient of sanction is positive and statistically significant, implying that sanctions increase the probability of war termination. Other variables have the same signs as with the hazard model.

[15] Many authors, including Collier & Hoeffler (1998, 2002, 2004), and Fearon (2005) use the primary commodity export to GDP ratio as the indicator of resource abundance in their estimation.

[16] In this case, AIC = -2(log likelihood) + 2(c + p + 1), where c = number of covariates, p = the number of model-specific ancillary parameters.

Table 4: Robustness Check: Other Controls

Variable	(1)		(2)		(3)		(4)		(5)		(6)	
Sanction	0.682***	(0.005)	0.683***	(0.005)	0.698***	(0.004)	0.690***	(0.005)	0.688***	(0.005)	0.703***	(0.007)
Log of population	-0.333***	(0.006)	-0.338***	(0.005)	-0.341***	(0.005)	-0.289**	(0.025)	-0.317**	(0.020)	-0.357**	(0.014)
Log of GDP per capita	-0.354**	(0.012)	-0.340**	(0.021)	-0.351**	(0.021)	-0.444**	(0.010)	-0.422**	(0.016)	-0.401**	(0.027)
Male secondary school enrolment	0.0140**	(0.017)	0.0143**	(0.015)	0.0146**	(0.016)	0.0153**	(0.012)	0.0147**	(0.018)	0.0139**	(0.036)
Oil rent to GDP	0.0409***	(0.000)	0.0401***	(0.000)	0.0409***	(0.000)	0.0427***	(0.000)	0.0423***	(0.000)	0.0408***	(0.000)
Diamond production per capita	3.073***	(0.002)	2.657**	(0.027)	2.702**	(0.025)	2.941**	(0.023)	2.769**	(0.034)	2.562**	(0.049)
Contraband	-1.341***	(0.000)	-1.422***	(0.000)	-1.479***	(0.000)	-1.476***	(0.000)	-1.516***	(0.000)	-1.447***	(0.000)
Son of civil war	-1.574***	(0.000)	-1.570***	(0.000)	-1.561***	(0.000)	-1.733***	(0.000)	-1.645***	(0.000)	-1.540***	(0.001)
Army size	-0.0533**	(0.028)	-0.0566**	(0.021)	-0.0587**	(0.018)	-0.0529**	(0.038)	-0.0527**	(0.041)	-0.0447*	(0.098)
Army size sq.	0.000468	(0.107)	0.000510*	(0.081)	0.000528*	(0.074)	0.000416	(0.184)	0.000409	(0.195)	0.000347	(0.290)
Log of battle death per year	-0.180**	(0.025)	-0.169**	(0.041)	-0.172**	(0.040)	-0.155*	(0.069)	-0.150*	(0.082)	-0.198**	(0.028)
Number of border	-0.0894	(0.138)	-0.0949	(0.120)	-0.0982	(0.111)	-0.114*	(0.073)	-0.0982	(0.129)	-0.0465	(0.525)
Gini index	-0.00963	(0.509)	-0.00796	(0.595)	-0.0105	(0.494)	-0.00997	(0.527)	-0.0118	(0.467)	-0.0114	(0.513)
Ethnic fractionalization			-0.0865	(0.969)	-0.130	(0.954)	1.133	(0.651)	0.850	(0.734)	-0.105	(0.967)
Ethnic fractionalization sq.			0.501	(0.826)	0.623	(0.787)	-0.634	(0.806)	-0.342	(0.894)	0.414	(0.874)
Religious fractionalization					-1.096	(0.848)	-1.867	(0.760)	-1.979	(0.747)	-3.655	(0.562)
Religious fractionalization sq.					0.238	(0.977)	1.078	(0.903)	1.305	(0.883)	5.264	(0.572)
Mountain							-0.00707	(0.228)	-0.00650	(0.269)	-0.00653	(0.275)
Forests							-0.00420	(0.487)	-0.00363	(0.554)	-0.00497	(0.455)
External intervention									-0.156	(0.625)	-0.166	(0.614)
Ethnic war											-0.324	(0.290)
Polity2											-0.00387	(0.874)
Constant	4.717***	(0.002)	4.442***	(0.006)	5.089**	(0.016)	5.280**	(0.017)	5.443**	(0.014)	6.038***	(0.009)
Log(p)	0.252***	(0.003)	0.256***	(0.003)	0.256***	(0.003)	0.261***	(0.002)	0.260***	(0.002)	0.296***	(0.001)
N	857		857		857		857		850		838	
Log likelihood	-121.7		-121.4		-121.2		-120.4		-120.4		-113.9	
AIC	273.4		276.8		280.4		282.8		284.7		275.8	

