Economic aid to post-conflict countries: a methodological critique of Collier and Hoeffler

Astri Suhrke , Espen Villanger & Susan L. Woodward

Published online: 20 Aug 2006.

Submit your article to this journal ☐

Analysis

Economic aid to post-conflict countries: a methodological critique of Collier and Hoeffler

Astri Suhrke, Espen Villanger and Susan L. Woodward

In the past five years, research sponsored by the World Bank on the economic aspects of civil war[1] under the research directorship of Oxford economist Paul Collier has had an extraordinary influence on the subsequent study of violent conflict and civil war and on international policy. The research project has now turned its attention to the problem of countries emerging from civil war and what Collier and his co-author, Anke Hoeffler, call 'a first systematic empirical analysis of aid and policy reform in the post-conflict growth process.'[2] Building on the influence of their earlier research and the lively interest currently in knowledge about and policy on post-conflict strategies, this work is likely to be equally influential on research, thinking, and policy. It is all the more important, therefore, to subject the research to critical examination before it becomes established as conventional wisdom. This note reports one such attempt to analyze some major methodological problems with the study and argues that the research cannot sustain the conclusions they draw or the resulting policy recommendations.

Introduction

The original research program equated civil war with rebellion and focused on individual-level motivations to rebel, arguing through econometric analyses of the various factors

Astri Suhrke and **Espen Villanger** are Senior Research Fellows at the Chr. Michelsen Institute, Bergen (Norway). **Susan Woodward** is professor of political science at the Graduate Center, City University of New York

correlated with civil-war onset that rationally calculated economic benefits were far better predictors than were grievances associated with the 'ancient ethnic hatreds' and 'clash of (religious) civilizations' schools. The next phase treated civil war as a collective action problem and identified the financial resources (specifically primary commodity export earnings) that enabled leaders to organize and sustain a rebellion. This step made it possible to propose an explanation for the outbreak of civil wars and policy recommendations for war termination that have informed programs such as UN-mandated targeted sanctions and the Kimberley process on conflict diamonds (Ballentine & Nitzschke, 2004). By this shift from a logic of individual motivations to one driven by opportunities for rebellion, Collier and his associates also entered more deeply into the policy world of conflict prevention by suggesting structural economic factors—economic decline, dependence on primary commodity exports, low and declining per capita income, a large pool of unemployed young men—that make countries more or less at risk of a civil war.[3]

Applying the same logic used for civil-war onset, what they call conflict 'proneness,' to post-conflict circumstances, Collier and his team argue, in a subsequent summary volume on the relation between development and conflict, that countries emerging from civil war are even more vulnerable to this 'conflict trap.'[4] Entitled *Breaking the Conflict Trap*, the report states at the outset that economic decline leads to civil war, which 'wrecks the economy and increases the risk of further war' (Collier *et al.*, 2003, p. 1). Economic growth is necessary to escape that trap. Turning to the literature on the effectiveness of external economic aid in stimulating growth, particularly the Dollar–Burnside studies at the World Bank,[5] Collier & Hoeffler then seek to identify the optimal timing of economic aid in post-conflict countries and sequencing of economic policy prescriptions to generate that growth.

Breaking the Conflict Trap, like the earlier research on civil-war onset, thus emphasizes the economic aspects of war termination and peace building. Its recommended policy package has four elements. External economic assistance based on the 2002 research on the relation between aid, policy and growth in post-conflict countries. Methods for governing financial resources (particularly revenues from natural resources and from diaspora, based on their research on civil-war onset) to prevent them from falling into the hands of potential new or revived rebel groups. International military intervention, not to create a credible commitment to peace, as other scholars have argued (Walter, 2001; Fortna, 2003), but to keep military expenditures low because high military expenditures in the first

period of peace are associated with a higher risk of conflict. Delays in the introduction of democratic political institutions until some economic growth have laid a stronger foundation for political stability.

Despite the newness of their focus on post-conflict questions, the Collier and Hoeffler conclusions already inform the aid policy of some major donors.[6] Although running counter to the current practice of most donors, their recommendations and the 'hard data' on which they are based seem to have wide appeal. In a policy realm increasingly characterized by pessimism and frustrating complexity, informed by interventions from Bosnia and Kosovo to Afghanistan and Iraq, the conclusions are simple and concrete, and apparently apolitical, credible, and universally applicable.

The Collier and Hoeffler study on optimal aid policies to post-conflict countries (henceforth, Collier & Hoeffler, 2004) raises two types of issues. Theoretically, as is already the case with their research on the causes of civil war, many will question whether the causal relations between aid and growth and between economic growth and peace have been sufficiently specified. In addition, whether large-N quantitative analysis is applicable to an issue with such great internal variation (types of civil wars and types of civil war endings) and, to practitioners, so sensitive to context, and whether an argument about the economic causes of civil war is in any way applicable to the causes of peace. Since even a casual reading of the study reveals questionable coding decisions, however, we decided to examine its methodological soundness first, before confronting these theoretical issues. This article reports our efforts to recode their sample, aiming to correct questionable decisions, make allowance for other factors relevant to post-war aid and recovery such as the relative impact of the war on the national economy and the level of destruction and displacement and create greater internal consistency in the units of analysis, and on that basis, to retest their study and its conclusions.

One issue we could not resolve in attempting to replicate their approach, unfortunately, was the effect of one of the key variables for their policy conclusions, namely the role of government policies and institutions that economists now widely consider critical to the aid-growth relationship. On this variable, their data belong to a confidential World Bank dataset (the CPIA), which is not available to outside scholars.[7] In terms of scientific rules and the principle of transparency, this is naturally of great concern, as has been emphasized in the scholarly community.[8] For the purpose of this study, Anke Hoeffler kindly agreed to run some basic regressions for us using the CPIA on our recoded sample,[9] but only sufficient to permit a partial retesting of their results.

The findings of Collier and Hoeffler (2002; 2004)

The Collier & Hoeffler forthcoming study examines the relationship between aid, economic growth, and economic policy variables in countries that have emerged from civil war. The main purpose is to identify the optimal magnitude and timing of aid in relation to economic growth. A related concern is to assess the role of host country policy on growth in the post-conflict period. Policy is divided into macro, structural, and anti-poverty components, and rated according to the composite CPIA index. The findings lead to three specific policy recommendations.

First, aid is 'considerably more effective' in generating growth in post-conflict countries than in countries that have not recently experienced civil wars (let us call them 'normal countries'). Since their working assumption is that growth reduces poverty (which they base on the Collier and Dollar, 2002 study), they conclude that '[f]or 'poverty efficiency,' aid volumes [in post-conflict situations] should be approximately double those in other situations' (2004, p. 1142).[10] Second, the timing of aid is critical to achieve aid-growth efficiency. To this end, donors should phase in aid gradually during the first four years after the conflict, and then 'gradually taper back to normal levels by the end of the first post-conflict decade' (loc. cit.). In reality, donors tend to do the opposite by providing large amounts of aid soon after the conflict when the situation commands world attention, and then reducing aid. Historically, aid 'has tapered out just when it should have been tapering in' (loc. cit.). Third, regarding policy priorities, Collier & Hoeffler find that improvement in anti-poverty measures ('social policies') in post-conflict situations has a positive impact on growth and, therefore, it should be given priority in the reform process.

These findings thus affirm that 'aid matters' and particularly in post-conflict situations. The first and third conclusions reinforce the comforting notion that there is no major trade-off between poverty and growth and that this is even more so in post-conflict societies than in 'normal' situations. The second conclusion regarding the actual timing of aid seems intuitively correct: we have all seen the massive media attention and donor focus on a society that is just emerging from war and the pledging conferences tied to a peace settlement (or their equivalent). The recommendation that aid should be phased in slowly seems more controversial. It conflicts, for instance, with the widely accepted notion that an immediate 'peace dividend,' rapid reconstruction, specific schemes such as financing reforms required in the peace agreement, and early, large-scale

employment of demobilized soldiers are important to solidify the peace (Boyce, 1996; Woodward, 2002).

Methodological issues in Collier & Hoeffler

All of these conclusions are questionable on methodological grounds. We will address sample size and the methodological issues in the aid-policy-growth regressions before turning to coding issues.

Sample size

The civil war sample used contains 14 countries and 17 conflicts (2004, Table 7). Of these, Collier & Hoeffler have data regarding the effects of aid in post-conflict situations for 13 conflicts in the first period after war, 13 cases in the following period, and only eight for the final period.[11] The dataset is not complete for every relevant post-conflict period, e.g. it is not the same set of post-conflict societies in the first period and in the second period after peace. As a result, their conclusions regarding the impact of aid and policy in post-conflict countries depend on between 8 and 13 observations. The small sample size in itself raises critical questions regarding the representativeness and validity of the conclusions.

While noting that the small sample makes for tentative findings,[12] the authors proceed to draw quite specific conclusions and make related policy recommendations. For instance, they recommend that 'the pattern of aid disbursements should probably gradually rise during the first four years, and gradually taper back to normal levels by the end of the first post-conflict decade' (2004, p. 1142)—that is, in the second full peace period for which they have only eight observations. This conclusion is incorporated in *Breaking the Conflict Trap*—'the high-impact phase is during the middle four or five years of the [post-conflict] decade (p. 158)—and is referenced to the Collier & Hoeffler study (2003 version). The conclusion that aid should be reduced in the second peace period because the growth benefits of aid taper off - and the location of its peak in the middle of the post-war decade—presumes that the shape of the curve into the second full peace period is known. Yet eight observations constitute an extremely thin basis of knowledge. Indeed, they acknowledge as much elsewhere in the paper, in a slightly different context. Referring to the Collier–Dollar (2002) study which they use for growth data, they note,

'There are only eight countries in the Collier–Dollar sample that have completed this peace period and such a sample is evidently too small for the approach' (2003 version, p. 9). As a result, it is 'unfortunately not feasible to investigate the effects during the second full peace episode' (p. 9).

