

“Economic Aid to Post-Conflict Countries: A Methodological Critique of Collier and Hoeffler”

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Chr. Michelsen Institute *Development Studies and Human Rights*

* Woodward is professor of political science at The Graduate Center, City University of New York. The research was assisted with a grant from the Research Council of Norway.

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In the past five years, research sponsored by the World Bank on the economic aspects of civil war¹ 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.

1. Introduction³

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 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 duration of civil wars and policy recommendations for war termination that have since been institutionalized in programs such as UN-mandated targeted sanctions and the Kimberley process on conflict diamonds. 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.⁴

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.”⁵ Economic decline leads to civil war which “wrecks the economy and increases the risk of further war” (p.1). Economic growth is necessary to

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,” will be published this year in the *European Economic Review* (hereafter C&H 2004).

³ This research note is based on a longer paper presented at the UNU/Wider conference, “Making Peace Work”, Helsinki, 4-5 June 2004. Anette Walstad Enes at the Chr. Michelsen Institute assisted in the preparation of this paper.

⁴ The critical literature this project has generated is massive. Useful examples include Sambanis (2004); Ballentine and Sherman (2003); Addison and Murshed (2003); Marchal (2004); Gutierrez Sanin (2004); and Fearon and Laitin (2003).

⁵ *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 spillover 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.

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,⁶ Collier and 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 peacebuilding. 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 (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; and delays in the introduction of democratic political institutions until some economic growth has 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. Although running counter to the current practice of most donors, their recommendations and the “hard data” on which they are said to be based 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 (henceforth, C&H 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, 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. Because 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 note 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 data set (the CPIA), which is not available to outside scholars. In terms of scientific rules and the principle of transparency, this is naturally of great concern, as has been emphasized in the

⁶ The particular formulation and data set of their study, as will be discussed below, is from the Collier-Dollar (2002) continuation of the Dollar-Burnside studies.

scholarly community.⁷ For the purpose of this study, Anke Hoeffler kindly agreed to run some basic regressions for us using the CPIA on our recoded sample,⁸ but this permitted only a partial retesting.

2. The Findings of Collier and Hoeffler (2002; 2004)

The C&H 2004 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” (p.14).⁹ 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” (p.14). 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” (p.14). Third, regarding policy priorities, C&H find that improvement in anti-poverty measures (“social policies”) in post-conflict situations has a positive impact on growth and therefore 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.

⁷ See symposium in *International Studies Perspectives*, no. 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.

⁸ We wish to thank Nils Petter Gleditsch for facilitating the arrangement.

⁹ Unless otherwise noted, all page references are to C&H 2004.

3. Methodological issues in C&H

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

3.1 Sample size

C&H list 31 cases of countries emerging from civil wars in their sample, although the number for which they have observations is slightly smaller - only 27. The effects of aid are observed in three periods after the war ends of four-year time periods each: “peace onset,” “post-conflict 1,” and “post-conflict 2.” Of the 27 countries, C&H have data regarding the effects of aid in post-conflict situations for only 13 cases in the first period after war, 13 cases in the following period, and only eight for the final period.¹⁰ Moreover, the data set 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. In the end, the conclusions regarding the effects of aid in post-conflict situations – that is, the principal findings listed in the first two conclusions above – depend on between 8 and 13 observations. The small sample size in itself raises critical questions regarding the representativeness and validity of the conclusions.

C&H give the usual caveats about the need for caution when generalizing from such a small sample. Nevertheless, they 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” (p.14) – 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 C&H study. 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” (p. 9). As a result, it is “unfortunately not feasible to investigate the effects during the second full peace episode” (p.9).

3.2 Methodological weaknesses in aid-policy-growth regressions

The policy recommendations in C&H 2004 stem from applying methods used in much of the econometric work that tries to apply interaction effects 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 C&H use and consider to what extent they also apply here.

¹⁰ C&H 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.

