Peace on quicksand? Challenging the conventional wisdom about economic growth and post-conflict risks

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Abstract

In a widely cited study, Collier, Hoeffler & Söderbom (2008) show that economic growth reduces the risk of post-conflict peace collapse – particularly when the UN is present with a peace mission. These findings are encouraging for interventionist international policy makers. We replicate their study using data from the UCDP/PRIO Armed Conflict Database instead of the Correlates of War database. We generate a series of different datasets on the basis of different coding criteria commonly used in the literature, and rerun a simplified version of their model. Surprisingly, our results do not support their findings regarding the risk-reducing effect of economic growth and UN involvement. At best, the results are mixed. Some of the models even suggest that economic growth may *increase* the risk of post-conflict peace collapse. Overall, we are forced to conclude that the impact of economic growth and UN involvement on the risk of post-conflict peace collapse may not be clear and simple. The differences in the results seem to be driven by two sources: the conflicts included in the original datasets and the coding of the start and end dates of the conflicts.

Most of the recent civil war onsets are due to reoccurrence of old conflicts (2008). According to Collier, Hoeffler & Söderbom (2008), the risk of conflict recurrence during the first post-conflict decade are 40%. Thus, effective handling of post-conflict periods is argued to be ‘the most important component in international efforts to bring down the incidence of civil war’ (Elbadawi, Hegre & Milante, 2008: 458). Recently, scholars have highlighted the role economic recovery plays in stabilizing post-conflict societies (for example Flores & Nooruddin, 2009: 3). Simply put, they claim that economic recovery reduces the risk of post-conflict collapse. In particular, the widely influential article by Collier, Hoeffler & Söderbom (2008), hereafter CHS, promotes the view that economic recovery, especially if supported by external military presence, offers a favorable route to creating long-lasting peace. Using the Correlates of War (COW) database, they estimate a series of piece-wise exponential survival models (with two time periods). They find that ‘faster growth directly and significantly reduces risk in the year which it occurs’ (Collier, Hoeffler & Söderbom, 2008: 469). Their estimate of the effect of economic growth is clear and precise. CHS find that the decade-risk of post-conflict collapse falls from 42.1% to 26.9% if the economy grows at a rate of 10% rather than remaining stagnant (all other variables held at their mean). This is a very encouraging finding that has been picked up by international policy-makers such as USAID (2009).

When findings, as in the case of CHS, are used to formulate clear and broad policy advice, robustness tests become particularly relevant. If results are sensitive to small changes in the model specification or the data being used, it is essential that the fragility is detected and well communicated. Basing policy decisions on fragile findings is risky, and can at worst backfire. Despite the fact that economic growth is often assumed to be a potential post-conflict peace stabilizer, CHS (2008) is, to our knowledge, the only study where this is directly tested. Moreover, relevant theory indicates that the relationship is not necessarily as straightforward as suggested by CHS. Collier & Hoeffler (2004) includes growth as a proxy for new income opportunities, which implies a negative relationship between growth and post-conflict risks. Fearon (2008) and Kocher (2004), on the other hand, argue that greater wealth will make war more attractive, as the potential gains from winning increases.[1] If this is correct, growth will not necessarily operate solely as a post-conflict peace stabilizer. In this article we test the robustness of CHS's main findings by first running their model on their own dataset, and then by changing some of the model specifications. These operations did not reveal any discrepancies. When we changed the dataset from COW to UCDP/PRIO Armed Conflict Database (ACD), however, we were unable to replicate their results. We then tested what caused the discrepancy between their results and ours, and found that more than half of the difference is caused by different start and end years.

Changing the model specifications

We started the analysis by running CHS's model on their own dataset, and were able to replicate their results. We adjusted the standard error to account for possible dependency between the observations (robust standard errors). When this was accounted for, the level of economic development was no longer significant at the 10% level. As CHS include a large battery of controls, some with a high proportion of missing values, we excluded all controls in column 3. To account for missing data, CHS employs a modified zero-order regression strategy, whereby all missing values are coded as zero, and distinguished from non-missing zeros with a dummy variable. This strategy allows them to keep all observations, but may hide some of the variability in the real-world data (Honaker & King, 2010). To reduce bias and uncertainty caused by missing data, while keeping the replication within the spirit of CHS, we simplified the model so that column 3 includes only level of economic development, economic growth and the presence of, and expenditure on, UN peacekeepers. In this reduced model, the effects of level of economic development and economic growth are comparable to the original results, and somewhat stronger, while the effects of UN presence and expenditure are somewhat weaker, but still significant at the 10% level. Hence, the key results of CHS are not driven by the inclusion of these control variables. This finding allowed us to use only the simplified model for the remainder of the robustness tests, reducing the bias due to missing data and making it easier to identify the possible sources of differences in results.