p values in parentheses: *p<.10, **p<.05, ***p<.01

Table 5: Robustness: Alternative Definitions of Resource Abundance

Variable	(1)	(2)	(3)	(4)	(5)
Sanction	0.679***	0.708***	0.597***	0.557**	0.618***
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)
Log of population	-0.303***	-0.433***	-0.345***	-0.309***	-0.296**
	(0.01)	(0.00)	(0.00)	(0.01)	(0.02)
Log of GDP per capita	-0.425***	-0.399***	-0.382***	-0.326***	-0.390***
	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)
Male secondary school enrolment	0.016***	0.017***	0.017***	0.017***	0.022***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Diamond production per capita	3.052***	2.822***	2.877***	2.483***	1.968**
	(0.00)	(0.00)	(0.00)	(0.01)	(0.04)
Contraband	-1.376***	-1.331***	-1.219***	-1.315***	-1.556***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Son of civil war	-1.655***	-1.158***	-1.409***	-1.399***	-1.372***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Army size	-0.054**	-0.056**	-0.047*	-0.055**	-0.053**
	(0.03)	(0.03)	(0.05)	(0.02)	(0.03)
Army size sq.	0.0005*	0.0003	0.0004	0.001**	0.001*
	(0.08)	(0.28)	(0.12)	(0.03)	(0.07)
Log of battle death per year	-0.189**	-0.168**	-0.186**	-0.157*	-0.143*
	(0.02)	(0.04)	(0.02)	(0.05)	(0.07)
Number of border	-0.100*	-0.019	-0.036	-0.009	0.006
	(0.10)	(0.80)	(0.55)	(0.87)	(0.92)
Oil rent to GDP	0.042***				
	(0.00)				
Oil production per capita		13.519**			
		(0.02)			
Oil exporter			0.872***		
			(0.01)		
Mineral exporter				0.688**	
				(0.02)	
Primary exports to GDP ratio					3.450***
					(0.00)
Constant	4.582***	5.077***	4.414***	3.539**	3.251**
	(0.00)	(0.00)	(0.00)	(0.01)	(0.03)
Log (p)	0.232***	0.245***	0.212**	0.193**	0.195**
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
N	927	863	927	927	912
LL	-130.0	-123.0	-135.8	-136.6	-131.3
AIC	288.0	274.0	299.6	301.2	290.6

p values in parentheses: *p<.10, **p<.05, ***p<.01

Endogeneity of Sanctions

To deal with possible problem of endogeneity, we carry three tests: selection bias test, unobserved heterogeneity test, and use instrumental variable technique. Table 7 presents the results of endogeneity tests. To test for selection bias, we include the variable 'non-imposed sanction threat' as a regressor in our model, and we find that the variable is not statistically significant (model 1). Thus, we can say that selection bias is not a problem in our estimation. To test for unobserved