Methodological weaknesses in aid-policy-growth regressions

The policy recommendations in Collier & Hoeffler (2004) stem from applying methods used in much of the econometric work that uses interaction analysis to show how policy and aid affect growth. These methods have some generally recognized weaknesses, and there are some standard ways of testing for their impact. As we could not perform these tests without access to the CPIA, a second-best option was to review the main methodological concerns expressed in connection with the kind of interaction terms that Collier & Hoeffler use and consider to what extent they also apply here.

Collier & Hoeffler use an almost identical interaction term to that in the Burnside and Dollar studies (1997; 2000). The Burnside and Dollar studies, which show that aid increases growth only if the macroeconomic policies are good, have generated heated debate.[13] Criticism on methodological grounds, as well as the policy implications of channelling aid only to countries that adopt 'good' policies, has been made by Hansen & Tarp (2000; 2001), McPherson (2000), Dalgaard & Hansen (2001), Dayton-Johnson & Hoddinott (2001), Guillaumont & Chauvet (2001), Lensink & White (2001), Lu & Ram (2001), Akhand & Gupta (2002), and Easterly, Levine & Roodman (2003). By contrast, the study by Collier & Dollar (2002) supports Burnside & Dollar (1997; 2000).

The main criticism against Burnside & Dollar (2000) is that the interaction term between aid and a measure of institutions and policies is very fragile and vulnerable to minor changes in the methodology. Such changes could be alterations in the specification of the econometric model, application of a different estimation method, or use of a different criterion for deleting outliers. Much of the criticism of the Burnside & Dollar study concerns misspecification. Insertion of new, relevant variables into their specifications caused the interaction term between aid and policy to lose significance, a point also recognized by Burnside & Dollar (2004). Due to the similarities in methodology, this is a source of concern for the Collier & Hoeffler study as well.

As the empirical growth literature emphasizes, different theoretical growth models may yield different predictions on which variables to include. Even identical theoretical models can serve as background for econometric models with different sets of explanatory variables. The standard solution to sorting out appropriate variables is robustness testing, but Collier & Hoeffler have not gone very far in this direction. Given the attention that this issue has drawn in the growth literature,[14] and the small and internally heterogeneous sample used in Collier & Hoeffler (2004), comprehensive robustness testing is appropriate in order to assess whether incremental change in variables affects the results. However, a robustness test without one of the most important variables, the CPIA, is not very informative, and therefore, we could not test the fundamentals of the model used by Collier & Hoeffler. Nevertheless, several methodological weaknesses can readily be identified and call into question the validity of the conclusions that Collier & Hoeffler draw.

Collier & Hoeffler undertake only limited testing to determine which combination of variables should be included in the final specification of the aid-policy-growth relationship. They include 'aid squared' but exclude 'aid' after initial regressions show the term to be insignificant. They make no attempt to estimate policy squared. This is a dubious methodological approach. Since the purpose is to investigate the effect of aid and policy on growth, the regression should include aid and aid squared, policy and policy squared, and the aid-policy interaction, and these, in turn, should be interacted with post-conflict dummies.[15] This more standard approach would yield eleven initial variables, rather than the seven that Collier & Hoeffler use. Omitting 'aid' in the regressions is particularly questionable. If aid has a positive effect on growth—as most other studies have found[16]—and 'aid' is used in virtually all other aid-growth studies, omitting this variable and only using 'aid squared' is likely to impose an odd form on the relationship and may well influence the core results of Collier & Hoeffler.

The Collier & Hoeffler method in arriving at their preferred specification also does not follow conventional econometric procedures. In their initial testing, they find that none of the four post-conflict variables (post-conflict 1, post-conflict 1 interacted with policy, aid squared, and aid-policy) are individually significant. Instead of testing for joint significance, which is standard procedure, Collier & Hoeffler exclude each of the insignificant post-conflict variables one at the time. The result is that the interaction variable between post-conflict and aid-policy becomes significant, which is the basis of the first two conclusions noted previously. This stepwise exclusion of variables is clearly

ad-hoc and, by omitting variables that could jointly be significant, it could cause substantial bias in the results.

A similar approach is adopted with respect to assessing the findings. Collier & Hoeffler test whether their results depend on certain influential data points by removing one of the 13 observations, one at a time. A more appropriate method would be to see whether deleting two or three observations simultaneously from the regressions would have any effect. Given that the Burnside & Dollar interaction effects were not significant until they excluded five outliers, it is important to see whether deleting a similar number of outliers would affect the Collier & Hoeffler interaction variables. Moreover, as Dalgaard & Hansen (2001), among others, point out, there may well be influential observations in the other dimensions of the growth relationship that affect the outcome and that need to be checked. In the Collier & Hoeffler framework, outliers and influential data points of interest that should be subjected to criteria for deleting outliers in the study are in the CPIA index, initial GDP, aid, governance, and interaction terms.

When carefully examined, the Collier & Hoeffler study also raises questions regarding internal consistency in the model with which they operate, at least implicitly. One of their findings is that aid to a typical post-conflict country is associated with an increase in economic growth that is almost 150% higher than the association between aid and growth in a typical 'normal' country (provided the timing is right). This remarkable impact of aid (although Collier & Hoeffler do not elaborate) appears to reflect a condition of post-conflict countries that makes them differ in fundamental ways from countries that have not recently emerged from war. If post-conflict conditions generate particularistic relations between aid and growth, however, the same pattern might be expected with respect to growth and poverty. On the growth-and-poverty relationship, however, Collier & Hoeffler assume that post-conflict countries conform to the general pattern that Collier & Dollar (2002) found for all countries. Although Collier & Hoeffler do not directly test the relationship between growth and poverty on their post-conflict sample, they nevertheless conclude with recommendations regarding the optimal level of aid to produce 'poverty-efficient' growth in post-conflict countries (p. 14). Apart from the inconsistency in explanatory logic, the conclusion is particularly open to question insofar as the standard literature on the poverty-efficiency of growth shows wide variations and different patterns (Kanbur, 2000).

Overall, there is a tendency in the Collier & Hoeffler paper to highlight conclusions that are likely to be welcomed in the aid community, while findings that seem to be

inconsistent with the literature or conventional wisdom are glossed over. In the latter category is their striking statistical result displayed in Collier & Hoeffler (2004, Table 6) that the coefficient of their interaction variable between macroeconomic policies and post conflict dummy is very large and negative, while the coefficient of social policies is large and positive. Their interpretation is that good macroeconomic policy is less important compared to good social policy in post-conflict situations relative to other situations, which is a valid reasoning. Alternatively, the high negative coefficient value can also imply that the overall impact of improving the macroeconomic environment in post-conflict societies could be to *reduce* economic growth. Such a result is counter-intuitive and contrary to the findings of the established literature, and needs to be investigated further. To do so, however, requires full access to the CPIA index.

Recoding

Fundamentally, of course, the results depend on the dataset, which makes coding a critical issue. Coding problems are well recognized in the civil war literature. As Sambanis has demonstrated, the application of different criteria to define a civil war and determine the time of onset produces quite different results with regard to identifying the conditions that are associated with the occurrence of such conflicts (Sambanis, 2002). Similarly, one would expect that the point in time taken as the end date of the war would affect findings regarding economic growth and the impact of aid and policy in the post-war period. Insofar as wars are destructive in ways that impinge on the task of post-war reconstruction (destruction of physical and social infrastructure, displacement of people, etc), one would also expect that the impact of small conflict episodes would differ from that of major civil wars. If the civil war ends but is followed by another kind of war (e.g. as civil war in revolutionary Iran was followed by the Iran-Iraq war), and the analysis seeks to assess the dynamic of post-war reconstruction, this second war would have to be taken into account in determining when the 'post-conflict' period starts. In short, there is a strong argument for tailoring the dataset to the problem to be investigated, as several civil war scholars indeed have recognized (Licklider, 1995, Hartzell, Hoddie & Rothchild, 2001). By contrast, Collier & Hoeffler use a conventional dataset on civil wars (Correlates of War, COW 1984, 1994) that is widely used for examining the *causes* of conflict. They make no adjustment for variables that arguably would affect the conditions for subsequent economic

reconstruction, above all the territorial scope of the war, its intensity, and the intrusion of different kinds of wars that were not included in their dataset.

The Collier & Hoeffler sample

The threshold of battle-related deaths provides the core of commonality in the Collier & Hoeffler sample. The COW dataset uses a definition of 'civil war' that entails 'an internal conflict between a government and an identifiable rebel organization that results in at least 1,000 combat-related deaths, of which at least 5% must be incurred on each side' (2004, p. 1127).[17]

In other respects, their sample is quite heterogeneous, notably with respect to duration, territorial scope of the fighting, the overall cost of the war, and the entities formally involved. For instance:

Duration: three conflicts are coded as lasting one month or less (Romania December 1989, Jordan September 1970, Burundi August 1988), while another three conflicts lasted 15 years or longer (Mozambique, Peru and Ethiopia).

Territorial scope: in some cases, the fighting was territorially concentrated with little impact outside the affected areas (e.g. the Muslim insurgency in the southernmost part of the Philippines; the conflict in Punjab in India); in other cases, the war engulfed the entire country (El Salvador, Nicaragua).

The formal entities involved: while COW initially made a distinction between civil wars and what they call 'extra-systemic wars'—including colonial wars and wars of independence—Collier & Hoeffler do not. They include two wars of independence (Southern Rhodesia/Zimbabwe and Guinea Bissau) and two wars of colonial expansion (Morocco in West Sahara, and Indonesia in East Timor). As Sambanis notes (2002), coding colonial wars entails particular problems regarding the location of the conflict and appropriate data. Thus, Indonesia's war of conquest in East Timor was fought only in East Timor—a tiny, peripheral area in relation to the metropolitan territory—and the effects of the war were hardly felt outside the occupied area. Nevertheless, since separate data for East Timor were not available, Collier & Hoeffler use data for Indonesia as a whole, which then becomes the post-conflict entity.