C&H 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.¹¹ 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 and Tarp (2000, 2001), McPherson (2000), Dalgaard and Hansen (2001), Dayton-Johnson and Hoddinott (2001, 2003), Guillaumont and Chauvet (2001), Lensink and White (2001), Lu and Ram (2001), Akhand and Gupta (2002), and Easterly, Levine and Roodman (2003). By contrast, the study by Collier and Dollar (2002) supports Burnside and Dollar (1997, 2000).

The main criticism against Burnside and 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 and 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 and Dollar (2004). Due to the similarities in methodology, this is a source of concern for the C&H 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 C&H have not gone very far in this direction. Given the attention that this issue has drawn in the growth literature,¹² and the small and internally heterogeneous sample used in C&H 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 we could therefore not test the fundamentals of the model used by C&H. Nevertheless, several methodological weaknesses can readily be identified and call into question the validity of the conclusions that C&H draw.

C&H 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.¹³ This more standard approach would yield eleven initial variables, rather than the seven which C&H use. Omitting “aid” in the regressions is particularly questionable. If aid has a positive effect on growth -- as most

¹¹ 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 and Warner 1995) or the CPIA.

¹² See Levine and Renelt (1992) for an extreme-bounds analysis applied to economic growth, and Salai-Martin, et al. (2003) for a less extreme robustness criterion (the Bayesian Averaging of Classical Estimates, BACE).

¹³ As Hansen and 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.

other studies have found¹⁴ -- 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 C&H.

The C&H 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. But instead of testing for joint significance, which is standard procedure, C&H 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 above. This stepwise exclusion of variables is clearly ad-hoc, and, by omitting variables that could be jointly significant, could cause substantial bias in the results.

A similar approach is adopted with respect to assessing the findings. C&H test whether their results depend on certain influential data points by removing one of the 13 observations, one at the time. A more appropriate method would be to see whether deleting two or three observations simultaneously from the regressions will have any effect. Given that the Burnside and 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 C&H interaction variables. Moreover, as Dalgaard and 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 C&H 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 C&H study also raises questions regarding internal consistency in the model with which, at least implicitly, they operate. 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 is a remarkable impact of aid, and -- although C&H 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, C&H 2004 assume that post-conflict countries conform to the general pattern that Collier and Dollar (2002) found for all countries. Although C&H 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 C&H paper to highlight conclusions that are likely to be welcomed in the aid community, while findings that are inconsistent with the literature or conventional wisdom are glossed over. In the latter category is their striking statistical finding

¹⁴ See Dalgaard, et al. 2000 for a survey of empirical analysis from the last 30 years.

that in post-conflict situations, “bad” macroeconomic policy is associated with rapid growth.¹⁵ This surprising result – which is not discussed in the text, nor are the policy implications elaborated -- is precisely the opposite of the general conclusions in the literature, above all Burnside and Dollar (2000). Very few would believe that improvements in macro policy, such as improving fiscal policies or macroeconomic management, would reduce economic growth. Most likely, the C&H result signifies multicollinearity, which, if not accounted and corrected for, is likely to produce systemic biases in the findings and thus affect all the results.

4. Recoding

4.1 The C&H sample

C&H use a conventional data set on civil wars, Correlates of War (COW (1984, 1994), as updated by Nicholas Sambanis. As they explain it, the data set 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” (p.3).¹⁶

The threshold of battle-related deaths provides the core of commonality in the C&H sample. In other respects the 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 12/1989, Jordan 9/1970, Burundi 8/1988), 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 – C&H 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: 21-24), 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, C&H use data for

¹⁵ Table 6 in C&H 2004 shows a negative coefficient of the dummy post-conflict interacted with macro policy, and it is significant at 1%.

¹⁶ C&H evidently take the figure to mean an *accumulated* threshold, i.e., “at least 1,000 battle deaths resulted during the civil war” (C&H 2003: 3). However, the COW data set 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 C&H sample terminated before 1994, for all practical purposes they are analyzing conflicts with annual death rates of at least 1,000.