Table 6: Different Parametric Models and Logit Model

Variable	Wei-bull	Exponential	Gompertz	Cox proportional	Logit
Sanction	0.679***	0.652***	0.666***	0.620***	0.732***
	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)
Log of population	-0.303***	-0.225**	-0.283**	-0.244**	-0.221*
	(0.01)	(0.04)	(0.02)	(0.04)	(0.07)
Log of GDP per capita	-0.425***	-0.360***	-0.410***	-0.376***	-0.413***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Male secondary school enrolment	0.016***	0.014***	0.015***	0.013**	0.015**
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Oil rent to GDP	0.042***	0.037***	0.040***	0.036***	0.047***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Diamond production per capita	3.052***	2.750***	2.919***	2.865***	3.201***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Contraband	-1.376***	-1.233***	-1.362***	-1.295***	-1.237***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Son of civil war	-1.655***	-1.413***	-1.624***	-1.389***	-1.418***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Army size	-0.054**	-0.044*	-0.051**	-0.044*	-0.048*
	(0.03)	(0.06)	(0.03)	(0.06)	(0.06)
Army size sq.	0.0005*	0.0004	0.0005*	0.0004	0.0005
	(0.08)	(0.12)	(0.09)	(0.15)	(0.12)
Log of battle death per year	-0.189**	-0.135*	-0.159**	-0.128*	-0.123
	(0.02)	(0.06)	(0.04)	(0.09)	(0.13)
Number of border	-0.100*	-0.101*	-0.096	-0.097	-0.123*
	(0.10)	(0.09)	(0.11)	(0.11)	(0.06)
Constant	4.582***	3.589**	4.484***		4.047***
	(0.00)	(0.01)	(0.00)		(0.01)
Log (p)	0.232***				
	(0.01)				
Gamma			0.027		
			(0.10)		
N	927	927	927	927	934
LL	-130.0	-133.5	-132.2	-332.3	-267.8
AIC	288.0	293.0	292.5	688.5	561.5

p values in parentheses: *p<.10, **p<.05, ***p<.01

heterogeneity, we estimate a frailty model of hazard function, which tests for unobserved variation in the hazard rate (model 2). Using likelihood ratio test we fail to reject the null hypothesis of no unobserved variation. Thus, we can claim that the unobserved heterogeneity is not present in our model.

Table 7: Endogenieity of Sanction

Variable	(1)		(2)		(3) Sanction		(4) Sanction		(5)		(6)		(7)	
Sanction	0.687***	(0.003)	0.679***	(0.003)					1.361**	(0.037)	1.295**	(0.050)	0.730***	(0.002)
Log of population	-0.307***	(0.009)	-0.303***	(0.010)	0.381***	(0.000)	0.502***	(0.000)	-0.385***	(0.003)	-0.383***	(0.003)	-0.358***	(0.005)
Log of GDP per capita	-0.424***	(0.001)	-0.425***	(0.001)										
Log of initial GDP per capita					0.538***	(0.000)	0.478***	(0.000)	-0.443***	(0.004)	-0.440***	(0.005)	-0.407***	(0.007)
Male secondary school enrolment	0.0159***	(0.003)	0.0160***	(0.002)	-0.0207***	(0.000)	-0.0194***	(0.000)	0.0165***	(0.006)	0.0164***	(0.006)	0.0157***	(0.009)
Oil rent to GDP	0.0425***	(0.000)	0.0423***	(0.000)	-0.0376***	(0.000)	-0.0346***	(0.000)	0.0417***	(0.000)	0.0414***	(0.000)	0.0394***	(0.000)
Diamond production per capita	3.073***	(0.001)	3.052***	(0.002)	2.974**	(0.025)	2.556*	(0.052)	1.980	(0.123)	2.104*	(0.096)	2.389*	(0.050)
Contraband	-1.381***	(0.000)	-1.376***	(0.000)	0.810***	(0.000)	0.923***	(0.000)	-1.569***	(0.000)	-1.561***	(0.000)	-1.446***	(0.000)
Son of civil war	-1.673***	(0.000)	-1.655***	(0.000)	0.407*	(0.081)	0.325	(0.166)	-1.740***	(0.000)	-1.731***	(0.000)	-1.689***	(0.000)
Army size	-0.0539**	(0.025)	-0.0539**	(0.025)	0.119***	(0.000)	0.130***	(0.000)	-0.0598**	(0.024)	-0.0597**	(0.027)	-0.0509**	(0.045)
Army size sq.	0.000494*	(0.082)	0.000494*	(0.083)	-0.00126***	(0.000)	-0.00136***	(0.000)	0.000522	(0.107)	0.000518	(0.114)	0.000419	(0.177)
Log of battle death per year	-0.191**	(0.015)	-0.189**	(0.017)	0.351***	(0.000)	0.329***	(0.000)	-0.210**	(0.021)	-0.204**	(0.025)	-0.176**	(0.039)
Number of border	-0.103*	(0.091)	-0.100*	(0.096)	0.156***	(0.000)	0.144***	(0.001)	-0.0820	(0.215)	-0.0806	(0.223)	-0.0691	(0.292)
Non-imposed sanction threat	0.404	(0.701)												
Ethnic fractionalization					2.490	(0.136)	2.603	(0.120)	-0.598	(0.789)	-0.470	(0.833)	-0.391	(0.861)
Ethnic fractionalization sq.					-2.675	(0.107)	-2.865*	(0.084)	0.841	(0.714)	0.707	(0.758)	0.636	(0.782)
Mountain					0.394**	(0.033)	0.405**	(0.030)	-0.306	(0.234)	-0.284	(0.265)	-0.260	(0.304)
Ethnic war					-0.00547	(0.167)	-0.00773*	(0.056)	-0.00622	(0.232)	-0.00613	(0.239)	-0.00632	(0.224)
Polity2					-0.0535***	(0.001)	-0.0477***	(0.003)	0.0143	(0.545)	0.0140	(0.555)	0.0108	(0.644)
Post-cold war					1.669***	(0.000)	1.577***	(0.000)						
Non-member of SC							0.687***	(0.001)						
Residual 1									-0.287	(0.307)				
Residual 2											-0.262	(0.364)		
Constant	4.634***	(0.002)	4.582***	(0.002)	-13.21***	(0.000)	-14.31***	(0.000)	5.764***	(0.002)	5.658***	(0.002)	5.001***	(0.003)
Log (p)	0.233***	(0.005)	0.232***	(0.006)					0.233***	(0.006)	0.235***	(0.005)	0.242***	(0.004)
Log (θ)			-14.29	(0.980)										
N	927		927		922		922		915		915		915	
LL	-129.9		-130.0		-505.6		-500.3		-123.5		-123.7		-124.1	
AIC	289.9		290.0		1047.2		1038.5		287.1		287.3		286.2	