[1] This theory is applied on civil war risks, and not post-conflict risks, but should be applicable here as well.

[Table I about here]

Replicating using a rival data source

CHS use the Correlates of War (COW) dataset to identify post-conflict peace spells, and define post-conflict peace by the absence of war. COW initially defined civil war as military action that causes at least 1,000 battle-deaths per year (Small & Singer, 1982: 213-215). For resistance to be considered efficient, the stronger side must suffer at least 5% of the number of causalities suffered by the weaker side. Unfortunately, as Gleditsch (2004: 234) and Sambanis (2004: 817) point out, it is no longer clear whether COW's 1,000 battle-deaths threshold continues to be an annual criterion, or whether it has become cumulative. While they disagree on which of the two makes up the major part of the dataset, they agree that it is unlikely that the same coding criterion has been used consistently throughout the various COW dataset updates. This is problematic, as it lowers the reliability and consequentially the credibility of CHS's findings. In the remainder of this article, we will test to what extent the encouraging results reported by CHS depend on peculiarities related to the coding of the COW dataset. To facilitate the comparison, we rely on the ACD database for which the coding criteria are consistent and well documented. We used GDP data from the World Bank (2006).

The ACD database defines armed conflict as 'a contested incompatibility that concerns government or territory or both where the use of armed force between two parties results in at least 25 battle-related deaths. Of these two parties, at least one is the government of a state' (Gleditsch et al., 2002: 618-619). One advantage of using ACD is that it includes the less intense conflicts, the ones that never reach 1,000 battle-deaths during any of the conflict years. Consequently, by using ACD, we could construct several post-conflict datasets based on COW using either an annual or a cumulative criterion, and run the analysis on these. In order to develop a dataset that resembles CHS, we considered four alternative battle-deaths criteria. Criteria 1 and 2 are based on an annual threshold. Criterion 1 is an annual criterion in its strictest form. Criterion 2 avoids the problem of coding a single-year dip in battle-deaths, e.g. from 1,000 to 800 and back to 1,000, as a post-conflict period. Criteria 3 and 4 are based on a cumulative threshold. They differ with respect to the lenience of the annual number of battle-deaths required, and are set to 100 and 500. These choices are of course somewhat arbitrary, but the particular values are not consequential for the robustness of the results (see the online version where we have added 25, 250 and 750).

Criterion 1: The 1,000 battle-death threshold is an annual criterion. The conflict starts the first year with more than 1,000 battle-deaths and ends as soon as there are fewer than 1,000 battle-deaths during one year.

Criterion 2: The 1,000 battle-deaths criterion continues to be annual. The war starts the first year with more than 1,000 battle-deaths, but in order to end there must be at least two subsequent years with fewer than 1,000 battle-deaths.

Criterion 3: The 1,000 battle-death criterion is a cumulative criterion. There must be 1,000 battle-deaths during consecutive active conflict years. A conflict year will be regarded as active if it includes more than 100 battle-deaths.

Criterion 4: The 1,000 battle-death threshold is cumulative, but stricter than number 3. There must be 1,000 battle-deaths during subsequent active conflict years. A conflict year will be regarded as active if it triggers more than 500 battle-deaths.

In order to construct these datasets, we needed annual data on battle deaths. Unfortunately, ACD does not include this information. Hence we used Lacina & Gleditsch (2005). They provide three estimates for battle-deaths: high, low, and best.[2] Which of these are used affects both the inclusion of post-conflict periods and their duration. In general, the high estimate yields shorter post-conflict peace episodes, and a greater risk that the post-conflict peace period ends. Applying the three estimates on the four criteria, we create 12 post-conflict datasets.

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[2] There were 34, 81 and 34 missing values for the low, best and high estimate. We have not excluded the missing values, but replaced them with 25 and 500.