The overall cost and relative cost: Collier & Hoeffler do not use any estimate of the overall human and economic costs of the war, but the marked differences in duration of conflicts

that all have the same minimum *annual* battle-related deaths indicate substantial differences in overall costs.[18] Such differences are of particular interest in relation to aid, growth, and policy reform in the post-conflict period. Similarly, the cost of the conflict relative to the overall size of the economy is presumably a significant variable in relation to an analysis of post-conflict recovery.

Variations of this kind matter little in a large sample, but in a small sample, they can have a substantial impact. Although the results may be statistically significant, it is unclear for what kinds of conflicts they are most relevant or whether there is a policy-relevant pattern of variation within the sample.

In sum, while COW might be useful for studying the *causes* of conflict, as Collier and Hoeffler elsewhere argue,[19] it is not self-evident that the dataset is equally useful for analyzing the *consequences* in terms of the impact of aid and policies on economic growth after civil wars. COW does not even distinguish between minor conflicts and major wars, a matter of evident significance for post-war economic reconstruction and growth. The Uppsala/PRIO dataset would be more useful for this purpose as it distinguishes between three types of conflicts—war, intermediate armed conflict, and minor armed conflict – based on a combination of quantitative and qualitative assessments, including the number of battle-related deaths.[20]

Recoding the data

There is currently no one civil war dataset that includes the kind of indicators related to intensity, destruction, displacement, and relative cost that would be particularly relevant to assess the impact of aid in the post-war period. Data to construct such indicators are available, but scattered.[21] To capture at least some of these dimensions—let us call it the war-impact factor—we recoded the Collier & Hoeffler sample in some simple ways. As described in more detail below, we introduced

- A quantitative measure of the duration of the conflict, and
- A qualitative assessment based on careful examination of the case literature, of material destruction, geographic scope, and population displacement related to the conflict.
- To qualify as a civil war, the violent events included in the Collier & Hoeffler sample had to last more than two months and cause enough material destruction and population

displacement over a sufficiently large area to make post-war reconstruction a recognized national policy task. The effect of this recoding was to reduce the heterogeneity of the sample in favour of larger civil wars. In addition, we adjusted the period for war termination in cases where the country literature and comparable datasets indicated equally or more reasonable alternative end dates, and cases of mistaken application of the original dataset.

Based on this recoding we constructed a new, core sample. Moreover, to assess the impact of marginal variations, we added individual borderline cases to produce three additional recoded samples. Altogether, the 4 different variations of the original Collier & Hoeffler sample permitted us to examine the empirical consequences of a range of coding changes. As the recoding exercise has broader methodological implications, it is described in more detail below.

Reducing heterogeneity

To reduce heterogeneity requires identifying a core in the recoded, more homogeneous set of units of analysis. Collier & Hoeffler examine the aftermath of 'civil wars,' hence we settled for 'civil wars' in a common sense understanding of the word. Without adopting an essentialist perspective, we note that 'civil war' is typically associated with major destruction and dislocation. As Sambanis notes, '[o]ne of the main distinctions between civil war and other forms of violence is that civil war is usually associated with large-scale destruction' (2002, p. 13). This is reflected in the terminology and the death-count criterion of the most widely used differentiated dataset (Uppsala/PRIO), where 'war' has a higher threshold of violence than 'intermediate conflict' and 'minor conflict.' The aftermath of civil wars—as distinct from lesser conflicts—would probably reflect this variation as well. To what extent the degree of destruction or dislocation in fact impacts on the growth-aid-policy interaction is an empirical question; the point here is that it can best be discovered by an initial sorting of cases that caused severe dislocations from those that did not.

To allow for the differential impact of wars on the post-war period and simultaneously reduce the heterogeneity of the sample, we chose to include only conflicts that entail substantial dislocations and destruction. Datasets with numbers of combat-related deaths are inadequate for this purpose. Death figures may be high, but the dislocations of wars that typically impinge on post-war economic growth—destruction of physical

infrastructure, forcible displacement of people, and replacement of 'normal' economies by wartime economies—may still be minimal. Take, for example, the 1971 uprising in Sri Lanka, which is included in the Collier & Hoeffler sample. Collier & Hoeffler, COW and other comparable datasets record the event as 'war' (see Appendix II). In the case literature, by contrast, the 1971 violence is uniformly referred to as an uprising or an insurrection, not a war (de Silva, 1981; Jupp, 1978; Phadnis, 1976). The violence easily meets the threshold of combat-related deaths in both the COW and Uppsala/PRIO dataset for war (an estimated 10,000 were killed, mostly rebels). Yet there was minimal material damage and little population dislocation (except among the rebels). The rebels (JVP) were concentrated in the southeastern coastal area. They were poorly trained and organized, and the government responded with decisive force. Most of the fighting was over in a matter of three days. Massive arrests and mopping-up operations followed (including execution of many detainees). As a result, issues of post-war reconstruction were only indirectly related to the bloody events insofar as the uprising itself was seen to discredit previous policies and encouraged a reassessment of macroeconomic policy.

Applying the quantitative rule of minimal duration of two months, and a qualitative rule of the scope of destruction and reconstruction challenge, we excluded four conflicts in the Collier & Hoeffler sample from our core sample. These were Sri Lanka (1971), Romania (1989), Burundi (1988) and Indonesia (1982).

The 1971 uprising in Sri Lanka was excluded from our core sample for reasons discussed above. As for Romania (1989), Collier & Hoeffler/COW are almost alone among comparable datasets to consider the 1989 events as 'civil war.' (See Appendix II.) The violence in question consisted of one to two weeks of street demonstrations and clashes between the demonstrators and the army, until the latter turned and triggered the downfall of the Ceausescu regime. There was minimal material destruction and displacement. Those who executed Ceausescu later argued with some reason that a civil war was in fact thereby prevented. Burundi (1988) is a more difficult borderline case but was in the end excluded. The case literature treats the 1988 ethnic massacres as one of several violent phases in the continuing conflict between the Hutus and the Tutsis. There was no clearly defined post-'war' reconstruction, and refugees did not return. In fact, another and much larger massacre occurred in October 1993, when thousands were killed or displaced. The fluid nature of Burundi's violence makes it difficult to identify discrete 'wars'—and the 1988 events appear differently in comparable datasets (Appendix II). Clearly, if 1988 is

treated as a 'war' year, so should 1993, which Collier & Hoeffler instead code as a 'post-conflict' year. Nigeria (1980–84) is a similar case involving an ebb and flow in violence between the federal government and a communal-religious movement, and various datasets record the conflict quite differently (see Appendix II). We decided to keep Nigeria in our core sample but excluded it from our samples 3 and 4 to assess the effect of inclusion.

Collier & Hoeffler include two cases of what COW calls 'extra-systemic, colonial war'—Indonesia (East Timor) and Morocco (Western Sahara). While COW excludes such wars in recognition of their distinct character, Collier & Hoeffler decided nevertheless to include them, probably to increase the size of the sample. The consequent problem of distortions arising from the use of national-level data, as noted above, is particularly severe in the case of Indonesia/East Timor. As a prototypical case of such distortions, it bears further elaboration:

The 1975 invasion by Indonesian forces and the early military campaign had devastating consequences for East Timor in terms of widespread deaths, destruction of infrastructure, torching of villages, and forcible relocation of nearly half the population. During the 1980s ('post-conflict' in Collier & Hoeffler terms), Indonesian authorities undertook reconstruction and development in the occupied territory. By some estimates, the GNP per capita of East Timor increased nearly five times. However, Collier & Hoeffler use national data for Indonesia on economic growth, aid, and policy ratings, thereby losing much of the effect of change within East Timor because of the huge difference in size between the metropolitan country and the occupied territory. At the time of the invasion, Indonesia's population was 134 million, that of East Timor, 650,000. After East Timor was officially declared one of Indonesia's 27 provinces in 1976, the occupation represented a net drain on the central state budget due to various subsidies, but in terms of the overall Indonesian economy, it was minimal. Only a small proportion of Indonesia's armed forces was deployed in East Timor (Schwarz, 1994). Less than 10% of the army participated in the invasion and the first military campaign (around 30,000 soldiers). Since a large part of the Indonesian military budget at the time was realized by 'unconventional financing'—mainly through enterprises and foundations run by the armed forces, including the army units in East Timor—the war and its aftermath were hardly felt in terms of official military expenditures (Crouch, 1978; 2000) or foreign aid. East Timor, as an Indonesian foreign minister said, was only 'a pebble in the shoe.' As for foreign aid in the 'post-conflict period,' there was certainly aid going into Indonesia in

the 1980s, but not directly to East Timor. The occupation was regarded as illegal by the UN, and until 1989, the Indonesian government generally prohibited outside access to the territory. Hence, any relationship between aid-growth-and-policy as shown in the national data for Indonesia in the 1980s would have only a spurious relationship to the war and post-war reconstruction in East Timor. Therefore, we excluded Indonesia from our core sample.

By contrast, while Morocco/West Sahara is generically a similar case of extra-systemic, colonial war, using national data on aid, growth, and policy is more defensible because of the huge impact of the war on the metropolitan polity and economy. The war lasted several years (Collier & Hoeffler use the October 1975–November 1989 period), virtually all of Morocco's army was tied down in Western Sahara, and the economic cost of the war at one stage led to the withdrawal of food subsidies in Morocco proper and serious riots. Hence, locating the war conceptually in Morocco and using aggregate data for the metropolitan country to represent the aftermath does make some sense. Therefore, we retained Morocco in our core sample.