Indonesia as a whole. Similar problems do not arise for wars of independence, where separate data for the new state are more readily available.¹⁷

The overall cost and relative cost: C&H do not use any estimate of the overall human and economic costs of the war, but the marked differences in duration of conflicts which 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. C&H apparently sacrificed homogeneity in order to get a larger set of numbers. Although they obtain some results that are 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.

While COW might be useful for studying the *causes* of conflict, as Collier and Hoeffler elsewhere argue,¹⁹ it is not self-evident that COW 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 data set 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. COW, by contrast, does not include indicators relevant for postwar reconstruction policy such as relative cost or intensity of the violence, the territorial scope of the conflict, or forcible displacement of persons.²⁰

4.2 Recoding the data

Although there is an argument for having common data sets that would provide a common basis for analysis of civil wars, when the analytical foci differ the argument for having differentiated data sets is even stronger.²¹ In consequence of this logic, Licklider (1995) constructed a separate data set to study how war ends, as did Hartzell, Hoddie, and Rothchild (2001) to test the determinants of the longevity of peace settlements. Other main data sets have been used to analyze causes of war, including COW and the Uppsala/PRIO sets, and

¹⁷ If the purpose was to explain the causes of war, rather than its consequence, a decision to include wars of independence raises similar problems of separate data availability for independent variables pertaining to the colonial territory (Sambanis 2002: 21-24).

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

¹⁹ Collier and Hoeffler (2002: 2).

²⁰ In the Uppsala/PRIO data set, "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, and "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 Annex 1.

²¹ Mack (2002) makes an argument for "anointing" one data set to serve as a common basis for analysis, rejecting the pluralist argument for multiple sets. However, unless the single data set is extraordinarily rich in specifications, the argument for having different data sets to suit different research designs is particularly strong when the analytical foci differ markedly, for instance, as between the causes and the consequences of civil war.

were developed in part for that purpose. There is currently no civil war data set that includes the kind of indicators related to intensity, destruction, displacement, and relative cost that would be especially relevant for assessing the impact of aid in the post-war period. Data to construct such indicators are available, but scattered.²²

For this study, we limited our modifications of the C&H sample to the goal of recoding their basic data set and replicating their design so as to examine the effects of introducing incremental, measured changes in their sample on the conclusions they draw. Further study of the impact of aid and policy on post-war reconstruction should construct more appropriate data sets. Our recoding of the C&H/COW sample is a step in this direction by suggesting alternatives for improved coding criteria.

In recoding the C&H data set, we had three aims: a) *reducing the heterogeneity of the sample* b) adjusting *the time period* when the case literature and comparable data sets indicated equally or more reasonable alternative end dates for the war, and c) *correcting mistakes* in the application of data sets. We then constructed a core sample based on recoding on all three accounts, and, in order to assess the impact of marginal variations, added individual borderline cases to produce three more recoded samples.

(a) Reducing heterogeneity

To reduce heterogeneity requires identifying a core in the recoded, more homogeneous set of units of analysis. C&H 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 data set (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. Data sets 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 C&H sample. C&H, COW and other comparable data sets record the event as "war" (see Annex II). In the case literature, by contrast, the 1971 violence is uniformly referred to as an

²² Several existing data sets each 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 Fearon (2001) and Collier, Hoeffler and Söderbom (2001), the State Failure (Political Instability) Task Force project, and De Soysa and Gleditsch (1999), in addition to UN agency statistics such as UNHCR data on forced displacement. Collier and 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.

uprising or an insurrection, not a war (K.M. de Silva 1981; Jupp 1978; Phadnis 1976). The violence easily meets the threshold of combat-related deaths in both the COW and Uppsala/PRIIO data set 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 south-eastern 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.

To capture at least some of these dimensions – let us call it the war-impact factor – we recoded according to an additional quantitative measure of the duration of the conflict and a qualitative assessment of the material destruction, geographic scope, and population displacement that was based on careful examination of the case literature. To qualify as a civil war, the violent events included in the C&H 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.