Replication of the analysis with data from the Armed Conflict Database

Table II reports the results from the piecewise exponential model on the 12 post-conflict datasets with robust standard errors. The effect of per capita income is negative in all models, and significant in 9 out of 12. By contrast, economic growth shows no sign of reducing the risk of post-conflict peace collapse in any of the models. In fact, in nine of the specifications, the coefficient is positive. It is significant at the 1% level in one model, the 5% level in a second, and the 10% level in a third. In the remaining three specifications, the direction of the coefficient is negative (in line with CHS), but not significant. This should caution against drawing firm inferences regarding the effect of economic growth as a post-conflict peace generator. Moving on to the effect of international intervention, we fail to find a negative effect. For both the mere presence of UN forces and UN Expenditures, the coefficient is positive in six of the models and negative in six. It is not significant in any of them.

[Table II about here]

The discrepancy in the results raises the question as to why this is so. As it is the effect of economic growth that has been most readily picked up by policy-makers, the remainder of this article is devoted to accounting for the differences in these results. We identify which of our twelve datasets resembles CHS's dataset the most, we conduct a pair-wise comparison between CHS and each of the twelve datasets based on the following criteria: (1) number of post-conflict peace episodes; (2) number of peace collapses; (3) number of countries included; (4) share of countries that are included in both datasets; (5) share of country-years included in both datasets.[3] Criterion 1, using the high battle-death estimate, generates the most similar dataset. We investigate six possible sources for the discrepancy in the result: (1) the other independent variables, (2) precision in the coding of start and end points (year vs. date), (3) influential observations, (4) inconsistencies between COW and CHS, (5) the conflicts included, and finally, (6) coding of start and end points. We maximize resemblance between our data and CHS in a stepwise manner

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[3] Table A.III in the replication material contains lists of the post conflict peace spells included in the different operationalizations of the dependent variable.

and aspects altered at an early stage are carried forward in the subsequent steps of the analysis.[4] A summary of the results are presented in Figure 1.[5]

[Figure 1 about here]

The figure further highlights our key argument: The effect of economic growth is not robust to changes in the underlying data. Even when we start with the operationalization that generates the most similar dataset of CHS, we do not get similar results. As we are interested in finding the causes of these differences, we compared the estimates of the effect of economic growth as we altered the various aspects mentioned above. The filled circle shows the point-estimate, the line indicates the 95% confidence interval around the estimate. Close to half of the difference between the results from the ACD criterion 1 dataset and the CHS dataset is caused by differences in which conflicts are included. The rest of the difference is due to the coding of start and end dates. The sensitivity of the results to the inclusion of specific conflicts and the coding of their start and end dates demonstrates the importance of clear and consistent coding criteria and critical evaluation of data-sources used in quantitative research on post-conflict societies. The uncertainty regarding the coding decisions made during the development of the COW dataset might raise concerns about the validity of findings based on these data.

Conclusion

Collier, Hoeffler & Söderbom (2008) argue that economic growth reduces the risk of post-conflict peace collapse. This result is very encouraging and has inspired policy-makers to focus on creating economic growth in post-conflict societies as a tool to securing the peace. The purpose of our study is to investigate the robustness of these results using alternative data-sources. More specifically, where CHS uses data from the Correlates of War Database, we relied on data from the UCDP/PRIO Armed Conflict Database. When we do this, we do not get similar results, regardless of the coding criteria

[4] This is not the case of the test of influential outliers as the exclusion of these did not alter the results, nor did they alter the results when taken out of the subsequent analyses.

[5] The replication material posted at http://prio.no/Research-and-Publications/Journal-of-Peace-Research/Replication-Data/ will reproduce the full results.

used to define post-conflict peace spells. At least when relying on this database, the effect of economic growth and UN involvement is less clear than presented in CHS. We are far from able to reach as clear-cut results with our rival operationalization of the dependent variable. The effect of economic growth seems to depend on the particular coding of start and end dates of conflicts. The main difference results from which conflicts are included and on the coding of start and end dates of these conflicts. Policy makers should hence be aware that the effect of economic growth on the risk of post-conflict peace collapse may be ambiguous.