p values in parentheses: *p<.10, **p<.05, ***p<.01. Likelihood-ratio test of theta=0: chibar2(01) = 0.00 Prob.>=chibar2 = 1.00

To deal with possible two-way causality, we use 2SRI method (suggested by Terza et al. 2008; and Atiyat, 2011) as described in section 3. First, we use 'post-cold war' variable as a single instrument and find that it significantly affects the likelihood of imposing sanction if we use logit regression (model 3). We estimate the residual from first-stage regression, and then include it as a regressor in the second-stage regression. We find that the coefficient of sanction variable increases significantly (compare model 5 and model 7 in Table 7) after correcting for endogeneity. This result implies that if we do not consider reverse causality and endogeneity, the true co-efficient of sanctions will be underestimated. Secondly, we use 'non-membership in Security Council' as a second instrument for sanction in the first-stage regression. We find that both instruments significantly predict sanctions (model 4). Again, we estimate residual from the first-stage, and include it as a regressor in the second-stage (model 6). We find similar results as with first case. Though the coefficient of sanction is slightly lower in the second case, it is still significantly higher than the case without considering endogeneity.

Since instrumental variable technique used in this paper is not standard, we can only take this result as indicative, rather than conclusive. However, our main finding does not change in this case. Thus, contrary to the most of the previous finding, we show that international sanctions do reduce the expected duration of civil wars.

4.2 Different Types of Sanctions

In this section, we consider different types of sanctions: total economic embargo, multilateral arms embargo, trade sanctions, aid end, and other sanctions. Table 8 presents the estimated coefficients of hazard rates for these sanctions. We find that the coefficient of total economic sanction or comprehensive sanction is positive, and statistically significant. This result implies that comprehensive sanctions that cut the total flow of funds to the conflicting parties are very effective in reducing war duration. Our results also show that arms embargo has positive and significant effect on hazard rate of war termination. This implies that restrictions on the supply of arms to the warring parties can lead to a shorter intrastate war. The coefficients of trade sanction and aid-end are positive, but are not statistically significant. Thus, our results suggest that trade sanctions and aid cancellation as tools for war termination are not effective. Others sanctions such as blockade, asset freeze, travel ban, suspension of economic agreement do not appear to have any significant effect on civil war duration either. We also estimate the effects of each category of sanctions individually without controlling for other categories (Table 10), and find that the coefficients of total economic embargo and arms sanction are still positive and statistically significant.