Following these criteria, the 1971 uprising in Sri Lanka was excluded from our core sample. For the same reasons, we excluded Romania (1989) and Burundi (1988). C&H/COW are almost alone among comparable data sets in considering the 1989 events in Romania a "civil war." (See Annex 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 difficult borderline case. 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 the violence makes it difficult to identify discrete "wars" – and the 1988 events appear differently in comparable data sets (Annex II). Clearly, if 1988 is treated as a "war" year, so should 1993, which C&H 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 data sets record the conflict quite differently (see Annex II). We decided to keep the Nigeria case in our core sample, but excluded it from sample 3 and 4 to assess its effect.

C&H 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, C&H decided to include them, perhaps 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 C&H 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. But C&H 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 were deployed in East Timor (Schwarz 1994). Less than 10 per cent 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 the territory was generally closed to outsiders by Indonesia until 1989. 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.

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 (C&H 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. We therefore 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 generates growth, which is exactly the reverse of what C&H conclude from their analysis of post-conflict situations. Had the C&H recommendations on policy priorities for post-conflict situations been followed, the result probably would have been the opposite of what C&H would have predicted. In the cases of Indonesia/East Timor and Morocco/Western Sahara, the implications for research design are somewhat similar. For purposes of studying the effects of war, data limitations suggest that colonial wars be recognized conceptually as a distinct category of civil wars. Yet, the distinction may be irrelevant in particularly intense conflicts, as in the Morocco/Western Sahara case. In either

case, coding criteria need to be applied with benefit of qualitative insight from the case literature.

b) 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 or more relevant markers may be institutional events that signal 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 entail 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 C&H methodology requires -- is even more exacting. As a result, some of the end dates used by C&H have a "soft" quality, a feature also found in other, comparable data sets and that is reflected in the variation among them (see Annex II). After carefully comparing the end dates used by C&H with those of three comparable data sets and in-depth examination of the case literature, we found five cases where a different end date was equally or more plausible (see Annex II). In most cases, we considered battle-related deaths in light of institutional characteristics of the kind suggested above. The changes were as follows:

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 <i>fedayeen</i> 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

c) Correcting mistakes in application

Two coding cases in the C&H set must be described as plain mistakes, stemming from a poor match between the data base and the research design. The C&H study, it will be recalled, focuses on the post-war period, yet the main data set they use (COW) includes only civil wars. In two of the cases used by C&H, the end date for the civil war is correct, but C&H make no allowance for the fact that an international war simultaneously commenced. As a result, time periods that C&H code as "peace onset" and "post-conflict" were manifestly not

so. This is most obvious with respect to Iran, where C&H 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.²³ A similar but less grotesque coding application is evident in the case of Somalia, where C&H use December 1992 as the end of the (civil) war. They ignore the fact that major fighting occurred in mid-1993 because now the conflict was not merely civil, but ranged US forces against the Somalis. In a narrow technical sense C&H are correct, yet in terms of their own research design the coding makes no sense. These cases again emphasize the pitfalls of using a data set primarily designed for studying the causes of war as a basis for assessing developments in the post-war period.

5. Results from the recoded data sets

After recoding, our core sample had five conflicts fewer than that used by C&H, 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 recoding samples are presented in Table 1. Countries in italics constitute the 13 observations that are the basis for the first two of the three C&H 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 C&H have no data at all are marked with an asterisk, and it is unclear 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 C&H 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.

²³ Iran's border region became a battlefield, forcing large number of people to flee. Teheran 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.