Replication Data

The codes generating the post-conflict datasets and models, datasets and the online version are available at http://www.prio.no/Research-and-Publications/Journal-of-Peace-Research/Replication-Data/

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Biodata

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Table I. Determinants of post-conflict peace collapse 1960–2002: COW data

	(1) (st.errors)	(2) (robust st. errors)	(3) (robust st.errors)
Per capita income	-0.43†	-0.43	-0.42^{*}
	(-1.72)	(-1.53)	(-2.20)
Economic growth	-3.55^{*}	-3.55^{**}	-3.56^{***}
	(-2.21)	(-2.76)	(-4.31)
Democracy	1.23^{*}	1.23^{*}	
	(2.43)	(2.54)	
Democracy missing	1.75^{**}	1.75^{**}	
	(2.68)	(2.64)	
Regional autonomy	-1.56	-1.56	
	(-1.43)	(-1.19)	
Regional autonomy missing	-0.25	-0.25	
	(-0.50)	(-0.49)	
Election shift	-0.71^{*}	-0.71†	
	(-1.97)	(-1.72)	
ln diaspora	-0.33^{**}	-0.33^{**}	
	(-2.82)	(-2.61)	
Diaspora missing	3.46^{*}	3.46^{**}	
	(2.46)	(2.78)	
Ethnic diversity	-1.04	-1.04	
	(-1.24)	(-1.37)	
Ethnicity missing	-15.20	-15.20^{***}	
	(-0.01)	(-13.09)	
ln UN peacekeeping expenditure	-0.41^{*}	-0.41^{**}	-0.34†
	(-2.38)	(-2.75)	(-1.94)
No UN PKO	-3.71^{*}	-3.71^{*}	-3.05†
	(-2.16)	(-2.23)	(-1.67)
UN data missing	-3.89^{*}	-3.89^{*}	-3.56^{*}
	(-2.09)	(-2.21)	(-1.99)
After 4 years	-0.48	-0.48	-0.71†
	(-1.12)	(-1.18)	(-1.77)
Constant	-5.76^{*}	-5.76†	-2.74
	(-2.00)	(-1.79)	(-1.29)
N	825	825	825

†<0.10 $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

Table II. Determinants of post-conflict peace collapse, 1960-2002: ACD data, with 4 different coding criteria

	1	***1***	***1***	***2***	***2***	***2***	***3***	***3***	***3***	***4***	***4***	***4***
	Low	***Best***	***High***	***Low***	***Best***	***High***	***Low***	***Best***	***High***	***Low***	***Best***	***High***
Per capita income	-0.41*	-0.50**	-0.62**	-0.22	-0.41*	-0.50*	-0.44†	-0.60*	-0.69**	-0.42†	-0.60*	-0.70**
	(-2.11)	(-2.70)	(-2.98)	(-1.15)	(-2.09)	(-2.51)	(-1.74)	(-2.27)	(-2.80)	(-1.88)	(-2.57)	(-3.06)
Economic growth	0.10	0.66	2.42	3.00**	4.16**	2.61	-1.30	0.13	-0.92	-1.76	2.02†	0.56
	(0.04	(0.19)	(1.41)	(2.62)	(2.85)	(1.45)	(-0.72)	(0.07)	(-0.43)	(-1.03)	(1.68)	(0.30)
ln UN expenditures	-0.25	0.09	0.12	-0.26	0.12	0.13	-0.29	-0.28	-0.27	-0.22	0.16	0.15
	(-0.71)	(0.30)	(0.39)	(-0.80)	(0.38)	(0.41)	(-1.17)	(-1.18)	(-1.15)	(-0.60)	(0.47)	(0.44)
No UN PKO	-1.67	1.81	2.14	-2.08	2.03	2.08	-2.42	-2.75	-2.47	-1.39	2.64	2.34
	(-0.48)	(0.52)	(0.63)	(-0.64)	(0.58)	(0.59)	(-1.00)	(-1.17)	(-1.06)	(-0.38)	(0.69)	(0.62)
UN missing	-1.31	2.23	2.63	-1.96	2.29	2.56	-2.38	-2.47	-2.12	-1.36	2.76	2.75
	(-0.38)	(0.65)	(0.78)	(-0.61)	(0.67)	(0.74)	(-0.98)	(-1.06)	(-0.91)	(-0.37)	(0.73)	(0.74)
After 4 years	-1.47***	-1.34***	-0.83*	-1.18*	-1.07*	-0.66	-1.61***	-0.78†	-0.54	-0.99*	-0.75†	-0.30
	(-3.42)	(-3.43)	(-2.33)	(-2.43)	(-2.45)	(-1.73)	(-3.36)	(-1.67)	(-1.42)	(-2.22)	(-1.73)	(-0.84)
Constant	1.58	-1.42	-1.28	0.54	-2.52	-2.09	2.65	3.31	3.70	1.23	-1.91	-1.18
	(0.42)	(-0.38)	(-0.35)	(0.16)	(-0.71)	(-0.57)	(0.89)	(1.21)	(1.36)	(0.31)	(-0.49)	(-0.30)
N	676	771	762	664	759	754	594	773	772	665	812	812