The recoding exercise so far shows the importance of tempering the search for a large-N sample by recognizing distinct sub-groups within. Consider the cases of Sri Lanka and Romania. In both Romania and Sri Lanka, economic policy, aid, and growth patterns did change in the aftermath of the conflict. The rationale and dynamic of the changes were not related to the damage caused by the war, however, but by quite different factors. In Romania, the violent demonstrations triggered the end of state socialism and the start of a transition to a market economy. The consequent liberalization of the economy eventually did generate aid and growth (World Bank, 2003). In Sri Lanka, the 1971 uprising had partly been a response to the stagnating economy. The government subsequently liberalized the economy, which in turn attracted aid and investment and contributed to a sharp increase in growth rates towards the end of the decade (University of Bergen, 1987). The policy implication in both cases is that good macroeconomic policy is highly significant in generating growth in 'post-conflict' countries, which differs from the Collier & Hoeffler conclusion to the effect that macroeconomic policy is less significant than 'social inclusion'. Had the Collier & Hoeffler recommendations on policy priorities for post-conflict situations been followed, the result probably might well have been the opposite of what Collier & Hoeffler would have predicted.

Adjusting end dates

End dates of war are as slippery as starting dates, as Sambanis (2002) notes. A sharp drop in battle-related deaths indicates an equivalent decline in fighting, but not necessarily the end of the war or a transition to reconstruction and peace. For purposes of identifying 'post-conflict' periods, equally significant on more relevant markers may be institutional events that signal that the war-to-peace transition is irreversible, thereby allaying fear of renewed violence and encouraging collective investment in the future. Such markers may be a formal peace agreement, a UN-supervised cease-fire, or mass arrest and trials of rebels.

Using battle-related thresholds entails other difficulties as well. Assessments may reasonably differ, given the difficulty of obtaining precise casualty figures in many civil war situations. Determining the exact *month* when the casualty rate drops below a given threshold—as the Collier & Hoeffler methodology requires—is even more exacting. As a result, some of the end dates used by Collier & Hoeffler have a 'soft' quality, a feature also found in other, comparable datasets and that is reflected in the variation among them. After carefully comparing the end dates used by Collier & Hoeffler with those of three comparable datasets and in-depth examination of the case literature, we found five cases where a different end date was equally or more plausible (see Appendix II). In most cases, we considered battle-related deaths in light of institutional characteristics of the kind suggested above. The changes were as follows (Table 1):

Correcting application mistakes

Two cases in the Collier & Hoeffler set must be described as plain mistakes of application, stemming from a poor match between the database and the research design. In the two cases, the end date for the civil war is reasonable, but Collier & Hoeffler make no allowance for the fact that an international war simultaneously commenced. As a result, time periods that Collier & Hoeffler code as 'peace onset' and 'post-conflict' were manifestly not so. This is most obvious with respect to Iran, where Collier & Hoeffler code 'the war' as ending in 1982, and the remaining 1980s as years of 'peace onset' and 'post conflict.' The 1980s, of course, was the period of the Iran–Iraq war (1982–88), which was enormously destructive and traumatizing for Iran. The consequences in terms of deaths, economic destruction, population dislocation, and social trauma were similar to those of a major civil war.[22] A similar but less egregious coding application is evident in the case of Somalia, where

Table 1.

Conflict	C&H date	Recoded	Reason
Morocco	11/1989	9/1991	UN-monitored ceasefire; cease-fire held
Guatemala	Coded as two wars: 7/1966−7/1972 and 3/1978−3/1984	One war: 7/1966−6/1993	Case literature uniformly treats the conflict as one war, violence declines in 1993, followed by break-through in negotiations
Jordan	9/1970	7/1971	Major fighting continued, decisive army assault on the *fedayeen* in 7/1971
Peru	12/1996	3/1993	Decline in violence, mass arrest of rebels, including the leadership
Philippines	12/1996	9/1996	Peace agreement with rebels concluded

Collier & Hoeffler use December 1992 as the end of the (civil) war. This ignores the fact that major fighting occurred in mid-1993, although now the conflict was not merely 'civil' but ranged US forces against the Somalis. In a narrow technical sense, Collier & Hoeffler are correct, yet in terms of their own research design, the coding makes no sense. These cases again emphasize the pitfalls of using a dataset primarily designed for studying the causes of war as a basis for assessing developments in the post-war period.

Results from the recoded datasets

After recoding, our core sample had five conflicts fewer than that used by Collier & Hoeffler, and the end date of war had changed in another five cases. In the other three samples, we made only minor additional changes to assess the effects of smaller variations. In Sample 2, alternative dates were used for two conflicts. In Sample 3, one alternative date was used and one conflict excluded. In Sample 4, two alternative dates were used and one conflict excluded.

The recoded samples are presented in Table 2. Countries in italics constitute the 13 observations that are the basis for the first two of the three Collier & Hoeffler conclusions, that is, where they have data regarding the impact of aid on growth after

the end of the war. The other countries represent additional observations regarding the actual donor pattern of providing aid in the post-war period. Countries for which Collier & Hoeffler have no data at all are marked with an asterisk, and it is unclear

Table 2. Collier & Hoeffler sample and recoded samples

Country	Collier & Hoeffler (end of war)	Sample 1 (Core)	Sample 2	Sample 3	Sample 4
Angola*	05/91	No data	No data	No data	No data
Burundi	12/73				
Burundi	08/88	No war	No war	No war	No war
Chad	08/88				
Congo*	10/97	No data	No data	No data	No data
El Salvador	01/92				
Ethiopia	05/91				
Guatemala	07/72	One war, ended 06/93	One war, ended 06/93	One war, ended 06/93	One war, ended 06/93
Guatemala	03/84				
Guinea-Bissau	12/74				
India	06/94				
Indonesia	09/82	No war	No war	No war	No war
Iran	05/82	08/88	08/88	08/88	08/88
Jordan	09/70		07/71	07/71	07/71
Morocco	11/89		09/91		09/91
Mozambique	10/92				
Nicaragua	07/79				
Nicaragua	04/90				
Nigeria	01/70				
Nigeria	08/84			No war	No war
Pakistan	07/77				
Peru	12/96	06/93	06/93	06/93	06/93
Philippines	12/96	09/96	09/96	09/96	09/96
Romania	12/89	No war	No war	No war	No war
Russia*	08/96	No data	No data	No data	No data
Rwanda*	07/94	No data	No data	No data	No data
Somalia	*12/92*	*12/93*	*12/93*	*12/93*	*12/93*
Sri Lanka	05/71	No war	No war	No war	No war
Sudan	02/72				
Uganda	04/88				
Zimbabwe	12/79				

The Collier & Hoeffler sample used here refers to their 2003 paper version, which was only available to us at the time. The 2003 version has a much larger list of countries than the 2004 version, but the same number of observations. The country list is simply pruned for the 2004 version to reflect the actual observations.

why these countries appear in the sample list. A blank space in the columns of our recoded samples 1–4 means we have accepted the Collier & Hoeffler coding. Where we have not accepted their coding, this is indicated accordingly either by 'no war' (taken out of the sample) or by our alternative date.

Setting aside for the moment the methodological constraints on the Collier & Hoeffler study discussed above, we tested the results by applying the exact same methodology on our recoded samples. We were only able to test empirically two of Collier & Hoeffler's three main conclusions: that aid is considerably more effective in augmenting growth in post-conflict situations than in other situations, and that aid disbursement should increase during the first four years after the conflict and then taper off towards the end of the decade after peace. Their third conclusion regarding policy priorities—i.e. that anti-poverty measures ('social policies') have a positive impact on growth in post-conflict situations, and therefore, should be given priority in the reform process—is based on analysis of individual components of the CPIA. As we did not have access to this dataset, we could not test this conclusion.

Applying exactly the same methodology as Collier & Hoeffler on a substantially recoded set of observations, and with a smaller number of conflicts, produced some similar and some different results. On their finding that aid has an extra effect on growth in post-conflict countries during the four to seven years after peace-onset, we find that the corresponding coefficients remain statistically significant in all four datasets (See Appendix I). In other words, our testing supports the conclusion that aid is more productive with respect to growth during the four-to-seven-year period after peace compared to other situations.[23] The aid-policy-post-conflict interaction variable thus seems quite robust to substantial changes in the post-conflict sample. Our results from the four recoded datasets also confirm that there are no extra effects of aid on growth during the first four years after peace-onset. None of the interaction terms from the regressions with the recoded data is significantly different from the findings of Collier & Hoeffler.

However, it is well known that statistical significance in itself does not imply that one has found an interesting economic relationship, even when there is a strong probability that the two variables are causally linked. If the magnitude of the coefficient does not correspond to economically meaningful measures, then a significant relationship may have little relevance for policy. We thus compare the coefficients to discuss Collier & Hoeffler's claim that aid is 'considerably more' effective in generating growth in post-conflict societies as compared to other societies.

Using our Sample 2, plus a slightly different and preferred specification of variables, we found results that differed markedly from those of Collier & Hoeffler.[24] The coefficient indicating the effect of aid was less than half of the coefficient preferred by Collier & Hoeffler. The implications for policy are important: using the original Collier & Hoeffler sample and their preferred specification suggests a much greater growth effect of aid in the 'peak period' (a 120% 'overstatement') than if the recommendations had been based on our sample 2 and a slight change in specification. These minor changes were sufficient to produce a great difference in the magnitude of the growth effect of aid. The Collier & Hoeffler conclusions in this regard are thus quite fragile (see Table 3).

It is evident that a study based on Sample 2 would give very different indications of the importance of phasing in aid during the four-to-seven-year period after the war. The results reveal that Collier & Hoeffler's preferred coefficient of aid-policy-post-conflict1 (Table 2, Appendix I column CH4) is 120% larger than the preferred coefficient in our Sample 2.[25] The implications for policy are obvious: using the original Collier & Hoeffler sample would suggest a much greater growth effect of aid in the 'peak period' than if the recommendations had been based on our Sample 2. Our Sample 2, it will be recalled, consisted of the core sample plus minor changes in the end dates of two conflicts. This small change was sufficient to produce a great difference in the magnitude of the growth effect of aid.