Table 1. C& H sample and recoded samples

Country	Collier and 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
<i>El Salvador</i>	<i>01/92</i>				
<i>Ethiopia</i>	<i>05/91</i>				
<i>Guatemala</i>	<i>07/72</i>	<i>One war, ended 06/93</i>	<i>One war, ended 06/93</i>	<i>One war, ended 06/93</i>	<i>One war, ended 06/93</i>
<i>Guatemala</i>	<i>03/84</i>				
Guinea-Bissau	12/74				
India	06/94				
<i>Indonesia</i>	<i>09/82</i>	<i>No war</i>	<i>No war</i>	<i>No war</i>	<i>No War</i>
Iran	05/82	08/88	08/88	08/88	08/88
Jordan	09/70		07/71	07/71	07/71
<i>Morocco</i>	<i>11/89</i>		<i>09/91</i>		<i>09/91</i>
Mozambique	10/92				
Nicaragua	07/79				
Nicaragua	04/90				
<i>Nigeria</i>	<i>01/70</i>				
<i>Nigeria</i>	<i>08/84</i>			<i>No war</i>	<i>No war</i>
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
<i>Somalia</i>	<i>12/92</i>	<i>12/93</i>	<i>12/93</i>	<i>12/93</i>	<i>12/93</i>
<i>Sri Lanka</i>	<i>05/71</i>	<i>No war</i>	<i>No war</i>	<i>No war</i>	<i>No war</i>
Sudan	02/72				
Uganda	04/88				
<i>Zimbabwe</i>	<i>12/79</i>				

Setting aside for the moment the methodological constraints on the C&H study discussed in section 2 above, we tested the results by applying the exact same methodology on our recoded samples. We were only able to test empirically two of C&H'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 data set, we could not test this conclusion.

Applying exactly the same methodology as C&H 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 data sets (See Annex 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.²⁴ 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 data sets 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 C&H.

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 C&H'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 C&H.²⁵ The coefficient indicating the effect of aid was less than half of the coefficient preferred by C&H. The implications for policy are important: using the original C&H 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 C&H conclusions in this regard are thus quite fragile.

²⁴ 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 C&H 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; C&H do not include this variable.

²⁵ Limited access to the CPIA data set prevented us from analyzing the recoded data sets 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.

Table 2. Interaction effects Sample 2

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

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 C&H's preferred coefficient of aid-policy-post-conflict1 (table 2, Annex I column CH4) is 120% larger than the preferred coefficient in our Sample 2.²⁶ The implications for policy are obvious: using the original C&H 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

²⁶ The coefficients of the aid-policy-post-conflict1 variable in the other recoded data sets are 0.111, 0.115, and 0.124 for Samples 1, 4, and 3, respectively.

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

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 impact of aid across all countries, and so

$$g^{extra} = \mathbf{b} AP \mathbf{q} \quad (2)$$

where \mathbf{b} is the coefficient

\mathbf{q} is the post-conflict 1 dummy

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

$$\frac{dg}{dA} = \mathbf{b} P \quad (3)$$

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. We therefore 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 C&H to be 2.88, while the average aid for these 13 observations of post-conflict societies is 2.14.

Using the C&H 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 C&H, namely that “aid disbursement should probably gradually rise during the first four years....” (p. 14) is not clearly grounded in their empirical results. They find, as we do in all the recoded data sets, 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 is 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 C&H 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. 14), 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 C&H does, that aid should “gradually taper back to normal” in the subsequent years.

6. Conclusions

The results of our retesting are in part subject to the same methodological shortcomings we have identified in the Collier and 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 C&H results based on a heterogeneous sample of conflicts. By including a number of smaller conflicts that had little influence on the national economy, C&H 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 C&H 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. C&H 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 C&H 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:

- C&H 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.

- C&H conclude that improvement in “social policies” (as measured by the CPIA)²⁷ has a positive impact on growth in post-conflict situations and therefore 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, which is not explained or further discussed, is so unusual as to require explanation.

As our recoding exercise has shown, the C&H 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, data sets must be appropriate to the research design; in this case, a data set with indicators relevant for the impact of war and post-war reconstruction 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 data set exists at present, but some criteria for selection of indicators have been suggested above. Second, 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 C&H study.