t statistics in parentheses †<0.10, **p* < 0.05, ** *p* < 0.01, *** p< 0.001

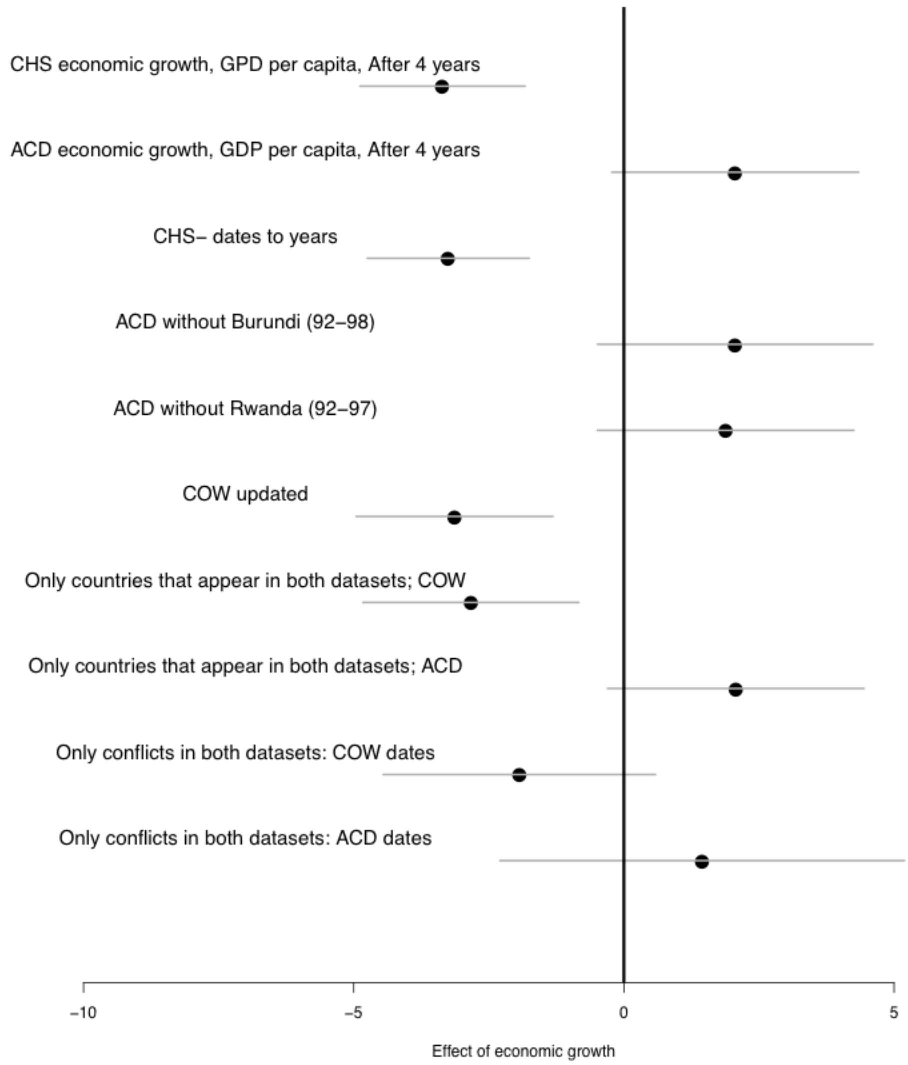

Figure 1. Effect of economic growth on post-conflict peace collapse (1960 – 2002)

The filled circle indicates the point estimate of the coefficient, while the line indicates the 95 % confidence interval.

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