We also estimate the effects of sanctions by dividing sanctions according to the types of sender of sanctions: unilateral and multilateral sanctions. Table 9 reports the estimated results and we find that both multilateral and unilateral sanctions have positive and significant effects on hazard rate of war termination. Thus, our results suggest that both multilateral and unilateral sanctions do reduce the duration of civil war. Note that in our sample vast majority (89%) of the unilateral sanctions were imposed by the United States. Since United States is the biggest military and economic power of the world, sanctions imposed by that country has significant effect on civil war termination. We also run separate regression for these two types of sanction, and find that the coefficients of both multi-lateral and unilateral sanctions are still positive and significant.

Table 8: Effects of Different Types of Sanctions on Civil War Duration

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total economic embargo	0.834**	0.568	0.381	0.536	0.701	0.647	0.921*	0.968**
	(0.03)	(0.15)	(0.37)	(0.22)	(0.12)	(0.15)	(0.05)	(0.04)
Aid suspension	0.188	0.075	0.264	0.230	0.104	0.179	0.362	0.318
	(0.55)	(0.81)	(0.42)	(0.48)	(0.75)	(0.60)	(0.29)	(0.36)
Trade sanction	-0.074	0.003	0.026	0.560	0.500	0.506	0.362	0.390
	(0.87)	(1.00)	(0.96)	(0.26)	(0.32)	(0.32)	(0.48)	(0.45)
Other sanctions	-0.291	-0.157	-0.005	-0.151	-0.117	-0.100	0.090	0.156
	(0.50)	(0.72)	(0.99)	(0.74)	(0.80)	(0.83)	(0.85)	(0.74)
Arms embargo	0.759***	0.649**	0.474*	0.610**	0.686**	0.638**	0.619**	0.583**
	(0.00)	(0.01)	(0.09)	(0.03)	(0.02)	(0.03)	(0.03)	(0.04)
Log of population		-0.282***	-0.343***	-0.352***	-0.194*	-0.254**	-0.331***	-0.266**
		(0.00)	(0.00)	(0.00)	(0.06)	(0.02)	(0.00)	(0.03)
Log of GDP per capita		0.022	-0.189	-0.318**	-0.349***	-0.317**	-0.402***	-0.416***
		(0.83)	(0.13)	(0.02)	(0.01)	(0.02)	(0.01)	(0.00)
Male secondary school enrolment			0.015***	0.013***	0.011**	0.012**	0.014**	0.013**
			(0.00)	(0.01)	(0.04)	(0.04)	(0.02)	(0.02)
Oil rent to GDP				0.027***	0.028***	0.030***	0.035***	0.042***
				(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Diamond production per capita				2.780***	2.494***	2.217**	2.537***	2.995***
				(0.00)	(0.01)	(0.02)	(0.01)	(0.00)
Contraband				-1.354***	-1.227***	-1.295***	-1.244***	-1.281***
				(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Son of civil war					-1.295***	-1.286***	-1.419***	-1.644***
					(0.00)	(0.00)	(0.00)	(0.00)
Army size						-0.031	-0.042*	-0.047*
						(0.20)	(0.09)	(0.06)
Army size sq.						0.0003	0.0004	0.0004
						(0.38)	(0.20)	(0.17)
Log of battle death per year							-0.209**	-0.200**
							(0.01)	(0.02)
Number of border								-0.085
								(0.17)
Constant	-2.403***	0.050	1.509	2.361**	1.104	1.696	4.363***	4.196***
	(0.00)	(0.96)	(0.15)	(0.04)	(0.34)	(0.18)	(0.01)	(0.01)
Log (p)	-0.082	0.003	-0.002	0.090	0.167**	0.196**	0.255***	0.251***
	(0.29)	(0.97)	(0.98)	(0.26)	(0.04)	(0.02)	(0.00)	(0.00)
N	997	997	930	928	915	915	915	915
LL	-183.8	-176.7	-160.7	-141.9	-132.8	-131.5	-128.4	-127.5
AIC	381.6	371.3	341.4	309.8	293.7	295.1	290.8	290.9

p values in parentheses: *p<.10, **p<.05, ***p<.01.