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²⁷ Social inclusion policy in the CPIA consists of a composite rating of three items: i) poverty monitoring and analysis, ii) pro-poor targeting and programs, and iii) safety nets.

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Annex I: Interaction Effects

Table 1: Interaction effects

[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.

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

Note: Robust standard errors in parentheses. *** indicates significance at 1%, ** significance at 5%, and * significance at 10%. All regressions include time dummies.

Table 2: Interaction effects, continued

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

Note: Robust standard errors in parentheses. *** indicates significance at 1%, ** significance at 5%, and * significance at 10%. All regressions include time dummies.

Annex II: Civil-War Coding of Comparable Datasets

	Sri Lanka		Somalia		Romania		Peru		Nigeria		Morocco		Jordan		Iran		Indonesia		Guatemala		Burundi	
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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 (1993). 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-Koalition, Mujahidin Nigeria: (1) Biafra; (2) Muslim fundamentalists
U/P:	Armed Conflict Dataset, 1946-2001. Uppsala Conflict Data Project and the International Peace Research Institute, Oslo (PRIO) 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 Indonesia: (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 casualties 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, <i>The State, War and the State of War</i> (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

Annex III: Countries, War Dates, and Observations, Table A1 in C&H 2004

Country	Start of the War	End of the War	Peace-onset	Post conflict 1	Post conflict 2
Angola	11/75	05/91			
Burundi	4/72	12/73		*	
Burundi	08/88	08/88		*	
Chad	03/80	08/88		*	*
Congo	97	10/97			
El Salvador	10/79	01/92	*	**	
Ethiopia	7/74	05/91	*	**	
Guatemala	07/66	07/72		**	
Guatemala	03/78	03/84	*	*	**
Guinea-Bissau	12/62	12/74		**	*
India	84	94	*		
Indonesia	06/75	09/82	*	**	**
Iran	06/81	05/82			*
Jordan	09/70	09/70		*	*
Morocco	10/75	11/89	*	**	**
Mozambique	07/76	10/92		*	
Nicaragua	10/78	07/79	*		
Nicaragua	03/82	04/90	*	**	
Nigeria	01/66	01/70		**	
Nigeria	12/80	08/84	*	**	**
Pakistan	01/73	07/77	*	**	**
Peru	03/82	12/96	*		
Philippines	09/72	12/96	*		
Romania	12/89	12/89		*	
Russia	12/94	08/96			
Rwanda	10/90	07/94			
Somalia	05/88	12/92	**		
Sri Lanka	04/71	05/71		**	**
Sudan	10/63	02/72		**	**
Uganda	10/80	04/88		*	*
Zimbabwe	12/72	12/79	*	**	**

Cols 2&4: two asterisks indicate that the post-conflict observations are included in the 344 sample, one asterisk indicates that the observation was also included in the 532 sample.

Summary

This paper retests the analysis of “Aid Policy and Growth in Post-Conflict Societies,” by Paul Collier and Anke Hoeffler (October 2002 and forthcoming in *European Economic Review*). It finds that their data and analysis do not support their conclusions and policy recommendations on the optimal timing and amounts of aid. These conclusions depend on very few observations (13 for the period of peace-onset, 13 for years 4 to 7 when a growth spurt is said to make aid particularly effective, and 8 for the period when aid should taper off); are vulnerable to the same methodological misspecifications identified in the Burnside and Dollar approach on which this analysis is based; and are not grounded in any theoretical formulation about the special relation between aid and growth in post-conflict conditions. Conventional econometric procedures are often not followed; recoding the sample to exclude cases that are not civil wars reduces the effect of aid on growth in post-civil war countries to less than half of what they claim; and the difference with the relationship for “normal” countries becomes negligible (0.26 percentage points), although it depends on identification of the sample. Their claims on the poverty-efficiency of aid are assumed, not analysed. The confidentiality of their policy measure (CPIA) prevented testing the aid-policy relationship.

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