Let us further quantify the economic implications of these differences. If we omit all of the post-conflict interaction dummies discussed earlier, the general growth regression for all countries takes the form

$$g = a + bA + cP + dAP - eA^2 + fP^2 + Z \qquad (1)$$

where

g = the growth rate

A = aid as a share of GDP

P = CPIA measure of policy and institutions

Z = a vector of control variables including their coefficients.

We are interested in whether aid is more productive in terms of growth in post-conflict societies compared to other societies. Thus, assume that g^{extra} is the extra impact from aid on growth that is found in post-conflict societies. In terms of the regression results, this effect is solely captured by the aid-policy-post-conflict interaction term. This implies that we separate the post-conflict component of the impact of aid on growth from the general

Table 3. Interaction effects Sample 2

	CH1	S2, CH1	CH2	S2, CH2	CH3	S2, CH3
Initial per	0.718	0.680	0.715	0.676	0.717	0.677
capita income	(0.627)	(0.640)	(0.621)	(0.637)	(0.618)	(0.636)
Governance	0.196	0.240	0.197	0.241	0.198	0.235
(ICRGE)	(0.160)	(0.157)	(0.157)	(0.155)	(0.157)	(0.154)
CPIA	0.991**	0.835**	0.991**	0.837**	0.988**	0.857**
	(0.397)	(0.413)	(0.396)	(0.413)	(0.390)	(0.408)
ODA $\times$ CPIA	0.134**	0.190*	0.134**	0.189*	0.134**	0.185*
	(0.066)	(0.112)	(0.066)	(0.112)	(0.065)	(0.111)
ODA		-0.184		-0.178		-0.168
		(0.367)		0.366		(0.365)
$(ODA/GDP)^2$	-0.028**	-0.026*	-0.028**	0.026*	-0.028**	0.026*
	0.012	(0.014)	(0.012)	(0.014)	(0.012)	(0.014)
South Asia	2.614***	2.668***	2.611***	2.666***	2.619***	2.623***
	(0.644)	(0.643)	(0.639)	(0.641)	(0.625)	(0.630)
East Asia	2.891***	3.023***	2.889***	3.022***	2.884***	3.002***
	(0.663)	(0.670)	(0.660)	(0.669)	(0.660)	(0.664)
Sub-Saharan	-0.440	-0.535	-0.442	-0.537	-0.442	-0.541
Africa	(0.821)	(0.817)	(0.817)	(0.815)	(0.816)	(0.813)
Middle East/	1.590***	1.571***	1.591***	1.569***	1.589***	1.571***
North Africa	(0.568)	0.560	(0.567)	(0.559)	(0.567)	(0.559)
Europe/Central	-0.400	-0.424	-0.402	-0.423	-0.403	-0.429
Asia	(1.059)	1.061	(1.056)	(1.060)	(1.054)	(1.058)
Post-conflict 1	1.385	-2.705	1.445	-2.615	0.913	2.015***
	(3.237)	(2.860)	(3.073)	(2.791)	(0.755)	(0.755)
(Post-conflict1)	-0.186	1.519	-0.180	1.550*		
$\times$ (CPIA)	(1.011)	(0.972)	(1.019)	(0.925)		
(Post-conflict1)	-0.009	-0.020				
$\times (ODA/GDP)^2$	(0.102)	0.094				
(Post-conflict1)	0.168	0.145	0.141***	0.084**	0.139***	0.105***
$\times$ (ODA/GDP)	(0.330)	0.303	(0.042)	(0.039)	(0.041)	(0.039)
$\times$ (CPIA)						
Observations	344	344	344	344	344	344
Post-conflict	13	11	13	11	13	11
observations						
R^2	0.38	0.38	0.38	0.38	0.38	0.38

impact of aid across all countries, and so

$$g^{extra} = \beta AP\theta \tag{2}$$

where β is the coefficient

θ is the post-conflict 1 dummy

Increasing aid yields the following impact on growth for a post-conflict society

$$\frac{dg}{dA} = \beta P \qquad (3)$$

Assume that policy makers are interested in how large the extra effect of aid on growth is in these societies, compared to the increase in growth that would occur in an average country in a normal situation. Therefore, we calculate the increase in growth from increasing the amount of aid to these countries in the period four to seven years after peace when this extra effect is found. The average CPIA for the countries in their first full period of post-conflict peace is given by Collier & Hoeffler to be 2.88, while the average aid for these 13 observations of post-conflict societies is 2.14.

Using the Collier & Hoeffler sample, increasing aid by 20% (50%) in the four-to-seven-year period after peace-onset increases the extra effect of aid on growth by 0.23 (0.57) percentage points. This is 120 percentage points above what the extra aid effect would be if our Sample 2 were used as a basis for calculation. Our Sample 2 coefficients indicate that increasing aid to the typical post-conflict country by 20% (50%) increases the economic growth by only 0.1 (0.26) percentage points above the normal. The comparison illustrates clearly the dangers of making policy recommendations based on results from a single sample.

The second conclusion in Collier & Hoeffler, namely that 'aid disbursement should probably gradually rise during the first four years.' (p. 1142) is not clearly grounded in their empirical results. They find, as we do in all the recoded datasets, that aid is just as effective in post-conflict societies during the first four years of peace as in normal societies. If growth-efficiency of aid were the overriding concern, the logical implication of this would be that no extra aid should be given in the immediate post-war period, rather than that aid should 'gradually rise.'

The Collier & Hoeffler conclusion that the extra effect of aid on growth in post-conflict societies is not sustained throughout the post-conflict decade, and hence should 'gradually taper back to normal levels by the end of the first post-conflict decade' (p. 1142), is problematic because of the small sample. While our findings confirm an extra effect of aid during the four to seven years after peace onset, we cannot say anything even reasonably conclusive about the effect during the subsequent period because of the limited number of observations (only eight). Hence there is insufficient empirical basis

for concluding, as Collier & Hoeffler do, that aid should 'gradually taper back to normal' in the subsequent years.

Conclusions

The results of our retesting are in part subject to the same methodological shortcomings we have identified in the Collier & Hoeffler paper, and as such, must be treated with caution. Nevertheless, we were able to demonstrate that there can be quite large variation in the possible extra effect of aid on growth during the post-conflict period depending upon the identification of the sample and slight changes in the specification of the variables. By recoding according to stricter measures of what constitutes civil war, and using an empirically derived, improved specification, we found that aid had up to less than half the effect on the growth spurt in the four-to-seven-year period after peace than in the Collier & Hoeffler results based on a heterogeneous sample of conflicts. By including a number of smaller conflicts that had little influence on the national economy, Collier & Hoeffler evidently watered down the impact of the war on the post-war recovery process. Our finding underlines the importance of increasing internal consistency in the sample if the analysis is to generate relevant policy recommendations. If donors had followed the recommendation of doubling aid volumes to post-war societies compared to 'normal' countries in order to achieve growth, and if our sample rather than that of Collier & Hoeffler were representative of the countries in question, the donors would be in for a surprise. Both in absolute and relative terms, the growth generated by the extra aid would be negligible (0.26 percentage points).

The policy implications of this finding are significant. If the extra growth generated by aid in the mid-decade period after peace-onset in reality is small or negligible, the trade-off between a growth-oriented aid policy and one that targets immediate peace dividend programs (e.g. demobilization, employment, or rehabilitation) is reduced.

Our recoding also produced different results with regard to the impact of policy. Collier & Hoeffler found that aid was more effective in generating growth in post-conflict countries than in 'normal' situations, and found no additional effects of policy (as measured by CPIA) in post-conflict situations. We found that policy had an additional positive effect, and this held across all of our four recoded samples.

Upon closer examination, then, it becomes clear that Collier & Hoeffler (2004) present conclusions and policy recommendations that their data do not fully support. Apart from the recommendations regarding magnitude and timing of aid, there is weak or curious evidence for two related conclusions:

- Collier & Hoeffler cite 'poverty-efficiency' of aid as an expected result of doubling the aid volume to post-conflict societies compared to 'normal' countries. Yet they do not analyze the impact of aid and growth on poverty in post-conflict societies, but refer to the general literature on the relationship between growth and poverty-reduction. This is a questionable procedure insofar as they find that aid, policy, and growth interact in unusual ways in post-conflict societies as compared to 'normal countries.' It would at least be reasonable to assume that this would equally apply to the aid, growth, and poverty interaction.

- Collier & Hoeffler conclude that improvement in 'social policies' (as measured by the CPIA)[26] has a positive impact on growth in post-conflict situations, and therefore, it should be given first priority in the reform process, whereas 'good' macroeconomic policy (as measured by the CPIA) should be the last priority in the temporal sequence. This conclusion rests on an interaction analysis, which shows a strong and negative relationship between (good) macroeconomic policy and growth. This particular finding is so unusual as to require further explanation.

As our recoding exercise has shown, the Collier & Hoeffler study illustrates the danger of drawing firm conclusions from a single sample in a field of quantitative analysis that has barely started to emerge and where the data are very limited. The very small sample used—in some cases only eight observations—accentuates this point and the related dangers of using it to derive general policy recommendations.

The testing exercise also brought out more general, methodological issues. First, datasets must be appropriate to the research design; in this case, a dataset with indicators related to the impact of the war (e.g. intensity) and challenge of post-war reconstruction (e.g. territorial scope) would have been more useful than the standard battle-related death counts so commonly used in the analysis of causes of war. No such comprehensive dataset exists at present, but some criteria for selection of indicators have been suggested above. Second, as we have shown, insight from the qualitative case literature is necessary to construct a sound dataset as well as to prevent application mistakes. Finally, a conclusion concerns the theoretical foundations for research in this

area. In the absence of a theoretically grounded model of how aid and policy variables are likely to influence economic growth after war, and how the relationships differ from those in 'normal' countries, an appropriate specification of relevant variables might be determined empirically. This requires extensive testing, and certainly more so than done in the Collier & Hoeffler study.