Table 9: Effects of Sanctions: Multi-lateral vs. Unilateral

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Multi-lateral sanctions	0.640***	0.541**	0.422*	0.486*	0.570**	0.571**	0.631**	0.642**
	(0.01)	(0.03)	(0.09)	(0.06)	(0.03)	(0.03)	(0.02)	(0.02)
Uni-lateral sanctions	0.153	0.148	0.245	0.501*	0.447	0.546*	0.760**	0.738**
	(0.55)	(0.57)	(0.36)	(0.07)	(0.11)	(0.06)	(0.01)	(0.02)
Log of population		-0.315***	-0.377***	-0.392***	-0.245**	-0.307***	-0.389***	-0.310***
		(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.01)
Log of GDP per capia		0.035	-0.200*	-0.345***	-0.382***	-0.347***	-0.427***	-0.432***
		(0.71)	(0.08)	(0.01)	(0.00)	(0.01)	(0.00)	(0.00)
Male secondary school enrolment			0.016***	0.016***	0.015***	0.015***	0.017***	0.016***
			(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)
Oil rent to GDP				0.026***	0.028***	0.030***	0.035***	0.042***
				(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Diamond production per capita				2.838***	2.483***	2.184**	2.520***	3.097***
				(0.00)	(0.01)	(0.02)	(0.01)	(0.00)
Contraband				-1.336***	-1.209***	-1.315***	-1.326***	-1.377***
				(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Son of civil war					-1.295***	-1.285***	-1.401***	-1.654***
					(0.00)	(0.00)	(0.00)	(0.00)
Army size						-0.037	-0.050**	-0.055**
						(0.12)	(0.04)	(0.02)
Army size sq.						0.0003	0.0005	0.001*
						(0.25)	(0.11)	(0.08)
Log of battle death per year							-0.202**	-0.193**
							(0.01)	(0.02)
Number of border								-0.099*
								(0.10)
Constant	-2.298***	0.333	1.899**	2.822***	1.698	2.336**	4.998***	4.712***
	(0.00)	(0.70)	(0.05)	(0.01)	(0.10)	(0.04)	(0.00)	(0.00)
Ln (p)	-0.127*	-0.015	-0.017	0.080	0.155*	0.185**	0.235***	0.232***
	(0.10)	(0.85)	(0.84)	(0.30)	(0.06)	(0.03)	(0.01)	(0.01)
N	1013	1013	943	940	927	927	927	927
LL	-192.0	-182.5	-164.9	-145.6	-136.2	-134.5	-131.4	-130.0
AIC	391.9	377.0	343.7	311.3	294.3	294.9	290.7	289.9

p values in parentheses: *p<.10, **p<.05, ***p<.01.

Table 10: Effects of Different Types of Sanctions Separately

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Total economic embargo	0.956**						
	(0.04)						
Aid suspension		0.284					
		(0.40)					
Trade sanction			0.296				
			(0.53)				
Other sanctions				0.402			
				(0.34)			
Arms embargo					0.649**		
					(0.02)		
Multilateral sanction						0.478*	
						(0.06)	
Unilateral sanction							0.547*
							(0.06)
Log of population	-0.270**	-0.331***	-0.316***	-0.326***	-0.283**	-0.276**	-0.331***
	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.00)
Log of GDP per capita	-0.423***	-0.387***	-0.387***	-0.387***	-0.319**	-0.374***	-0.434***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)
Male secondary school enrolment	0.015***	0.018***	0.016***	0.016***	0.014**	0.014***	0.018***
	(0.01)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)
Oil rent to GDP	0.041***	0.038***	0.039***	0.039***	0.038***	0.041***	0.041***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Diamond production per capita	3.203***	3.074***	2.995***	2.901***	2.952***	2.899***	3.290***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Contraband	-1.121***	-1.158***	-1.168***	-1.170***	-1.286***	-1.259***	-1.235***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Son of civil war	-1.680***	-1.578***	-1.608***	-1.595***	-1.650***	-1.662***	-1.654***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Army size	-0.046*	-0.053**	-0.048**	-0.047*	-0.048*	-0.042*	-0.052**
	(0.06)	(0.04)	(0.05)	(0.06)	(0.05)	(0.08)	(0.03)
Army size sq.	0.0004	0.0005	0.0004	0.0004	0.0004	0.0004	0.0005
	(0.17)	(0.11)	(0.15)	(0.17)	(0.15)	(0.21)	(0.11)
Log of battle death per year	-0.176**	-0.163**	-0.143*	-0.152**	-0.162**	-0.141*	-0.176**
	(0.02)	(0.04)	(0.06)	(0.05)	(0.04)	(0.06)	(0.03)
Number of border	-0.097	-0.086	-0.094	-0.091	-0.084	-0.102*	-0.097
	(0.11)	(0.15)	(0.12)	(0.13)	(0.15)	(0.09)	(0.11)
Constant	4.275***	4.464***	4.273***	4.359***	3.582**	3.769**	4.846***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.02)	(0.01)	(0.00)
Ln (p)	0.241***	0.234***	0.226***	0.235***	0.245***	0.228***	0.226***
	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)
N	921	921	921	915	926	927	927
LL	-130.7	-132.3	-132.4	-132.0	-130.2	-132.7	-132.7
AIC	289.5	292.6	292.8	292.1	288.5	293.4	293.4