Acknowledgements

Endnotes

1. The project was titled the Economics of Political and Criminal Violence.

2. First reported in a World Bank Policy Research Working Paper circulated in October 2002, their article, 'Aid, Policy, and Growth in Post-Conflict Societies,' the paper was in 2003 posted on the website of the Centre for the Study of African Economics, Oxford University until it was published in 2004 in the *European Economic Review*. There are some minor differences between the two versions of their work, but the conclusions are identical. In the present article, the paper version will be referred to as Collier & Hoeffler (2003) and the published version as Collier & Hoeffler (2004).

3. The critical literature this project has generated is massive. Useful examples include Sambanis, 2004; Ballentine & Sherman, 2003; Addison & Murshed, 2003; Marchal, 2004; Sanin, 2004; and Fearon & Laitin, 2003.

4. *Breaking the Conflict Trap* (Collier, *et al.*, 2003) summarizes their findings on civil war onset, duration, termination, and post-war aid together with the results of other quantitative research on civil war, including the long-term economic and health costs, the spill-over effects to neighbourhoods and the world, and the role of international peacekeeping. Its aim, however, is to promote the 'moral right and practical duty' (p. 8) of international intervention to reduce the global incidence of civil war.

5. The particular formulation and dataset of their study, as will be discussed below, is from the Collier & Dollar (2002) continuation of the Dollar–Burnside studies.

6. For instance, the aid-policy-growth findings were prominently cited by the Norwegian Minister of Development Cooperation at a high-level Norwegian-Japanese seminar on peace building in Oslo, October 2003. Collier has been invited to present his findings to several fora involving donors and UN agencies, including a presentation to the UN Foundation that entered into the work of the UN Secretary-General's High-level panel on Threats, Challenges and Change(http://www.un-globalsecurity.org/pdf/Collier _paper_Security_Development.pdf), as well as presentations to seminars for UN officials and country delegations organized in New York (International Peace Academy, 2002), and to seminars at the Norwegian Ministry of Foreign Affairs (2003).

7. A highly abridged version of the CPIA has recently been made public, but the data are not comparable with the internal World Bank version used by Collier & Hoeffler. The public version has only quintile data for ranked countries (while Collier & Hoeffler used raw scores for individual countries), and has data only for one year (2004)—which of course is useless for a longitudinal analysis (the Collier & Hoeffler data go from 1966 to 1998). The public version is available from http://web. worldbank.org/wbsite/external/extaboutus/ida/ 0,contentmdk:20052347 ~ menu PK:116699 ~ page PK:51236175 ~ piPK:437394 ~ theSitePK:73154,00. html The CPIA index ranks countries based on policy performance in four categories: (i) macroeconomic management, (ii)structural policies for sustainable and equitable growth, (iii) policies for social inclusion, and (iv) public sector management.

8. See symposium in *International Studies Perspectives*, **1**, 2003, based on a session and the 2002 annual conference of the ISA, where editors of four prominent academic journals in international relations made a common

commitment to publish only articles based on open data that could be replicated. We wish to thank Nils Petter Gleditsch for bringing this to our attention.

9. We wish to thank Nils Petter Gleditsch for facilitating the arrangement.

10. Unless otherwise noted, all page references are to Collier & Hoeffler, 2004.

11. Collier & Hoeffler do not use annual data, but only an average for the 4-year period. The peace-onset period is 'discounted' by the month/year in which the war ended.

12. 'We should stress that these results are tentative: with time, the sample of post-conflict countries will increase and the results should be re-assessed.' (2004, p. 1136).

13. The term 'good macroeconomic policies' as used in this literature is either a composite index constructed from a country's budget surplus, inflation, and the Sachs–Warner indicator of openness (Sachs & Warner, 1995) or the CPIA.

14. See Levine & Renelt (1992) for an extreme-bounds analysis applied to economic growth, and Sala-i-Martin, *et al.* (2004) for a less extreme robustness criterion (the Bayesian Averaging of Classical Estimates, BACE).

15. As Hansen & Tarp (2000) demonstrate, a precise second-order Taylor approximation of a standard theoretical growth equation yields an empirical reduced form where both aid and aid squared are present. Moreover, a complete, second-order polynomial response surface in the growth-aid-policy space is defined by aid, aid squared, policy, policy squared, and aid interacted with policy.

16. See Dalgaard, *et al.*, 2000 for a survey of empirical analysis from the last 30 years.

17. Collier & Hoeffler evidently take the figure to mean an *accumulated* threshold, i.e. 'at least 1,000 battle deaths resulted during the civil war' (Collier & Hoeffler, 2004, p. 1127). However, the COW dataset originally used a threshold of 1,000 deaths *per annum*, and only in 1994 lowered the threshold 1,000 for the entire conflict (Sambanis, 2002, p. 6). Since all but three of the conflicts in the Collier & Hoeffler sample ended before 1994, for all practical purposes they are analyzing conflicts with annual death rates of at least 1,000.

18. Of course, short wars can be destructive, especially international wars between organized state entities (e.g. the Six-Day War).

19. Collier & Hoeffler (2002, p. 2).

20. In the Uppsala/PRIO dataset, 'war' involves at least 1,000 battle-related deaths per year. 'Intermediate conflict' has between 25 and 1,000 deaths per year and a cumulative total for the entire conflict of at least 1,000 deaths, but fewer than 1,000 in any given year. 'Minor armed conflict' has at least 25 deaths per year and fewer than 1,000 deaths accumulated during the course of the conflict. The qualitative criteria refer to the type of incompatibility among the parties (a conflict over government or territory) and their organization. See Appendix 1.

21. Several existing datasets have one or more quantitative measures that would be useful in constructing a composite indicator of the intensity of the violence, such as geographic spread of the fighting, human cost, and displacement. These include Collier, Hoeffler & Søderbom (2001), the State Failure (Political Instability) Task Force project, and De Soysa & Gleditsch (1999), in addition to UN agency statistics such as UNHCR data on forced displacement. Collier & Hoeffler (2001) suggest that geographic spread of the fighting and differences in GNP before and after the war would be useful indicators, while Sambanis (2002) calls for measures of relative cost, notably displacement and deaths relative to the total population.

22. Iran's border region became a battlefield, forcing large number of people to flee. Tehran and other cities were bombed and attacked. Iraq's tanker-war severely hurt Iran's oil exports. The Iranian military's strategy of attacking with 'human waves' produced massive casualties and a traumatized population.

23. Note, however, that we find that aid squared is not significant in any of the specifications that include peace-onset variables, in contrast to the Collier & Hoeffler specifications where it is significant at the 5% level. This discrepancy in significance may be due to the inclusion of the aid term in our regressions; Collier & Hoeffler do not include this variable.

24. Limited access to the CPIA dataset prevented us from analyzing the recoded datasets with our preferred specification, i.e., regressions with control variables plus post-conflict1-CPIA and post-conflict1-aid-CPIA. Nevertheless, the limited testing that was done for us using CPIA produced strong, illustrative figures. Using Sample 2 as an illustration, we can see from table 2 that the CPIA-post-conflict1 variable is significant when the aid-policy-post-conflict1 variable is included. Thus, the CPIA-post-conflict1 variable should not be excluded, and the best specification for Sample 2 is given in column S2, CH2 in Table 2, where both of these variables are included.

25. The coefficients of the aid-policy-post-conflict1 variable in the other recoded datasets are 0.111, 0115, and 0.124 for Samples 1, 4, and 3, respectively.

26. For CPIA conflict, see note 7.

References

Addison, Tony and Mansoob Murshed, S., 2003. 'Explaining Violent Conflict: Going Beyond Greed versus Grievance, UNU/WIDER Special Issue on Conflict'. *Journal of International Development* 15(4), 391–396.

Akhand, H.A. and Gupta, K.L., 2002. *Foreign Aid in the Twenty-First Century*. Kluwer Academic Publishers, Boston.

Ballentine, Karen, Nitzschke, Heiko, 2004 'The Political Economy of Civil War and Conflict Transformation. Berghof Research Center for Constructive Conflict Management', Available at: www.berghof-handbook.net/articles.

Ballentine, Karen and Sherman, Jake (eds), 2003. *The Political Economy of Armed Conflict: Beyond Greed and Grievance*. Lynne Rienner, Boulder and London.

Boyce, James K., 1996. *Economic Policy for Building Peace*. Lynne Rienner, Boulder and London.

Burnside, C. and Dollar, D., 1997. 'Aid, Policies and Growth'. *World Bank Policy Research Working Paper* no. 1777.

Burnside, Craig and Dollar, David, 2000. 'Aid, Policies, and Growth'. *American Economic Review* 90(4), 847–868.

Burnside, C. and Dollar, D., 2004. 'Aid, Policies and Growth: Revisiting the evidence'. *World Bank Policy Research Working Paper* (no. 3251), World Bank.

Collier, P. et al., 2003. *Breaking the Conflict Trap. Civil War and Development Policy*. Oxford University Press and The World Bank, Washington, DC and New York.

Collier, P. and Dollar, D., 2002. 'Aid Allocation and Poverty Reduction'. *European Economic Review* **46**, 1475–1500.

Collier, P., Hoeffler, A., 2001 "Data Issues in the Study of Conflict', Paper prepared for the Conference on 'Data Collection on Armed Conflict,' Uppsala, 8–9 June 2001'

Collier, P. and Hoeffler, A., 2002 'Data Issues in the study of conflict', CSAE Econometric and Data Discussion Paper 2002–01.

Collier, P. and Hoeffler, A., 2004 'Aid, Policy and Growth in Post-conflict Societies', *European Economic Review*, 48, 1125–1145.

Collier, P., Hoeffler, A. and Söderbom, M., 2001. 'On the Duration of Civil War', World Bank Working Paper 2681.