p values in parentheses: *p<.10, **p<.05, ***p<.01.

5. Conclusion

This paper examines empirically the effects of international sanctions on the duration of civil conflicts. Using civil wars and sanctions data for the period of 1960-2008, we estimate the hazard rate of war termination due to sanctions. Contrary to most earlier studies, we find that sanctions in aggregate reduce the expected duration of civil wars. However, not all types of sanction are equally successful in shortening conflicts. Total economic embargoes and arms sanctions are effective, but trade sanctions, aid suspension, and other sanctions are not. Both multilateral and unilateral sanctions shorten civil wars. Thus, our results suggest that in the current globalized system, sanction could be an effective tool for the international community to reduce the duration of civil war.

Like most studies, our study is not without limitations. Our data on sanctions include all imposed sanctions during the conflict. We do not have sufficient information about whether these sanctions were imposed because of civil war or for some other reasons (e.g., democracy, human rights issue, violation of international law). Our sanction variables are dummy variables, they measure whether intervention is present or absent in a given war or in a given year, they do not measure the extent of the intervention. For practical purpose the intensity of intervention might be an important determinant of war duration. Another limitation is the potential endogeneity of sanctions. To deal with the endogeneity, we use an instrumental variable technique suggested by Terza et al. (2008), and Atiyat (2011). Since there is no standard methodology to use instrumental variable technique in case of hazard model, our results can be taken as indicative, rather than conclusive.

References

Allison, P. D. (1984). *Event History Analysis: regression for longitudinal event data*. Newbury Park, California: Sage Publications.

Ang, A. U-Jin and Peksen, D. (2007). When do economic sanctions work? Asymmetric perceptions, issue salience, and outcomes. *Political Research Quarterly* 60(1): 135-145.

Atiyat, M. (2011). Instrumental Variable Modeling in a Survival Analysis Framework. A doctoral dissertation in Statistics submitted in The Graduate School at the The Pennsylvania State University.

Balch-Lindsay, D., Enterline, A. J., and Joyce, K. A. (2008). Third-party intervention and the civil war process. *Journal of Peace Research* 45(3): 345-363.

Bapat, N. A. and Morgan, C. (2007). Multilateral versus unilateral sanctions reconsidered: A test using new data. Manuscript, University of North Carolina.

Bennett, D. S. (1999). Parametric models, duration dependence, and time-varying data revisited: Comment on J. M. Box-Steffensmeier and B. S. Jones. *American Journal of Political Science* 43(1): 256-70.

Berman, E., Shapiro J. N., and Felter J. H. (2008). Can Hearts and Minds Be Bought? The Economics of Counterinsurgency in Iraq. National Bureau of Economic Research Working Paper Series, No. 14606.

Besley, T. and Persson, T. (2008). The Incidence of Civil War: Theory and Evidence. Working Paper, London School of Economics.

Blattman, C. and Miguel, E. (2010). Civil war. *Journal of Economic Literature* 48(1), 3-57.

Box-Steffensmeier, J. M. and Jones, B. S. (2004). *Event history modeling: A guide for social scientists*. Cambridge, UK: Cambridge University Press.

Collier, P. and Hoeffler, A. (1998). On Economic Causes of Civil War. *Oxford Economic Papers* 50(4): 563-573.