Crouch, H., 1978. *The Army and Politics in Indonesia*. Cornell University Press, Ithaca.

Crouch, H., 2000. 'The TNI and East Timor Policy', in Fox, J.F. and Soares, D.B., (eds), *Out of the Ashes. Destruction and Reconstruction of East Timor*. Crawford, Adelaide, pp. 151–179.

de Silva, K.M., 1981. *A History of Sri Lanka*. Oxford University Press, Delhi.

de Soysa, I. and Gleditsch, N.P. 1999. 'To Cultivate Peace: Agriculture in a World of Conflict', *The Woodrow Wilson Centre Environmental Change & Security Report* 5, 15–25.

Dalgaard, C.-J. and Hansen, H., 2001. 'On Aid, Growth, and Good Policies'. *Journal of Development Studies* 37, 17–41.

Dayton-Johnson, J. and Hoddinott, J., 2001 'Optimal Allocation of Development Aid Across Countries', unpublished manuscript, Dalhousie University.

Easterly, W., Levine, R. and Roodman, D., 2003 'New Data, New Doubts: A Comment on Burnside and Dollar's Aid, Policies, and Growth', National Bureau of Economic Research, Inc., Working Paper 9846.

Fearon, James D. and Laitin, David D., 2003. 'Ethnicity, Insurgency, and Civil War'. *American Political Science Review* 97(1), 75–90.

Fortna, VirginiaPage, 2003. *Peace Time; Cease-Fire Agreements and the Durability of Peace*. Princeton University Press, Princeton, NJ.

Gleditsch, N.P., Eriksson, M., Sollenberg, M. and Strand, H., 2002. 'Armed Conflict 1946–2001: A New Dataset'. *Journal of Peace Research* 39(5), 615–637.

Guillaumont, P. and Chauvet, L., 2001. 'Aid and Performance: A Reassessment'. *Journal of Development Studies* 37(6), 66–92.

Hansen, H. and Tarp, F., 2000. 'Aid Effectiveness Disputed'. *Journal of International Development* 12(3), 375–398.

Hansen, H. and Tarp, F., 2001. 'Aid and Growth Regressions'. *Journal of Development Economics* 64, 547–570.

Hartzell, C., Hoddie, M. and Rothchild, D., 2001. 'Stabilizing the Peace After Civil War: An Investigation of Some Key Variables'. *International Organization* 55(1), 183–208.

Holsti, K.J., 1996. *The State, War and the State of War*. Cambridge University Press, Cambridge.

Jupp, J., 1978. *Sri Lanka: Third World Democracy*. Frank Cass, London.

Kanbur, R., 2000. 'Income Distribution and Development', in Atkinson and Bourguignon, (ed), *Handbook of Income Distribution*. North Holland, Elsevier.

Lensink, R. and White, H, 2001. 'Are There Negative Returns to Aid?'. *Journal of Development Studies* 37(6), 42–65.

Levine, R. and Renelt, D., 1992. 'A Sensitivity Analysis of Cross-Country Growth Regressions'. *American Economics Review* 82(4), 942–963.

Licklider, R., 1995. 'The Consequences of Negotiated Settlements in Civil Wars, 1945–1993'. *American Political Science Review* 89(3).

Licklider, R., 'Civil War Termination Data Version 2.1, downloaded from', ~ licklide/">http://www.rci.rutgers.edu/~licklide/

Lu, S. and Ram, R., 2001. 'Foreign Aid, Government Policies, and Economic Growth: Further Evidence from Cross-Country Panel Data for 1970–93'. *Economia Internazionale* 54(1), 14–29.

Marchal, Roland, 2004. 'The Economic Analysis of Civil War According to Paul Collier: A Sociologist's View,' paper presented to the SSRC conference on The Economics of Conflict, Washington, DC, 19–20 April 2004

(an abridged version of 'Une lecture symptomale de quelques théorisations récentes des guerres civiles'. *Critique internationale* (16).

McPherson, Malcolm, (2000) unpublished manuscript, Harvard University 'Aid, Policies, and Growth: A Comment'

Phadnis, U., 1976. *Religion and Politics in Sri Lanka*. Manohar, Delhi.

Sachs, J.D. and Warner, A., 1995. 'Economic Reform and the Process of Global Integration'. *Brookings Papers on Economic Activity* 1, 1–118.

Sala-i-Martin, X., Doppelhofer, G. and Miller, R., 2004 'Determinants of Long-Term Growth: A Bayesian Averaging of Classical Estimates (BACE) Approach', *American Economic Review* 94(4), 813–835.

Sambanis, N., (2002) 'What is Civil War? Conceptual and Empirical Complexities of an Operational Definition', http://www.ligi.ubc.ca/_conferences/030812mecw/ media/ downloaded 21 August 2003. Published in *Journal of Conflict Resolution* 46, 6pp. 814–858

Sambanis, N., 2004. 'Using Case Studies to Expand Economic Models of Civil War'. *Perspectives on Politics* 2(2), 259–279.

Sanin, Francisco Gutierrez, (2004) '10 Reasons for doubting,' paper presented to the SSRC conference on The Economics of Conflict, Washington, DC, 19–20 April'

Schwarz, A., 1994. *A Nation in Waiting. Indonesia in the 1990s*. Westview Press, Boulder.

Singer, J.D., Small, M., (1994) 'Correlates of War Project: International and Civil War Data, 1816-' Computer File. Ann Arbor, MI: J. David Singer and Melvin Small, Producers, 1993. Ann Arbor, MI: Inter-university Consortium for Political and Social Research, Distributor, 1994'

University of Bergen, 1987. *Sri Lanka. Country Study and Norwegian Aid Review*. Centre for Development Studies, Bergen.

Walter, Barbara F., 2001. *Committing to Peace: The Successful Settlement of Civil Wars*. Princeton University Press, Princeton, NJ.

Woodward, Susan L., 2002. 'Economic Priorities for Successful Peace Implementation', in Stephen, Stedman, et al., (eds), *Ending Civil Wars*. Lynne Rienner, Boulder and London.

World Bank, (2003), Romania. Country Brief. 2003. http//lnweb18.worldbank.org. downloaded April 2004

Appendix I: Interaction effects

Table 1. Interaction effects

	CH1	Recoding CH1	CH2	Recoding CH2
Initial per capita	0.718	[0.669, 0.680]	0.715	[0.669, 0.676]
Income	(0.627)	[(0.642, 0.640)]	(0.621)	[(0.642, 0.637)]
Governance	0.196	[0.208, 0.240]	0.197	[0.209, 0.241]
(ICRGE)	(0.160)	[(0.158, 0.157)]	(0.157)	[(0.156, 0.155)]
CPIA	0.991**	[0.835**, 0.880**]	0.991**	[0.837**, 0.882**]
	(0.397)	[(0.414, 0.415)]	(0.396)	[(0.413, 0.415)]
ODA × CPIA	0.134**	[0.184*, 0.190*]	0.134**	[0.183*, 0.189*]
	(0.066)	[(0.112, 0.112)]	(0.066)	[(0.112, 0.112)]
$(ODA/GDP)^2$	− 0.028**	(− 0.024*, − 0.026*)	− 0.028**	(− 0.024*, − 0.026*)
	0.012	[(0.014, 0.014)]	(0.012)	[(0.014, 0.014)]
South Asia	2.614***	(2.636***, 2.668***)	2.611***	(2.633***, 2.666***)
	(0.644)	[(0.645, 0.643)]	(0.639)	[(0.642, 0.640)]
East Asia	2.891***	(2.974***, 3.023***)	2.889***	(2.973***, 3.022***)
	(0.663)	[(0.670, 0.670)]	(0.660)	[(0.669, 0.669)]
Sub-Saharan	− 0.440	(− 0.454, − 0.535)	− 0.442	(− 0.453, − 0.537)
Africa	(0.821)	[(0.818, 0.817)]	(0.817)	[(0.817, 0.815)]
Middle East/	1.590***	(1.567***, 1.582***)	1.591***	(1.566***, 1.579***)
North Africa	(0.568)	[(0.573, 0.562)]	(0.567)	[(0.573, 0.561)]
Europe/Central	− 0.400	(− 0.408, − 0.429)	− 0.402	(− 0.405, − 0.429)
Asia	(1.059)	[(1.060, 1.055)]	(1.056)	[(1.059, 1.053)]
Post-conflict 1	1.385	(− 0.314, − 2.705)	1.445	(− 0.335, − 2.615)
	(3.237)	[(4.539, 2.860)]	(3.073)	[(4.626, 2.791)]
(Post-conflict1) ×	− 0.186	(0.561, 1.519)	− 0.180	(0.605, 1.550*)
(CPIA)	(1.011)	[(1.897, 0.972)]	(1.019)	[(1.767, 0.925)]
(Post-conflict1) ×	− 0.009	(0.008, − 0.039)		
$(ODA/GDP)^2$	((0.102)	[(0.968, 0.097)]		
(Post-conflict1) ×	0.168	(0.076, 0.219)	0.141***	(0.102**, 0.095***)
(ODA/GDP) ×	(0.330)	[(0.316, 0.319)]	(0.042)	[(0.051, 0.037)]
(CPIA)				
Observations	344	344	344	344
Post-conflict	13	10−11	13	10−11
Observations				
R^2	0.38	0.38	0.38	0.38

Note: Robust standard errors in parentheses. ***significance at 1%; **significance at 5%; *significance at 10%. All regressions include time dummies. [x,y] indicates that x is the value of the coefficients that is least significant among the four coefficients from the for different recoded data sets. Similarly, y refers to the value of the coefficient that is the most significant among the four coefficients. Then [(v,z)] indicates that x has a robust standard error of v, while y has a robust standard error of z.

Table 2. Interaction effects, continued

	CH3	Recoding CH3	CH4	Recoding CH4
Initial per capita	0.717	[0.665, 0.677]	0.712	[0.659, 0.659]
Income	(0.618)	[(0.637, 0.636)]	(0.617)	[(0.636, 0.636)]
Governance	0.198	[0.207, 0.235]	0.172	[0.183, 0.182]
(ICRGE)	(0.157)	[(0.155, 0.154)]	(0.155)	[(0.156, 0.156)]
CPIA	0.988**	[0.857**, 0.890**]	1.021***	[0.928**, 0.933**]
	(0.390)	[(0.408, 0.408)]	(0.064)	[(0.412, 0.412)]
ODA × CPIA	0.134**	[0.182*, 0.185*]	0.127*	[0.170, 0.174]
	(0.065)	[(0.112, 0.111)]	(0.064)	[(0.112, 0.112)]
$(ODA/GDP)^2$	−0.028**	(−0.024*, −0.026*)	−0.028**	(−0.024*, −0.024*)
	(0.012)	[(0.014, 0.014)]	(0.012)	[(0.014, 0.014)]
South Asia	2.619***	(2.616***, 2.614***)	2.662***	(2.633***, 2.636***)
	(0.625)	[(0.631, 0.630)]	(0.620)	[(0.633, 0.633)]
East Asia	2.884***	(2.965***, 3.002***)	2.880***	(2.923***, 2.921***)
	(0.660)	[(0.664, 0.664)]	(0.660)	[(0.664, 0.663)]
Sub-Saharan	−0.442	(−0.455, −0.541)	−0.366	(−0.376, −0.385)
Africa	(0.816)	[(0.815, 0.813)]	(0.809)	[(0.814, 0.813)]
Middle East/	1.589***	(1.577***, 1.571***)	1.606***	(1.600***, 1.619***)
North Africa	(0.567)	[(0.572, 0.559)]	(0.563)	[(0.566, 0.560)]
Europe/Central	−0.403	(−0.410, −0.430)	−0.365	(−0.391, −0.394)
Asia	(1.054)	[(1.058, 1.053)]	(1.053)	[(1.057, 1.054)]
Post-conflict 1	0.913	(1.324, 2.015***)		
	(0.755)	[(0.991, 0.755)]		
(Post-conflict1) × (CPIA)				
(Post-conflict1) × $(ODA/GDP)^2$				
(Post-conflict1) × (ODA/GDP) × (CPIA)	0.139***	(0.105***, 0.124***)	0.186***	(0.199***, 0.188***)
	(0.041)	[(0.039, 0.040)]	(0.046)	[(0.055, 0.051)]
Observations	344	344	344	344
Post-conflict observations	13	10 − 11	13	10 − 11
R^2	0.38	0.38	0.38	0.38

Appendix II: Civil war coding of comparable datasets.

	Burundi	Guatemala	Indonesia	Iran	Jordan	Morocco	Nigeria	Peru	Romania	Somalia	Sri Lanka
	COW (2) · COW (1) · Holsti (2) · CWT · U/P · C&H	COW (2) · COW (1) · Holsti (1) · CWT · U/P · Holsti	C&H · COW · U/P (2) · U/P (3) · CWT · Holsti (1)	C&H · COW · U/P (1) · U/P (2) · U/P (3) · CWT · Holsti (1)	COW · COW · CWT · Holsti	C&H · COW · U/P · CWT · Holsti	COW · COW (2) · COW (2) · U/P (1) · U/P (2) · CWT (1) · CWT (2) · Holsti	C&H · COW · U/P · CWT · Holsti (1) · Holsti (2)	COW · C&H · U/P · CWT · Holsti	COW · COW · CWT · U/P · Holsti (2) · Holsti (1)	Holsti · CWT · U/P · COW · C&H
1961		X									
1962		X									
1963		X									
1964		X									
1965		M X	M X								
1966		X X M X X X		X	X		X M				
1967		X X M X X X		X	X		X X X X X				
1968		X X I X X		X	X		X X X X X				
1969		X X X X X		X			X X X X X				
1970		X X X X X		X	X X X X		X X X X X				
1971		X X X X X		X							X X X X X
1972	X X X X	X X X X X		X							
1973	X		X X X		X						
1974			X X X E	X							
1975			X X X X X E X	X X X		X E X X X					
1976			X X X X X E X X	X X X		X E X X X					
1977			X X X X X E X X	X X X		X E X X X					
1978		X	X X X X X X X X	X X X	X X X	X E X X X				M	
1979		X	X X X X X I	X X X	X M M X X X X	X E X X X					
1980		X	X X X X X I	X X X	M M X X	X E X X X X X X X	M X				
1981		X	X X X X X I	X X X X X X I X		X E I X X X X X	X X X			M	
1982		X	X X X X X I	X X X X X X X X		X E I X X X X	X X X X X			X M	
1983		X	X X X X I	X X I X		X E I X X X X	X X X X X			X M	
1984		X	X X X X I	X X I X		X I X X X X X	X X X X X X			X M	
1985			X X X I	X X I X		X I X X	X X X X X X			X M	
1986			X X X I	X X I I X		X I X X	X X I X X X			X M	
1987			X X X I	X X I I X		X I X X	X X I X X X			X I	
1988	X X X		I X X I	X X I I X		X I X X	X X X X X X			X X I X	
1989	X		I X X I M X X	X X		X I X X	X X X X X X X X M			X X X X X	X
1990	M X		I X X X X X	X X		X X	X X X X X X			X X X X X X	I
1991	X M X		I X X I X X	X X		X X	X X X X X X			X X X X X X	
1992	M X		X * X I X X	X X		X	X * X * X X			X X X * X	
1993			I X X X	X I X		X	X I X X			X I X	X
1994			I X X X	X X			X I X X			X I X	X
1995	M		I	X			X I			X I	
1996	M						X I			X I	
1997	I		I				I			X	
1998	X		I				I				
1999	I		I				I				
2000	X		I	I							
2001	X		I	I							

X Civil War
E Extra Systemic War

M   Minor Armed Conflict
I   Intermediate Armed Conflict
*   War ongoing by the end of the study
C&H   Collier and Hoeffler (2003), Table A1
          Operationalization of Civil War: (a) an internal conflict between a government
          and an identifiable rebel organization that results in at least 1,000 combat-related
          deaths, (b) of which at least 5% must be incurred on each side
COW   Correlates of War Project, see (1993). Singer and Small (1994). This older version
          of the COW-Project is what Collier and Hoeffler (2003) give as their source for
          use in the A1 table.
          Operationalization of Civil War: (a) military action was involved, (b) the
          national government at the time was actively involved, (c) effective resistance
          (as measured by the ratio of fatalities of the weaker to the stronger forces)
          occurred on both sides, and (d) at least 1,000 battle deaths resulted during the
          civil war.
          Note: Guatemala: (1) Government vs. Indians, (2) Government vs. leftists of
                    1978. COW also refers to a third war, 70-71: Government vs. leftists of 1970.
                    Indonesia: East Timor
                    Iran: Anti-Shan-Coalition, MujahidinNigeria: (1) Biafra; (2) Muslim
                    fundamentalists
U/P: Armed Conflict Dataset, 1946-2001. Uppsala Conflict Data Project and the
          International Peace Research Institute, Oslo (PRIO). Gleditsch et al. (2002)
          Operationalization of Civil War (Internal Armed Conflict): Internal armed conflict
          occurs between the government of a state and internal opposition groups without
          intervention from other states. Minor Armed Conflict: at least 25 battle-related
          deaths per year and fewer than 1,000 battle-related deaths during the course of the
          conflict. Intermediate Armed Conflict: at least 25 battle-related deaths per year and
          an accumulated total of at least 1,000 deaths, but fewer than 1,000 in any given year.
          War: at least 1,000 battle-related deaths per year.
          Note: Burundi: Tutsi Supremacists
                    Indondesia: (1) East Timor; (2) West Papua; (3) Aceh
                    Iran: (1) Government vs. Mujahideen e khalq; (2) Territory of Arabistan;
                    (3) Territory of Kurdistan
                    Jordan: 1970 is not coded as war, but is mentioned in their list of unclear
                    cases. Explanation: unclear incompatibility
                    Morocco: Territory of Western Sahara
                    Nigeria: (1) Military faction; (2) Territory of Biafra
                    Nigeria: the period of 1980-84 is mentioned in their list of unclear cases.
                    Explanation: Unclear level of organization and unclear incompatibility
                    Peru: Peruvian government vs. Shining Path, Red Path, MRTA
CWT: Civil War Termination Data, Licklider (1995)
          Operationalization of Civil War: involvement of large-scale violence, killing
          people. (a) 1,000 battle deaths or more per year and (b) effective resistance, that is,
          at least two sides must have been organized for violent conflict before the war
          started or else the weaker part must have imposed causalities on its opponent equal
          to at least 5% of its own.
          Note: Indonesia: (1) East Timor

Nigeria: (2) Muslim fundamentalists

Holsti: Major armed conflicts by region and type, 1945-1995, Appendix to K. Holsti, The State, War and the State of War (1996)

Operationalization of civil war: (a) state vs. nation wars: including armed resistance by ethnic, language or religious groups, often with the purpose of secession or separation from the state; or (b) internal wars based on ideological goals.

Note: Burundi: (1) Tutsi-led government vs. Hutu rebels (2) Tutsi government massacre of Hutu civilians

Guatemala: Guatemalan government vs. URNG rebels

Indonesia: (1) Indonesian government vs. OPM rebels in Irian Jaya. (2) Indonesian government vs.. FRETILIN rebels

Iran: (1) Iranian (Saha) government vs. fundamental Islamic (Khomeini) rebels; (2) Iranian government vs. Kurdian rebels

Morocco: Irredenta / secession / resistance. Morocco, Mauritania (to 1979) governments vs. POLISARIO rebels

Nigeria: Nigerian government vs. Ibo rebels in Biafra

Peru: (1) Peruvian government vs. Sendero Luminoso rebels; (2) Peruvian government vs. MRTA rebels.