Collier, P. and Hoeffler, A. (2002). On the Incidence of Civil War in Africa. *Journal of Conflict Resolution* 46(1): 13-28.

Collier, P. and Hoeffler, A. (2004). Greed and grievance in civil war. *Oxford Economic Papers* 56(4): 563-595.

Collier, P., Hoeffler, A., and Söderbom, M. (2004). On the Duration of Civil War. *Journal of Peace Research* 41(3): 253-273.

Cortright, D. and Lopez, G. A. (2002). Smart Sanctions: Targeting Economic Statecraft. New York: Rowman & Littlefield.

Crost, B., Joseph F., and Patrick J. (2014). Aid under Fire: Development Projects and Civil Conflict. *American Economic Review* 104(6): 1833-56.

DeRouen Jr., Karl, R., and Sobek, D. (2004). The dynamics of civil war duration and outcome. *Journal of Peace Research* 41(3): 303-320.

de Ree, J. and Nillesen, E. (2009). Aiding Violence or Peace? The Impact of Foreign Aid on the Risk of Civil Conflict in sub-Saharan Africa. *Journal of Development Economics* 88(2): 301-13.

Drezner, D. (1999). The Sanctions Paradox: Economic Statecraft and International Relations. Cambridge: Cambridge University Press.

Elbadawi, I. A. and Sambanis, N. (2000). External interventions and the duration of civil war. World Bank Policy Research Working Paper, 2433.

Escribà-Folch, A. (2010). Economic sanctions and the duration of civil conflicts. *Journal of Peace Research* 47(2): 129-141.

Fearon, J. D. (2004). Why Do Some Civil Wars Last So Much Longer Than Others? *Journal of Peace Research* 41(3): 275-301.

Fearon, J. D. (2005). Primary commodities exports and civil war. *Journal of Conflict Resolution* 49(4): 483-507.

Fearon, J. D. and Laitin, D. D. (2003). Ethnicity, Insurgency, and Civil War. *American Political Science Review* 97(1): 75-90.

Hufbauer, G. C, Schott, J. J., and Elliott, K. A. (1990). Economic Sanctions Reconsidered: History and Current Policy. Washington, DC: Peterson Institute for International Economics.

Hufbauer, G. C, Schott, J. J., Elliott, K. A., and Oegg, B. (2007). Economic Sanctions Reconsidered. Washington, DC: Peterson Institute for International Economics.

Le Billon, P. and Eric, N. (2007). Ending 'Resource Wars': Revenue Sharing, Economic Sanction or Military Intervention? *International Peacekeeping* 10(5): 613-632.

Lektzian, D. and Souva, M. (2007). An institutional theory of sanctions onset and success. *Journal of Conflict Resolution* 51(6): 848-871.

Mason, D., Weingarten, J., and Fett, P. J. (1999). Win, lose, or draw: Predicting the outcome of civil wars. *Political Research Quarterly* 52(2): 239-268.

Masuhara, H. (2013). Semiparametric duration analysis with an endogenous binary variable: an application to hospital stays. CIS discussion paper series: 597, Tokyo: Center for Intergenerational Studies.

Nooruddin, I. (2002). Modeling selection bias in studies of sanctions efficacy. *International Interactions* 28(1): 59-75.

Pape, R. A. (1997). Why economic sanctions do not work. *International Security* 22(2): 90-136.

Terza, J. V., Basu, A., and Rathouz, P. J. (2008). Two-stage residual inclusion estimation: Addressing endogeneity in health econometric modeling. *Journal of Health Economics* 27: 531-543

Thyne, C. L. (2006). Cheap signals with costly consequences: The effect of interstate relations on civil war. *Journal of Conflict Resolution* 50(6): 937-961.

Regan, P. (2002). Third-party interventions and the duration of intrastate conflicts. *Journal of Conflict Resolution* 46(1): 55-73.

Strandow, D. (2006). Sanctions and civil war: Targeted measures for conflict resolution. Uppsala: Department of Peace and Conflict Research, Uppsala University.

Wallensteen, P., Eriksson, M., and Strandow, D. (2006). Sanctions for conflict prevention and peace building. Uppsala: Department of Peace and Conflict Research, Uppsala University.

World Bank (2011). Conflict, Security, and Development. World Development Report, 2011. The World Bank,Washington, DC.